NBER WORKING PAPER SERIES

THE EFFECTS OF TEACHER STRIKES ON COMPENSATION, WORKING CONDITIONS, AND PRODUCTIVITY

Melissa Arnold Lyon
Matthew A. Kraft
Matthew P. Steinberg

Working Paper 32862
http://www.nber.org/papers/w32862

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2024, Revised February 2025

This manuscript was previous circulated with the title, "The Causes and Consequences of U.S. Teacher Strikes." We are very grateful for research assistance from Zoe Beckman, Eunice Chong, Summer Dai, Stephanie Tu, Sarah Newberger, Hyesang Noh, Adam Shephardson, and Natalie Truong. This research also benefited immensely from the helpful feedback of Jesse Bruhn, Jake Rosenfeld, Sarah Anzia, and the seminar participants at Michigan State University, the 2023 APSA Comparative Labor Politics Workshop, and the 2023 APSA and 2022 AEFP Annual Meetings. This research was supported by a NAEd/Spencer Postdoctoral Fellowship awarded to Lyon. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

NBER working papers are circulated for discussion and comment purposes. They have not been peer-reviewed or been subject to the review by the NBER Board of Directors that accompanies official NBER publications.

© 2024 by Melissa Arnold Lyon, Matthew A. Kraft, and Matthew P. Steinberg. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

The Effects of Teacher Strikes on Compensation, Working Conditions, and Productivity
Melissa Arnold Lyon, Matthew A. Kraft, and Matthew P. Steinberg
NBER Working Paper No. 32862
August 2024, Revised February 2025
JEL No. I22, J30, J45, J52

ABSTRACT

We examine how teacher strikes in the United States affect compensation, working conditions, and productivity with an original dataset of 745 teacher strikes between 2007 and 2024. Using an event study framework, we find that the average strike leads to a 6% ($7,629) increase in combined annual wages and benefits and a 0.5 student (3.2%) decline in pupil-teacher ratios after five years. There is little evidence of sizable impacts on student achievement up to five years post-strike, though strikes lasting 10 or more days decrease math achievement in the short-term.

Melissa Arnold Lyon
SUNY Albany
mlyon@albany.edu

Matthew P. Steinberg
Accelerate
matthew@matthewpsteinberg.com

Matthew A. Kraft
Brown University
Department of Education
and NBER
mkraft@brown.edu

I. Introduction

Organized labor has experienced substantial declines in the U.S. over the last half century. The percentage of unionized workers has fallen by more than 60% since 1970 (Naidu 2022). Labor activism experienced an even starker drop in past decades, with estimates of a 90% decrease in worker participation in strikes (Massenkoff and Wilmers 2024). President Reagan's decision to fire and permanently replace striking air traffic controllers in 1981 opened the doors for many firms to adopt hardline responses to strikes (Rosenfeld 2006). Technological changes and offshoring of traditional union sector jobs have further undercut union strength. Massenkoff and Wilmers (2024) find that in the post Professional Air Traffic Controllers Organization (PATCO) strike era, striking has not resulted in real wage gains for workers, on average (see also Rosenfeld 2006). Such trends have caused observers to pronounce America's unions "basically dead" (Ghilarducci 2015). However, a resurgence of labor activism in the U.S. in recent years among teachers, graduate students, hotel workers, actors, and autoworkers belies these historical trends, raising new questions about the efficacy of strikes as a source of worker power.

In this paper, we examine how strikes affect compensation, working conditions, and productivity in the context of U.S. K-12 public education. Public school teachers comprise one of the largest occupational fields in the United States, making up approximately 5% of the college-educated workforce with over 3.7 million teachers nationally. Teachers also constitute a sizable fraction of all unionized workers in the United States, where nearly one in five union members (18%) is a public school teacher (Hirsch et al. 2024), and have been at the forefront of the resurgence in labor activism. A wave of high-profile teacher strikes in 2018 resulted in the largest work stoppages in the United States in a generation (Bureau of Labor Statistics 2018).

We construct and analyze an original, hand-collected database of 745 teacher strikes between the 2007-08 and 2023-2024 school years. We use this dataset to describe the landscape of teacher strikes, elucidating a central feature of the evolving U.S. labor movement (Cowen and Strunk 2015; Naidu 2022). We find that teacher strikes have been a recurring phenomenon with a median number of strikes per year at 14, impacting roughly 13.4 million students across our 17-year panel. Most strikes are brief, with the modal strike lasting a single day. Using data from press releases and news reports on unions' stated motivations, we also demonstrate that compensation (i.e., salaries and benefits) has been a key focus of teacher strikes, with teachers advocating for higher wages and benefits in 89% of strikes. Additionally, over half of the strikes were motivated by working conditions, including class sizes, educational expenditures and non-instructional staff (e.g., school nurses), and school buildings and school facilities.

We estimate the causal effects of teacher strikes on compensation, working conditions, and productivity using doubly robust differences-in-differences (DiD) estimators in an event study framework (Callaway and Sant'Anna 2020; Sant'Anna and Zhao 2020). Results are robust to a range of approaches to constructing the comparison group. These include synthetic control methods as well as restricting our comparison districts to only those in states where strikes occurred or only those in states that have never experienced a strike. Results remain consistent in a series of further specification tests including: systematically modifying sets of covariates, excluding large-scale strikes and those oriented around labor policy, accounting for multiple strike events, and dropping Great Recession and pandemic years, among others.

We find that teacher strikes in the United States increase teacher compensation by 6% on average by the fifth year after a strike. This translates to an annual increase in salary and benefit expenditures with an economic value of $7,629 per teacher in real 2018 dollars. Working

conditions also improve, with pupil-teacher ratios decreasing by 0.5 students (3.2%) and an approximately 5% increase in expenditures dedicated to the compensation of non-instructional staff who support the work of teachers. We find that these improvements in compensation and working conditions arise from increased revenues, rather than reallocations of funds within existing education budgets. Strikes cause both per pupil revenues and expenditures to increase by 7%.

We then examine how strikes affect school district productivity, measured by student achievement. Despite effects on both compensation and working conditions, we find no evidence of sizable positive or negative effects on student achievement, on average, up to five years post-strike. However, our estimates cannot rule out the possibility of small positive effects of 0.05 or smaller. We can rule out negative effects, on average, as small as -0.004 SD for reading and -0.02 standard deviations (SD) for math in the year of and immediately following a strike. However, consistent with both theory and the prior research, exploratory dosage analyses reveal that strikes lasting two or more weeks are associated with declines in math achievement, with estimates ranging between -0.03 and -0.05 SD in the year of the strike and the following year. These declines are not sustained beyond the first year after a strike. Heterogeneity analyses further suggest that strikes are most effective at raising teacher compensation when they involve more teachers and when districts spend and pay less than other districts in the same state.

This study makes three primary contributions. First, it revisits the question of whether strikes are an effective tactic of organized labor for raising compensation and improving working conditions with current evidence from the public education sector. The sizable effects of strikes we find on wages and benefits stand in stark contrast to the decline in the effectiveness of strikes across sectors in the 1980s and 1990s (Massenkoff and Wilmers 2024; Rosenfeld 2006).

Second, our paper expands the literature on how teacher collective action affects production in the public education sector (Baron 2018; Biasi 2021; Biasi and Sarsons 2022; Brunner et al. 2019; Hoxby 1996; Lovenheim 2009; Lovenheim and Willén 2019; Roth 2019). Prior research has largely focused on the academic and labor market effects of long-lasting strikes in the international context (Baker 2013; Belot and Webbink 2010; Jaume and Willén 2019; 2021; Johnson 2011). We study the effects of teacher strikes in the United States, where the structure of organized labor and public education differs considerably from other countries.

Finally, we contribute to the broader economics of education literature examining the relationship between teacher wages and student achievement. Prior studies have examined this question in the context of deregulating teacher labor markets (Burgess et al. 2022; Willén 2021; Biasi 2021), changes in relative wages (Britton and Propper 2016) and unconditional teacher wages increases (Cook et al. 2021; De Ree et al. 2018). We develop a simple conceptual model to illustrate how the effect of strikes on student achievement could operate through prolonged school cancellations as well as strike-induced changes in district resource levels and allocations. The typically short duration of teacher strikes in the U.S. context allows us to examine how strikes affect student achievement via changes in resource-based mechanisms that are not confounded with large amounts of lost instructional time. Though we find no sizable effects on student achievement for the typical U.S. strike, our dosage analyses based on strike duration affirm the findings of prior research outside the U.S. that longer strikes are associated with immediate negative consequences for student learning (Baker 2013; Belot and Webbink 2010; Jaume and Willén 2019; 2021; Johnson 2011).

II. Conceptual Framework

We develop a simple toy model of public sector teacher strikes building on the industrial relations literature as well as Freeman and Medoff 's (1984) monopoly versus collective voice framework. We represent teacher welfare as a stylized function of salary (s), benefits (b), and school inputs (X) that shape teachers' working conditions. Teacher unions aim to maximize their members' welfare (W), subject to a district budget constraint (B) as follows:

$$max\ \mathrm{W} = f(\mathrm{s}, \mathrm{b}, \mathrm{X}) \ \text{ subject to B.} \tag{1}$$

Two things make this stylized model of teacher welfare maximization uniquely different from industrial labor actions in the private sector. Economic research traditionally frames strikes as conflicts between workers and firms that result from information asymmetry due to workers' lack of knowledge about firm surplus (Card 1990; Cramton and Tracy 1992; Krueger and Mas 2004; Mas 2008). Sustained disruptions caused by strikes can compel firms to disclose information regarding their profit margins and capacity to enhance compensation (Cramton and Tracy 1992). However, firms in the public education sector (e.g. districts) are not profit maximizing actors that adjust their budget based on the marginal revenue product of labor. Instead, district budgets are a function of the local propensity to fund education through municipal taxes, the size and allocation of state education aid, and federal funding programs such as Title I.

Teachers can improve their welfare by advocating for a larger allocation of the district budget *and* they can lobby local, state, and federal elected officials to relax the district budget constraint by increasing funding for public education. Strikes in this context serve dual purposes: direct bargaining leverage and broader signaling to policymakers and voters about service provision and funding priorities (Lyon and Kraft 2024).

A second key difference is that in maximizing their welfare, teacher strikes have the potential to positively or negatively affect the education production process (Brunner et al. 2019). We adapt the canonical education production function model (Todd and Wolpin 2007) to illustrate how strikes shape key inputs of student success, which we quantify as student achievement on standardized tests:

$$\mathrm{A} = f[\mathrm{H}, \mathrm{TE}(\mathrm{s}, \mathrm{b}, \mathrm{X}), \mathrm{X}, \tau]. \quad (2)$$

Here, achievement is a function of students' human capital (H), the effectiveness of the teaching staff (TE) (which is shaped to various degrees by the three factors that also influence teachers' welfare), the direct effects of school inputs on students (X), and the instructional time available to schools (τ).[1] Research that employs discrete choice experiments documents teachers' substantial willingness to pay for improved working conditions such as smaller class sizes and additional support staff (Johnston 2025; Lovison and Mo 2024).[2] These school inputs can also directly benefit students (Angrist and Lavy 1999; Chetty et al. 2011; Hoxby 2000; Krueger 1999; Mulhern 2023; Jackson et al. 2015; Lafortune et al. 2018; Jackson and Mackevicius 2024).

Early studies that explore the association between strikes and student achievement cross-sectionally find a mixed and inconclusive pattern of results (e.g., Lytle and Yanoff 1973; Caldwell and Jeffreys 1983; Thornicroft 1994). In several more recent studies, scholars attempt to take advantage of variation across localities and over time to generate more credible estimates of the effects of teacher strikes outside of the United States. Two studies of Canadian teacher strikes demonstrate that strikes lasting more than 10 days reduce student achievement in certain

[1] Our model allows for school inputs to affect student achievement directly as well as indirectly through teachers. For example, hiring more or better school counselors can benefit students directly as well as allow teachers to focus on core instruction by supporting students' social-emotional needs.

[2] Teacher strikes may also improve school working conditions due to strategic alliances and coordinated bargaining strategies. In some districts, teachers' unions negotiate contracts alongside other unions representing instructional support staff and noninstructional staff.

grades[3] by roughly three percent of a SD, particularly in the year that the strike occurs (Baker 2013; Johnson 2011). Two additional studies of exposure to strikes in Belgium (Belot and Webbink 2010) and Argentina (Jaume and Willén 2019) demonstrate that the loss of roughly 4-5 months of instructional time has long-term negative consequences for students' human capital development and long-run labor market outcomes. This is in part because of the long-term disruptions to childcare arrangements that cause mothers to drop out of the workforce (Jaume and Willén 2021). These results are also likely driven by the negative effects of large amounts of lost instructional time (τ) that is well documented in the literature (Kraft and Novicoff 2024).

We complement these studies by estimating the impacts of hundreds of strikes in the United States and examining a broader set of mechanisms through which teacher strikes might affect student achievement and exploring how these effects evolve over time. A primary avenue through which strikes might affect student achievement is through changes in resource levels and allocations that affect the composition and performance of the teacher workforce (TE) and school inputs (X). The United States presents an advantageous context to study this broader range of mechanisms and dynamics given that strikes are typically short in duration. In this context where lost instructional time is unlikely to be a first-order effect, the hypothesized effects are decidedly ambiguous given that strikes represent a bundled treatment.

One potential first order effect in this context is if strikes lead to expanded education funding thereby relaxing the budget constraint districts face. Teacher strikes may expand education funding through public signaling; two recent studies document how teacher strikes in the U.S. have notable political effects on campaign priorities and public opinion, dramatically increasing the prevalence of education in politics and elevating support for teacher demands

[3] Baker (2013) finds negative effects in grades five and six, and Johnson (2011) finds negative effects in grades three and six.

(Hertel-Fernandez et al. 2021; Lyon and Kraft 2024). If strikes significantly expand funding, we might expect to see downstream improvements in student achievement. Work by Jackson and Mackivicious (2024) shows the impact of expanded investments in public education: increased district spending by $1,000 per pupil per year over four consecutive years improves test scores by 0.03 SD, on average, particularly when that spending goes to instructional salaries. Small class sizes and additional support staff have also been shown to raise achievement (Angrist & Lavy, 1999; Kruger, 2000; Reback, 2010; Carrell & Hoekstra, 2014; Mulhern, 2023).

The use of additional funds to increase teacher compensation can benefit student achievement through changes in the composition of the teacher workforce and the effort teachers expend on the job. Efficiency wage theory suggests that if teachers are compensated at their marginal product, then any increases in compensation should lead to increases in teacher productivity (Akerlof 1982; Shapiro and Stiglitz 1984). Higher wages can expand teacher supply (Edwards et al. 2024) and improve morale and retention (Hendricks 2014). Additionally, almost one in five teachers nationally works a second job outside the school system (Will 2022). Strike-induced salary increases may make secondary jobs less necessary, leaving teachers with more time and energy to dedicate to instructional effort. These labor market effects would take several years to emerge, illustrating the value of studying the dynamic impacts of strikes over time.

It is also possible that any increased teacher compensation secured via strikes might not translate into improved student achievement. Prior studies have found that across-the-board pay raises for teachers (Cook et al. 2021; De Ree et al. 2018) have not resulted in improved achievement. If teacher effort has already been capitalized in test scores in the pre-strike period, districts may be realizing rents by not paying teachers commensurate with their productivity. In this context, wage gains may not drive further effort. Second, wage gains alone without

corresponding investments in on-the-job skill development may leave teachers who are motivated by newly won compensation without a productive pathway for improving their performance (Murnane and Cohen 1985). If strike-induced contract concessions are funded by reallocating funding instead of expanding the education funding pool via new resources, then their effects again depend on whether any funding allocated to increased teacher compensation and school inputs is rent-seeking or efficiency-enhancing (Hoxby 1996).

III. Data

A. Original National Strike Database

We construct an original database of 745 teachers' strikes in 592 unique public school districts in 27 U.S. states from July 2007 to 2024. To create this dataset, our research team of three principal investigators and seven research assistants relied primarily on data collected through our own original search efforts involving the review of ~90,000 news articles alongside longitudinal administrative data on strikes from two states with large numbers of strikes and other public and proprietary sources, which we detail in Online Appendix A. Lyon and Kraft (2024) use a smaller portion of this dataset at the media market-by-month level in a related study of teacher strikes as public signals.

We define a teacher strike as a coordinated labor action involving the withholding of teacher labor and resulting in school cancellation for some time period. This includes legal, illegal, coordinated, and individual strikes, as well as "wildcat" strikes, "walk-outs," or "sick-outs" that lead to the cancellation of school. In the 37 states where striking is illegal, teachers' unions can effectively strike when teachers collectively walk out of their classrooms (a "walk-out"). Another option is a "sick out," in which teachers call in sick *en masse*. In states like Washington strikes are illegal but nevertheless quite common, perhaps related to the lack of a

specified penalty. In illegal teacher strikes, organizers often attempt to capture the attention of state policymakers or the public to create a situation in which imposing legal penalties becomes unmanageable or undesirable. For example, thousands of teachers prominently chose to participate in work stoppages in 2018 and 2019 under the banner of "#RedforEd," despite strikes being illegal in nearly every one of the states that experienced such strikes (e.g., Arizona, Kentucky, Oklahoma, North Carolina, and West Virginia).

B. Outcomes

We merge our strike dataset with several measures of compensation, working conditions, and productivity at the district-year level. We use data on compensation, working conditions, and district characteristics from the Common Core of Data (CCD), maintained by the National Center for Education Statistics (NCES). Our primary fiscal measure is logged teacher compensation, which we estimate by dividing total instructional salary and benefit expenditures in a district by the number of full-time equivalent (FTE) teachers.[4] We also show average teacher salary and benefits separately.

We use three primary measures of teacher working conditions: pupil-teacher ratios; working conditions expenditures per pupil; and capital expenditures per pupil. We use pupil-teacher ratios and capital expenditures per pupil calculated by the NCES. Note that capital

[4] NCES defines full-time equivalent as "The state's (or district's) FTE value for a teacher. FTE is the amount of time required to perform a teaching assignment stated as a proportion of a full-time position; it is computed by dividing the amount of time employed by the time normally required for a full-time position. FTE is not necessarily linked to contract days" (Noel 2010). The Census of Governments (2017) defines employee benefit expenditures as "the employer share of state or local employee retirement contributions, social security contributions, group life and health insurance, unemployment and worker's compensation, and any tuition reimbursements." The instructional salaries and benefits measures include salaries and benefits for both teachers and instructional assistants and aides. We use the count of FTE teachers as the denominator in our primary measure because the extent to which districts hire and report instructional aides varies wildly, and instructional aides make substantially less than teachers. Data on FTE in the state of Wisconsin were missing for the school year ending in 2015, so we use a district-year linear interpolation to estimate the FTE values for 2015. We also create an alternate measure of average instructional staff compensation measured as instructional salary and benefit expenditures/FTE teachers + instructional aides and paraprofessionals). Results are not meaningfully different with this outcome, as shown in Appendix Figure B1.

expenditures are not included in current expenditures and thus are independent of all other expenditure measures. We then characterize total current expenditures per pupil, with the exception of teacher salary and benefits, as a proxy measure of the investments that districts make in teacher working conditions, broadly construed. This includes the salary and benefits of all non-teacher district employees as well as funds for professional development, instructional materials, and more. We show results with the two component parts of this measure as well: noninstructional salary and benefit expenditures per pupil and expenditures per pupil not spent on salaries and benefits (i.e., non-salary and benefit expenditures per pupil). We inflation adjusted all fiscal measures to reflect real 2018$ and then logged them to derive estimates that approximate percent changes. We use data on fiscal measures for the 2007-2008 to 2019-2020 school years and pupil-teacher ratios from 2007-2008 to 2021-22.

We also use the Stanford Education Data Archive Version 5.0 student achievement data at the district-by-year level (SEDA; Reardon et al. 2024). These data are from state standardized tests and have been normed to the National Assessment of Education Progress (NAEP) exam, allowing for cross-state comparisons despite the tests (and proficiency standards) differing across states. SEDA currently includes measures of 3rd- to 8th-grade academic performance in mathematics and reading at the district-grade level across all states for the spring of 2009 to 2019. These test scores are standardized to the nationwide population of school districts. We use these tests scores as informative proxy measures of teacher and school productivity, recognizing that student achievement is also a product of their individual characteristics and out-of-school influences. We merge the SEDA data with strike data covering the time period included in the SEDA data (2008-2009 to 2018-2019 school years), by generating district-by-year math and

reading achievement averages using precision weights, as described by Shores and Steinberg (2019).

