The Effects of School Finance Reforms on Teacher Salary and Turnover: Evidence from National Data

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Abstract

In recent decades, parallel literatures have documented the magnitudes and effects of teacher turnover and the impact of state school finance reforms (SFRs). In this paper, we examine SFRs as possible mechanisms to improve teacher salary, turnover, and job satisfaction by using nationally representative data from 2000 to 2016 and leveraging variation in SFR timing. We find that SFRs increased teacher salaries by approximately $4,000 and reduced teacher turnover by three percentage points, on average, though gains in both outcome measures took up to a decade to emerge. We observe larger salary gains among teachers in high-poverty and high-minority school contexts, though declines in their corresponding turnover behaviors were similar to average findings.

Keywords: School Finance Reform, Teacher Salary, Teacher Turnover

Introduction

A longstanding body of literature has documented the "revolving door" of teacher turnover in P-12 public schools (Ingersoll, 2001). Much recent evidence has established the deleterious effect of teacher turnover on student outcomes, particularly in schools which serve large shares of economically disadvantaged and minority students (Hanushek et al., 2016; Ronfeldt et al., 2013; Steele et al., 2015). For example, teacher turnover is 50% higher in high-poverty school contexts (Ronfeldt et al., 2013) where it is especially pronounced in certain critical subject areas like STEM (Nguyen & Redding, 2018). Though a range of policy mechanisms have been attempted to reduce teacher turnover, especially through financial interventions, turnover remains a critical issue of policy import. The improved understanding of the policies which may improve (or fail to improve) teacher turnover, therefore, may guide future policy efforts and the allocation of scarce educational resources.

Though teacher salaries represent the largest P-12 school expenditure (Ingersoll et al., 2018), low salaries long have been considered an important determinant of turnover decisions (i.e., Loeb et al., 2005). Many of the policies crafted to reduce teacher turnover have centered on either targeted or broad teacher salary interventions. Targeted interventions often focus on specific teaching subjects like STEM or on high-poverty school contexts (Clotfelter et al., 2008), pay for performance (Yuan et al., 2013), or on teacher recruitment (Fowler, 2003; Liu et al., 2004). Many school districts, however, may be constrained in their abilities to apply such interventions by limited capacities to raise additional revenues. Districts may also allocate existing revenues according to preferences for other education expenses including class size reduction, support staff, or non-instructional expenditures.

In recent decades, state school finance reforms (SFRs) have injected substantial investments in P-12 schools in attempts to decouple local property wealth and school funding levels, a relationship which rendered many districts ill-equipped to increase school resources through local revenue sources (Reschovsky, 1994). Much of this spending has been directed to broad-based increases in teacher salaries and teacher hiring (Brunner et al., 2019). SFRs, particularly those precipitated by the court-ordered overturning of existing state funding systems, generated substantial improvements in cross-district spending inequality (Card & Payne, 2002). Indeed, the *Rose v. Council for Better Education* 1989 Kentucky ruling ushered in the so-called "adequacy era," after which courts directed adequacy-based financing mandates to aid districts serving the largest shares of low-income students (e.g., Candelaria & Shores, 2019; Sims, 2011). In turn, SFRs produced large positive effects on student achievement and long-term outcomes, particularly for economically disadvantaged student populations, stemming from increased state funding to low-income school districts (Candelaria & Shores, 2019; Jackson et al., 2016; Lafortune et al., 2018). Though much of the increases in spending were directed to teacher salaries, these increases may not have been distributed uniformly across states and districts.

One source of potential spending heterogeneity lies in teacher union characteristics. Researchers have considered the relationship between teacher quality, salary, and union status. For example, Figlio (2002) found that higher teacher salaries attract higher-qualified teachers, but only in non-union school districts. In the context of SFRs, recent work has extended this focus to investigate teacher outcomes including impacts on salary and new teacher hiring within contexts of varying prevailing teacher union strength (Brunner et al., 2019). Specifically, Brunner and colleagues (2019) find that union strength is a key determinant of the allocation of SFRs to teacher salaries, new teacher hiring, and local tax relief. Though SFRs may direct large

increases in state aid to low-income districts, their effects on teachers and students may be blunted through the allocation of aid to non-educational goods.

In this paper, we seek to connect the heretofore disparate literatures pertaining to school finance reforms, teacher turnover, and teacher union strength, including an examination of potentially important heterogeneity in education contexts. Namely, do school finance reforms affect teacher turnover, and, if so, do effects vary by school characteristics (i.e., shares of economically disadvantaged students and minority students), union strength, and teacher characteristics (i.e., experience)? Utilizing nationally representative, repeated cross-sectional data from five School and Staffing and National Teacher and Principal Survey waves from 1999-2000 to 2015-16, we leverage variation in the timing of SFRs (Jackson et al., 2016; Lafortune et al., 2018) to estimate the effects of SFRs on teacher salaries, turnover, and intent to leave a teaching position. Within our event study and difference-in-differences frameworks, we examine the intersection of SFRs and teacher union strength (Brunner et al., 2019) while accounting for key contemporaneous teacher accountability policy reforms (Kraft et al., 2020). Further, we evaluate heterogeneity in response to SFRs along several dimensions including school poverty and racial composition, and teacher subject matter and experience.

Our analyses indicate SFRs increased teacher salaries, with average gains of approximately $4,000 and larger gains concentrated for teachers in low-income and high-minority schools, though each took up to a decade to emerge following state reforms. We estimate positive effects for teachers in other contexts including high-income and low-minority schools, findings typically smaller or less precisely estimated. Highly experienced teachers may have witnessed gains at the expense of novice teachers. Consistent with findings documented by Brunner et al. (2019), teacher salary gains were concentrated in district contexts of high teacher

union strength. Similar to salary estimates, we observe reductions in teacher turnover and teacher-reported intentions to leave a teaching position, but only a decade following state reforms, corresponding with the timing of increases to teacher salaries. Our estimates suggest that though SFR-driven gains in teacher salaries were concentrated in the school contexts of their greatest need, they took significant time to develop following state reforms, as did meaningful changes to teacher turnover behaviors. These findings may bear on future policy interventions related to teacher turnover in heterogeneous state, district, and school contexts.

Related Literature

School Finance Reforms and Teacher Unions

Over the past five decades, nearly every state in the country revised its school funding formula, many at the direction of state supreme courts (Jackson, 2018) and the remainder at the behest of state legislatures. Many of these SFRs were initiated with the expressed intent to more equitably distribute state resources to low-income school districts by attempting to decouple local school funding from local property wealth. In recent years, an abundance of research has documented the effects of these reforms on critical student outcomes. In short, SFRs generated large positive treatment effects across a range of student outcomes including academic achievement, educational attainment, and long-term income, particularly among students in low-income districts (i.e., Candelaria & Shores, 2019; Jackson et al., 2016; Lafortune et al., 2018). SFR-driven funding changes also modestly reduced racial funding gaps between white and non-white student populations (Rothbart, 2020).

Despite their strong average effects on students, the literature indicates SFRs did not generate uniform impacts on teacher outcomes. Much of this heterogeneity relates to the degree to which SFR-generated state aid flowed to schools. State aid allocated to a district through an

SFR may augment school budgets, provide local tax relief, or provide some combination of the two effects. In the economics literature, increases in intergovernmental aid resulting in additional resource provision demonstrate a "flypaper effect," whereby intergovernmental aid sticks to its intended destination; increases in aid that provide tax relief constitute a "crowding-out effect," whereby intergovernmental aid leads to a decline in prevailing local funding (Hines & Thaler, 1995). Several studies present empirical estimates of flypaper and crowding out effects stemming from increases in state education aid (e.g., Hoxby, 2001; Papke, 2005) and federal aid (e.g., Cascio et al., 2013; Gordon, 2004). Card and Payne (2002) find that increases to state aid in the wake of 1980s judicial orders led to district spending increases of 53 to 66 cents per additional dollar of state aid. Conversely, other recent state-specific studies have found increases in state aid were nearly entirely crowded out by commensurate reductions in local revenue (i.e., Lutz, 2010; Steinberg et al., 2016). These studies demonstrate the range of funding outcomes which may result from SFRs.

In light of heterogeneous local funding outcomes, an emerging line of research has examined the effects of state funding reforms on policies relevant to teacher labor including salaries and hiring practices. SFR aid which ultimately reaches schools may be spent in different ways, particularly as it relates to teachers. Brunner and colleagues (2019) examine a critical element of this heterogeneity in spending, the intersection of SFRs and local teacher union practices. They find SFRs generated state aid increases of $500 to $1,000 per pupil, driven by increases of up to $1,500 per pupil in districts in the lowest income tercile. Allocation of SFR-generated state aid, however, varied starkly across union environments as measured by state-specific prevailing teacher union strength. At the mean level of teacher union power, 64 cents of every dollar of SFR aid flowed to school districts. This average effect masked considerable

underlying variation. In state contexts of the strongest teacher unions (the 90th percentile), nearly every dollar of SFR state aid flowed to schools; 80% of these funding increases augmented the compensation of existing teachers. A one standard deviation increase in union power increased SFR allocation to teacher compensation by 51 cents beyond the statistically insignificant 32-cent average estimate. Conversely, in state contexts of weak teacher union strength (the 10th percentile), districts spent 80% of additional state aid on local tax relief and only funded small increases to support new teacher hires; student-teacher ratios remained unchanged. With these estimates in mind, the intersection of teacher union power and SFRs may warrant closer scrutiny with respect to important teacher outcomes.

Studies in individual state settings shed additional light on SFR spending heterogeneity. Funding reforms in Kentucky in the early 1990s led to short-term increases in teacher salary (Streams et al., 2011). Over the medium- to long-term, however, funding increases were spent on modest reductions to class size and on non-teaching staff instead of salary increases to experienced teachers. Following a 2013 reform in California, $1.1 billion in new yearly funding in the Los Angeles Unified School District (LAUSD) led to the increased use of novice and probationary teachers. California's reform only reduced LAUSD class sizes modestly, while achievement gaps for low-income and English language-learner students actually increased (Lee & Fuller, 2020).