C. Covariates and Contextual Measures

We merge our strike data with district and state-level information for use in our doubly robust estimation process. We include measures of district student enrollment, unemployment rates, child poverty rates, and socioeconomic status. Student enrollment counts are derived from the CCD as described above. Measures of child poverty are from Urban Institute's Data Explorer. To measure local labor market conditions, we also include SEDA measures of unemployment rates and socioeconomic status (SES). The socioeconomic measure is calculated by Reardon et al. (2024). SEDA documentation describes this as the "first principal component score of the following measures (each standardized): median income, percent of adults ages 25 and older with a bachelor's degree or higher, poverty rate for households with children ages 5-17, SNAP receipt rate, single mother headed household rate, and employment rate for adults ages 25-64." When data are missing on covariates, we use a district-level linear interpolation to estimate the missing value.

To examine the context surrounding teacher strikes, we also use district demographic characteristics derived from the CCD, as described above. These include the percentages of special education students; English language learners; White, Black, Hispanic, Asian/Pacific Islander, Native American, and Multi-racial students; the school age population in poverty; and students living in urban, suburban, and town or rural areas. We supplement our panel data with several state-level measures. We use a measure of teachers' union density from the Schools and Staffing Survey in 2007-2008. We also include information on whether a state prohibits strikes from our own data collection efforts. We use measures of public opinion on economic and social

policy issues from Caughey and Warshaw's (2018) mass economic policy preferences and mass social policy preferences (where a higher number indicates greater liberalism).

IV. National Landscape of Teacher Strikes

Teacher strikes have occurred with regular frequency in the United States over the last 17 years. Across our panel, the median annual number of such strikes is 14 strikes per year, corresponding to a median of 89 total strike days per year. Over this period, strikes have accounted for the loss of over 71 million student-days in schools. Figure 1 displays the frequency of strikes at the district-by-event level since 2007. We separate individual and coordinated strikes because coordinated strikes, by definition, involve multiple districts. Strikes have occurred in every year of our panel with spikes in activity corresponding with the #RedForEd coordinated strikes of the 2017-18 and 2018-19 school years, though not all strikes in this period were coordinated across districts. The modal strike lasts only a single day; the median strike lasts 2 days; 65% of strikes ending in five days or less; and 90% ending in fewer than 10 days (see Figure 2).

Teacher strikes overwhelmingly focus on teacher compensation. Table 1 shows the frequency of strike reasons in three broad, non-mutually exclusive categories (compensation, working conditions, and common good), as well as their respective sub-categories. Appendix Figure B7 shows the prevalence of these reasons over time. Nine out of every ten (89%) teacher strikes involve demands for increasing teacher salaries or benefits, and this has been static over time. Over half of strikes (56%) include a focus on working conditions such as general school expenditures, infrastructure and maintenance, noninstructional staff, and/or labor rights. One in six strikes (17%) include demands for increases to non-instructional staffing or pay, specifically. The focus on working conditions has increased over time.

Table 2 shows baseline differences between striking and non-striking districts.[5] Striking and non-striking districts look fairly similar, particularly in terms of compensation, pupil-teacher ratios, and capital expenditures. Striking districts tend to have lower current expenditures and revenues per pupil at baseline, are larger, and are more urban. They also tend to serve larger proportions of students who are non-white, in poverty, and English Language Learners. Such baseline differences are not indicative of a failure of the core assumption discussed above. Indeed, many of these differences are unlikely to vary much over time and therefore are accounted for by the district fixed effects in our empirical specification. We show that results are nearly identical even after controlling for these demographic differences in the robustness section and Appendix Figures B3-6.

Geographically, strikes have occurred in 27 states with notable clustering on the west coast, the mid-Atlantic, and the Midwest (see Figure 3). Appendix Table B4 shows the number of strikes in each state overall and separated between individual and coordinated strikes. Individual strikes have occurred in 22 states, and coordinated strikes have taken place in 11 states in 2011, 2015, 2018, and 2019. Coordinated strikes are more common in states where strikes are illegal (see Appendix Table B5), with 90% of coordinated taking place in states with prohibitions on teacher strikes, compared to 28% of individual district strikes. The coordinated strikes reflect a more social movement style of teacher mobilization, and teachers' unions or associations have tended to be less central (Blanc 2019).

[5] We use the dollar (rather than logged) versions of compensation and expenditures variables here for clarity. We use means from the 2007-2008 school year, the first year in which we have strike data. We impute missing district-year observations using linear interpolation but show that results are nearly identical without the imputation in Appendix Table B2. After imputation 4.3% of districts were missing data because they did not report measures of interest for any time period, reducing our sample of striking districts to 555. We show separate baseline descriptives for individual and coordinated strikes in Appendix Table B3.

Appendix Table B5 provides more information on state contexts, demonstrating that strikes occur in states with both weak and strong teachers' unions. Most strikes (517 strikes or 71%) have taken place in one of the 37 states where they are illegal. Additionally, 178 teacher strikes have occurred in one of the six states where majorities of public school teachers are not members of their teachers' union or association (North Carolina: 80 strikes, Arizona: 89 strikes, Arkansas: 2 strikes, and South Carolina: 7 strikes). Indeed, four out of the six states with the lowest shares of unionized teachers have experienced at least one strike. However, strikes have also occurred in states where teachers' unions are very strong including five out of the six states where over 97% of public school teachers have joined their local teachers' union or association.

V. Econometric Approach

We estimate the effect of strikes on compensation, working conditions and productivity by exploiting the staggered timing of strikes across districts via a dynamic and doubly robust two-way fixed effects (i.e., event study) framework. This approach compares changes in schooling inputs and outcomes pre and post strikes with contemporaneous changes in untreated districts that never experience teacher strikes. Critically, TWFE estimators identify credible treatment effects even when mean differences exist across treated and untreated units as long as their average outcomes evolve in the same pattern. The Callaway and Sant'Anna (2020) doubly robust estimator that we use further addresses potential endogeneity issues by better aligning the untreated group with the striking group using both outcome regression and inverse probability weighting (IPW). Our approach identifies the causal effect of strikes under three primary assumptions: 1) that changes in outcomes in the weighted sample of non-striking school districts provide a valid counterfactual for the changes that would have occurred in striking districts had they not experienced strikes (i.e., parallel trends), 2) that the timing of teacher strikes in striking

districts is not confounded with other concurrent shocks to teacher compensation and student achievement, and 3) that the effect of striking does not influence outcomes in non-striking districts.

Conceptually, the parallel trends assumption is likely to hold in our context given idiosyncrasies in the *timing* of strikes due to processes that operate on fixed timelines such as the duration of teacher contracts and the timing of school board elections. Prior causal work on teacher strikes has used a similar framework relying on stronger assumptions. Notably, studies of Canadian teacher strikes use fixed effects approaches (Baker 2013; Johnson 2011), and studies of strikes in Belgium (Belot and Webbink 2010) and Argentina (Jaume and Willén 2019) use DiD approaches without inverse probability weighting. Potential spillover effects, which we examine in our robustness section, are likely to cause our estimates to understate the full magnitude of any effects given non-striking districts may responded in ways to minimize the possibility of a strike in their district.

Our core specification is a dynamic event study estimator as follows:

$$Y_{dt} = \sum_{r=-5}^{5} \beta_r \mathrm{I}(t - t_d^{strike} = r) + \pi_d + \delta_t + \varepsilon_{dt}, \quad (3)$$

where *Y* is the average outcome (e.g., teacher compensation, pupil-teacher ratios, achievement in math or reading) for district *d* during school year *t*, and t_d^{strike} indicates the year of the first[6] strike for district *d*. β_r represents the effect of the strike r years later (or before if $r < 0$) relative to the year before the strike. The terms π_d and δ_t represent district and year fixed effects, respectively. A benefit of this approach is that the coefficients β_{-5} to β_{-1} dynamically test for differences in trends prior to strikes between treated and comparison districts, thus embedding a

[6] We focus on the first strike in a given district for our primary specification but show in Appendix Table B1 that results are substantively unchanged if we use the full set of strikes.

falsification test for the key assumption noted above.[7] The β_0 to β_5 coefficients then map out the effect of strikes over time non-parametrically. [8] We cluster standard errors at the district level.

Traditional two-way fixed effects (TWFE) DiD estimators can be biased in the presence of staggered treatment timing and treatment effect heterogeneity. The doubly robust DiD estimation that we use addresses potential biases due to possible negative weights in settings with staggered treatments and potentially heterogeneous effects by aggregating individual estimates of 2x2 group-time average treatment effects on the treated (ATTs) where the comparison group is restricted to never treated districts (Sant'Anna and Zhao 2020; Callaway and Sant'Anna 2020). We do not include not-yet treated districts in our comparison groups, though their inclusion does not affect results.

As noted briefly above, the Callaway and Sant'Anna doubly robust estimator is particularly advantageous in our context where treated districts differ from untreated districts. It combines both outcome regression among untreated units to estimate control regression coefficients and project the outcome evolution in the treated group, as well as inverse probability weighting based on propensity score estimates to better balance treated and comparison groups. To implement this, we include a vector of pre-strike district covariates for dynamic local labor market conditions (student enrollment, unemployment rates, child poverty rates, and socioeconomic status). For both striking and non-striking units, the earliest-period covariates are used for the estimation of the propensity score and outcome regressions. For all post-strike ATTs, this is the year before the strike. For pre-treatment ATTs, this is year *t*. We show that results are consistent when we remove these covariates in Appendix Figures B3-B6.

[7] We also show results examining β_{-8} to β_{-1} in Appendix Figure B2, though the sample composition changes significantly when we attempt to estimate falsification tests in pre-periods more than 5 years prior to a strike.
[8]Across all our analyses, we censor districts that strike in the last year of a given outcome-specific analytic dataset because we have no post-strike outcome data for these districts.

VI. How Do Strikes Affect Compensation, Working Conditions, and Productivity?

A. Compensation

We find that teacher strikes cause large increases in teacher compensation, which comprises roughly 53% of total district expenditures. Figure 4 shows estimated effects for compensation and breaks out compensation into salaries and benefits. First, we see that the estimates in the five years *prior* to strike occurrence are consistently very small in magnitude (1% or less), hover around zero, and are almost always non-significant. This suggests that prior to strike occurrence, compensation in striking districts were largely trending in parallel with compensation in non-striking districts. However, after the strike, we see that compensation shifts dramatically.

Panel A in Figure 4 shows that strikes cause average compensation to increase by 3-6% in the 5 years after a strike. We present point estimates for this and all other outcomes in Appendix Table B6. Appendix Table B7 also shows estimated strike effects on fiscal measures in 2018 dollars (not log transformed), demonstrating that teacher strikes cause annual compensation to increase by $2,500-$7,600 in the 1-5 years after the strike. In Panels B and C, we show that these increases occur in both salaries and benefits. On average, strikes lead to a 2-4% (~$1,300-$4,000) increase in annual teacher salaries in the 1-5 years after the strike. Strikes also cause annual teacher benefits to increase by 4-8% (~$1,000- $4,000) over the same period.

The magnitude of these wage gains is impressive when put in context. Based on data collected by the NEA, the national average starting and top salary for public school teachers with master's degree during the 2017-18 school year were $42,927 and $66,919, respectively (National Education Association 2019). Drawing on our estimated effects in real 2018 dollars in Appendix Table B7, the average wage gain five years after a teacher strike is equivalent to 32%

of the total difference in salary between novices and the highest paid teachers, holding education levels constant.

In Appendix Figure B8, we examine whether these effects on teacher salaries are driven by increases in total expenditures on instructional salaries and benefits (i.e., the numerator) scaled per pupil or by decreases in total teacher FTE (i.e., the denominator). We find that strikes increase instructional salary and benefit expenditures by 4-9% in the five years after a strike. We find no evidence that districts reduce the number of teachers in response to strikes, suggesting that gains in compensation are driven by real increases in wages and benefits rather than by cutting teaching jobs or leaving vacant positions unfilled.

How could short strikes produce these sustained increases in compensation? Intuition suggests that in the private sector, longer strikes are more likely to produce larger wage gains given the cumulative pressure that lost profits have on firms over time. However, strike length is endogenous to the bargaining process and prior studies have found that in practice, shorter strikes have produced large wage gains (Card and Olson 1995). Teacher strikes are felt immediately by entire communities as parents must take off from work or scramble to secure childcare, and student lose instructional time. Even a single day of teacher strikes can galvanize broader constituencies of parents and community members to place intense pressure on districts to resolve the negotiations quickly. The extreme level of disruption that teacher strikes cause, shuttering public services widely used within communities, means that teacher strikes can attract attention and effectively signal the need for policy change, even in the absence of prolonged shutdowns.

Past research has also found that strikes in the 1970s produced even larger gains in wages, 8%, but that these gains were immediate rather than staggered (Massenkoff and Wilmers

2024). What might explain the sustained increases we observe? On average, salaries and benefits for K-12 education comprise roughly 30% of direct general municipal expenditures at the local level making immediate wage jumps untenable for most municipal budgets.[9] Instead, contract negotiations between teachers' unions and districts typically focus on two dimensions of teacher salaries—the pay teachers earn at a given level of experience (step) and education (lane) as well as an annual cost-of-living adjustment (COLA). For example, an increase in the value of advancing a "step" in the contract of 0.3% annually and increasing the COLA by 0.5% annually would produce the 4% wage gains, which is roughly what we observe after five years. Negotiations also commonly focus on two dimensions of teacher benefits—contributions to health insurance and retirement plans. Most collective bargaining agreements last for three years, but teachers' unions often start at the status quo and bargain up from there, rarely agreeing to a lower COLA rate and often pushing to incrementally increase it further (Scott 1997).

Public sector strikes may also signal the need for increased educational expenditures and teacher compensation to political elites and the public, leading to increased investments in public education funding over time (Lyon and Kraft 2024). Studies of court-ordered education finance reforms find similar patterns of extended incremental increases in expenditures for lower-spending districts post-reforms (Jackson et al. 2015; Lafortune et al. 2018).

B. Working Conditions

In Figure 5, we examine the effect of teacher strikes on five measures of working conditions. In Panel A, we find that, on average, teacher strikes lead to a decline in pupil-teacher ratios of about half of a student in 3-5 years after a strike (3.2% relative to a base of 15.86

[9] This estimate draws on analyses from the Urban Institute that find 39% of all local expenditures go to K-12 education (*Elementary and Secondary Education Expenditures* 2021) and F-33 school finance data from the National Center for Education Statistics that shows that 76% of K-12 expenditures go to employee salary and benefits.

students; see Table 2). This suggests that districts slightly decrease class sizes in response to teacher strikes, which is a common goal of teacher collective bargaining. Appendix Figure B9, Panel B shows large, positive but imprecise strike effects on the number of FTE teachers, suggesting district lower class sizes by hiring more teachers.

As a second, proxy measure of working conditions, we examine expenditures per pupil that are *not* spent on instructional salaries and benefits. Panel B demonstrates that teacher strikes increase working conditions expenditures by 4-5% in the 3-5 years following the strike. In Panels C and D, we disaggregate this proxy measure into two funding categories: non-instructional salary and benefit expenditures per pupil (25% of total current expenditures), and expenditures per pupil not spent on salaries and benefits (for either instructional or non-instructional staff; 19% of total current expenditures). We find that strikes increase non-instructional salary and benefit expenditures per pupil by 3-8%.[10] Finally, in Panels D and E, we find no evidence that strikes affect non-salary and non-benefit expenditures per pupil (e.g., instructional materials and technology) or capital expenditures per pupil, which NCES measures separately from other expenditures.

C. Revenues or Reallocations

Funding for compensation and working conditions improvements could either come from new revenues or reallocations from existing sources (e.g., raising class sizes or employing fewer workers). As we show above, districts do not offset higher salaries with larger class sizes or workforce reductions, as we find a small decrease in pupil-teacher ratios and no declines in

[10] Auxiliary analyses in Appendix Figure B9 demonstrate that these increases reflect real increases in noninstructional compensation, rather than the hiring of new noninstructional staff members. Districts may have raised noninstructional staff compensation alongside teacher compensation as part of coordinated bargaining efforts, or they may have initially attempted to hire additional noninstructional staff but had trouble at pre-strike salary levels and needed to increase compensation.

overall teacher FTEs. Districts could generate new revenues by, for example, increasing local property taxes. States are also key funders of public education, and strikes may lead states to increase their funding of public education through direct negotiation and by signaling to policymakers and voters (Lyon and Kraft 2024). We examine the effects of strikes on total expenditures and revenues per pupil in Figure 6. Results show that strikes increase yearly per pupil expenditures and revenues by 2-7% in the five years after a strike. This equates to an increase of $1,057 in revenues and an increase of $780 in expenditures per pupil in the fifth year after the strike. These findings suggest that strikes cause policymakers to expand the education budgets with new revenues rather than reallocations.

D. Productivity

We find no definitive evidence that strikes affect reading or math achievement in the year of the strike or in the subsequent five years after the strike, on average. Figure 7 displays the results of Equation 3 for student achievement in reading and math. We can rule out average negative effects in the year of the strike as small as -0.004 SD for reading and -0.02 SD for math. In the medium run, where we might expect increases in district expenditures to positively affect student achievement, the estimates are less precise. There is some suggestive evidence of a positive effect on reading scores, particularly in the third year after a strike, though the event-study estimates are too noisy to provide definite evidence.

Why didn't increased expenditures and smaller classes drive improvements in achievement?— In a recent meta-analysis of the impact of school funding, Jackson and Mackevicius (2024) find that increasing *noncapital* expenditure by $1,000 per pupil in 2018$ for four consecutive years increases student test scores by 0.034 SD. There is also a long-standing literature on the academic benefits of class size reductions (e.g., Angrist and Lavy 1999; Hoxby

2000; Krueger 1999; Chetty et al. 2011). Results from the Tennessee STAR class size experiment find that reducing kindergarten class sizes by 7 students, from 22 to 15, raises tests scores by 0.20 SD (Krueger 1999). However, the incremental treatment effects of strikes on expenditures that increase over time limit our ability to detect effects within the event-study window that our panel data allows. We estimate that strikes increase instructional spending per pupil by ~$780 in the fifth year after a strike and reduce class sizes by 0.5 students. These do not approach the magnitude for which we might expect an effect on achievement, particularly in the short to medium term. Furthermore, we cannot rule out that strikes had plausibly small positive effects on achievement given that the upper bound of our confidence intervals hover around 0.05 SD for both subjects.

Why didn't lost instructional time cause decreases in achievement?— As noted in our conceptual framework, there is clear reason to suspect that the immediate effects of teacher strikes on student acheivement may differ by strike duration given the negative effects of lost instructional time. Prior literature has found that very long strikes cause major declines in student outcomes due in part to labor disruptions amongst parents, and that strikes lasting 10 days or more cause small declines in achievement in some grades (Jaume and Willén 2019; 2021; Baker 2013; Belot and Webbink 2010; Johnson 2011). However, in our context, less than 10% of teacher strikes last 10 days or more, suggesting that the differences between our findings and previously estimated strike effects may be due to their differing duration.

To explore this, we re-estimate our preferred models using a dosage model where we subset the data to include only treated districts that went on strike for either less than 10 days (two weeks) or two weeks or more. In Figure 8, we show evidence that strikes lasting two weeks or more are associated with immediate declines in student achievement in math in the year of the

strike and the subsequent year (see Panel A). Estimates for reading reveal no negative shocks to achievement in the year of the strike but a similar, albeit imprecise, negative estimate in the following year (see Panel B).[11] These dosage analyses are suggestive of a negative causal relationships between lost learning time and student acheivement, but we cannot rule out other factors correlated with strike length that may be driving these patterns.

VII. Where and When Are Public Sector Strikes Most Effective?

Our results above document the average effect of strikes across a range of diverse contexts. Here, we conduct an exploratory, descriptive examination of where strikes are most effective. Specifically, we examine potential heterogeneity in strike effects depending on a district's relative expenditures and its political contexts. We conduct these tests using state and district contextual factors measured pre-strike. These analyses inform our understanding of key strike mechanisms as well as the generalizability and policy relevance of our findings. We focus our discussion on teacher compensation and report results on tests for heterogeneous effects on working conditions and achievement in Appendix Figures B11-13.

A. District Relative Expenditures

We hypothesize strikes are more effective in districts that spend less on school inputs and teacher wages relative to other districts in their state. Teachers may be able to make a more compelling case to both policymakers and the public if district spending levels are low compared to other districts in the state. Here, the threat of teachers leaving for higher wages in other districts in the state without having to obtain a new teacher license may also be more credible.

[11] In supplemental analyses, we find no evidence that short and long strikes have differential effects on compensation and working conditions, though we do find suggestive evidence that effects on pupil-teacher ratios are driven by short strikes. See Appendix Figure B10.

Descriptively, districts have similar strike rates (~4.7%) regardless of whether they have relatively high expenditures or salaries within their state at baseline.

We first investigate whether strike impacts vary based on initial 2008 per-pupil expenditures relative to the rest of the state, categorized as high or low for districts above and below the median expenditures per pupil in their state in the baseline year. We do the same with baseline 2008 teacher salaries relative to public school teachers in the rest of the state. In Figure 9, we find that teacher strikes lead to larger spending increases in districts that were initially lower spending and had lower salaries at baseline. To be clear, average teacher salaries increase in both high and low spending districts; however, for both spending and salaries, all point estimates are higher for the initially lower spending and lower salary districts.

B. Political Contexts

Finally, we hypothesize that strikes will be more effective in states where the political context is more favorable to unions. As noted in the conceptual framework above, how strikes are received by the public shapes the extent to which teachers are able to achieve wins at the bargaining table or in broader education politics. The political climate in a state might affect how families, the public, district leadership, and policymakers perceive and respond to teacher strikes.

We operationalize the favorability of the state political context in two ways. First, we examine whether the effect of strikes on compensation varies by whether teacher strikes are legal or prohibited by law. We expect that teacher strikes will be more effective in states where they are legal, though it is possible that the riskiness of illegal strikes may actually make them more effective tools for influencing educational policy and politics. Furthermore, illegality is an imperfect deterrant of teacher strikes, as the severity of consequences vary substantially across

states and are not always enforced.[12] We find that teacher strikes are only effective at increasing compensation when they occur in states where teacher striking is prohibited by state law (Figure 9). Compensation rises by as much as 17% four years after a strike in states where strikes are illegal, relative to small, negatively signed estimated effects for strikes in states where such actions are legal for teachers. This pattern may reflect a selection mechanism where strike legality changes the threshold for a union's willingness to go on strike. Baseline relative salaries and expenditures are 4% and 6% lower respectively in districts that go on strike where it is illegal compared to where striking is legal.

State partisan context and culture may also shape how the public and political leaders respond to teacher strikes. Teachers' unions have long allied themselves with the Democratic party, and we expect that teacher strikes would be more effective in states that lean Democratically given that the public views protests through ideological lenses (Gillion 2020). In our panel, 55.5% of strikes were in Republican states. We find in Panel B that teacher strikes cause increases in teacher compensation in both Republican and Democratic leaning states based on the vote share for the Republican presidential candidate George W. Bush in 2004 (the last presidential election prior to the start of our panel). However, we see that these increases are quicker, smaller, and more short-lived when they occur in states that voted for Bush in the 2004 presidential elections, whereas compensation increases more monotonically and to a larger degree over time in states that voted for John Kerry.

These exploratory analyses suggest that strikes are most impactful when they occur in states where strikes are illegal. Strikes also vary considerably in how they affect compensation depending on partisan context: teacher strikes in more conservative states lead to quicker and

[12] For a deeper discussion of strike legality, see Appendix C.

more temporary effects—persisting for roughly the length of a single contract term—whereas strikes in more liberal states have longer-term effects, perhaps a result of public signaling and other political effects.

VIII. Robustness

A. Testing Parallel Trends through Alternative Comparison Groups

In our preferred model, we use the Callaway and Sant'Anna estimator that applies outcome regression and IPW to build a "doubly robust" weighted composition of the comparison group. Here we present results from alternative specifications of the comparison group to further test the robustness of our parallel trends assumption. We start by implementing the synthetic DiD (SDiD) approach developed by Arkhangelsky and colleagues (2021). This complements the propensity-score IPW approach incorporated into the Callaway and Sant'Anna estimator by reweighting estimates explicitly based on pre-treatment trends for outcome measures across the treatment and comparison group, instead of the estimated propensity to be in the treatment group.[13] However, SDiD is not our preferred approach because it requires dropping a substantial amount of data to achieve a strongly balanced panel to implement in the process recently outlined by Arkhangelsky and colleagues (2021). Using the SDiD approach, the trends prior to the strike in treated and untreated district are unequivocally flat and near-zero. After the strike, we find very consistent results, with estimated effects on teacher salary that are even larger than our preferred estimate, reaching 10% in the fifth year after a strike (see Figure 10, Panel A). We continue to find a reduction in class size, though it is attenuated and estimated less precisely in

[13] To refine the comparison group further, we also examine synthetic DiD specifications that include state-by-year fixed effects that account for state-level shocks including policy changes, economic conditions, or demographic shifts that could confound strike effects. We do these only for individual district strikes because of the small number of control units within state-years for coordinated strikes. In the case of West Virginia, for example, all districts in the state were involved in the strike. Results are in Appendix Figure B14.

the fourth and fifth years after a strike. We continue to find no average effects on test scores (see Panels B-D).

Next, we explore two other approaches to restricting our comparison groups that serve as plausible bounds for any potential spillover effects of the treatment on comparison units. It is possible that the effects of an individual strike may be felt by surrounding districts, attenuating the treatment-control contrast through three mechanisms: (a) states increase spending on K-12 education across the board, (b) school boards in close proximity to a striking district may more directly fear something similar could happen in their districts and proactively take action to avoid strikes, (3) a strike in one district might also increase the general credibility of the threat to strike in other districts. We estimate the upper and lower bounds of such spillover effects in Appendix Table B8.

First, we limit comparison districts to those only in states that never experienced a strike in our panel. This comparison group is less likely to be contaminated by spillover. Indeed, our estimated effects increase in magnitude when we limit comparison districts to those outside of striking states. Specifically, we find an 8% increase in teacher compensation and an average decrease of 0.54 students in pupil-teacher ratios after 5 years. We also see some evidence that teacher strikes positively impact achievement once we drop potential spillover districts. This suggests that our preferred estimates may understate the full effect of strikes, given spillover to neighboring districts.

Second, we limit our comparison group to include only districts in striking states to estimate lower bound effects. We continue to find a positive and significant effect of teacher strikes on compensation, though the magnitude is reduced to 3% in the fifth year after a strike. Estimated effects on working conditions tend to be larger but noisier than the main specification.

Effects on achievement continue to be close to zero and do not reach standard levels of statistical significance. This suggests that strikes cause smaller but still meaningful compensation increases even when compared to other districts that experience some positive spillover effects of the strike.

B. Sensitivity Analyses

We test the robustness of our findings to model specification choices regarding covariates and statistical inference in a range of ways and display the results in Appendix Figures B3-B6. For one, we address potential concerns that strikes may be associated with the composition of students by controlling for additional school sociodemographic characteristics: the percentage of students who are English Language Learners, have special needs, live in urban, suburban, and town or rural areas, and are Black, Hispanic, White, or Asian (with other races as the uncoded comparison group). We also show results with no covariates, removing the enrollment and local labor market control variables from our preferred specification. Third, the inclusion of coordinated strikes in our sample raises questions about the most appropriate way to model the non-independence of these clusters of collaborating districts when estimating our standard errors. We therefore show that our results are consistent when we only examine independent strikes that take place in a single district. We also show that results are extremely robust to dropping Great Recession (2006-2007 to 2008-2009) years and to dropping Covid-19 pandemic years (2020-2021 to 2022-2023). Next, we recognize that one in ten strikes was motivated by labor rights (see Table 1), such as in Wisconsin in 2011 when teachers went on strike to oppose Act 10 (Baron 2018; Biasi 2021; Biasi and Sarsons 2022; Roth 2019). We therefore drop districts that experienced those Wisconsin strikes and all other strikes motivated by labor policy concerns. Finally, a large portion of the strikes in our sample took place in 2018 and 2019, so we show

results that drop all districts with strikes in those years. Across all tests, estimated effects and standard errors are nearly identical to our preferred specification across all four outcomes.

C. Multiple Events

Multiple events are fairly common in our panel, with 121 of the 592 striking districts experiencing multiple strikes. Though this is frequently an issue in DiD analyses, a generally accepted strategy for dealing with multiple events in DiD analyses does not exist, and prior literature has tended to favor simple and objective methods for dealing with this challenge (Lafortune et al. 2018; Lyon and Kraft 2024). In our preferred specification, we focus on the first strike in a given district. In Appendix Table B1, we confirm that our results are consistent when we model the full set of strikes across our panel. We create a copy of each school district experiencing multiple events for each strike event after the first one, estimate the effect of each strike separately, and then aggregate all of these effects with weights to correct for overrepresentation of districts with multiple strikes. We replace the district fixed effects with district-by-event fixed effects and continue to include year fixed effects and covariates, as described above. Effects are substantively unchanged when we use this approach, suggesting that subsequent strikes within a district have similar effects to earlier ones.

IX. Discussion

Using an original dataset of 745 U.S. teacher strikes between 2007 and 2024, we catalog where and why strikes take place. We then examine how they affect compensation, working conditions, and productivity. Strikes have occurred across the U.S. in both Republican and Democratic states. The overwhelming majority of strikes occur to secure higher compensation. Our causal analysis shows that, on average, teacher strikes achieve this goal during the period we study. We estimate that teacher strikes lead to a 6% increase in annual teacher compensation, on

average, by the fifth year after a strike, which corresponds to an increase of $7,629 in combined salary and benefits that year (in real $2018). We find no immediate or sustained effects of strikes on student achievement in math or reading on average, an unsurprising finding given the modal teacher strike is in the U.S. is only two days.

If strikes effectively raise compensation in the K-12 education sector, then why don't teachers strike more often? Four plausible explanations include 1) the threat of strikes, 2) costly negative spillovers of strikes, 3) organizational time costs, and 4) strike illegality with strong consequences. It is possible that unions have found an optimal equilibrium where some minimum number of actual strikes creates a sufficient credible threat such that not every union that votes to authorize a strike needs to enact it to achieve their bargaining goals. Strikes are divisive acts that and divide communities and may bring reputational harm to teachers. Teachers also rely on the support of the community directly, via family cooperation, parent-teacher organizations that often raise funding for schools and teachers, as well as indirectly via the politics of elected school board members. Third, strikes require considerable time and effort to organize. Strikes are illegal in 37 states, and coordinating large groups of teachers—public service professionals—to act illegally is likely difficult. Perhaps because of the illegal nature of strikes in so many states, 23 states have never experienced a strike.

Our findings also have some clear policy implications. For one, we find that strikes often occur in states where they are illegal. In illegal strikes, organizers often attempt to mobilize such a large-scale collective action that imposing legal penalties (such as firing all teachers and/or revoking their licenses) becomes untenable. Teachers and their unions can sometimes negotiate away penalties in the context of their collectively bargained agreements (e.g., agreeing that striking teachers will not be fined), or via other less formal agreements ending a teacher strike if

collective bargaining is not in place. These practices are reflective of the maxim among labor organizers that "there is no such thing as an illegal strike, only an unsuccessful one" (Reddy 2021; see Appendix C for more details on strike legality). Indeed, teacher strikes in states where they are illegal tend to have larger effects on compensation, suggesting that strike illegality is not always a strong deterrent. In short, many strike prohibitions—often enacted alongside collective bargaining expansions in the 1960s and 70s—are not serving their intended function. From a policy perspective, it is likely that the penalties for illegal strikes (and the feasibility of enacting them) are more salient than whether strikes are legal or not.

Second, our exploratory dosage analysis also demonstrates that atypically long strikes (those leading to the loss of weeks of instructional time) have negative consequences for students. We find that strikes lasting 10 or more days lead to reductions in math achievement in the year of and immediately following the strike. Notably, only 10% of teacher strikes in our dataset lasted 10 days or more, the threshold at which both our study and prior literature start to detect a small, negative effect (Baker 2013; Johnson 2011; Belot and Webbink 2010; Jaume and Willén 2019). Although other factors correlated with strike duration could explain this pattern, these findings suggest that policymakers could focus more acutely on how they address atypically long strikes. Policy solutions could include graduated enforcement mechanisms where financial penalties escalate proportionally to the duration of a work stoppage or compulsory mediation after strikes exceed a predetermined threshold (e.g., 10 calendar days). In short, policymakers might consider moving beyond unenforceable, punitive approaches toward more nuanced, context-sensitive dispute resolution mechanisms.

X. Conclusion

The modern labor movement in the U.S. has faced substantial challenges since the pivotal decision of President Reagan to break the PATCO strike (Massenkoff and Wilmers 2024). Unionization among private sector workers has been in decline for decades, while public sector unions have seen their membership grow but their overall coverage begin to erode (Naidu 2022). Our analyses demonstrate that despite the limited success of labor actions in the two decades following the PATCO strike, more recent teacher strikes have been a potent form of leverage for achieving compensation gains in the public sector.

References

Akerlof, George A. 1982. “Labor Contracts as Partial Gift Exchange.” *The Quarterly Journal of Economics* 97 (4): 543–69. https://doi.org/10.2307/1885099.

Angrist, Joshua D., and Victor Lavy. 1999. “Using Maimonides’ Rule to Estimate the Effect of Class Size on Scholastic Achievement.” *The Quarterly Journal of Economics* 114 (2): 533–75.

Arkhangelsky, Dmitry, Susan Athey, David A. Hirshberg, Guido W. Imbens, and Stefan Wager. 2021. “Synthetic Difference-in-Differences.” *American Economic Review* 111 (12): 4088–118. https://doi.org/10.1257/aer.20190159.

Baker, Michael. 2013. “Industrial Actions in Schools: Strikes and Student Achievement.” *Canadian Journal of Economics/Revue Canadienne d’économique* 46 (3): 1014–36. https://doi.org/10.1111/caje.12035.

Baron, E. Jason. 2018. “The Effect of Teachers’ Unions on Student Achievement in the Short Run: Evidence from Wisconsin’s Act 10.” *Economics of Education Review* 67 (December): 40–57. https://doi.org/10.1016/j.econedurev.2018.09.003.

Belot, Michèle, and Dinand Webbink. 2010. “Do Teacher Strikes Harm Educational Attainment of Students?” *LABOUR* 24 (4): 391–406. https://doi.org/10.1111/j.1467-9914.2010.00494.x.

Biasi, Barbara. 2021. “The Labor Market for Teachers under Different Pay Schemes.” *American Economic Journal: Economic Policy* 13 (3): 63–102. https://doi.org/10.1257/pol.20200295.

Biasi, Barbara, and Heather Sarsons. 2022. “Flexible Wages, Bargaining, and the Gender Gap*.” *The Quarterly Journal of Economics* 137 (1): 215–66. https://doi.org/10.1093/qje/qjab026.

Blanc, Eric. 2019. *Red State Revolt: The Teachers’ Strike Wave and Working-Class Politics*. Verso.

Britton, Jack, and Carol Propper. 2016. “Teacher Pay and School Productivity: Exploiting Wage Regulation.” *Journal of Public Economics* 133 (January): 75–89. https://doi.org/10.1016/j.jpubeco.2015.12.004.

Brunner, Eric, Joshua Hyman, and Andrew Ju. 2019. “School Finance Reforms, Teachers’ Unions, and the Allocation of School Resources.” *The Review of Economics and Statistics*, 1–47. https://doi.org/10.1162/rest_a_00828.

Bureau of Labor Statistics. 2018. *Work Stoppages*. Translated by United States Department of Labor.

Burgess, Simon, Ellen Greaves, and Richard Murphy. 2022. “Deregulating Teacher Labor Markets.” *Economics of Education Review* 88 (June): 102253. https://doi.org/10.1016/j.econedurev.2022.102253.

Caldwell, William E., and Loretta M. Jeffreys. 1983. “The Effect of Teacher Strikes on Student Achievement: New Evidence.” *Government Union Review* 4 (1): 40–58.

Callaway, Brantly, and Pedro H. C. Sant’Anna. 2020. “Difference-in-Differences with Multiple Time Periods.” *Journal of Econometrics*, ahead of print, December 17. https://doi.org/10.1016/j.jeconom.2020.12.001.

Card, David. 1990. “Strikes and Bargaining: A Survey of the Recent Empirical Literature.” *American Economic Review* 80 (2): 410–15.

Card, David, and Craig A. Olson. 1995. “Bargaining Power, Strike Durations, and Wage Outcomes: An Analysis of Strikes in the 1880s.” *Journal of Labor Economics* 13 (1): 32–61. https://doi.org/10.1086/298367.

Caughey, Devin, and Christopher Warshaw. 2018. “Policy Preferences and Policy Change: Dynamic Responsiveness in the American States, 1936–2014.” *The American Political Science Review* 112 (2): 249–66. http://dx.doi.org/10.1017/S0003055417000533.

Census of Governments. 2017. “2017 CENSUS OF GOVERNMENTS SURVEY OF SCHOOL SYSTEM FINANCES.” https://nces.ed.gov/ccd/pdf/17f33.pdf.

Chetty, Raj, John N. Friedman, Nathaniel Hilger, Emmanuel Saez, Diane Whitmore Schanzenbach, and Danny Yagan. 2011. “How Does Your Kindergarten Classroom Affect Your Earnings? Evidence from Project Star.” *The Quarterly Journal of Economics* 126 (4): 1593–660. https://doi.org/10.1093/qje/qjr041.

Cook, Jason, Stéphane Lavertu, and Corbin Miller. 2021. “Rent-Seeking through Collective Bargaining: Teachers Unions and Education Production☆.” *Economics of Education Review* 85 (December): 102193. https://doi.org/10.1016/j.econedurev.2021.102193.

Cowen, Joshua M., and Katharine O. Strunk. 2015. “The Impact of Teachers’ Unions on Educational Outcomes: What We Know and What We Need to Learn.” *Economics of Education Review* 48 (October): 208–23. https://doi.org/10.1016/j.econedurev.2015.02.006.

Cramton, Peter C., and Joseph S. Tracy. 1992. “Strikes and Holdouts in Wage Bargaining: Theory and Data.” *The American Economic Review* 82 (1): 100–121. JSTOR.

De Ree, Joppe, Karthik Muralidharan, Menno Pradhan, and Halsey Rogers. 2018. “Double for Nothing? Experimental Evidence on an Unconditional Teacher Salary Increase in Indonesia*.” *The Quarterly Journal of Economics* 133 (2): 993–1039. https://doi.org/10.1093/qje/qjx040.

Edwards, Danielle Sanderson, Matthew A. Kraft, Alvin Christian, and Christopher A. Candelaria. 2024. “Teacher Shortages: A Framework for Understanding and Predicting Vacancies.” *Educational Evaluation and Policy Analysis*, April 1, 01623737241235224. https://doi.org/10.3102/01623737241235224.

Elementary and Secondary Education Expenditures. 2021. Urban Institute. https://www.urban.org/policy-centers/cross-center-initiatives/state-and-local-finance-initiative/state-and-local-backgrounders/elementary-and-secondary-education-expenditures.

Freeman, Richard B., and James L. Medoff. 1984. *What Do Unions Do?* Basic Books.

Ghilarducci, Teresa. 2015. “Unions Are Basically Dead and That’s Really Bad.” Business. *The Atlantic*, October 28. https://www.theatlantic.com/business/archive/2015/10/unions-are-basically-dead/412831/.

Gillion, Daniel Q. 2020. *The Loud Minority: Why Protests Matter in American Democracy*. Princeton University Press. www-degruyter-com.libproxy.albany.edu. https://www-degruyter-com.libproxy.albany.edu/document/doi/10.1515/978 0691201726/html.