In addition to their contributions to the SFR literature, Brunner and colleagues (2019) contribute to a mixed body of evidence concerning the effects of teacher unions on school outcomes. Attributes of teacher union contract restrictiveness, timing of union contracts, and other measures of union power may influence the allocation of school resources to teacher pay. Lovenheim (2009) finds teacher unions increase teacher employment but do not affect teacher

pay. Conversely, Cook et al. (2020) attribute the allocation of new tax revenues in large part to the timing of collective bargaining agreements. Examining Ohio tax elections, they find bargaining negotiations made immediately after tax increases resulted in higher teacher salaries, whereas those made before tax increases led to increased expenditures on support services and on new teachers. Only the latter resulted in improved student achievement. These estimates would appear to support prior findings by Hoxby (1996) that the rent-seeking attributes of teacher unions may exert negative impacts on student achievement.

Additional research documents the relationship between union contract flexibility and spending outcomes (Strunk, 2011) and the effect of bargaining laws on student and teacher outcomes (Brunner & Squires, 2013; Lovenheim & Willén, 2019). Whereas union contract flexibility may not exert any influence on teacher salaries (Strunk, 2011), state laws mandating collective bargaining reduce long-term male student earnings and labor market participation (Lovenheim & Willén, 2019). Further, in states which mandate bargaining, there exists a positive relationship between more powerful unions and both starting salaries and returns to experience, and a negative relationship with teacher-student ratios, relationships which reverse in states which prohibit bargaining (Brunner & Squires, 2013).

Teacher Mobility

In addition to the previous research demonstrating the importance of teacher quality to improving student's academic achievement (Kane & Staiger, 2008), non-cognitive abilities (Jackson, 2018), and long-term outcomes (Chetty et al., 2014), a substantial literature has concentrated on the factors related to keeping teachers in the classroom (Nguyen et al., 2020). Teacher attrition is especially concerning in high-minority schools and high poverty schools, which can least afford the cost of bringing in new teachers (Steele et al., 2015; Nguyen &

Redding, 2018). This literature is large enough that several research teams have aggregated the empirical evidence into systematic reviews and meta-analyses (Guarino et al., 2006; Nguyen et al., 2020). To date, however, no previous work has considered how the external shocks of SFRs affect teacher attrition. Despite this gap in the literature, much of the previous work on teacher attrition informs a theory of how SFRs mechanically would impact attrition rates, both for mobility between schools and exiting the profession entirely.

Considering teacher mobility from an economic lens, incentives which encourage a teacher to stay or leave incorporate both supply and demand dimensions (Grissom et al., 2015). Teachers choose to supply their labor with expectations about the benefits that they will receive, including salary, non-salary monetary benefits (e.g., health insurance), and non-pecuniary benefits, like overall job satisfaction. Similarly, the decisions made by school leaders to demand teacher labor also are multifaceted, including the types of resources that will be allocated directly to teachers (salary and benefits), resources to benefit job performance and satisfaction (e.g., teaching materials, professional development, smaller class sizes, support staff, and the number of class preparations), and how decisions are made to retain teachers, such as tenure, evaluation, or counseling teachers out of the profession. While some of these pieces of the labor market decision are based on non-monetary personal interactions, such as job satisfaction resulting from a positive principal and teacher fit (Bartanen & Grissom, 2019), or on factors beyond teacher and school leader control, such as student demographics (Lankford et al., 2002; Newton et al., 2018), many factors that impact a teacher's and school leader's labor decision are at least in part impacted by financial considerations. An implemented SFR could serve to raise the amount of financial resources that are used by school leaders to benefit teachers.

In our research, we consider the previous work that has shown how different inputs into these decisions have impacted teacher attrition. The most widely examined portion of this literature includes studies that have considered how compensation levels for teachers affect attrition rates. In studies that have considered the association of teacher salary and turnover, the overall finding is that an increase in salary has a small, yet significant effect on lowering teacher attrition rates (Nguyen et al., 2020). Across these different studies, a more nuanced picture emerges about which types of teachers benefit the most from salary increases. For example, a salary increase relative to a nearby district does more to lower attrition rates than higher salaries for all teachers, as higher salaries attract teachers who are willing to switch schools, but do not necessarily reduce the number of teachers leaving the profession altogether (Hanushek et al., 2004; Imazeki, 2005). From a comparative perspective, the movement of teachers between schools can have a sorting effect, as high-quality teachers tend to respond more to the racial or socioeconomic characteristics of a school, rather than to higher salaries, though incentive programs can induce movement (Clotfelter et al., 2008; Clotfelter et al., 2011). So, while higher salaries can reduce teacher turnover, the price of a large reduction in teacher turnover may be quite costly and might be only a secondary consideration informing teacher decisions.

While salary increases are a broad tool that would be experienced by all teachers, another monetary intervention is the use of merit-pay or other types of teacher bonuses. Merit-pay systems, output-based approaches that reward teachers through positive evaluations, have been shown to lower teacher attrition rates (Nguyen et al., 2020), with even larger reductions found in hard-to-staff schools (Hough, 2012). On the other hand, bonus programs represent an input-based policy that rewards teachers who are willing to work in hard-to-staff positions, like math

or special education, with loan forgiveness or monetary bonuses; this type of program has shown potential for large, significant reductions in teacher attrition rates (Feng & Sass, 2018).

In addition to their benefit to teachers through direct payments or direct benefits that lead to lower levels of teacher attrition, school leaders may opt to use additional funding for services that complement or improve teachers' abilities in the classroom. First, class size reductions may decrease rates of teacher attrition (Isenberg, 2010). In examples where class size reduction interventions were not analyzed directly, smaller class sizes were associated with reduced teacher turnover in some circumstances (Djonko-Moore, 2016), though not in others (Nguyen et al., 2020). Second, investments in the professional development of teachers could lead to lower levels of teacher attrition. Previous research found evidence to this effect specifically for math and science teachers (Ingersoll & May, 2012; Allen & Sims, 2017). Third, direct interventions into the classroom practice through investments in teacher mentoring have been associated with lowering teacher mobility (Helms-Lorenz et al., 2016; Ronfeldt & McQueen, 2017), as senior teachers aid novice teachers during their early years when the likelihood of teacher turnover is highest. Across all of these interventions, lower performing teachers in terms of academic student achievement are the most likely to leave the profession (Feng & Sass, 2017; Goldhaber et al., 2011), so developing and aiding teachers can serve the dual purpose of bettering student outcomes and lowering teacher mobility.

Finally, school accountability reforms may serve as a driver of teacher turnover behavior. Increased accountability often leads to increased turnover, particularly in low-performing schools (Clotfelter et al., 2004; Ingersoll et al., 2016). In the post-No Child Left Behind era, every state passed some type of change to its teacher accountability policies, reforms contemporaneous to many SFRs. These changes included reforms to teacher evaluation, tenure,

collective bargaining, the payment of union dues, and teacher examinations pertaining to basic skills, core content, and pedagogical knowledge. In addition, many states won Race to the Top Grants and implemented Common Core State Standards, programs which required changes to accountability practices. Kraft and colleagues (2020) examine variation in the timing of the passage of these state policies to estimate the effect of accountability practices, many including high-stakes consequences, on the supply of teachers and on teacher job satisfaction. They find that accountability reforms reduced the supply of new teachers while harming teacher perceptions of job security and autonomy.

While much has been documented related to SFRs and teacher turnover, the relationship between the two remains a crucial area of study. SFRs generated large gains in spending in low-income school districts, though spending increases varied by union contexts. Additional exploration of the effects of finance reforms on teacher salaries, especially in hard-to-staff schools and hard-to-staff subject areas, may inform district and state labor practices. Further, the relationship between finance reforms and teacher mobility presents an important area to which little attention has been devoted, particularly contextualized within the expanding literature concerning teacher unions.

Data & Empirical Methods

This paper uses a unique dataset from 2000 to 2016 combining data from the National Center for Education Statistics (NCES), the Bureau of Labor Statistics (BLS), the U.S. Census Bureau, and documented lists of state policy changes and financial reforms. In particular, from NCES we use the Schools and Staffing Survey (SASS) and its supplement, the Teacher Follow-Up Survey (TFS)[1], as well as the new iteration of the SASS, the National Teacher and Principal

[1] We use the TFS principal report on teachers' statuses the year following the baseline survey rather than the current and former teacher questionnaires. This allows us to generate the mobility status for every teacher in the SASS.

Survey (NTPS). More specifically, we use the 1999-2000, 2003-2004, 2007-2008, 2011-2012, and 2015-2016 waves from SASS and NTPS. Utilizing a stratified probability sampling design based on the Common Core of Data, the SASS and NTPS consist of nationally representative samples of teachers and schools for public schools in the United States.[2] They include a comprehensive set of teacher and school characteristics and, critically important for our analyses, teacher salary (i.e., the teacher's base salary for the entire school year) and their intention to leave teaching. We also observe teacher reports of whether they are provided adequate school materials and teacher salary satisfaction. From the TFS, we observe the teachers' actual turnover behavior (whether they stay in their current school, switch school, or leave the profession). We note the NTPS 2015-2016 survey wave does not include turnover behavior. In short, we have measures of teacher salary and teacher intentions from 2000 to 2016 and turnover behaviors from 2000 to 2012.

Measures of School Finance Reforms, Union Power, Salary, Intentions, and Turnover

We obtained a comprehensive list of SFRs from Jackson et al. (2016), Lafortune et al. (2018), and Brunner et al. (2019). Our primary coding is based on the coding developed by Lafortune and colleagues.[3] We leverage SFRs for two reasons. First, as discussed previously, SFRs led to large increases in school spending, spending which often was focused on teachers (i.e., Brunner et al., 2019). Second, the plausibly exogenous timing of SFRs (Jackson et al., 2016; Lafortune et al., 2018) avails an identification strategy whereby effects linked to spending

[2] We note that the SASS's stratified probability sampling design allows the SASS to be representative at the state and national level with the appropriate sampling weights.

[3] Lafortune et al. (2018) include both court-ordered and legislative SFRs in their analyses. Importantly, a number of states passed multiple SFRs, many in close temporal succession, and other states passed SFRs that generated little, if any, meaningful change in state funding. Lafortune et al. (2018) employ Monte Carlo simulations to identify "the most consequential reform" in each of these states (p. 8); these simulations test for the possibility of multiple significant reforms, possibly due to "political and legal maneuvering with little consequence for school spending or student achievement" (Online Appendix D, p. vii). We rely on their identification of consequential SFRs and their respective timing.

increases may be interpreted in a causal manner. This empirical approach allows us to identify and further analyze findings regarding heterogeneous SFR spending on teacher salaries and to document the heretofore unexplored relationship between SFRs and teacher turnover. Also, as demonstrated by Lafortune et al. (2018), we note many states passed several SFRs over the considered analytic time period. Because many of these SFRs had little to no impact on state education finances, we consider the SFR that had the highest impact on state education spending as the relevant intervention.[4] As such, though SFRs were not identical in different states, we focus on the SFRs which produced meaningful changes to school spending regimes across diverse contexts.