Hendricks, Matthew D. 2014. “Does It Pay to Pay Teachers More? Evidence from Texas.” *Journal of Public Economics* 109 (January): 50–63. https://doi.org/10.1016/j.jpubeco.2013.11.001.

Hertel-Fernandez, Alexander, Suresh Naidu, and Adam Reich. 2021. “Schooled by Strikes? The Effects of Large-Scale Labor Unrest on Mass A Ttitudes toward the Labor Movement.”

Perspectives on Politics 19 (1): 73–91. DOI.org (Crossref). https://doi.org/10.1017/S1537592720001279.
Hirsch, Barry, David Macpherson, and William Even. 2024. "Union Membership and Coverage Database from the CPS (Unionstats.Com)." unionstats.com.
Hoxby, Caroline M. 1996. "How Teachers' Unions Affect Education Production." *The Quarterly Journal of Economics* 111 (3): 671–718.
Hoxby, Caroline M. 2000. "The Effects of Class Size on Student Achievement: New Evidence from Population Variation." *The Quarterly Journal of Economics* 115 (4): 1239–85.
Jackson, C. Kirabo, Rucker C. Johnson, and Claudia Persico. 2015. "The Effects of School Spending on Educational and Economic Outcomes: Evidence from School Finance Reforms *." *The Quarterly Journal of Economics* 131 (1): 157–218. https://doi.org/10.1093/qje/qjv036.
Jackson, C. Kirabo, and Claire L. Mackevicius. 2024. "What Impacts Can We Expect from School Spending Policy? Evidence from Evaluations in the United States." *American Economic Journal: Applied Economics* 16 (1): 412–46. https://doi.org/10.1257/app.20220279.
Jaume, David, and Alexander Willén. 2019. "The Long-Run Effects of Teacher Strikes: Evidence from Argentina." *Journal of Labor Economics* 37 (4): 1097–139. journals.uchicago.edu (Atypon). https://doi.org/10.1086/703134.
Jaume, David, and Alexander Willén. 2021. "The Effect of Teacher Strikes on Parents." *Journal of Development Economics* 152: 102679.
Johnson, David R. 2011. "Do Strikes and Work-to-Rule Campaigns Change Elementary School Assessm Ent Results?" *Canadian Public Policy* 37 (4): 479–94. https://doi.org/10.3138/cpp.37.4.479.
Johnston, Andrew C. 2025. "Preferences, Selection, and the Structure of Teacher Pay." *American Economic Journal: Applied Economics*, ahead of print. https://doi.org/10.1257/app.20210763.
Kraft, Matthew A., and Sarah Novicoff. 2024. "Time in School: A Conceptual Framework, Synthesis of the Causal Research, and Empirical Exploration." *American Educational Research Journal*, May 30, 00028312241251857. https://doi.org/10.3102/00028312241251857.
Krueger, Alan B. 1999. "Experimental Estimates of Education Production Functions*." *The Quarterly Journal of Economics* 114 (2): 497–532. https://doi.org/10.1162/003355399556052.
Krueger, Alan B., and Alexandre Mas. 2004. "Strikes, Scabs, and Tread Separations: Labor Strife and the Production of Defective Bridgestone/Firestone Tires." *Journal of Political Economy* 112 (2): 253–89. https://doi.org/10.1086/381479.
Lafortune, Julien, Jesse Rothstein, and Diane Whitmore Schanzenbach. 2018. "School Finance Reform and the Distribution of Student Achievement." *American Economic Journal: Applied Economics* 10 (2): 1–26. DOI.org (Crossref). https://doi.org/10.1257/app.20160567.
Lovenheim, Michael F. 2009. "The Effect of Teachers' Unions on Education Production: Evidence from Union Election Certifications in Three Midwestern States." *Journal of Labor Economics* 27 (4): 525–87. https://doi.org/10.1086/605653.

Lovenheim, Michael F., and Alexander Willén. 2019. "The Long-Run Effects of Teacher Collective Bargaining." *American Economic Journal: Economic Policy* 11 (3): 292–324. https://doi.org/10.1257/pol.20170570.
Lovison, Virginia S., and Cecilia Hyunjung Mo. 2024. "Investing in the Teacher Workforce: Experimental Evidence on Teachers' Preferences." *American Educational Research Journal* 61 (1): 108–44. https://doi.org/10.3102/00028312231208956.
Lyon, Melissa Arnold, and Matthew A. Kraft. 2024. "Teacher Strikes as Public Signals." *Journal of Human Resources*, April 8, 0722-12437R2. https://doi.org/10.3368/jhr.0722-12437R2.
Lytle, James H., and Jay M. Yanoff. 1973. "The Effects (If Any) of a Teacher Strike on Student Achievement." *The Phi Delta Kappan* 55 (4): 270–270.
Mas, Alexandre. 2008. "Labour Unrest and the Quality of Production: Evidence from the Construction Equipment Resale Market." *The Review of Economic Studies* 75 (1): 229–58. https://doi.org/10.1111/j.1467-937X.2007.00461.x.
Massenkoff, Maxim, and Nathan Wilmers. 2024. "Economic Outcomes of Strikers in an Era of Weak Unions." *Journal of Labor Economics* 42 (1): 25–51. https://doi.org/10.1086/722743.
Mulhern, Christine. 2023. "Beyond Teachers: Estimating Individual School Counselors' Effects on Educational Attainment." *American Economic Review* 113 (11): 2846–93. https://doi.org/10.1257/aer.20200847.
Murnane, Richard J., and David K. Cohen. 1985. *Merit Pay and the Evaluation Problem: Understanding Why Most Merit Pay Plans Fail and a Few Survive*. https://eric.ed.gov/?id=ED270842.
Naidu, Suresh. 2022. "Is There Any Future for a US Labor Movement?" *Journal of Economic Perspectives* 36 (4): 3–28. https://doi.org/10.1257/jep.36.4.3.
National Education Association. 2019. *State Teacher Salary Benchmark Data Highlights for 2017-2018*. https://www.nea.org/sites/default/files/2020-06/2017-2018%20Teacher%20Benchmark%20Data.pdf.
Noel, Amber M. 2010. "Documentation for the NCES Common Core of Data Teacher Compensation Survey (TCS) Agency-Level Public-Use Data File, School Year 2006–07." https://nces.ed.gov/ccd/pdf/tcs_lea_2006071a_gen.pdf.
Reardon, Sean F., A.D. Ho, B.R. Shear, E.M. Fahle, D. Kalogrides, and J. Saliba. 2024. "Stanford Education Data Archive (Version 5.0)." https://purl.stanford.edu/cs829jn7849.
Reddy, Diana S. 2021. "'There Is No Such Thing as an Illegal Strike': Reconceptualizing the Strike in Law and Political Economy." *The Yale Law Journal Forum* 130: 421.
Rosenfeld, Jake. 2006. "Desperate Measures: Strikes and Wages in Post-Accord America." *Social Forces* 85 (1): 235–65. JSTOR.
Roth, Jonathan. 2019. "Union Reform and Teacher Turnover: Evidence from Wisconsin's Act 10." Unpublished manuscript.
Sant'Anna, Pedro H.C., and Jun Zhao. 2020. "Doubly Robust Difference-in-Differences Estimators." *Journal of Econometrics* (LAUSANNE) 219 (1): 101–22. https://doi.org/10.1016/j.jeconom.2020.06.003.
Scott, Steven J. 1997. "Status Quo Doctrine: An Application to Salary Step Increases for Teachers." *Cornell Law Review* 83 (1).
Shapiro, Carl, and Joseph Stiglitz. 1984. "Equilibrium Unemployment as a Worker Discipline Device." *American Economic Review* 74 (3): 433–44.

Shores, Kenneth, and Matthew P. Steinberg. 2019. “Schooling During the Great Recession: Patterns of School Spending and Student Achievement Using Population Data.” *AERA Open* 5 (3): 2332858419877431. https://doi.org/10.1177/2332858419877431.

Thornicroft, Kenneth Wm. 1994. “Teacher Strikes and Student Achievement: Evidence from Ohio.” *Journal of Collective Negotiations in the Public Sector* 23 (1): 1–1. https://doi.org/10.2190/65X6-ETU0-3MQ5-27Q9.

Todd, Petra E., and Kenneth I. Wolpin. 2007. “The Production of Cognitive Achievement in Children: Home, School, and Racial Test Score Gaps.” *Journal of Human Capital* 1 (1): 91–136. JSTOR. https://doi.org/10.1086/526401.

Will, Madeline. 2022. “Long Hours, Second Jobs: New Federal Data Give a Snapshot of the Teaching Profession.” Teaching & Learning, Teaching Profession. *Education Week*, December 13. https://www.edweek.org/teaching-learning/long-hours-second-jobs-new-federal-data-give-a-snapshot-of-the-teaching-profession/2022/12.

Willén, Alexander. 2021. “Decentralization of Wage Determination: Evidence from a National Teacher Reform.” *Journal of Public Economics* 198 (June): 104388. https://doi.org/10.1016/j.jpubeco.2021.104388.

Tables and Figures

Table 1. Reported Reasons for Strikes, 2007-2024

	All	Coordinated	Individual
Teacher Compensation	89%	90%	86%
Teacher Salaries	80%	81%	80%
Teacher Benefits	28%	25%	33%
Working Conditions	56%	70%	29%
General School/Student Expenditures	45%	56%	21%
Non-Instructional Staff	17%	22%	7%
Labor Rights	11%	13%	5%
School/Classroom Infrastructure	0%	0%	1%
Common Good (e.g., Housing, Immigration)	11%	16%	1%
Other	54%	69%	24%
Total Strikes	*745*	*503*	*242*

Notes: Data are described at the strike level. Districts experiencing multiple strikes are observed multiple times. Strike reasons are not mutually exclusive. General School/Student Expenditures reasons include general expenditures, class size, or other student-focused demands. Other reasons range from ending teacher drug testing to the restoration of electives like art and music.

Table 2. Baseline Sociodemographic Characteristics of Striking and Non-Striking Districts (2007-8)

	Striking Districts	Non-Striking Districts	Difference
Average Teacher Compensation ($1,000s)	92.80	96.02	-3.21
Average Teacher Salaries ($1,000s)	69.08	72.33	-3.25
Average Teacher Benefits ($1,000s)	23.69	23.90	-0.22
Pupil-to-Teacher Ratio	16.48	15.86	0.63
Total Current Expenditures per Pupil ($1,000s)	10.73	13.07	-2.34
Instr. Salary and Benefit Expenditures per Pupil ($1,000s)	5.75	6.88	-1.13
Working Conditions Expenditures per Pupil ($1,000s)	4.25	5.74	-1.49
Noninstr. Salary and Benefit Expenditures per Pupil ($1,000s)	2.87	3.28	-0.41
Non-Salary and Benefit Expenditures per Pupil ($1,000s)	2.08	3.20	-1.12*
Capital Expenditures per Pupil ($1,000s)	1.34	1.51	-0.17
Total Revenues per Pupil ($1,000s)	12.55	16.49	-3.94
Total Student Enrollment (1,000s)	11.99	2.77	9.22***
% Special Education Students	13.83	14.12	-0.30
% English Language Learners	7.02	4.09	2.94***
% White Students	66.83	70.46	-3.63**
% Black Students	10.25	11.56	-1.31
% Hispanic Students	15.40	11.77	3.63***
% Asian/Pacific Islander Students	2.75	2.15	0.60**
% Native American Students	3.66	3.14	0.52
% Multi-Racial Students	0.24	0.48	-0.24+
% School Age Population in Poverty	0.17	0.16	0.01**
% Urban	19.64	12.59	7.05***
% Suburban	26.49	20.50	5.98***
% Town/Rural	53.87	66.91	-13.03***
Unemployment Rate	0.06	0.06	0.00***
Socioeconomic Status (SD)	-0.12	0.01	-0.13**
Number of Districts	555	14,895	15,450

Notes: All finance measures are in real 2018 dollars. Socioeconomic Status, Mass Economic Policy Preferences, and Mass Social Policy Preferences are standardized (z-scored). Data on district (state) characteristics from missing years are extrapolated at the district (state) level using linear trends within districts. Results without this imputation are presented in the appendix. Differences within states come from a series of OLS regressions with each characteristic regressed on a dichotomous indicator of whether there is ever a strike. *** Significant at the 0.1 percent level. ** Significant at the 1 percent level. * Significant at the 5 percent level. + Significant at the 10 percent level.

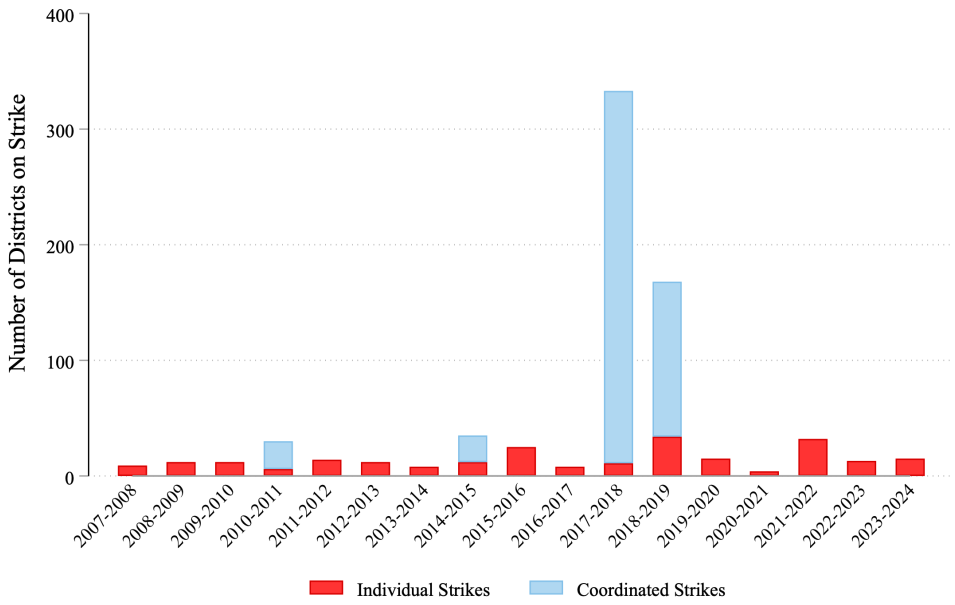


Figure 1. Number of Districts on Strike Over Time

Notes: Figure displays the number of strikes in each school year. Strike data are from the authors' compilation.

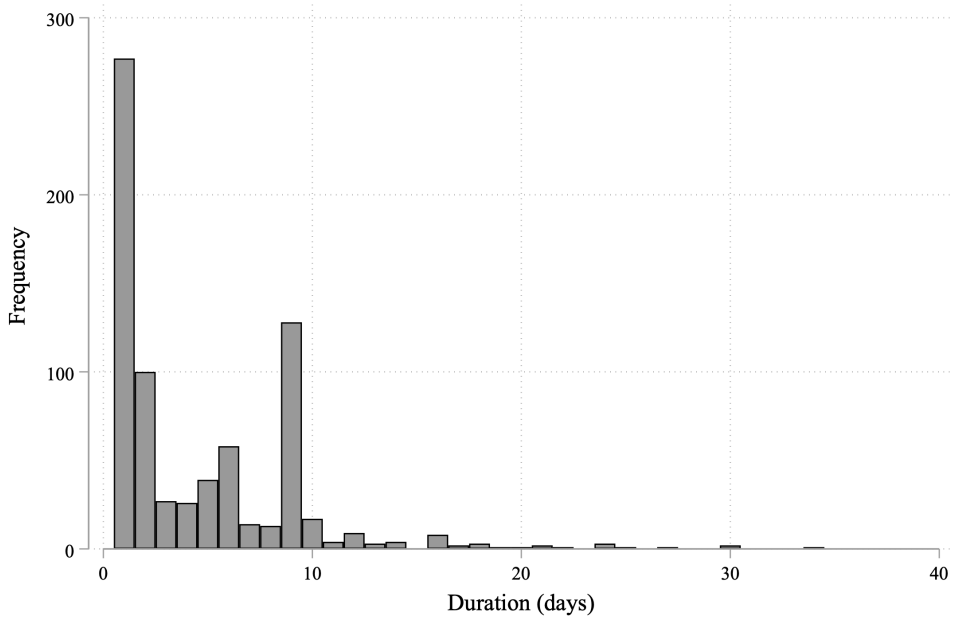


Figure 2. Count of Strikes by Duration

Notes: Figure displays the distribution of strikes by duration. Strike data are from the authors' compilation.

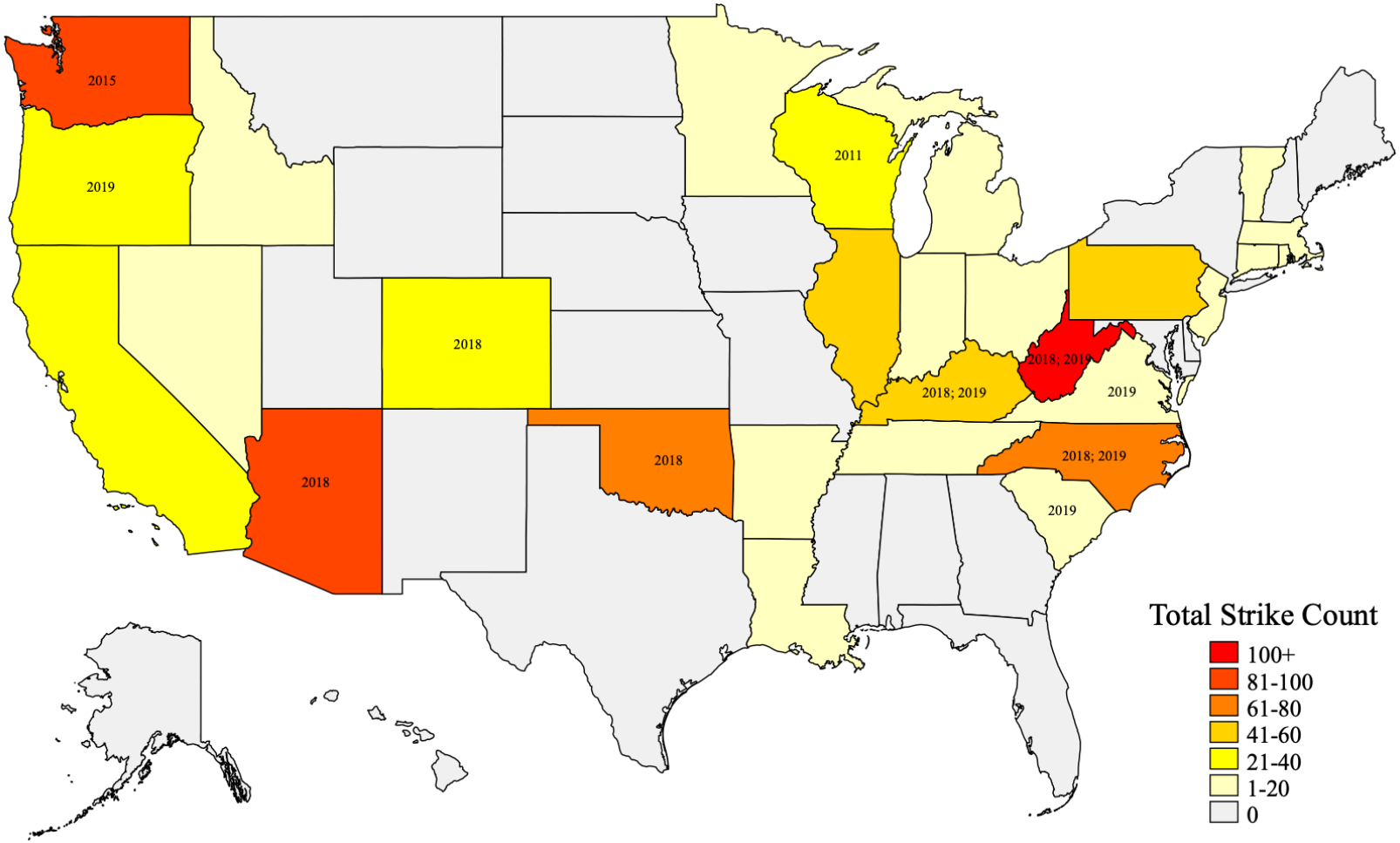


Figure 3. National Map of U.S. Teacher Strikes, 2007-2024

Notes: Data are from the authors' compilation. Strikes are counted at the district-event level. Dates indicate the year of a coordinated strike across at least some districts within the state. Data years range from the fall of 2007 to the spring of 2024.

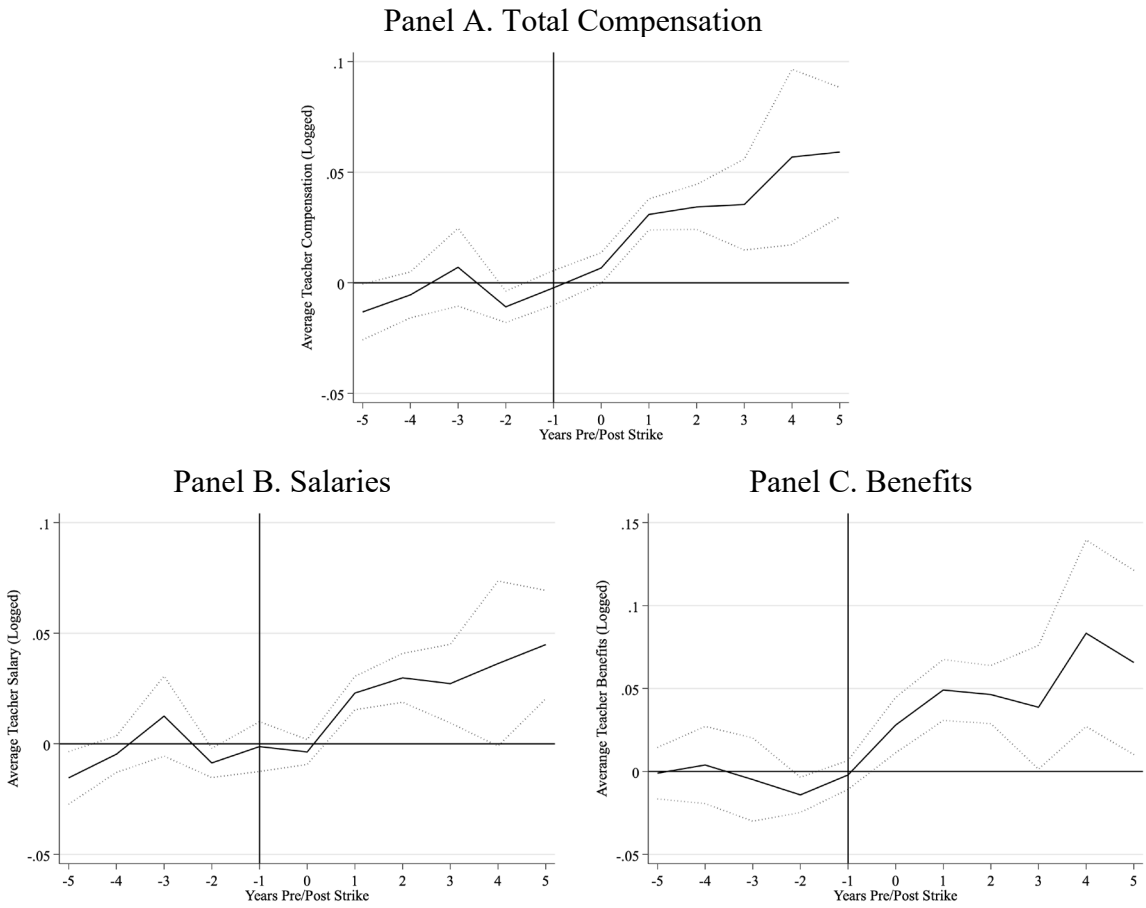
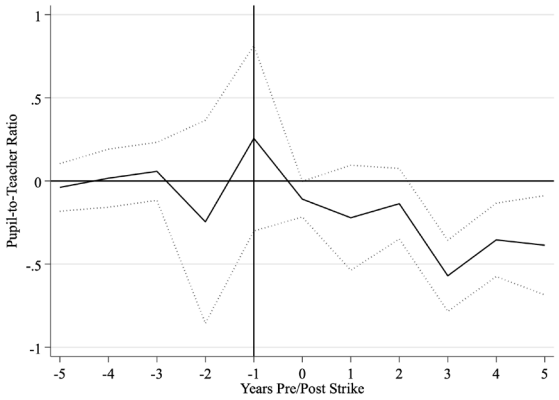


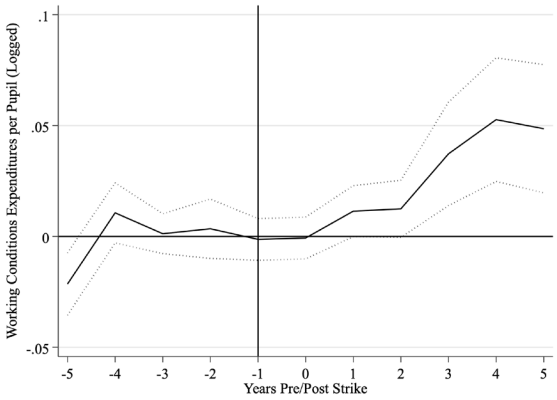
Figure 4. Effect of Strikes on Teacher Compensation

Notes: Solid line indicates the estimate from Equation 3 with school-district and school-year fixed effects. We use the specification recommended by Callaway and Sant'Anna (2020) to estimate an average treatment effect across distinct group-time estimates. Dotted line indicates the 95% confidence interval. Data are from the authors' compilation, as well as the NCES.

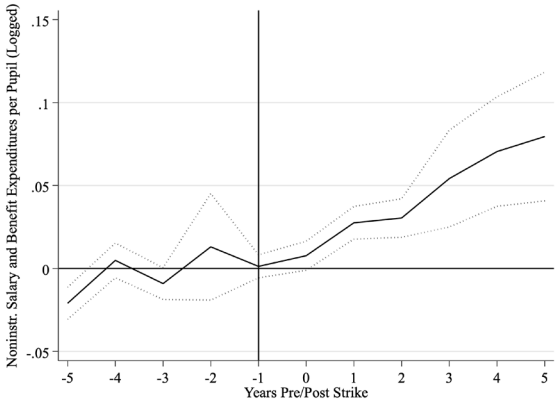
Panel A. Pupil-Teacher Ratios



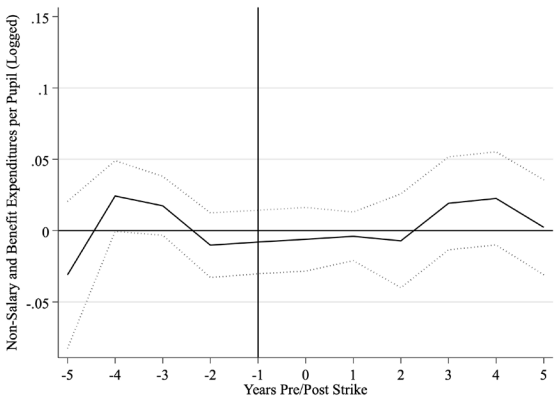
Panel B. Working Conditions Expenditures



Panel C. Noninstr. Salary and Benefit Expenditures



Panel D. Expenditures not on Salaries and Benefits



Panel E. Capital Expenditures

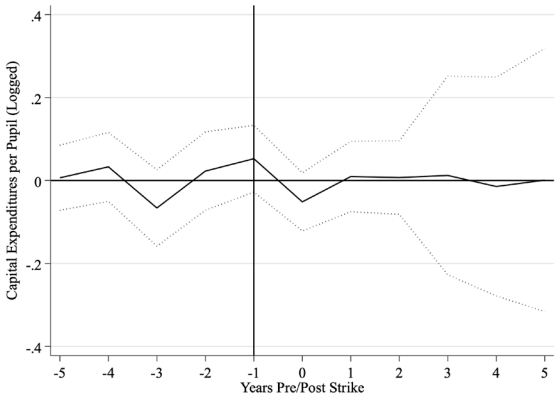
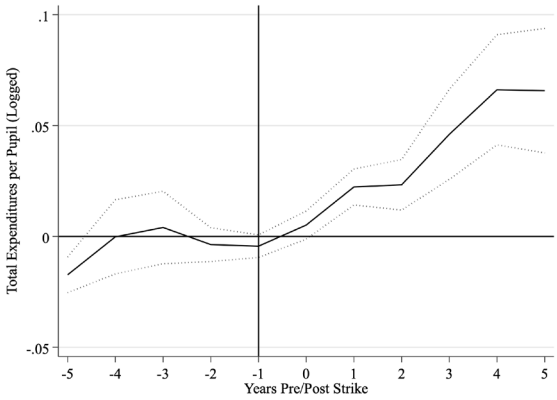


Figure 5. Effect of Strikes on Working Conditions

Notes: Solid line indicates the estimate from Equation 3 with school-district and school-year fixed effects. We use the specification recommended by Callaway and Sant'Anna (2020) to estimate an average treatment effect across distinct group-time estimates. Dotted line indicates the 95% confidence interval. Data are from the authors' compilation, as well as the NCE

Panel A. Total Expenditures per Pupil



Panel B. Total Revenues per Pupil

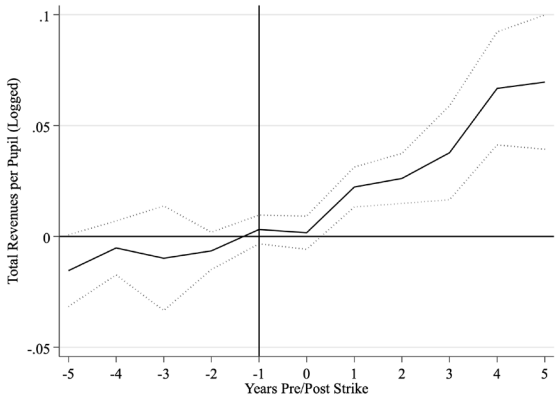
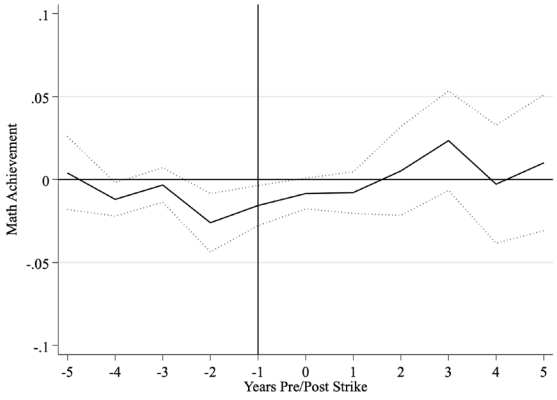


Figure 6. Effect of Strikes on Expenditures and Revenues

Notes: Solid line indicates the estimate from Equation 3 with school-district and school-year fixed effects. We use the specification recommended by Callaway and Sant'Anna (2020) to estimate an average treatment effect across distinct group-time estimates. Dotted line indicates the 95% confidence interval. Data are from the authors' compilation, as well as the NCES.

Panel A. Math Achievement



Panel B. Reading Achievement

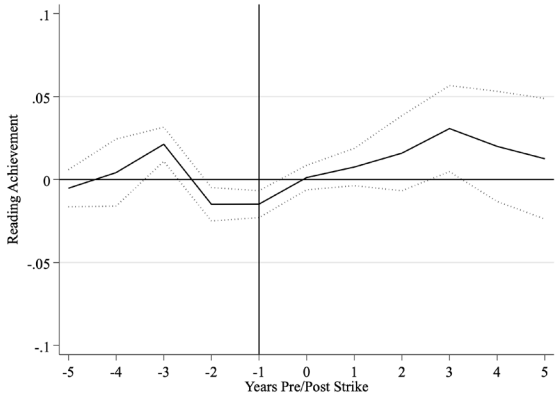


Figure 7. Effect of Strikes on Student Achievement

Notes: Solid line indicates the estimate from Equation 3 with school-district and school-year fixed effects. We use the specification recommended by Callaway and Sant'Anna (2020) to estimate an average treatment effect across distinct group-time estimates. Dotted line indicates the 95% confidence interval. Data are from the authors' compilation, as well as the SEDA 5.0

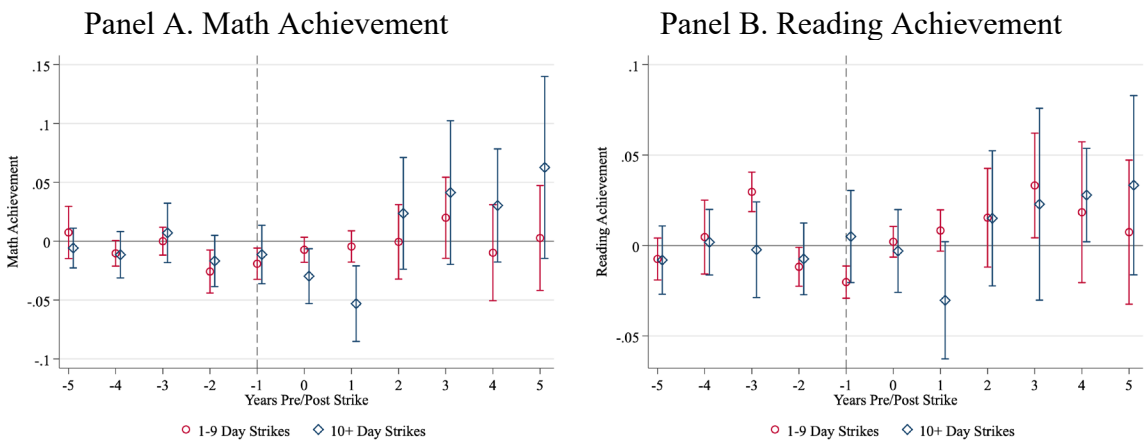


Figure 8. Dosage Effects on Achievement

Notes: To produce differential estimates by strike length, we subset the data to include only treated districts that went on strike for either less than two weeks (1-9 days) or two weeks or more (10+ days). For each analysis, we use the specification recommended by Callaway and Sant'Anna (2020) to estimate an average treatment effect across distinct group-time estimates. Capped line indicates the 95% confidence interval. Data are from the authors' compilation, as well as the NCES and the SEDA 5.0.

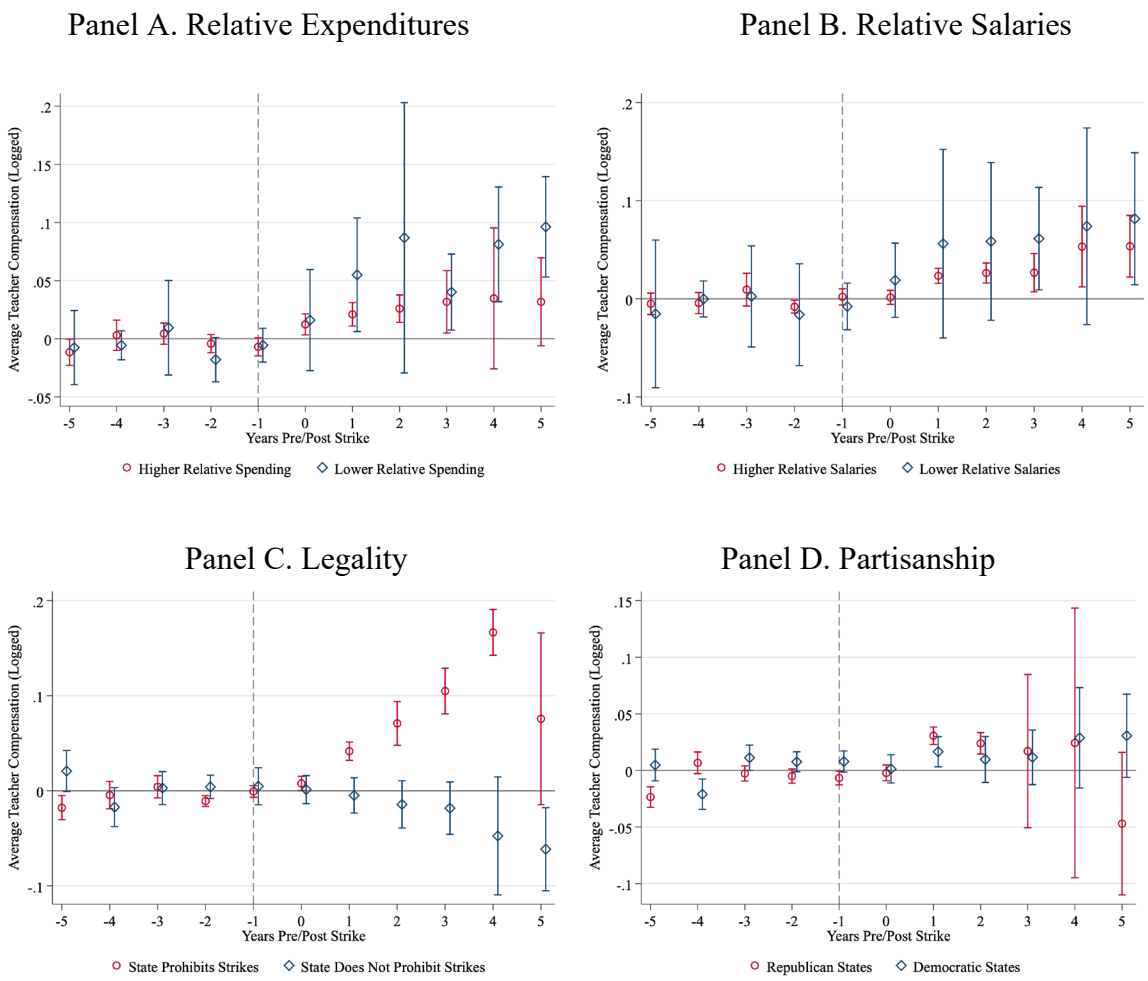


Figure 9. Heterogeneity

Notes: For each analysis, we use the specification recommended by Callaway and Sant'Anna (2020) to estimate an average treatment effect across distinct group-time estimates. Capped line indicates the 95% confidence interval. Data are from the authors' compilation, as well as the NCES and the SEDA 5.0. Expenditures and salaries relative to the rest of the state are categorized as higher or lower for districts above and below the median in their state in the baseline year, respectively. State partisanship is measured by vote share in the presidential election just prior to the start of our panel (2004).

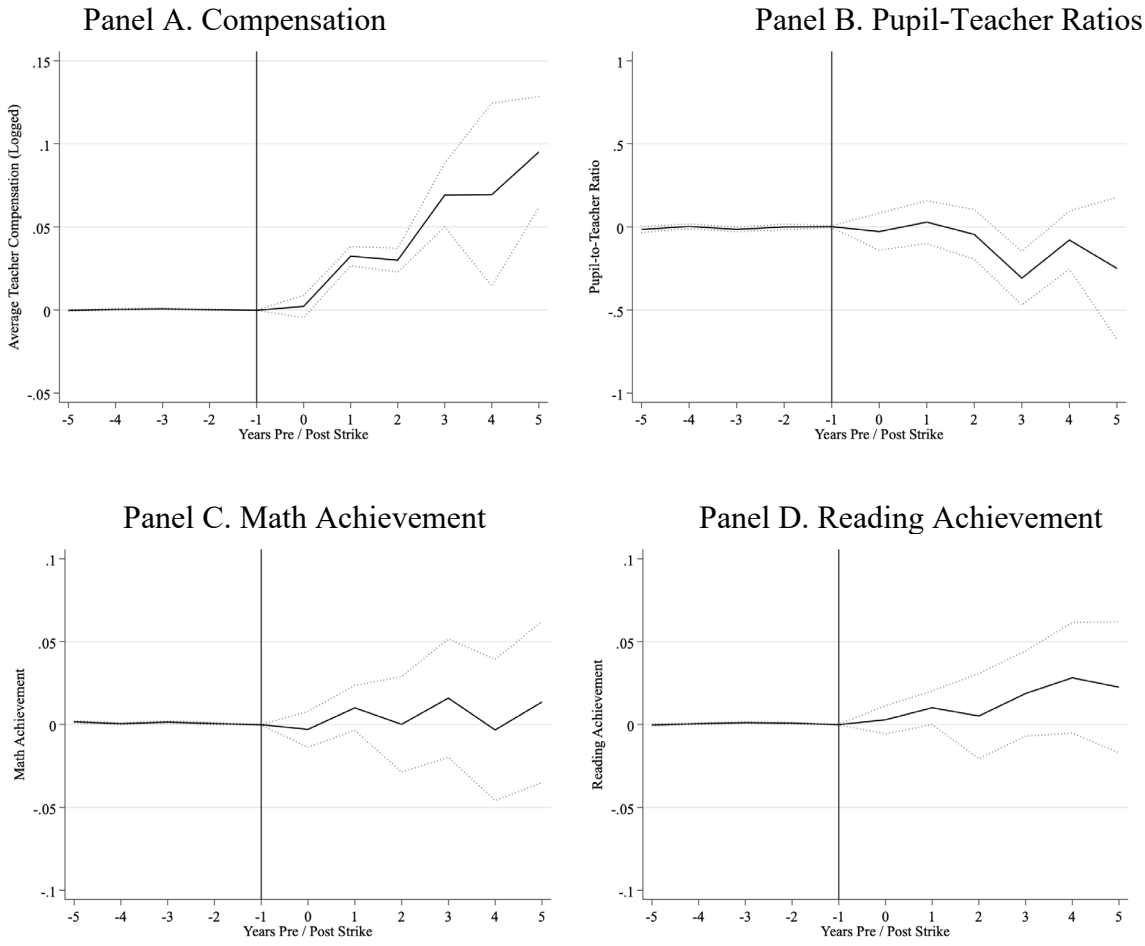


Figure 10. Effects of Strikes on Compensation, Working Conditions, and Productivity Using Synthetic Differences-in-Differences

Notes: For each analysis, we use the synthetic DiD specification recommended by Arkhangelsky et al. (2021) to estimate an average treatment effect across distinct group-time estimates with bootstrap inference. Results are identical with placebo inference. Capped line indicates the 95% confidence interval. Data are from the authors' compilation, as well as the NCES and the SEDA 5.0.