Our primary measure of union power is based on the Fordham Institute index that combines administrative and original survey data across five important areas of union power: 1) resources and membership, 2) involvement in politics, 3) scope of bargaining, 4) state policies, and 5) perceived influence (Winkler et al., 2012). Although this measure of union strength does not vary longitudinally in our dataset, the included measure draws on data from close to the time that many of the analyzed SFRs were first implemented.

Our dependent variables of interest include teacher salary, teacher intentions to leave teaching, and turnover behaviors. Teacher salary is the teacher's report of their annual salary. Teacher's intent to leave teaching is a binary variable where a 1 indicates that teachers reported they plan to leave teaching as soon as possible and 0 otherwise, including options such as "staying until eligible for retirement benefits" (see Appendix Table A1). We categorize teacher turnover behaviors as movers and stayers. Stayers are teachers who remain in the same school as

[4] Indeed, per Lafortune et al. (2018), over our analytic time period some states passed inconsequential reforms without passing a consequential reform (i.e., Arizona, Connecticut, Idaho, Missouri, New Jersey, Ohio, Oregon, South Carolina, Tennessee, Texas) while others passed consequential and inconsequential reforms (i.e., Arkansas, Maryland, Montana, New Hampshire, New York, North Carolina, Washington).

in the baseline year and movers are those who do not remain in the same school. In some analyses, we separate movers into leavers and switchers where switchers are teachers who switched to a new school and leavers are teachers who left the teaching profession.

State, School, and Teacher Controls

Since the Fordham index was created in 2012, there are two primary concerns: 1) the index may be endogenous to the SFR, and 2) it is a static measure that does not change over time. To address the first concern of endogeneity, we supplement our analysis with measures of state mandated collective bargaining (CB) and state right-to-work status (RTW) (Kraft et al., 2020). Additionally, we also include a set of plausibly exogenous control variables to account for state-specific political and educational conditions such as measures of the political ideology of the state senate and state house of representatives, evaluation reform implementation, elimination of teacher tenure, implementing Common Core State Standards, requiring teacher candidates to take basic skills licensure tests or content area licensure tests, and winning a Race-to-the-Top grant (Caughey & Warshaw, 2018; Kraft et al., 2020).

Since our primary outcomes vary with school and teacher characteristics, we also account for this variation by employing a comprehensive set of school and teacher controls. For teacher characteristics, we include the teacher's gender, race/ethnicity, age, whether they have graduate degrees, the selectivity of their undergraduate institution, having a standard certification, and union membership. With regards to school characteristics, we control for the urbanicity of their school, the size of the school, the grade level, the percent of students eligible for free or reduced-price lunch (FRPL), the percent of minority students, the percent of students with individualized education programs (IEP), and the percent of students classified as limited English proficiency (LEP). The full list of controls can be found in Appendix Table A1.

Empirical Approach

To examine the effects of SFRs on teacher salary, teacher intentions, and turnover, there are two main analytic approaches, one using a standard difference-in-differences (DID) model and one using a dynamic event study. As each approach entails strengths and weaknesses, we employ both in our study. First, we discuss the standard DID model:

$$Y_{ist} = \beta_0 + \beta_1 SFR_i + X_{st}\beta_2 + \pi_s + \lambda_t + T_{it}\beta_3 + S_t\beta_4 + \varepsilon_{ist} \quad (1)$$

where Y_{ijst} is an outcome of interest for teacher *i* from state *s* in year *t*, *SFR* is the main independent variable that equals 1 in all years post-policy adoption, X_{st} is a vector of time-varying state covariates, π_s and λ_{st} are state and year fixed effects, respectively. T_i is a vector of teacher characteristics, S_j is a vector of school characteristics, and ε_{ijst} are error terms. The coefficient of interest is β_1, which represents the effect of an SFR on our outcomes of interest. We use heteroskedastic-robust standard errors clustered at the state level. Table 1 details the states with an SFR and timing of the implementation within our analytic time frame.

<<Insert Table 1 Here>>

The main drawback with the standard DID model is that it does not provide estimates for how the effects of SFR may vary over time; in other words, it provides a weighted average treatment estimate of the effects of SFRs over the post-policy periods. Moreover, if there are heterogenous treatment effects by groups, the estimate may be biased (Goodman-Bacon, 2021). Relatedly, as SFRs were implemented at various times, a staggered DID is more econometrically appropriate (Marcus & Sant'Anna, 2021). As such, we rely on the staggered DID or event study approach as our main specification to examine the effects of SFRs. This non-parametric event study specification can be written as:

$$Y_{ist} = \alpha + \sum_{j=2}^{J} \beta_j (Lag\ j)_{ist} + \sum_{k=1}^{K} \gamma_k (Lead\ k)_{ist} + \pi_s + \lambda_t + T_{it}\beta_1 + S_t\beta_2 + e_{ist} \quad (2)$$

Lags and leads are binary variables indicating that a particular state was a given number of time periods away (i.e., SASS/NTPS survey wave) from implementing its SFR in the respective time period. J and K lags and leads are included respectively. A single lag, the first lag prior to SFR implementation where j=1, is omitted. The coefficients of interest are the lags, indicating differences in pre-treatment trends between SFR states and non-SFR states, and the leads where each lead k represents the effects of SFRs in the k^{th} period after implementation relative to non-SFR states. Non-significant estimates on the lags would suggest that SFR and non-SFR states were on similar trends prior to treatment, and significant estimates on the leads would suggest that SFRs impacted teacher outcomes. In conventional DID parlance, significant findings on the lags would suggest non-parallel trends between SFR and non-SFR states, weakening our claim that these two groups are comparable prior to treatment.

While the staggered DID is our preferred specification, it does have its limitations, particularly in terms of analyzing the effects of SFRs with respect to union status and union strength. Since the available data on union strength does not vary over time, we are unable to use a staggered DID approach to explore the differential effects of SFRs based on union strength. Instead, using a standard DID allows for this analysis. As such, we employ standard DID as a secondary approach to probe how union status may influence the effects of SFRs. Moreover, using a standard DID also allows us to conduct spillover analyses that a staggered DID would not. Specifically, we assume the behavior of the treated group does not influence that of teachers across state lines in control states, causing a spillover effect of the treatment. While this potentially could be problematic, we note that state educator licensing laws and teacher pension implications render the possibility of this spillover difficult, where if an SFR generated higher salaries in one state, neighboring state teachers would not be able to cross state lines easily,

raising turnover in those neighboring states. Still, we examine this possibility in Appendix Table A5.

Pre-treatment differences and parallel trends

As we show in the Results section, in the event study specification (Figures 1a to 1f) we find little to no evidence of pre-treatment differences in our outcome measures between SFR and non-SFR states, providing evidence that treated and non-treated groups are comparable prior to treatment. With respect to the standard DID approach, we examine parallel trends across treatment with multiple time periods and some never-treated states using the Callaway and Sant'Anna (2021) approach, considering both parallel trends of SFR states compared to: 1) states that are never treated within the analytic timeframe; and 2) a "supergroup" of both never-treated states and states that are not yet treated but will be treated later in the analytic timeframe. This is accomplished by regressing the outcome on the never treated states or the supergroup. Full results can be found in Appendix Table A2. We find no violations of the parallel trends assumptions at traditional significance levels, though our 2008 treated states do experience a two percent decline in teacher turnover prior to SFR interventions that is significant at the 10% level. Despite this relatively small difference, the observed pre-treatment stability of the outcome trends before intervention lends further credibility to our findings.

Results

Table 2 presents teacher and school characteristics for the overall sample as well as for the SFR and non-SFR samples weighted for national representation (unweighted characteristics are reported in Appendix Table A3). Nationally, about three quarters of teachers are women, 84% are white, 10% are novice teachers, 50% have graduate degrees, three quarters have union membership, a quarter of schools are urban, 39% are low-income schools (majority-FRPL) and

28% are majority minority schools. Descriptively, most teacher and school characteristics for the SFR and non-SFR samples are similar to the national sample except teachers in the SFR sample are more likely to have attended selective colleges, have graduate degrees, have union membership, and teach in urban schools.

<<Insert Table 2 Here>>

Examining our outcome measures of interest (Table 3), we observe the average teacher salary is about $54,770, 2% of teachers indicate they intend to leave their teaching positions as soon as possible, and the turnover rate is about 14% over the analytic time frame. We also examine these outcomes for various subgroups of interest. We find, as expected, novice teachers earn less than teachers near retirement and are less likely to indicate they intend to leave teaching. Both groups are more likely to turn over than the full sample of teachers. Teachers in weak union states report an average salary around $47,750, relative to $60,980 for teachers in strong union states. Moreover, they are also more likely to turn over relative to their counterparts. In low-income schools, teachers, on average, are paid $52,780 compared to $56,040 for teachers in high-income schools. In comparison, teachers' salaries in low-minority schools, schools that are more likely to be located in rural areas, are about $54,250 compared to $56,110 for teachers in majority minority schools, which are more likely to be located in urban areas.

<<Insert Table 3 Here>>

In our regression estimates, first we examine the effect of SFRs on teacher salary nationally and for the various subgroups (Table 4). We find that SFR impacts on teacher salaries vary significantly by time period. Specifically, consistent gains approaching or exceeding $4,000 are reached by *Lead 3* (Model 1 of Table 4), or approximately eight to 12 years following state

reforms. As discussed previously, SFRs may have differential effects for novice and veteran teachers who are near retirement as states may earmark salary increases for more veteran teachers. We examine whether SFRs have differential effects for these two groups in Models 2 and 3. In Model 2, we observe there is some marginally significant evidence that, while SFRs may increase salaries for all teachers on average, they likely accrue more quickly and more substantially for veteran teachers than novice teachers. With respect to low-income schools where the majority of students are eligible for free-and-reduced-price lunch, we find SFRs increased average teacher salary by $4,500 to $6,000 approximately a decade following reforms, with magnitudes between 150% to twice as large as those for teachers in high-income schools. These findings are closely mirrored by salary estimates for teachers in high- and low-minority schools. Taken together, these results indicate that SFRs were used to give targeted supports through increased teacher salaries in traditionally under-resourced and disadvantaged schools.