Appendix A. Data Collection Details

Our original search efforts involved three primary data collection approaches: (1) 186 Boolean searches on Google that produced over 42,500 news articles that our team reviewed, (2) 50 Boolean ProQuest searches of news documents producing roughly 43,500 news articles that our team reviewed, and (3) reviews of all NEA and AFT state affiliate websites at three points in time. We focused primarily on news sources because strikes are typically accompanied by press releases or news reporting of closed schools to inform parents not to send their children to school. We then validated and supplemented this search process with several additional sources, including administrative data from PA and IL, tracking by the Office of the Secretary Treasurer at the American Federation of Teachers (AFT), and Cornell University's publicly available labor action tracking since 2021.

First, our Google searches used the keyword "strike" with the News filter and the additional Tools to customize time ranges for each month and year from 7/1/2007 to 12/31/2022. A member of our research team read all search results until the content became no longer relevant to labor strikes. On average, each of these monthly searches produced 23 pages of relevant content with 10 articles per page, leading us to review over 42,500 articles. To review these articles, we focused first on the headline and short preview sentences to determine if the article was at all related to labor strikes. If so, a member of the research team read the entire article to look for information regarding teacher strikes. Second, in ProQuest, we searched News Documents between 7/1/2007 and 12/31/2022 using the term: "teacher strike" AND STATE. We read all headlines of search results for each state to determine if it was related to a strike we had not previously identified. Each year that we searched produced, on average, 2,900 articles to review, leading us to review roughly 43,500 articles. Third, we reviewed the NEA and AFT state

affiliate websites at three moments in time (in 2017, 2019, and 2022) to check for any documentation of strikes that we had not previously found.

We then cross-referenced and expanded our dataset with several additional sources. We collected administrative data from the states of PA (2007-8 through 2016-17; retrieved through Freedom of Information Requests) and IL (2010-2021; retrieved from public documentation of the Illinois Educational Labor Relations Board Annual Reports). We were also able to use these sources to validate our search process, suggesting that our search procedures described above uncovered at least 85% of strikes. We also obtained data on teacher strikes provided directly by the Office of the Secretary Treasurer at AFT, and we supplemented and cross-referenced our dataset with the Labor Action Tracker published by Cornell University's School of Industrial and Labor Relations, which has tracked strikes since 2021. We also reviewed the National Bureau of Labor Statistics for additional documentation of teacher strikes, though their efforts focus exclusively on strikes involving over 1,000 workers. We also reviewed the work stoppage information collected and published by the Federal Mediation and Conciliation Service (FMCS) set up in 1947, though we found very few public school teacher strikes reported, perhaps due to the lack of coverage for public sector workers under the National Labor Relations Act of 1935.

For each strike, we coded the school district, state, school year, start date, end date, duration (days), the primary data source, and the stated reason(s). We also coded whether each strike involved a single district (i.e., individual) or multiple districts coordinating within a state (i.e., coordinated). We determined stated reasons based on the news articles covering the strikes, grouping them into three categories: (i) compensation; (ii) working conditions; and (iii) common good. We define compensation as comprising demands related to salary and/or benefits, including healthcare, retirement, and time off. Working conditions include class size,

infrastructure and maintenance (e.g., new buildings, building new classrooms within old buildings, ventilation, and draining), noninstructional staff (e.g., counselors, nurses, and social workers), labor rights, and/or other general school expenditures not specifically related to salaries and benefits. Common good provisions are defined as out-of-school conditions that affect communities (e.g., housing affordability, broader social safety net protections, tax increases on higher incomes, Medicaid expansion). Though we were able to uncover an end date for every strike, we tried but could not gather information on how the strikes were resolved. Often, such information was not reported at all.

Appendix B. Appendix Tables and Figures

Appendix Table B1. Addressing Multiple Events

	(1)	(2)	(3)	(4)
Years Since Strike	Teacher Compensation (Logged)	Pupil-Teacher Ratios	Math Achievement	Reading Achievement
-5	-0.016***	-0.043	0.010	-0.005
	(0.004)	(0.065)	(0.006)	(0.004)
-4	-0.000	-0.012	-0.011*	0.009
	(0.004)	(0.060)	(0.005)	(0.006)
-3	0.001	0.042	-0.002	0.023***
	(0.004)	(0.082)	(0.005)	(0.005)
-2	-0.007**	-0.083	-0.020***	-0.011**
	(0.002)	(0.129)	(0.005)	(0.004)
-1	-0.004	0.133	-0.019***	-0.015***
	(0.003)	(0.153)	(0.005)	(0.004)
0	0.009**	-0.112*	-0.009	0.001
	(0.003)	(0.052)	(0.004)	(0.004)
1	0.031***	-0.170	-0.001	0.009
	(0.003)	(0.103)	(0.006)	(0.005)
2	0.039***	-0.203**	0.003	0.018
	(0.004)	(0.077)	(0.014)	(0.012)
3	0.035***	-0.547***	0.026	0.030*
	(0.010)	(0.079)	(0.015)	(0.013)
4	0.055**	-0.358***	-0.004	0.022
	(0.021)	(0.101)	(0.018)	(0.017)
5	0.055***	-0.347*	0.014	0.013

	(0.015)	(0.144)	(0.021)	(0.019)

Notes: + p<.10, * p<0.05, ** p<0.01, *** p<0.001. Robust standard errors clustered at the district level are in parentheses. We create a copy of each school district experiencing multiple events for each strike event after the first one, estimate the effect of each strike separately, and then aggregate all of these effects with weights to correct for overrepresentation of districts with multiple strikes. Models include district-by-event and year fixed effects, as well as controls for student enrollment and local labor market conditions.

Table B2. Baseline Sociodemographic Characteristics of Striking and Non-Striking Districts (Non-Imputed)

	Striking Districts	Non-Striking Districts	Difference
Average Teacher Compensation ($1,000s)	92.77	92.27	0.50
Average Teacher Salaries ($1,000s)	69.05	69.23	-0.18
Average Teacher Benefits ($1,000s)	23.59	23.20	0.39
Pupil-to-Teacher Ratio	16.48	15.38	1.10
Total Current Expenditures per Pupil ($1,000s)	10.71	12.47	-1.76***
Instr. Salary and Benefit Expenditures per Pupil ($1,000s)	5.75	6.71	-0.96***
Working Conditions Expenditures per Pupil ($1,000s)	4.25	5.16	-0.91***
Noninstr.Salary and Benefit Expenditures per Pupil ($1,000s)	2.86	3.11	-0.25***
Non-Salary and Benefit Expenditures per Pupil ($1,000s)	2.08	2.82	-0.74***
Capital Expenditures per Pupil ($1,000s)	1.27	1.19	0.08
Total Revenues per Pupil ($1,000s)	12.54	15.37	-2.83
Total Student Enrollment (1,000s)	11.99	2.79	9.20***
% Special Education Students	14.58	14.49	0.10
% English Language Learners	6.69	3.67	3.02***
% White Students	66.83	70.42	-3.59**
% Black Students	10.25	11.53	-1.28
% Hispanic Students	15.40	11.79	3.61***
% Asian/Pacific Islander Students	2.79	2.11	0.68**
% Native American Students	3.66	3.14	0.52
% Multi-Racial Students	0.19	0.35	-0.16+
% School Age Population in Poverty	0.17	0.16	0.01**
% Urban	19.64	12.59	7.05***
% Suburban	26.49	20.50	5.98***
% Town/Rural	53.87	66.91	-13.03***
Unemployment Rate	0.06	0.06	0.00***
Socioeconomic Status (SD)	-0.17	0.01	-0.18***
Districts	555	14,895	

Notes: All finance measures are in real 2018 dollars. Socioeconomic Status, Mass Economic Policy Preferences, and Mass Social Policy Preferences are standardized (z-scored). Data reflect the first year of data for a given district. Differences within states come from a series of OLS regressions with each characteristic regressed on a dichotomous indicator of whether there is ever a strike.

Table B3. Baseline Sociodemographic Characteristics of Striking Districts (2007-8)

	Coordinated Striking Districts	Individual Striking Districts
Average Teacher Compensation ($1,000s)	86.37	109.60
Average Teacher Salaries ($1,000s)	64.57	80.66
Average Teacher Benefits ($1,000s)	21.76	28.43
Pupil-to-Teacher Ratio	16.33	16.96
Total Current Expenditures per Pupil ($1,000s)	10.15	12.18
Instr. Salary and Benefit Expenditures per Pupil ($1,000s)	5.40	6.63
Working Conditions Expenditures per Pupil ($1,000s)	4.04	4.77
Noninstr.Salary and Benefit Expenditures per Pupil ($1,000s)	2.78	3.11
Non-Salary and Benefit Expenditures per Pupil ($1,000s)	1.96	2.36
Capital Expenditures per Pupil ($1,000s)	1.30	1.42
Total Revenues per Pupil ($1,000s)	11.71	14.59
Total Student Enrollment (1,000s)	10.45	17.02
% Special Education Students	13.24	14.99
% English Language Learners	7.57	5.92
% White Students	66.51	67.29
% Black Students	9.02	13.24
% Hispanic Students	16.52	13.01
% Asian/Pacific Islander Students	2.25	4.11
% Native American Students	4.80	0.66
% Multi-Racial Students	0.00	0.82
% School Age Population in Poverty	0.18	0.14
% Urban	19.65	21.60
% Suburban	18.91	45.68
% Town/Rural	61.44	32.72
Unemployment Rate	0.06	0.06
Socioeconomic Status (SD)	-0.21	0.13
Districts	402	162

Notes: Coordinated strikes are cross-district teacher work stoppages, typically directed at state government. Individual strikes are those for which teachers in a single district went on strike, typically as part of stalled collective bargaining negotiations. See Table 1 for other notes.

Appendix Table B4. Strike Count by State

State	Individual Strikes	Coordinated Strikes	Total Strikes
Arizona	1	88	89
Arkansas	2	0	2
California	38	0	38
Colorado	8	27	35
Idaho	1	0	1
Illinois	45	0	45
Indiana	2	0	2
Kentucky	1	48	49
Louisiana	3	0	3
Massachusetts	8	0	8
Michigan	8	0	8
Minnesota	2	0	2
Nevada	2	0	2
New Jersey	3	0	3
North Carolina	2	79	81
Ohio	9	0	9
Oklahoma	0	68	68
Oregon	5	26	31
Pennsylvania	59	0	59
Rhode Island	1	0	1
South Carolina	0	7	7
Tennessee	1	0	1
Vermont	7	0	7
Virginia	1	4	5
Washington	33	23	56
West Virginia	0	109	109
Wisconsin	0	24	24
Total	*242*	*503*	*745*

Appendix Table B5. Strikes and State Characteristics

State Name	Number of Strikes	Districts with Strikes (%)	Prohibits Strikes	Teacher Union Members (%)	Economic Policy Preferences	Mass Social Policy Preferences
Alabama	0	0	1	85.6	-0.34	-1.94
Alaska	0	0	0	89.3	-0.63	0.07
Arizona	89	15%	1	44.5	-0.55	0.33
Arkansas	2	1%	1	34.7	-0.77	-1.74
California	37	2%	0	93.6	0.93	0.97
Colorado	34	16%	1	62.4	-0.85	0.94
Connecticut	1	1%	1	98.8	0.64	1.50
Delaware	0	0	1	90.1	0.93	0.40
Florida	0	0	1	55.8	-0.12	-0.10
Georgia	0	0	1	54.8	-0.03	-1.01
Hawaii	0	0	0	96.7	0.31	1.17
Idaho	1	1%	1	62.2	-1.77	-0.42
Illinois	43	4%	0	96.2	0.54	0.43
Indiana	2	1%	1	73.7	0.27	-0.97
Iowa	0	0	1	73.3	-0.85	-0.23
Kansas	0	0	1	54.7	-1.09	-0.42
Kentucky	49	24%	1	58.4	-0.08	-1.50
Louisiana	2	2%	0	57.8	0.32	-1.62
Maine	0	0	1	77.1	-0.07	1.11
Maryland	0	0	1	84.8	2.02	0.87
Massachusetts	6	1%	1	92.8	1.24	2.40
Michigan	7	0%	1	92.0	1.83	0.21
Minnesota	2	0%	0	95.7	-0.39	0.28
Mississippi	0	0	1	36.7	-0.01	-2.21
Missouri	0	0	1	76.6	-0.55	-1.13
Montana	0	0	0	82.5	-1.61	-0.26
Nebraska	0	0	1	85.3	-1.29	-0.71
Nevada	1	6%	1	74.6	-0.91	0.75
New Hampshire	0	0	1	84.4	-0.10	1.47
New Jersey	3	0%	1	97.1	1.43	1.36
New Mexico	0	0	1	41.0	-0.60	-0.35
New York	0	0	1	98.4	1.63	1.30
North Carolina	80	22%	1	49.5	0.44	-1.02
North Dakota	0	0	1	74.7	-1.57	-1.09
Ohio	8	1%	0	91.5	0.00	-0.37
Oklahoma	67	12%	1	57.5	-1.63	-1.52
Oregon	30	14%	0	95.2	-0.54	0.89
Pennsylvania	59	7%	0	93.4	0.38	0.10
Rhode Island	1	3%	1	97.4	0.75	1.92
South Carolina	7	7%	1	26.9	-0.29	-1.41
South Dakota	0	0	1	54.1	-1.40	-0.42
Tennessee	1	1%	1	58.7	-1.17	-1.70
Texas	0	0	1	62.7	-0.66	-0.60
Utah	0	0	1	63.6	-1.79	-1.28
Vermont	0	0	0	82.4	0.98	2.36
Virginia	4	3%	1	51.1	0.23	-0.29
Washington	52	15%	1	98.0	-0.37	1.14
West Virginia	109	100%	1	68.2	-0.29	-2.15
Wisconsin	24	5%	0	98.3	-0.11	0.15
Wyoming	0	0	0	53.2	-2.70	-1.01

Note: Characteristics are at baseline (2007-8 school year). Teacher Union Members (%) measures the percent of public school teachers that are union members from the Schools and Staffing Survey.

Table B6. Event Study Point Estimates

Years Since Strike	Logged Teacher Compensation	Logged Teacher Salary	Logged Teacher Benefits	Pupil-Teacher Ratios	Logged Working Conditions Expenditures	Logged Per Pupil Non-Instr. Salary & Benefit Expenditures	Logged Per Pupil Non-Salary & -Benefit Expenditures	Logged Capital Expenditures	Logged Per Pupil Expenditures	Logged Per Pupil Revenues	Math Achievement	Reading Achievement
-5	-0.01*	-0.02*	-0.00	-0.04	-0.02**	-0.02***	-0.03	0.01	-0.02***	-0.02	0.004	-0.005
	(0.01)	(0.01)	(0.01)	(0.07)	(0.01)	(0.00)	(0.03)	(0.04)	(0.00)	(0.01)	(0.011)	(0.006)
-4	-0.01	-0.00	0.00	0.02	0.01	0.00	0.02	0.03	-0.00	-0.01	-0.012*	0.004
	(0.01)	(0.00)	(0.01)	(0.09)	(0.01)	(0.01)	(0.01)	(0.04)	(0.01)	(0.01)	(0.005)	(0.010)
-3	0.01	0.01	-0.00	0.06	0.00	-0.01	0.02	-0.07	0.00	-0.01	-0.003	0.021***
	(0.01)	(0.01)	(0.01)	(0.09)	(0.00)	(0.00)	(0.01)	(0.05)	(0.01)	(0.01)	(0.005)	(0.005)
-2	-0.01**	-0.01*	-0.01**	-0.25	0.00	0.01	-0.01	0.02	-0.00	-0.01	-0.026**	-0.015**
	(0.00)	(0.00)	(0.01)	(0.31)	(0.01)	(0.02)	(0.01)	(0.05)	(0.00)	(0.00)	(0.009)	(0.005)
-1	-0.00	-0.00	-0.00	0.26	-0.00	0.00	-0.01	0.05	-0.00	0.00	-0.016*	-0.015***
	(0.00)	(0.01)	(0.00)	(0.28)	(0.00)	(0.00)	(0.01)	(0.04)	(0.00)	(0.00)	(0.006)	(0.004)
0	0.01	-0.00	0.03**	-0.11*	-0.00	0.01	-0.01	-0.05	0.01	0.00	-0.008	0.001
	(0.00)	(0.00)	(0.01)	(0.05)	(0.00)	(0.00)	(0.01)	(0.04)	(0.00)	(0.00)	(0.005)	(0.004)
1	0.03***	0.02***	0.05***	-0.22	0.01	0.03***	-0.00	0.01	0.02***	0.02***	-0.008	0.008
	(0.00)	(0.00)	(0.01)	(0.16)	(0.01)	(0.01)	(0.01)	(0.04)	(0.00)	(0.00)	(0.006)	(0.006)
2	0.03***	0.03***	0.05***	-0.14	0.01	0.03***	-0.01	0.01	0.02***	0.03***	0.005	0.016
	(0.01)	(0.01)	(0.01)	(0.11)	(0.01)	(0.01)	(0.02)	(0.05)	(0.01)	(0.01)	(0.014)	(0.012)
3	0.04***	0.03**	0.04*	-0.57***	0.04**	0.05***	0.02	0.01	0.05***	0.04***	0.023	0.031*
	(0.01)	(0.01)	(0.02)	(0.11)	(0.01)	(0.01)	(0.02)	(0.12)	(0.01)	(0.01)	(0.015)	(0.013)
4	0.06**	0.04	0.08**	-0.35**	0.05***	0.07***	0.02	-0.01	0.07***	0.07***	-0.003	0.020
	(0.02)	(0.02)	(0.03)	(0.11)	(0.01)	(0.02)	(0.02)	(0.13)	(0.01)	(0.01)	(0.018)	(0.017)
5	0.06***	0.04***	0.07*	-0.39*	0.05***	0.08***	0.00	0.00	0.07***	0.07***	0.010	0.013
	(0.01)	(0.01)	(0.03)	(0.15)	(0.01)	(0.02)	(0.02)	(0.16)	(0.01)	(0.02)	(0.021)	(0.019)

Notes: + p<.10, * p<0.05, ** p<0.01, *** p<0.001. Robust standard errors clustered at the district level are in parentheses. Controls include time-varying district-level indicators of student enrollment, unemployment rates, socioeconomic status, and the share of children living in poverty.