<<Insert Table 4 Here>>

Second, we examine the potentially heterogeneous effects of SFRs in strong and weak union states, finding that SFRs resulted in higher salaries for teachers in strong union states (Table 5), averaged across the post-policy periods. In a simple dichotomous analysis, states with above-median union power experienced average teacher salary increases that were around $3,500 more than states with below-median union power. We also operationalized union power across quartiles, and found that while the lowest quartile of union power states saw a significant increase in teacher salaries of $1,430, the highest quartile union power states witnessed teacher salary increases of $4,340. As mentioned previously, we note these findings are averages of all post-policy estimates, as the time-invariant nature of the included measure of teacher union strength does not allow for SASS survey wave-specific estimates.

<<Insert Table 5 Here>>

Third, we turn to whether SFRs significantly impacted a teacher's intention to leave their current school and the subsequently observed turnover behavior (Table 6). Evidence indicating SFRs caused reductions in teachers' intentions to leave their respective schools and reductions in observed turnover behavior emerge only after three and four survey waves, respectively (approximately eight to 12 years later). On the other hand, these findings are highly consistent with those pertaining to teacher salary, corresponding to the timing of the increase in salary. Regarding a teacher's intent to leave, we estimate null effects until four survey waves following treatment, when we find suggestive evidence of a one percentage point reduction in turnover intention. There is some indication that reductions in leaving intentions are driven by SFR-state teachers who serve in schools with majority FRPL students, finding that these teachers are two percentage points less likely to say that they plan on leaving the classroom. We found no differences in stated intentions across other classifications, including teachers in schools that are high-income, low-minority, or high-minority. We also observed turnover behavior, again finding statistically significant evidence of SFR-driven reductions in turnover only after three survey waves, of three percentage points. Across all of the different subgroups we analyzed, there is no clear grouping that behaved differently in SFR states.

<<Insert Table 6 Here>>

We also examined whether SFRs had any effect on intention and turnover based on union strength (Table 7), finding little statistically significant evidence to suggest that SFRs had differential effects on these outcome measures based on union influence. These null findings come despite having well-powered subgroups, with the smallest subgroups containing nearly 32,000 observations. While comparable studies of teacher turnover behavior were able to detect

turnover changes associated with salary increases using smaller sample sizes (e.g., Fulbeck, 2014), our analyses highlight the potentially critical nature of time-specific estimates in measuring first the effects of finance reforms on teacher salaries and, second, cotemporaneous shifts in teacher intentions and actual turnover behavior. If reforms do take time to be implemented, then it stands to reason that effects born out of those reforms would take a similar, if not longer, period of time to develop.

<<Insert Table 7 Here>>

Robustness Checks and Sensitivity Analyses

To examine how sensitive our results are to different modeling choices and specifications, we conduct several robustness checks and sensitivity analyses. First, some might argue political ideology, state characteristics, or teacher characteristics may be endogenous to the likelihood of adopting an SFR or teacher union membership may be related to union strength in the state. If true, then it would be better to exclude them as controls in the model. As such, we run a series of models that do not include teacher and school covariates or state-level policy covariates (Appendix Table A4). These results are substantively similar to the main results in both direction and statistical significance, easing concerns that our covariates are overly restrictive or that they include endogenous regressors.

Next, we explore whether states bordering SFR states would mirror their practices to match their neighbors (i.e., spillover effects); we find no evidence to support this hypothesis (Models 1 and 2 of Appendix Table A5). Similarly, we also examine whether there are "pre-treatment" effects or "post-treatment" effects by examining SFR states one wave before or after actual treatment, respectively, by arbitrarily reassigning SFRs to occur one wave earlier in the "pre-treatment" analysis and one wave later in the "post-treatment" analysis, each relative to the

actual timing of SFR implementation in state *s*. These analyses help address the possibility that additional state SFRs in close temporal proximity to those analyzed (i.e., those not deemed consequential by Lafortune et al.) may drive our results. We find a $2,100 post-treatment effect but no pre-treatment effect on salary or labor outcomes (Models 3 and 4 of Appendix Table A5). These results suggest that SFRs do increase teacher salaries averaged across the post-policy time periods and do not provide evidence indicating that additional SFRs motivate our findings. However, using the available repeated cross-sectional data rather than longitudinal data, we are unable to explore this fully.

In addition to the observed effects of SFRs on overall turnover, SFRs may also affect various forms of turnover in a heterogeneous manner. As such, we explore whether SFRs affect rates of switching and leaving teaching positions separately in Appendix Table A6. On the one hand, we find SFRs reduced rates of switching positions to a greater extent than leaving, though improvements were witnessed most notably in high-income and low-minority schools. On the other hand, though we estimate null overall effects on rates of leaving, we do observe significant declines in leaving behavior among teachers in low-income schools, six percentage points after three survey waves. Results without weights are substantively similar (available upon request). In short, these results largely reinforce our main findings, particularly with respect to the timeline over which SFR effects emerge.

Finally, we leverage SASS data on teacher opinions of school materials, salary satisfaction, and general satisfaction to assess alternate or complementary mechanisms through which SFRs may have affected teachers. In Table 8, our results indicate that teachers who experienced an SFR were more likely to indicate they have adequate materials, with results most concentrated in low-income schools and high-minority schools. Relatedly, teachers also

indicated more satisfaction with salary a decade after reforms, findings consistent with observed increases in their average salaries. Similarly, salary satisfaction results were most pronounced in high-minority schools. Effects on teachers' general satisfaction were more modest and also concentrated in high-minority schools. These exploratory analyses suggest teachers were more likely to express general satisfaction and satisfaction with their salaries and materials due to SFR changes, likely motivating observed effects on teacher intentions and turnover. In particular, these findings also are concentrated among teachers in historically disadvantaged school settings, the primary focus of SFRs.

Discussion and Conclusion

Our findings may be categorized in three domains. First, we find that after the introduction of increased state spending due to the exogenous shock of SFRs, there is a heterogeneous impact on teacher salaries across school settings and the time since reforms took place. The majority of significant results took about a decade to emerge, with average gains of approximately $4,000 in salary after three survey waves. Teachers in low-income and high-minority settings witnessed larger impacts, salary increases of approximately $4,500 to $6,000 annually. There is also suggestive evidence that gains were captured mostly by experienced teachers, as near retirement teachers received a sizeable salary bump after an SFR. These delayed salary effects are consistent with existing SFR literature. For example, Lafortune et al. (2018) find that increases to state investments in the lowest quintile of school districts by income peaked approximately nine years after reforms took place. Similarly, Jackson et al. (2016) find that investments in the bottom three quartiles of districts by prevailing spending took time to improve following court-ordered reforms, reaching statistically significant levels after seven years; spending improvements in the districts predicted to increase spending the most following

reforms took at least six years to emerge. In short, the somewhat delayed effects of SFRs on teacher salaries are consistent with findings related to total spending.

Second, confirming previous findings (Brunner et al., 2019), strong unions successfully capture the increased spending due to SFRs in the form of higher salaries, with suggestive evidence that SFR states with strong unions saw experienced teachers gain more than experienced teachers in weak union states. This may be interpreted as rent-seeking behavior, as the employed experienced teachers have a concerted interest in capturing salary gains, while future novice teachers were not present to self-advocate for higher salaries or increased hiring. Brunner et al. (2019) find that these strong union states also experienced stronger student achievement gains. We hypothesize this may be due to a range of factors, including reductions in teacher turnover and improvements in teacher resources. Still, we also know that many states ushered in finance reforms alongside a host of other education policy changes as state houses witnessed political change, which makes this singular story less clear (Kraft et al., 2020; Candelaria et al., 2021).

Third, in light of the observed increases in teacher salaries, particularly for teachers in schools with concentrations of FRPL-eligible students or minority students, we find significant average reductions in teacher turnover rates approximately a decade after the statewide SFR took place. On the other hand, we are unable to definitively parse these results among teachers in different school types, especially for teachers in schools targeted by statewide reforms. We find suggestive evidence that teachers in stronger union contexts reduced their turnover more than those in weaker contexts. Our additional exploratory analyses suggest some teachers, particularly those in high-minority schools, were more satisfied with their salaries due to SFRs and similarly

reported increased general satisfaction. Consistent with salary and turnover findings, results related to teacher satisfaction emerged a decade or more after reforms took place.

One of the mechanisms by which monetary investments in current teachers can produce better student outcomes is through lower teacher turnover (Ronfeldt et al., 2013; Steele et al., 2015). Previous research anticipates that while higher salaries can have a positive effect on teacher turnover behavior, the preponderance of the research evidence shows that a $1,000 increase in salary only reduces the risk of teacher turnover slightly (Nguyen et al., 2020), suggesting that a substantially larger increase may be needed to generate an economically significant impact. Other research specifically concerned with teachers' opportunity costs predicts only a slight increase in teacher retention (0.48%, an increase from 82.12% to 82.6%) if teachers' salaries were raised to be equivalent with competing professions in terms of wages ($3,160, or a 12.2% raise) (Feng, 2009), a slightly smaller average increase than identified here.

The previous literature that considers the ties between teacher salaries and teacher mobility considers between-school teacher salary comparisons and the effects on teacher mobility. The intervention considered here, SFRs, provided increased per-pupil spending in the districts that previously had the lowest resources (Candelaria & Shores, 2019), but did not necessarily raise teacher salaries to be competitive across districts. These larger salary interventions have been shown to be one of the chief mechanisms for discouraging teacher movement between schools (Imazeki, 2005). Future research should seek to understand what effect parity in salaries can have in terms of teacher turnover across districts in substantially different contexts.

Despite the previous literature linking SFR-generated spending increases to a range of positive student outcomes (Jackson et al., 2016; Lafortune et al., 2018), there remain unexplored

mechanisms by which this increase in funds may have generated improved student outcomes. The present research explores one of those mechanisms, further lending support for a positive relationship between SFR-induced teacher salary increases and reductions to teacher turnover rates, though several years after reforms took place. As we continue to understand how increased financial resources can lower teacher attrition rates and produce better student outcomes, more research is needed to understand which factors contributed to changes in teacher attrition decisions and which did not. Future research remains necessary to better understand how SFRs and other school finance interventions may shape teacher behavior, evidence which could provide insights into how best to confront resource constraints faced by schools and teachers.

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Tables and Figures

Table 1. Year of SFR implementation

State	Year of SFR implementation
Arkansas	2002
California	2004
Colorado	2000
Indiana	2011
Kansas	2005
Maryland	2002
Montana	2005
New Hampshire	2008
New York	2006
North Dakota	2007
Vermont	2003
Washington	2010
Wyoming	2001

Note. Year of SFR implementation drawn from Lafortune et al. (2018).

Table 2. Descriptive statistics of teacher and school characteristics

	(1) National pooled	(2) SFR	(3) Non-SFR
Female	0.76	0.75	0.76
Black	0.07	0.05	0.07
Asian	0.02	0.04	0.01
American Indian	0.01	0.01	0.01
Hispanic	0.07	0.09	0.07
White	0.84	0.82	0.85
Teacher age	42.40	43.17	42.27
Novice teacher	0.10	0.08	0.10
Graduate degree	0.50	0.58	0.49
No certification	0.02	0.01	0.02
Salary ($1,000s)	54.77	61.64	53.54
Union member	0.76	0.85	0.75
Urban school	0.26	0.34	0.25
K-12 enrollment	820.98	856.04	814.70
Secondary school	0.32	0.34	0.32
Combined elementary and secondary school	0.04	0.05	0.04
Percent FRPL	0.43	0.45	0.43
Low-income school	0.39	0.41	0.39
Percent minority students	0.32	0.34	0.32
High-minority school	0.28	0.32	0.27
Percent of students with an IEP	0.13	0.12	0.13
Percent of students LEP	0.06	0.08	0.06
Most selective college	0.09	0.13	0.09
Very selective college	0.19	0.22	0.19
Student discipline problem (std)	0.00	0.03	-0.01
Administrative support	0.00	-0.02	0.00
Teacher cooperation	0.00	-0.03	0.01
Observations	151,290	21,520	129,770

Note. Nationally-representative weights are employed. See Appendix Table 1 for description of included variables. Observations have been rounded to the nearest 10 per IES compliance. Source: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS) and National Teacher and Principal Survey (NTPS)

Table 3. Salary, intent to leave, and turnover

	(1) National	(2) Novice	(3) Near Retirement	(4) Weak Unions	(5) Strong Unions	(6) High-Income Schools	(7) Low-Income Schools	(8) Low-Minority Schools	(9) High-Minority Schools
Salary ($1,000s)	54.77 (17.44)	40.84 (10.01)	62.17 (20.11)	47.75 (11.81)	60.98 (19.17)	56.04 (18.18)	52.78 (16.01)	54.25 (17.60)	56.11 (16.94)
Intent to leave	0.02	0.01	0.04	0.02	0.02	0.02	0.02	0.02	0.02
Turnover	0.14	0.23	0.24	0.16	0.13	0.13	0.16	0.13	0.17
Observations	151,290	15,770	6,240	76,450	74,850	100,780	50,510	118,860	32,440

Note. Nationally-representative weights are employed. Observations have been rounded to the nearest 10 per IES compliance. Source: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS) and National Teacher and Principal Survey (NTPS). $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 4. Effects of SFRs on teacher salary

	(1) National	(2) Novice	(3) Near retirement	(4) High-income schools	(5) Low-income schools	(6) Low-minority schools	(7) High-minority schools
Lag 3	1.74^{+}	0.73	2.61	2.00^{+}	1.18	2.24*	-1.38
	(0.98)	(0.74)	(2.62)	(1.13)	(1.22)	(1.00)	(1.69)
Lag 2	-1.06	-0.58	-1.60	-1.03	-1.57	-0.12	-3.27*
	(1.26)	(0.61)	(1.50)	(1.37)	(1.16)	(1.08)	(1.28)
Lead 1	0.75	0.40^{+}	3.96**	0.45	1.47	0.70	0.97
	(0.89)	(0.23)	(1.45)	(0.69)	(1.51)	(0.62)	(1.62)
Lead 2	1.68	0.12	0.80	1.56	2.17	1.85	2.14
	(1.48)	(0.90)	(1.46)	(1.56)	(1.63)	(1.45)	(1.77)
Lead 3	4.01**	2.22^{+}	3.14	2.59*	5.82**	2.70*	4.86**
	(1.06)	(1.25)	(1.92)	(1.24)	(1.19)	(1.30)	(1.38)
Lead 4	3.73*	0.62	6.88	3.17*	4.58*	2.57	5.76*
	(1.42)	(1.22)	(4.22)	(1.43)	(1.96)	(1.71)	(2.52)
Observations	151,290	15,770	6,240	100,780	50,510	112,060	39,240

Note. Lag 1 is the comparison group. Nationally-representative weights are employed. Heteroskedastic-robust standard errors clustered at the state level are in parentheses. All models include teacher and school controls as well as state controls with state and year fixed effects. Observations have been rounded to the nearest 10 per IES compliance. Source: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS) and National Teacher and Principal Survey (NTPS). $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 5: Teacher salary by union strength

	(1) Low versus high union strength	(2) Q1 union power	(3) Q4 union power
SFR	-0.28	1.43*	4.34**
	(1.04)	(0.56)	(0.23)
Union strength	-3.17+		
	(1.86)		
SFR # Union strength	3.50**		
	(1.19)		
Constant	27.50**	29.33**	14.14**
	(1.97)	(2.67)	(3.05)
Observations	151,290	38,620	37,170

Note. Nationally-representative weights are employed. Heteroskedastic-robust standard errors clustered at the state level are in parentheses. All models include teacher and school controls as well as state controls with state and year fixed effects. Observations have been rounded to the nearest 10 per IES compliance. Source: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS) and National Teacher and Principal Survey (NTPS). $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 6. Effects of SFRs on teacher intentions and turnover behaviors

	(1) National	(2) Novice	(3) Near retirement	(4) High-income schools	(5) Low-income schools	(6) Low-minority schools	(7) High-minority schools
Panel A: Intention to leave							
Lag 3	-0.01*	0.01	-0.04*	0.00	-0.03**	-0.00	-0.00
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)
Lag 2	-0.00*	0.01	0.01	-0.00	-0.01**	-0.00	-0.01*
	(0.00)	(0.01)	(0.03)	(0.00)	(0.00)	(0.00)	(0.00)
Lead 1	0.00	-0.00	0.03+	-0.00+	0.01	-0.00	0.01
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)
Lead 2	0.00	0.00	0.01	0.00	0.00	0.00	0.00
	(0.00)	(0.01)	(0.02)	(0.00)	(0.01)	(0.00)	(0.01)
Lead 3	-0.00	0.01	0.02	-0.00	-0.00	0.00	-0.00
	(0.00)	(0.01)	(0.03)	(0.00)	(0.01)	(0.00)	(0.01)
Lead 4	-0.01+	0.00	-0.02	0.00	-0.02*	-0.01	-0.01
	(0.00)	(0.01)	(0.04)	(0.00)	(0.01)	(0.00)	(0.01)
Observations	151,290	15,770	6,240	100,780	50,510	112,060	39,240
Panel B: Turnover							
Lag 3	-0.01	-0.10**	-0.05	-0.01	-0.02	-0.01	0.02
	(0.01)	(0.02)	(0.04)	(0.01)	(0.02)	(0.01)	(0.06)
Lag 2	0.00	-0.06**	0.04	-0.01	0.03	-0.00	0.03
	(0.01)	(0.02)	(0.07)	(0.01)	(0.02)	(0.01)	(0.03)
Lead 1	-0.00	0.02	0.01	-0.00	0.00	-0.00	0.00
	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
Lead 2	-0.01	-0.03	-0.03	-0.01	0.00	-0.02	0.03
	(0.01)	(0.03)	(0.03)	(0.01)	(0.02)	(0.01)	(0.02)
Lead 3	-0.03*	-0.08	-0.05	-0.02	-0.05	-0.02+	-0.02
	(0.02)	(0.05)	(0.06)	(0.01)	(0.03)	(0.01)	(0.05)
Observations	132,820	13,900	5,160	91,240	41,570	100,380	32,440

Note. Nationally-representative weights are employed. Heteroskedastic-robust standard errors are in parentheses. All models include teacher and school controls as well as state controls with state and year fixed effects. Observations have been rounded to the nearest 10 per IES compliance. Source: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS) and National Teacher and Principal Survey (NTPS).
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 7: Teacher intentions and turnover by union strength

	(1) Low versus high union strength	(2) Q1 union power	(3) Q4 union power
Panel A: Intention to leave			
SFR	-0.00	-0.00	0.02
	(0.02)	(0.01)	(0.02)
Union strength	-0.06*		
	(0.03)		
SFR # Union strength	0.02		
	(0.02)		
Constant	0.48**	0.25**	0.49**
	(0.04)	(0.03)	(0.07)
Observations	151,290	38,620	37,170
Panel B: Turnover			
SFR	0.01	0.03	-0.01
	(0.01)	(0.02)	(0.01)
Union strength	0.05		
	(0.03)		
SFR # Union strength	-0.02+		
	(0.01)		
Constant	0.17**	0.31**	0.28**
	(0.02)	(0.09)	(0.05)
Observations	132,820	32,980	31,710

Note. Nationally-representative weights are employed. Heteroskedastic-robust standard errors clustered at the state level are in parentheses. All models include teacher and school controls as well as state controls with state and year fixed effects. Observations have been rounded to the nearest 10 per IES compliance. Source: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS) and National Teacher and Principal Survey (NTPS). $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 8. Effects of SFRs on adequate materials, satisfaction with salary, and general satisfaction