Table B7. Event Study Point Estimates, Real 2018$

Years Since Strike	Teacher Compensation	Teacher Salary	Teacher Benefits	Working Conditions Expenditures	Per Pupil Non-Instr. Salary & Benefit Expenditures	Per Pupil Non-Salary & -Benefit Expenditures	Capital Expenditures	Per Pupil Expenditures	Per Pupil Revenues
-5	-1224.95*	-1045.24**	-244.08	-64.75	-54.13**	-62.14	11.23	-132.43*	-206.36*
	(570.09)	(391.83)	(204.96)	(38.92)	(18.33)	(48.45)	(56.66)	(52.90)	(102.73)
-4	-891.40*	-402.23	-383.06	-49.21	-18.04	17.13	43.29	-126.48	-169.16
	(445.01)	(288.64)	(291.82)	(88.94)	(27.05)	(31.78)	(59.67)	(150.99)	(143.94)
-3	532.91	792.72	-418.05	-89.49	-91.42	9.76	-27.68	-48.73	-148.71
	(950.67)	(706.74)	(276.17)	(52.70)	(56.48)	(19.51)	(76.37)	(49.74)	(145.62)
-2	-740.61*	-410.60*	-310.63	22.76	71.58	-20.84	62.30	-91.69**	-216.07**
	(300.20)	(190.61)	(171.01)	(38.13)	(74.84)	(27.33)	(67.97)	(29.58)	(80.84)
-1	-27.02	18.83	-45.52	-53.99*	-10.43	-34.69*	139.27*	-82.34*	29.30
	(517.37)	(413.71)	(153.38)	(23.39)	(13.66)	(17.24)	(59.01)	(33.58)	(51.94)
0	736.56	-147.32	822.28**	-79.37	7.79	-21.45	-14.16	-3.40	-40.55
	(416.61)	(195.12)	(280.90)	(49.55)	(13.10)	(33.84)	(48.82)	(49.54)	(56.99)
1	2595.80***	1314.05***	1268.53***	-58.95	54.75**	-30.40	-0.43	103.97	102.76
	(453.78)	(228.11)	(376.74)	(66.61)	(18.72)	(34.49)	(55.35)	(69.10)	(81.95)
2	2841.10***	1683.12***	1136.89***	-89.51	45.01	-11.07	51.38	84.74	46.01
	(452.75)	(341.84)	(241.31)	(89.22)	(26.73)	(41.02)	(55.95)	(108.49)	(125.96)
3	3986.87***	2046.02**	1838.81**	176.12*	169.03**	28.62	99.73	517.59***	491.31**
	(1111.65)	(688.59)	(684.80)	(72.15)	(51.52)	(50.62)	(162.11)	(134.94)	(152.02)
4	8514.18***	4126.33**	4430.22***	332.26*	238.02***	29.09	113.56	858.60***	970.28***
	(1904.17)	(1312.54)	(1002.07)	(130.57)	(61.59)	(37.63)	(180.36)	(190.62)	(185.85)
5	7628.79***	3968.15***	3536.46**	231.38*	269.53***	-16.08	7.79	779.71***	1057.46***
	(1718.64)	(1055.68)	(1082.84)	(91.36)	(70.81)	(42.17)	(208.21)	(197.27)	(263.12)

Notes: + p<.10, * p<0.05, ** p<0.01, *** p<0.001. Robust standard errors clustered at the district level are in parentheses. Controls include time-varying district-level indicators of student enrollment, unemployment rates, socioeconomic status, and the share of children living in poverty.

Appendix Table B8. Bounding Spillover

	Teacher Compensation (Logged)			Pupil-Teacher Ratios			Math Achievement			Reading Achievement		
Years Since Strike	Main	Drop Spillover Districts	Only Striking States	Main	Drop Spillover Districts	Only Striking States	Main	Drop Spillover Districts	Only Striking States	Main	Drop Spillover Districts	Only Striking States
-5	-0.012*	-0.015***	-0.001	0.003	-0.005	0.117	0.004	0.006	-0.025	-0.008	-0.008	0.005
	(0.006)	(0.004)	(0.022)	(0.066)	(0.056)	(0.250)	(0.012)	(0.006)	(0.021)	(0.006)	(0.005)	(0.015)
-4	-0.005	-0.005	0.024	0.000	-0.051	0.402	-0.012*	-0.016**	-0.015	0.002	0.015***	-0.012
	(0.005)	(0.004)	(0.015)	(0.070)	(0.059)	(0.302)	(0.005)	(0.005)	(0.008)	(0.011)	(0.004)	(0.009)
-3	0.006	0.002	0.047	0.060	-0.044	-0.992	-0.002	-0.005	-0.029**	0.026***	0.026***	-0.011
	(0.008)	(0.003)	(0.026)	(0.073)	(0.048)	(0.702)	(0.006)	(0.006)	(0.010)	(0.006)	(0.005)	(0.009)
-2	-0.011**	-0.001	0.003	-0.217	-0.013	-1.428	-0.025*	-0.016***	-0.030	-0.013*	-0.003	0.014
	(0.003)	(0.003)	(0.016)	(0.285)	(0.058)	(1.816)	(0.010)	(0.005)	(0.019)	(0.006)	(0.004)	(0.023)
-1	-0.002	0.000	-0.018	0.290	-0.028	1.434	-0.018**	-0.014**	-0.032	-0.018***	-0.015***	0.002
	(0.004)	(0.002)	(0.019)	(0.270)	(0.051)	(1.643)	(0.007)	(0.005)	(0.018)	(0.005)	(0.004)	(0.016)
0	0.007	0.008**	0.027	-0.121*	-0.188***	0.067	-0.010	-0.004	-0.055**	0.002	0.010*	0.000
	(0.003)	(0.003)	(0.021)	(0.053)	(0.056)	(0.259)	(0.005)	(0.005)	(0.018)	(0.004)	(0.004)	(0.013)
1	0.031***	0.038***	0.075**	-0.220	-0.237***	-0.996	-0.008	-0.000	-0.055*	0.008	0.022***	0.023
	(0.003)	(0.003)	(0.028)	(0.152)	(0.066)	(1.282)	(0.006)	(0.007)	(0.022)	(0.006)	(0.005)	(0.019)
2	0.035***	0.038***	0.009	-0.161	-0.526**	0.572	0.005	0.012	0.001	0.016	0.024*	0.008
	(0.005)	(0.004)	(0.026)	(0.093)	(0.172)	(0.394)	(0.014)	(0.014)	(0.014)	(0.012)	(0.012)	(0.012)
3	0.036***	0.050***	0.022*	-0.556***	-0.995***	-0.126	0.023	0.031*	0.028	0.031*	0.044***	0.014
	(0.010)	(0.011)	(0.011)	(0.098)	(0.223)	(0.299)	(0.015)	(0.016)	(0.025)	(0.013)	(0.013)	(0.026)
4	0.057**	0.080***	0.031	-0.336**	-0.811***	0.091	-0.003	0.001	0.004	0.020	0.031	0.006
	(0.020)	(0.021)	(0.022)	(0.105)	(0.170)	(0.263)	(0.018)	(0.019)	(0.021)	(0.017)	(0.017)	(0.018)
5	0.060***	0.080***	0.034*	-0.396**	-0.539***	-0.476	0.010	0.026	0.015	0.013	0.030	-0.002
	(0.015)	(0.016)	(0.017)	(0.153)	(0.156)	(0.272)	(0.021)	(0.022)	(0.023)	(0.019)	(0.019)	(0.019)

Notes: + p<.10, * p<0.05, ** p<0.01, *** p<0.001. Robust standard errors clustered at the district level are in parentheses. Models include district and year fixed effects, as well as controls for student enrollment and local labor market conditions. Within each outcome, the first column displays results from the preferred specification estimating the effect of the first strike in a given district. The second column, limits comparison districts to those outside of striking states. The third column limits analyses to only states that experience strikes.

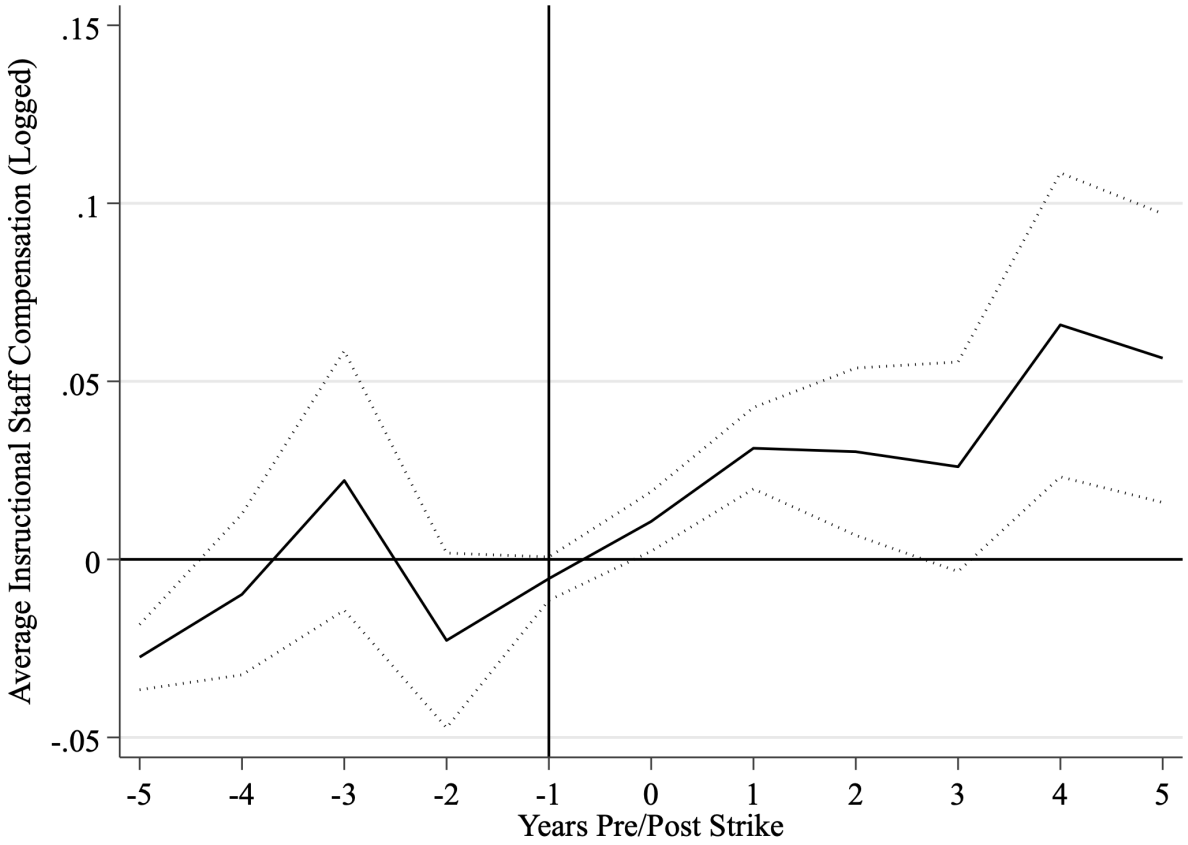


Figure B1. Effect of Strikes on Instructional Salaries and Benefits per Instructional Staff Member
Notes: Solid line indicates the estimate from Equation 3 with school-district and school-year fixed effects. We use the specification recommended by Callaway and Sant'Anna (2020) to estimate an average treatment effect across distinct group-time estimates. Dotted line indicates the 95% confidence interval. Data are from the authors' compilation, as well as the NCES.

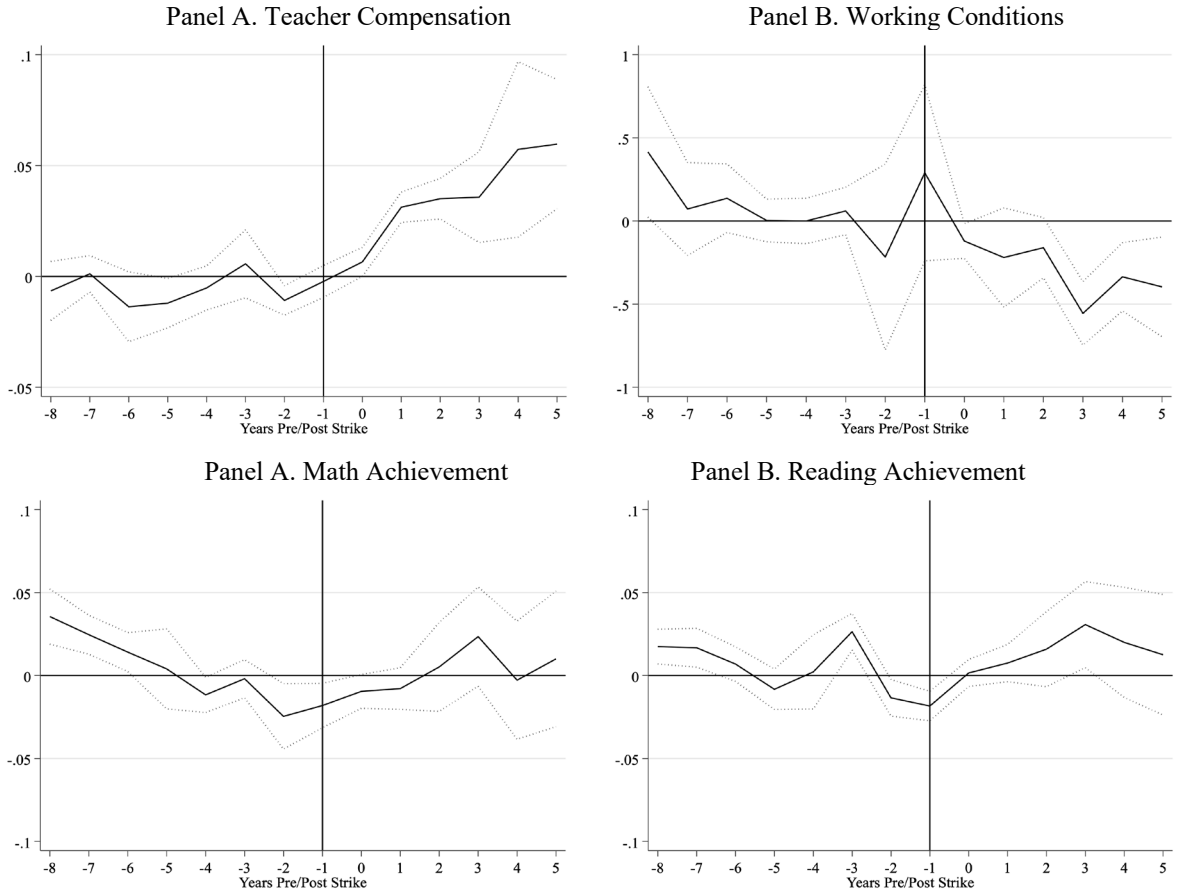


Figure B2. Effect of Strikes, Estimating Eight Years Pre-Strike

Notes: Solid line indicates the estimate from Equation 3 with school-district and school-year fixed effects. We use the specification recommended by Callaway and Sant'Anna (2020) to estimate an average treatment effect across distinct group-time estimates. Dotted line indicates the 95% confidence interval. Data are from the authors' compilation, as well as the NCES.

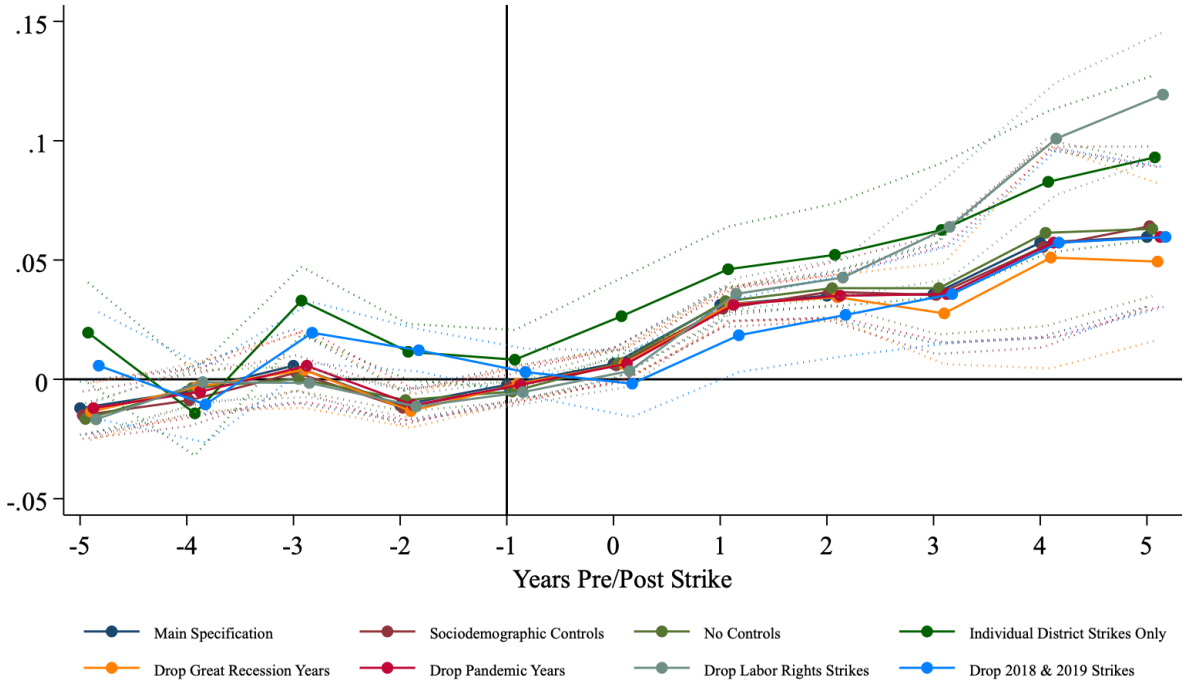


Figure B3. Sensitivity Analyses for Strike Effect on Teacher Compensation

Notes: This figure demonstrates the robustness of our main findings across multiple specification changes. We test: (1) the main specification from Figure 4; (2) adding sociodemographic controls (ELL status, special needs, urbanicity, racial composition); (3) removing all covariates; (4) Individual district strikes only; (5) excluding Great Recession years (2006-2009); (6) excluding COVID-19 years (2020-2023); (7) removing strikes motivated by labor rights policies; and (8) excluding the high-strike years of 2018-2019.

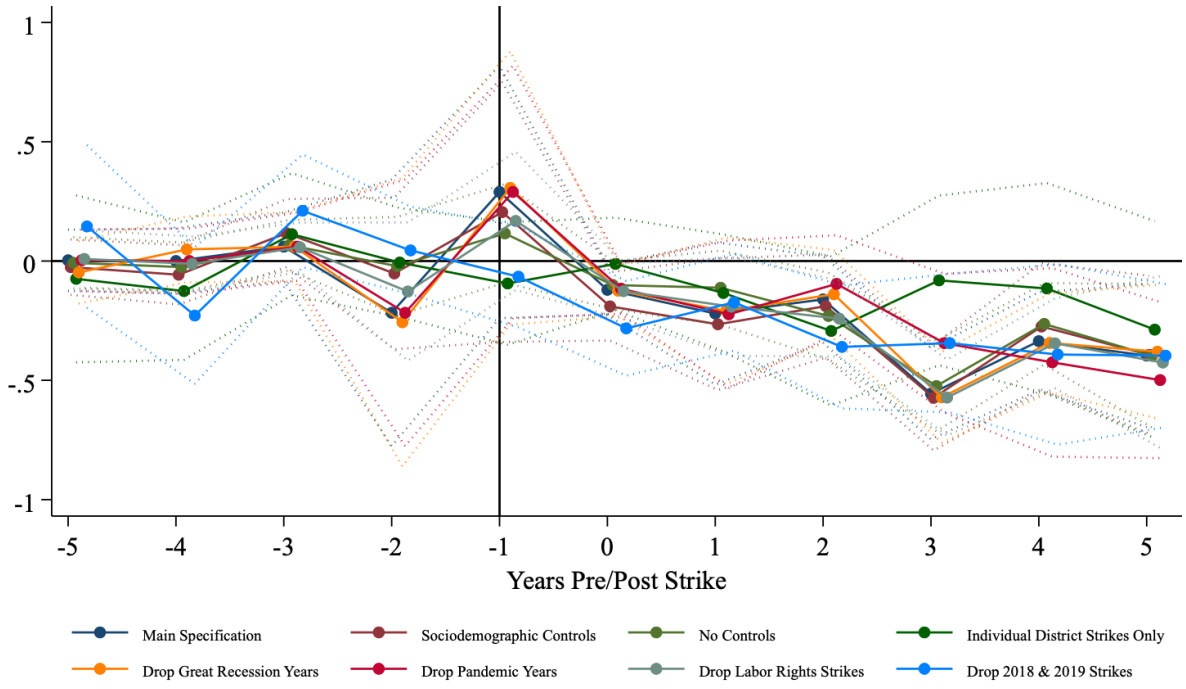


Figure B4. Sensitivity Analyses for Strike Effect on Pupil-Teacher Ratios

Notes: This figure demonstrates the robustness of our main findings across multiple specification changes. We test: (1) the main specification from Figure 5, Panel A; (2) adding sociodemographic controls (ELL status, special needs, urbanicity, racial composition); (3) removing all covariates; (4) Individual district strikes only; (5) excluding Great Recession years (2006-2009); (6) excluding COVID-19 years (2020-2023); (7) removing strikes motivated by labor rights policies; and (8) excluding the high-strike years of 2018-2019.