	(1) National	(2) Novice	(3) Near retirement	(4) High-income schools	(5) Low-income schools	(6) Low-minority schools	(7) High-minority schools
Adequate materials							
Lag 3	0.11*	0.20**	0.02	0.08+	0.23	0.11*	0.01
	(0.05)	(0.07)	(0.15)	(0.04)	(0.17)	(0.05)	(0.20)
Lag 2	0.02	0.10*	0.01	-0.00	0.07	0.00	0.04
	(0.04)	(0.04)	(0.08)	(0.04)	(0.04)	(0.05)	(0.05)
Lead 1	0.10**	0.19**	0.08	0.07*	0.16*	0.05*	0.16**
	(0.03)	(0.06)	(0.07)	(0.03)	(0.06)	(0.02)	(0.06)
Lead 2	0.07*	0.07	0.12	0.06+	0.08+	0.06+	0.06
	(0.03)	(0.05)	(0.09)	(0.03)	(0.04)	(0.03)	(0.05)
Lead 3	0.11*	0.14	0.24*	0.00	0.24**	0.03	0.18**
	(0.05)	(0.10)	(0.11)	(0.04)	(0.05)	(0.04)	(0.06)
Lead 4	0.18**	0.01	0.12	0.17*	0.22**	0.08	0.31**
	(0.07)	(0.13)	(0.15)	(0.07)	(0.08)	(0.07)	(0.07)
Observations	151,290	15,770	6,240	100,780	50,540	112,060	39,240
Satisfaction with salary							
Lag 3	0.06	-0.07	0.12	0.07	0.03	0.08	-0.07
	(0.08)	(0.14)	(0.16)	(0.08)	(0.08)	(0.08)	(0.07)
Lag 2	-0.04	-0.01	0.17	-0.03	-0.09	0.02	-0.13**
	(0.05)	(0.07)	(0.16)	(0.03)	(0.07)	(0.03)	(0.04)
Lead 1	0.00	-0.05	0.17	0.02	-0.04	0.02	-0.02
	(0.05)	(0.08)	(0.10)	(0.04)	(0.08)	(0.04)	(0.09)
Lead 2	0.03	-0.08	0.09	0.05	0.00	0.05	0.06
	(0.05)	(0.07)	(0.08)	(0.05)	(0.07)	(0.06)	(0.06)
Lead 3	0.08	0.13	0.12	0.03	0.12+	0.02	0.12+
	(0.06)	(0.08)	(0.11)	(0.06)	(0.07)	(0.08)	(0.07)
Lead 4	0.19*	0.09	0.05	0.19+	0.19+	0.08	0.34**
	(0.08)	(0.15)	(0.21)	(0.10)	(0.11)	(0.14)	(0.10)
Observations	151,290	15,770	6,240	100,780	50,510	112,060	39,240

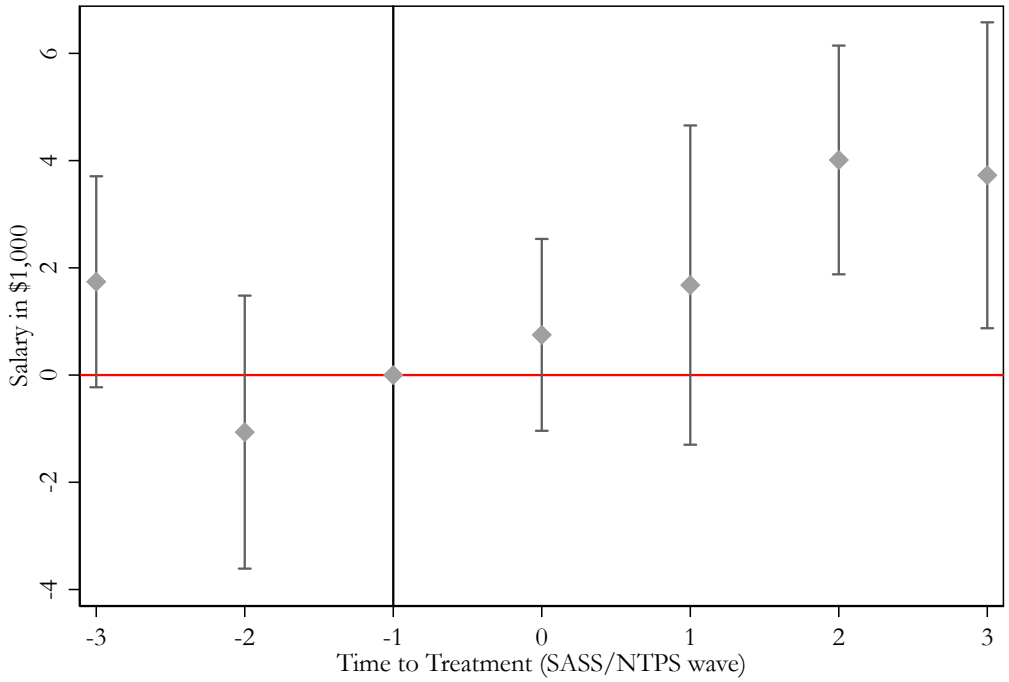
General satisfaction							
Lag 3	0.01	0.09*	-0.03	0.02	-0.02	0.02	-0.05
	(0.02)	(0.04)	(0.13)	(0.02)	(0.05)	(0.02)	(0.07)
Lag 2	-0.02	-0.04	0.00	-0.02	-0.03	-0.01	-0.03+
	(0.01)	(0.05)	(0.05)	(0.02)	(0.02)	(0.02)	(0.02)
Lead 1	0.00	-0.01	-0.14	-0.00	-0.01	-0.01	-0.00
	(0.01)	(0.04)	(0.08)	(0.01)	(0.04)	(0.01)	(0.03)
Lead 2	-0.01	0.02	-0.05	-0.00	-0.04	-0.02*	-0.02
	(0.01)	(0.04)	(0.05)	(0.01)	(0.02)	(0.01)	(0.03)
Lead 3	0.04	0.04	-0.03	-0.00	0.07*	-0.02	0.08**
	(0.03)	(0.09)	(0.07)	(0.04)	(0.03)	(0.03)	(0.03)
Lead 4	0.07*	0.09	0.06	0.07**	0.09	0.03	0.13**
	(0.03)	(0.08)	(0.10)	(0.02)	(0.06)	(0.03)	(0.05)
Observations	151,290	15,770	6,240	100,780	50,510	112,060	39,240

Note. Nationally-representative weights are employed. Heteroskedastic-robust standard errors are in parentheses. All models include teacher and school controls as well as state controls with state and year fixed effects. Observations have been rounded to the nearest 10 per IES compliance. Source: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS) and National Teacher and Principal Survey (NTPS).

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

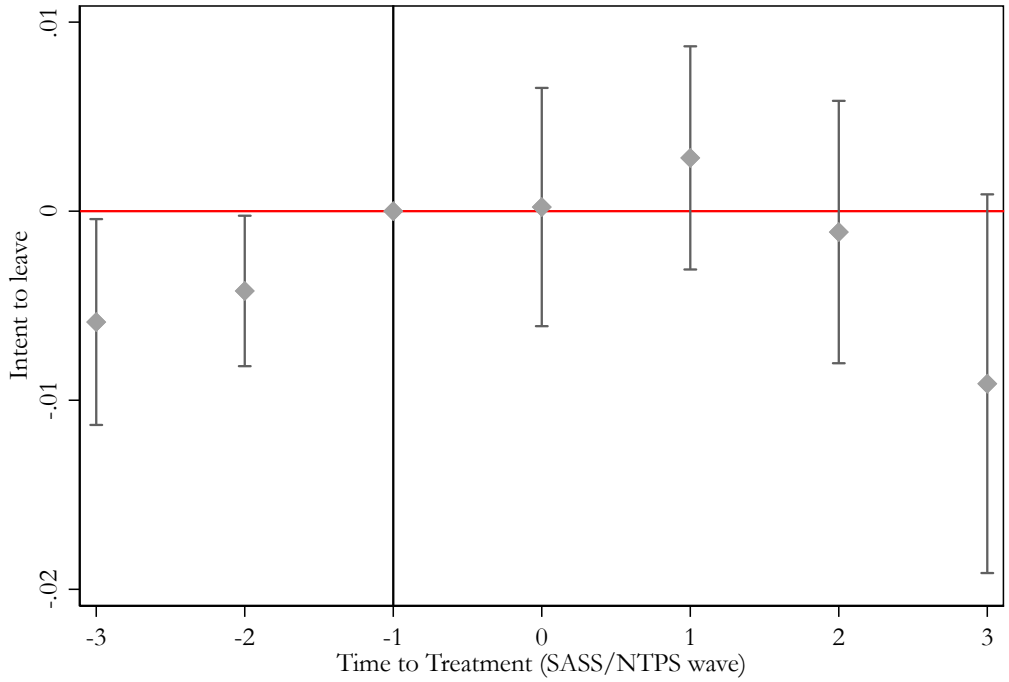
Figure 1. Teacher salary and turnover trends, dynamic staggered DID analysis

Panel A: Teacher salary



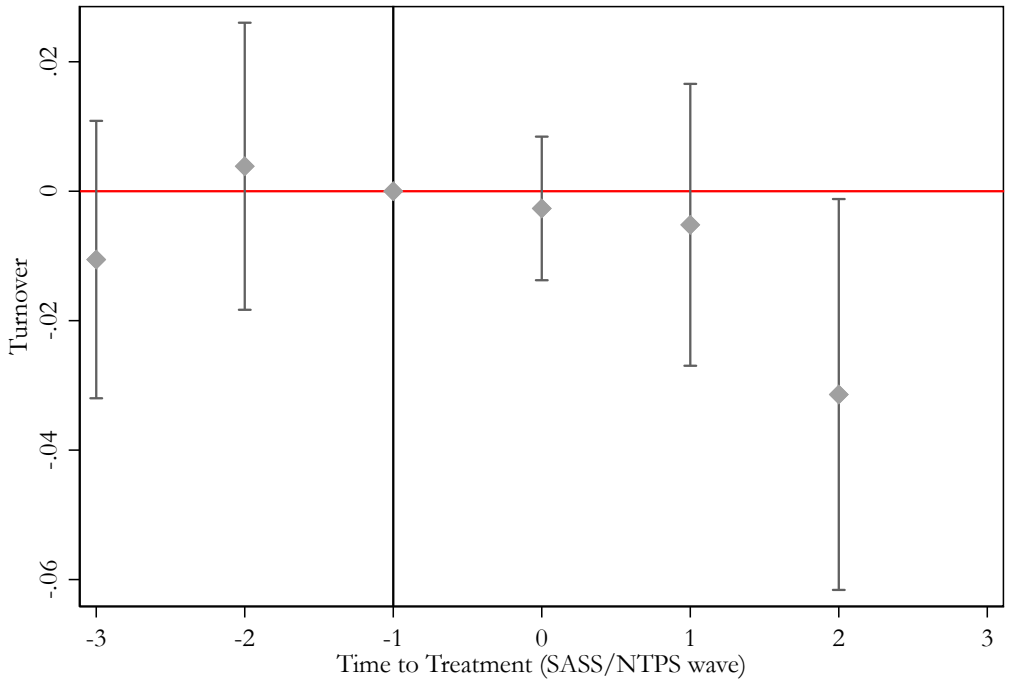
95% Confidence Intervals Shown

Panel B: Teacher intention to leave a teaching position



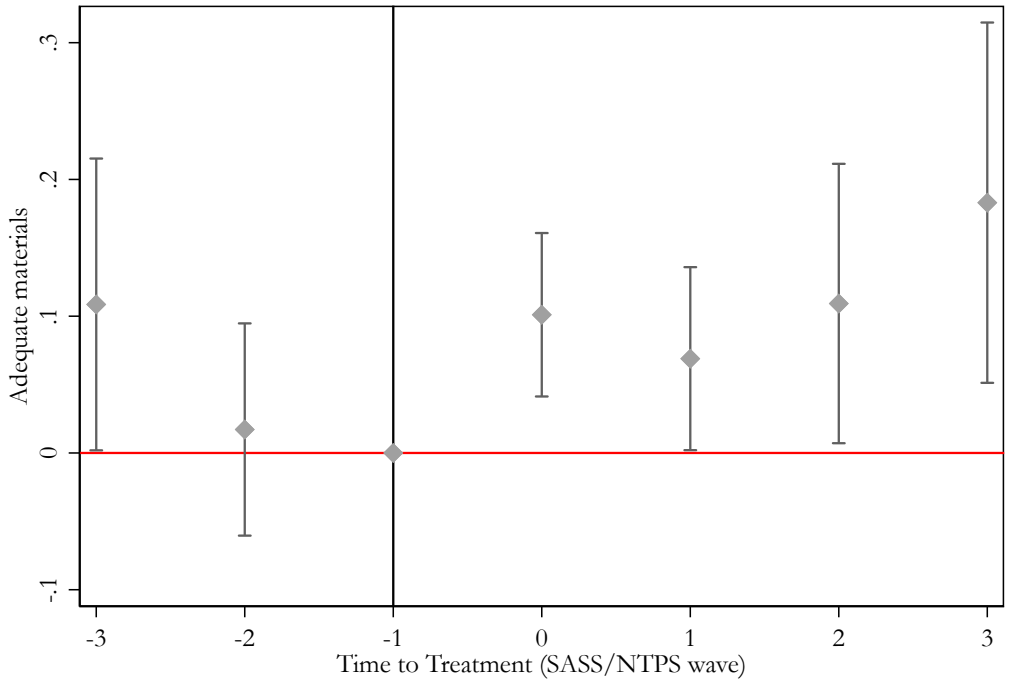
95% Confidence Intervals Shown

Panel C: Teacher turnover



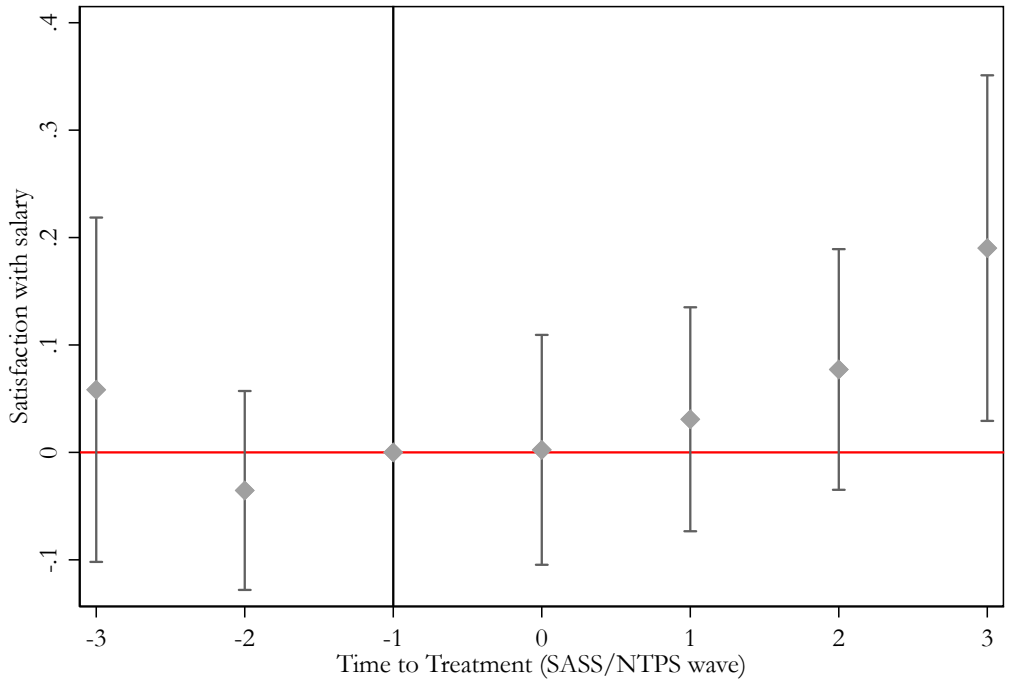
95% Confidence Intervals Shown

Panel D: Adequate materials

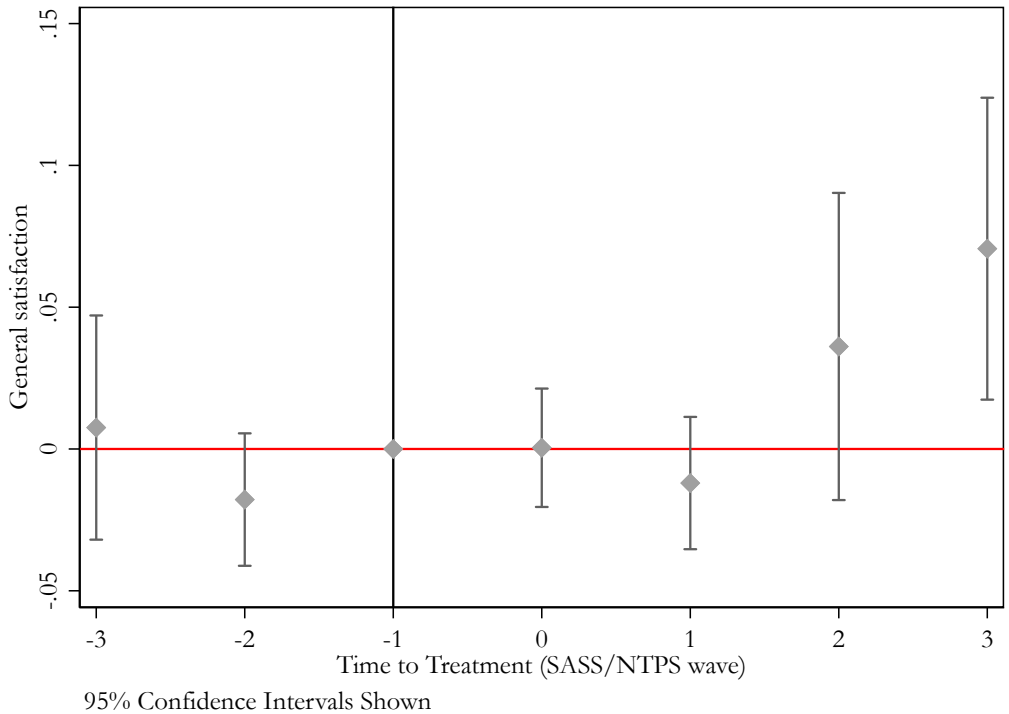


95% Confidence Intervals Shown

Panel E: Satisfaction with salary



Panel F: General satisfaction



Note. Nationally-representative weights are employed.

Online Appendix Table A1. Variable descriptions

Dependent variable/Outcomes	
Salary ($1,000)	A continuous variable of the base teaching salary for the entire school year, scaled in $1,000s, and in constant 2016 dollar.
Intent to leave	A dichotomous variable where 1 = definitely plan to leave teaching as soon as I can and 0 = other responses, which includes until I am eligible for retirement benefits, will probably continue unless something better comes along or specific life event (e.g. parenthood, marriage), as long as I am able, or undecided at this time
Leavers, Switchers, Movers and Stayers	Leavers are teachers who left the teaching profession, switchers are teachers switched to a new school, movers are teachers who left their current school (leavers+switchers) and stayers are teachers who are currently teaching in same school.
Teacher Characteristics	
Female	A dichotomous variable where 1 = female and 0 = male.
Black	A dichotomous variable where 1 = Black and 0 = non-Black.
Asian	A dichotomous variable where 1 = Asian and 0 = non-Asian.
American Indian	A dichotomous variable where 1 = American Indian and 0 = non-American Indian.
Hispanic	A dichotomous variable where 1 = Hispanic and 0 = non-Hispanic.
White	A dichotomous variable where 1 = White and 0 = non-White.
Age	A continuous variable of teacher's age.
Novice	A dichotomous variable where 1 = teacher has less than three years of teaching experience and 0 = teacher has three or more years of teaching experience.
Graduate degree	A dichotomous variable where 1 = teacher has graduate degree and 0 = no graduate degree.
No certification	A dichotomous variable where 1 = teacher has no certification and 0 = teacher has any certification.
Most selective college	A dichotomous variable where 1 = teacher's undergraduate college/university has Barron's classification of most competitive or highly competitive and 0 = Barron's classification is competitive, less competitive, or noncompetitive.
Very selective college	A dichotomous variable where 1 = teacher's undergraduate college/university has Barron's classification of very competitive and 0 = Barron's classification is competitive, less competitive, or noncompetitive.
Satisfy w/ salary (std)	On a scale of 1 = strongly disagree and 4 = strongly agree, teachers report on how satisfied they are with their salary. Measure standardized for each wave.
Union member	A dichotomous variable where 1 = teacher is a union member and 0 = teacher is not a union member.
School Characteristics	
Urban school	A dichotomous variable where 1 = school is classified as urban by U.S. census and 0 = non-urban areas as classified by U.S. census.
Suburban school	A dichotomous variable where 1 = school is classified as sub-urban by U.S. census and 0 = non-suburban areas as classified by U.S. census.
K-12 enrollment	A continuous variable of the size of school where the teacher is teaching in the base year.
Secondary school	A dichotomous variable where 1 = the school is classified as a secondary school and 0 = the school is not classified as a secondary school.
Combined elem-sec	A dichotomous variable where 1 = the school is classified as a combined elementary and secondary (K-8) school and 0 = the school is not classified as a combined elementary and secondary school.

Percent FRPL students	Percentage of students eligible for the federal free or reduced-price lunch program.
Low-income schools (majority FRPL)	A dichotomous variable where 1 = the majority of students at the school is eligible for federal free or reduced-price lunch and 0 = the majority of students at the schools is not eligible for federal free or reduced-price lunch (also referred to as low-income schools).
Percent minority students	Percentage of non-White students enrolled in a school.
High-minority schools (Majority minority)	A dichotomous variable where 1 = the majority of students at the school is non-White and 0 = the majority of students at the school is White.
Percent IEP	Percentage of students with Individualized Education Plans (IEP).
Percent LEP	Percentage of students classified as Limited English Proficient (LEP).
Student discip (std)	On a scale of 1 = never happens to 5 = happens daily, the principal reports of six kinds of student discipline problems: physical conflict, robbery or theft, vandalism, student use of alcohol, drug use, and possession of weapons.
Administrative support	On a scale of 1 = strongly disagree and 4 = strongly agree, teachers report on the school administration's behavior toward the staff is supportive and encouraging. Measure standardized for each wave.
Teacher cooperation	On a scale of 1 = strongly disagree and 4 = strongly agree, teachers report on the level of cooperative effort among the staff members. Measure standardized for each wave.
State and policy characteristics	
Eval_pass	Calendar year in which teacher evaluation reform law was passed
Eval_implement	Teacher evaluation reform implemented, regardless of whether included in formal teacher evaluation scores
Probabation_incrperiod	Tenure probationary period increased
Tenure_elim	Eliminated teacher tenure
Rttt_won	Race-to-the-Top Grant won
Cb-weaken	Mandated collective bargaining eliminated or substantially restricted
Senate Republicans	Measurement of state Republican Senate members ideology using the roll call voting of the state delegation
House Republicans	Measurement of state Republican House members ideology using the roll call voting of the state delegation
CC_implement	Common Core State Standards implemented and maintained at least 85% of standards if standards are renamed and/or modified

Note. State and policy characteristics are drawn from Kraft et al. (2020). Senate and House Republicans data are drawn from Klarner (2013).

Online Appendix Table A2: Pretreatment differences/parallel trends for salary and turnover

	(1) Supergroup 2004	(2) Treated 2004	(3) Supergroup 2008	(4) Treated 2008	(5) Supergroup 2012	(6) Treated 2012	(7) Supergroup 2016	(8) Treated 2016
Panel A: Salary								
Pre-treatment differences	-1.19	-1.19	2.61	2.75	-0.18	0.87	-3.96	-1.30
	(2.73)	(2.73)	(5.75)	(5.77)	(1.80)	(1.75)	(2.91)	(2.46)
Constant	51.58**	51.58**	49.90**	49.76**	52.12**	51.06**	53.08**	50.42**
	(1.31)	(1.31)	(1.04)	(1.12)	(1.53)	(1.47)	(2.13)	(1.46)
Panel B: Turnover								
Pre-treatment differences	0.01	0.01	-0.02+	-0.02+	-0.01	-0.02	.	.
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	.	.
Constant	0.15**	0.15**	0.15**	0.15**	0.14**	0.14**	.	.
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	.	.
Observations	38,170	38,170	32,660	30,020	31,720	25,630	25,150	20,110

Note. Nationally-representative weights are employed. Heteroskedastic-robust standard errors clustered at the state level are in parentheses. Observations have been rounded to the nearest 10 per IES compliance. Source: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS) and National Teacher and Principal Survey (NTPS). $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Online Appendix Table A3. Descriptive statistics of teacher and school characteristics without weights

	(1) National pooled	(2) SFR	(3) Non-SFR
Female	0.69	0.69	0.69
Black	0.05	0.04	0.06
Asian	0.02	0.02	0.02
American Indian	0.02	0.02	0.02
Hispanic	0.04	0.05	0.04
White	0.87	0.88	0.87
Teacher age	42.63	43.33	42.51
Novice teacher	0.10	0.10	0.11
Graduate degree	0.48	0.50	0.48
No certification	0.02	0.01	0.02
Salary ($1,000s)	51.26	54.14	50.79
Union member	0.72	0.75	0.72
Urban school	0.21	0.24	0.21
K-12 enrollment	822.93	800.34	826.68
Secondary school	0.50	0.48	0.51
Combined elementary and secondary school	0.10	0.11	0.09
Percent FRPL	0.40	0.40	0.40
Low-income school	0.33	0.31	0.34
Percent minority students	0.32	0.34	0.32
High-minority school	0.26	0.27	0.26
Percent of students with an IEP	0.14	0.13	0.14
Percent of students LEP	0.05	0.07	0.05
Most selective college	0.08	0.09	0.07
Very selective college	0.18	0.22	0.18
Student discipline problem (std)	0.09	0.04	0.09

Administrative support	0.00	0.02	-0.00
Teacher cooperation	-0.05	-0.05	-0.05
Observations	151,290	21,520	129,770

Note. Nationally-representative weights are not employed. Source: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS) and National Teacher and Principal Survey (NTPS)

Online Appendix Table A4. Effects of SFRs on salary, teacher intentions, and turnover, excluding covariates

	Salary				Intent to leave				Turnover			
	(1) High-income schools	(2) Low-income schools	(3) Low-minority schools	(4) High-minority schools	(5) High-income schools	(6) Low-income schools	(7) Low-minority schools	(8) High-minority schools	(9) High-income schools	(10) Low-income schools	(11) Low-minority schools	(12) High-minority schools
Lag 3	2.62	1.30	2.47	0.85	-0.00	-0.03**	-0.01*	-0.01**	-0.02+	-0.03	-0.02**	-0.01
	(1.82)	(1.90)	(1.73)	(2.20)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.02)	(0.01)	(0.07)
Lag 2	-1.35	-1.63	-0.42	-3.46+	-0.00	-0.01**	-0.00+	-0.01**	-0.01	0.01	-0.01+	0.01
	(1.97)	(1.75)	(1.51)	(2.02)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.02)	(0.00)	(0.03)
Lead 1	0.59	1.83	0.79	1.29	-0.00+	0.01	-0.00+	0.01	-0.01	-0.00	-0.00	-0.01
	(0.84)	(1.68)	(0.69)	(2.10)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Lead 2	2.04	2.95	2.32	2.58	0.00	0.00	0.00	-0.00	-0.01	-0.01	-0.02	-0.00
	(2.16)	(2.48)	(1.98)	(2.75)	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)	(0.02)	(0.01)	(0.02)
Lead 3	3.27*	8.53**	3.20*	7.42**	-0.00	-0.00	-0.00	-0.00	-0.04**	-0.07+	-0.04**	-0.07+
	(1.24)	(1.40)	(1.20)	(1.49)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	(0.04)	(0.01)	(0.04)
Lead 4	3.02	5.83**	2.61+	5.75+	-0.00	-0.01	-0.00	-0.01				
	(1.96)	(1.98)	(1.36)	(3.24)	(0.00)	(0.01)	(0.00)	(0.01)				
Observations	100,780	50,510	112,060	39,240	100,780	50,510	112,060	39,240	91,240	41,570	100,380	32,440

Note. Nationally-representative weights are employed. Heteroskedastic-robust standard errors are in parentheses.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Online Appendix Table A5: Border and time robustness checks

	(1) Border exclude	(2) Border include	(3) Post-treatment	(4) Pre-treatment
Panel A: Salary				
Pre-treatment differences	-0.23 (0.50)	-0.39 (0.42)	2.10** (0.68)	1.77+ (0.97)
Observations	129,770	151,290	151,290	151,290
Panel B: Turnover				
Pre-treatment differences	-0.01 (0.01)	-0.01 (0.01)	-0.00 (0.01)	-0.01 (0.01)
Observations	115,410	132,820	132,820	132,820

Note. Nationally-representative weights are employed. Heteroskedastic-robust standard errors clustered at the state level are in parentheses. All models include teacher and school controls as well as state controls with state and year fixed effects. Observations have been rounded to the nearest 10 per IES compliance. Source: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS) and National Teacher and Principal Survey (NTPS). $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Online Appendix Table A6. Effects of SFRs on teacher mobility

	(1) National	(2) Novice	(3) Near retirement	(4) High-income schools	(5) Low-income schools	(6) Low-minority schools	(7) High-minority schools
Panel A: Switcher							
Lag 3	-0.02*	-0.05+	-0.10**	-0.01	-0.03+	-0.01	-0.04
	(0.01)	(0.03)	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)
Lag 2	-0.01	-0.05+	-0.05	-0.01	-0.01	-0.01	-0.01
	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.01)	(0.02)
Lead 1	-0.01	-0.02	-0.05*	-0.01	-0.00	-0.01	-0.01
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Lead 2	-0.00	-0.03	-0.07+	-0.01	0.01	-0.01	0.02
	(0.01)	(0.03)	(0.03)	(0.01)	(0.02)	(0.01)	(0.02)
Lead 3	-0.02*	-0.07	0.02	-0.03**	0.00	-0.03*	0.02
	(0.01)	(0.06)	(0.04)	(0.01)	(0.02)	(0.01)	(0.04)
Observations	122,430	12,350	3,930	84,500	37,930	93,190	29,240
Panel B: Leaver							
Lag 3	0.00	-0.07**	0.02	-0.00	0.01	-0.00	0.06
	(0.01)	(0.03)	(0.05)	(0.01)	(0.02)	(0.01)	(0.05)
Lag 2	0.02*	-0.03	0.07	0.00	0.04**	0.01	0.04*
	(0.01)	(0.03)	(0.09)	(0.01)	(0.02)	(0.01)	(0.02)
Lead 1	0.01	0.05**	0.05	0.01+	0.01	0.01	0.02+
	(0.01)	(0.02)	(0.05)	(0.01)	(0.01)	(0.01)	(0.01)
Lead 2	-0.00	-0.00	0.02	-0.00	-0.00	-0.01	0.02
	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.02)
Lead 3	-0.01	-0.03	-0.06	0.01	-0.06*	0.00	-0.04
	(0.01)	(0.03)	(0.07)	(0.01)	(0.03)	(0.02)	(0.03)
Observations	122,800	12,010	4,970	84,930	37,870	93,200	29,600

Note. Nationally-representative weights are employed. Heteroskedastic-robust standard errors are in parentheses. All models include teacher and school controls as well as state controls with state and year fixed effects. Observations have been rounded to the nearest 10 per IES compliance. Source: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS) and National Teacher and Principal Survey (NTPS).

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$