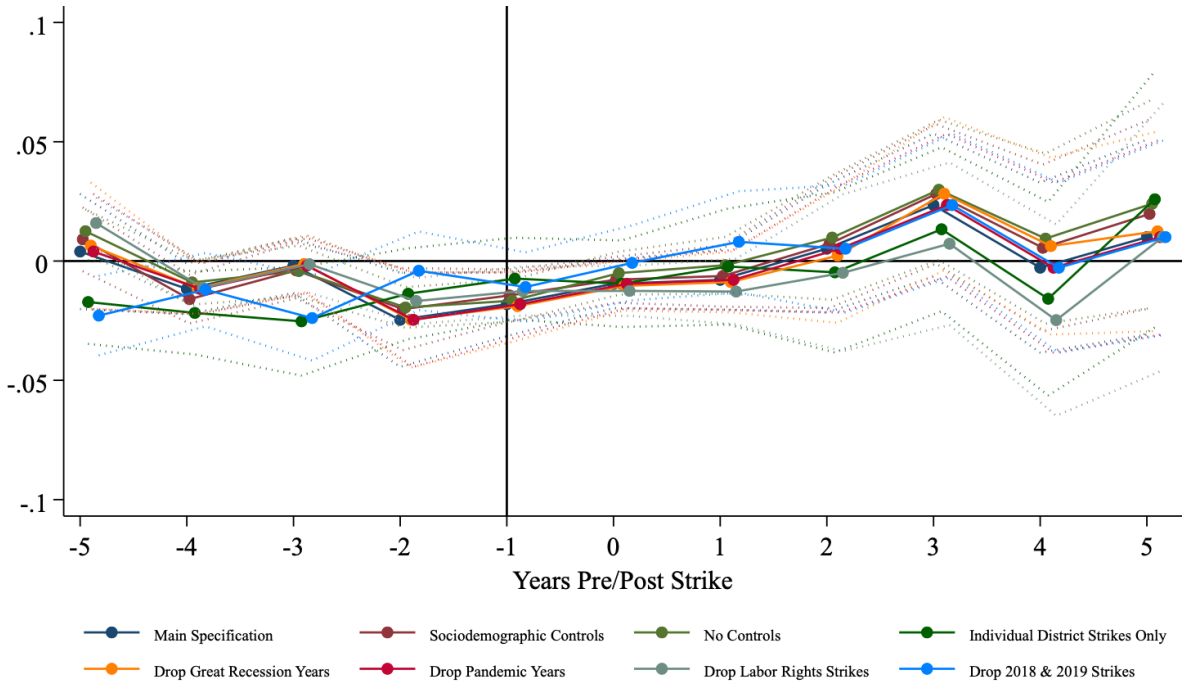


Figure B5. Sensitivity Analyses for Strike Effect on Math Achievement

Notes: This figure demonstrates the robustness of our main findings across multiple specification changes. We test: (1) the main specification from Figure 7, Panel A; (2) adding sociodemographic controls (ELL status, special needs, urbanicity, racial composition); (3) removing all covariates; (4) Individual district strikes only; (5) excluding Great Recession years (2006-2009); (6) excluding COVID-19 years (2020-2023); (7) removing strikes motivated by labor rights policies; and (8) excluding the high-strike years of 2018-2019.

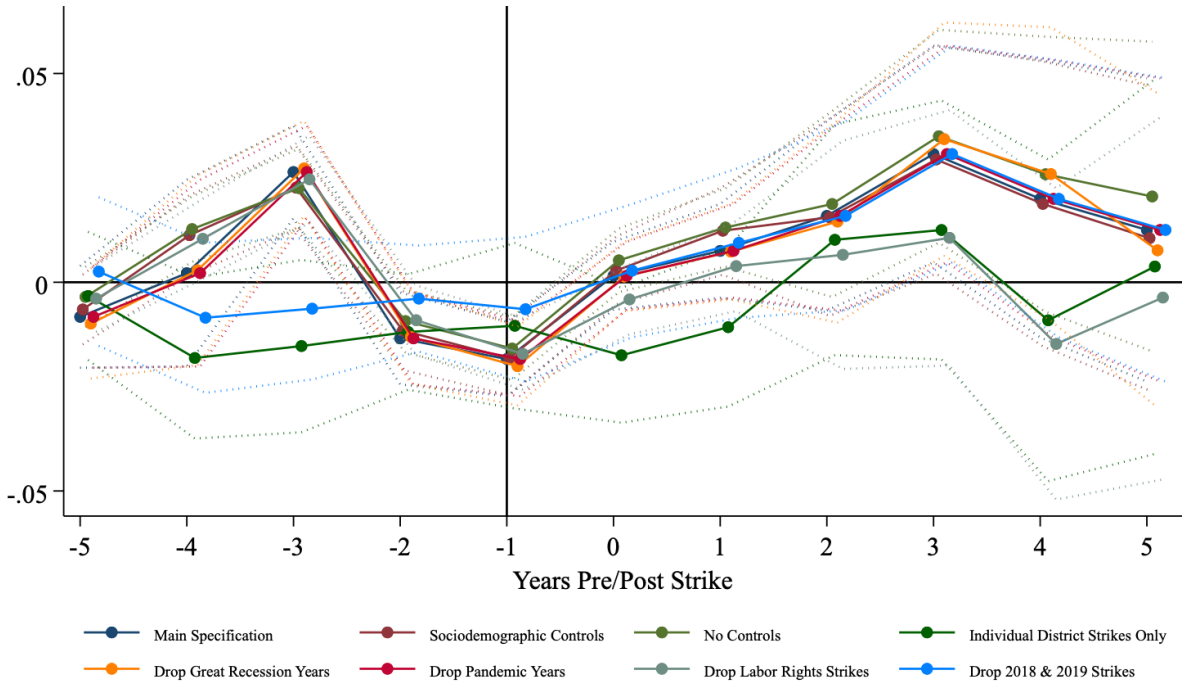


Figure B6. Sensitivity Analyses for Strike Effect on Reading Achievement

Notes: This figure demonstrates the robustness of our main findings across multiple specification changes. We test: (1) the main specification from Figure 7, Panel A; (2) adding sociodemographic controls (ELL status, special needs, urbanicity, racial composition); (3) removing all covariates; (4) Individual district strikes only; (5) excluding Great Recession years (2006-2009); (6) excluding COVID-19 years (2020-2023); (7) removing strikes motivated by labor rights policies; and (8) excluding the high-strike years of 2018-2019.

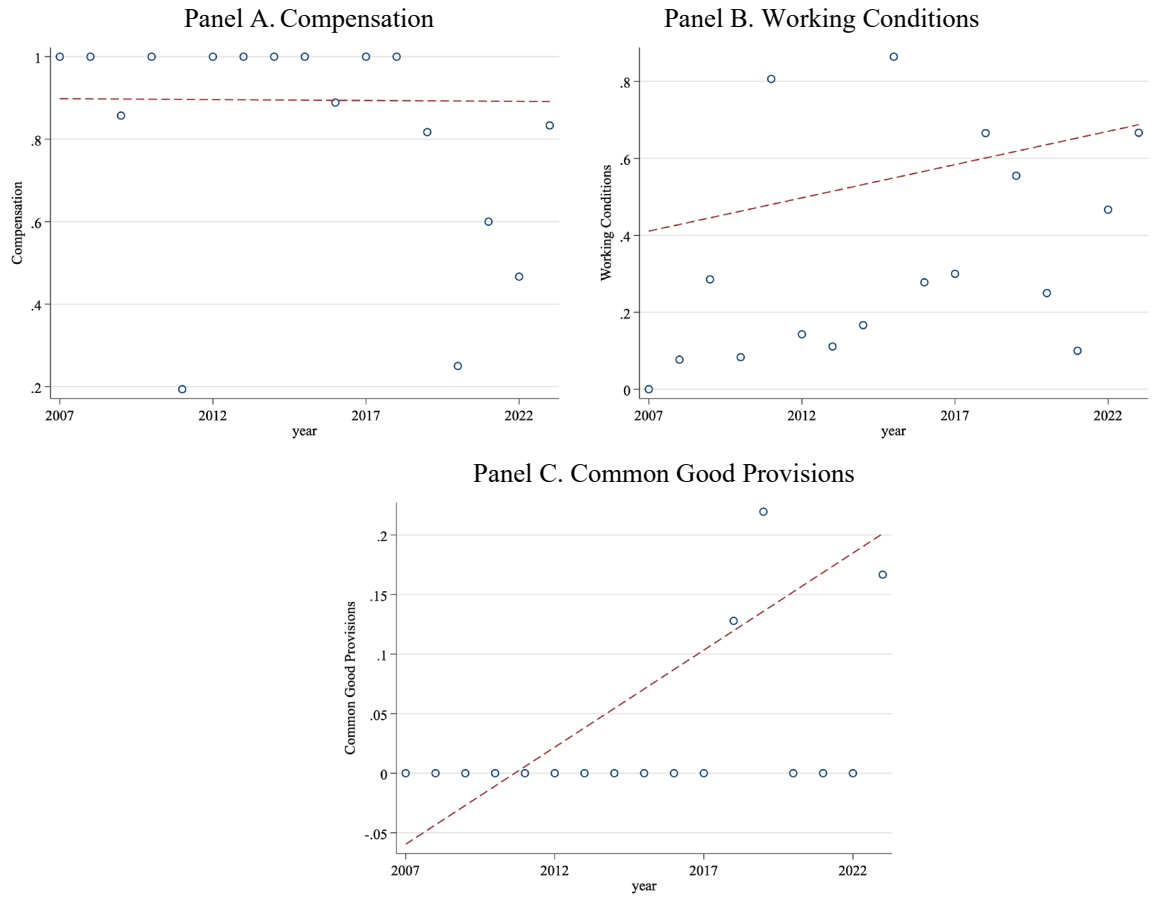


Figure B7. Prevalence of Strike Reasons over Time

Notes: Data are from the authors' compilation.

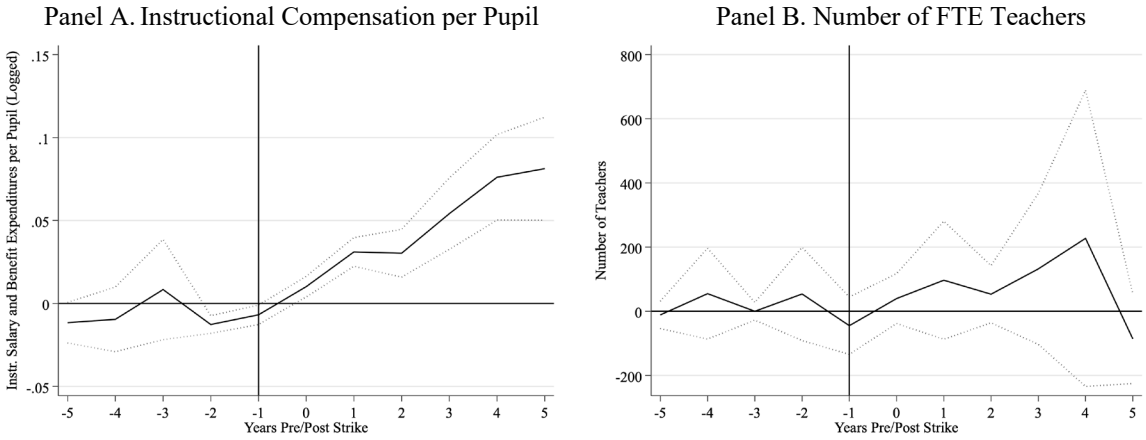


Figure B8. Effect of Strikes on Instructional Salaries and Benefits per Pupil and Number of Teachers

Notes: Solid line indicates the estimate from Equation 1 with school-district and school-year fixed effects. We use the specification recommended by Callaway and Sant'Anna (2020) to estimate an average treatment effect across distinct group-time estimates. Dotted line indicates the 95% confidence interval. Data are from the authors' compilation, as well as the NCES.

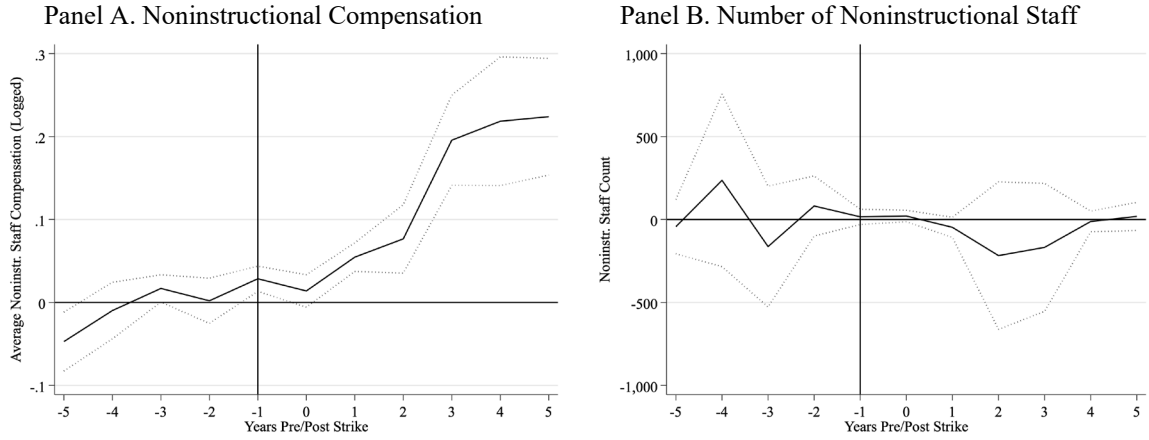


Figure B9. Effect of Strikes on Noninstructional Compensation and Number of Staff

Notes: Solid line indicates the estimate from Equation 1 with school-district and school-year fixed effects. We use the specification recommended by Callaway and Sant'Anna (2020) to estimate an average treatment effect across distinct group-time estimates. Dotted line indicates the 95% confidence interval. Data are from the authors' compilation, as well as the NCES.

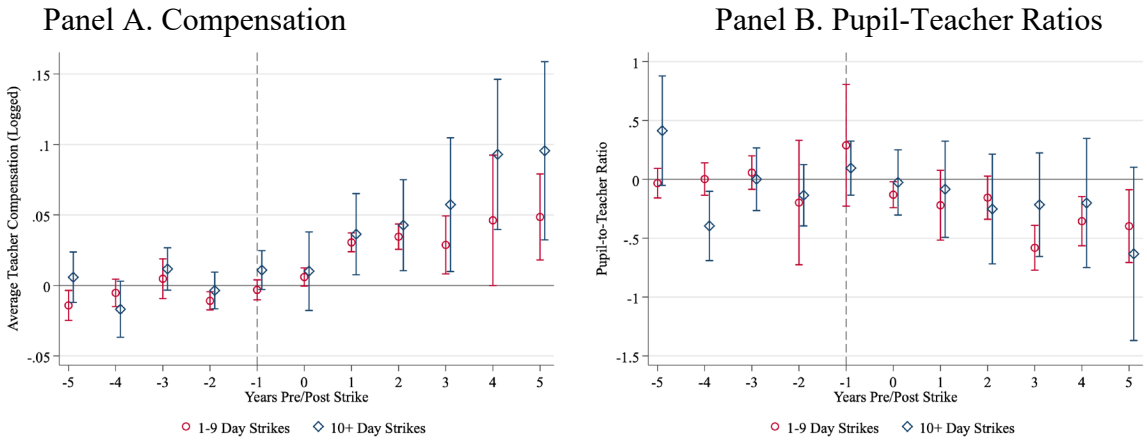


Figure B10. Dosage Effects for Compensation and Working Conditions

Notes: To produce differential estimates by strike length, we subset the data to include only treated districts that went on strike for either less than two weeks (1-9 days) or two weeks or more (10+ days). For each analysis, we use the specification recommended by Callaway and Sant'Anna (2020) to estimate an average treatment effect across distinct group-time estimates. Capped line indicates the 95% confidence interval. Data are from the authors' compilation, as well as the NCES and the SEDA 5.0.

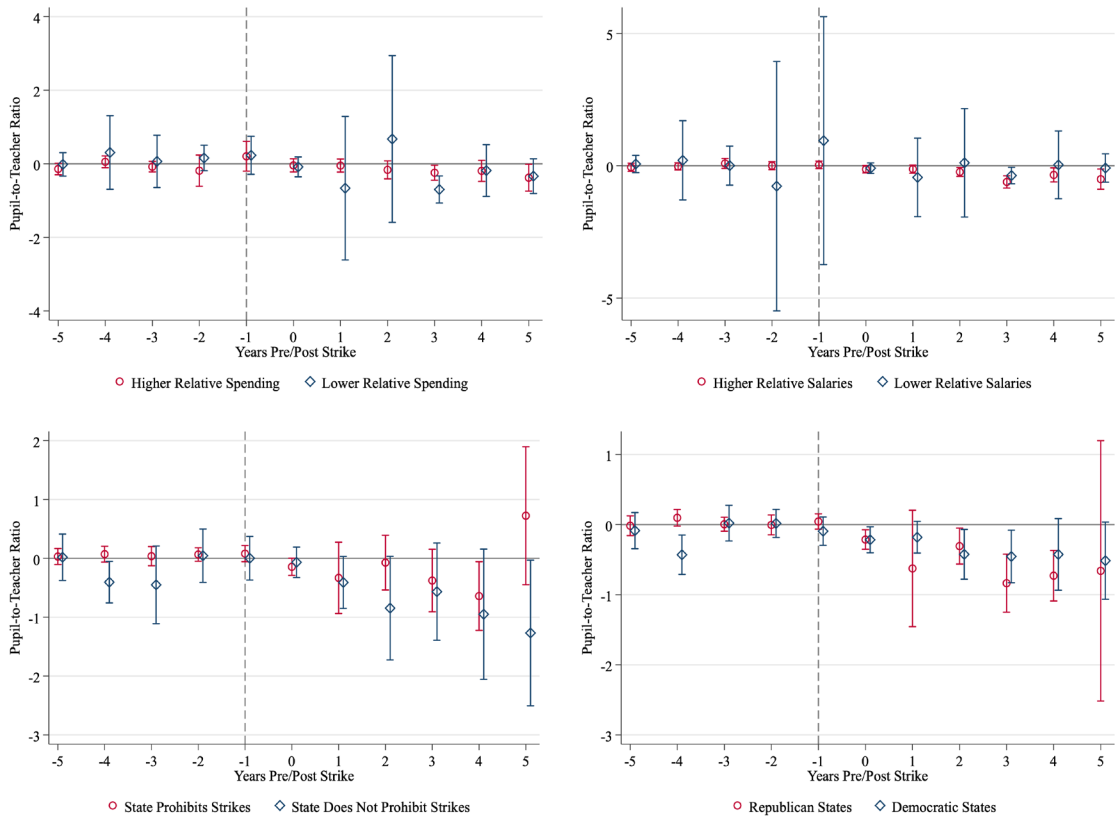


Figure B11. Heterogeneity Analysis for Pupil-Teacher Ratios

Notes: See notes on Figures 9-12 in the main text.

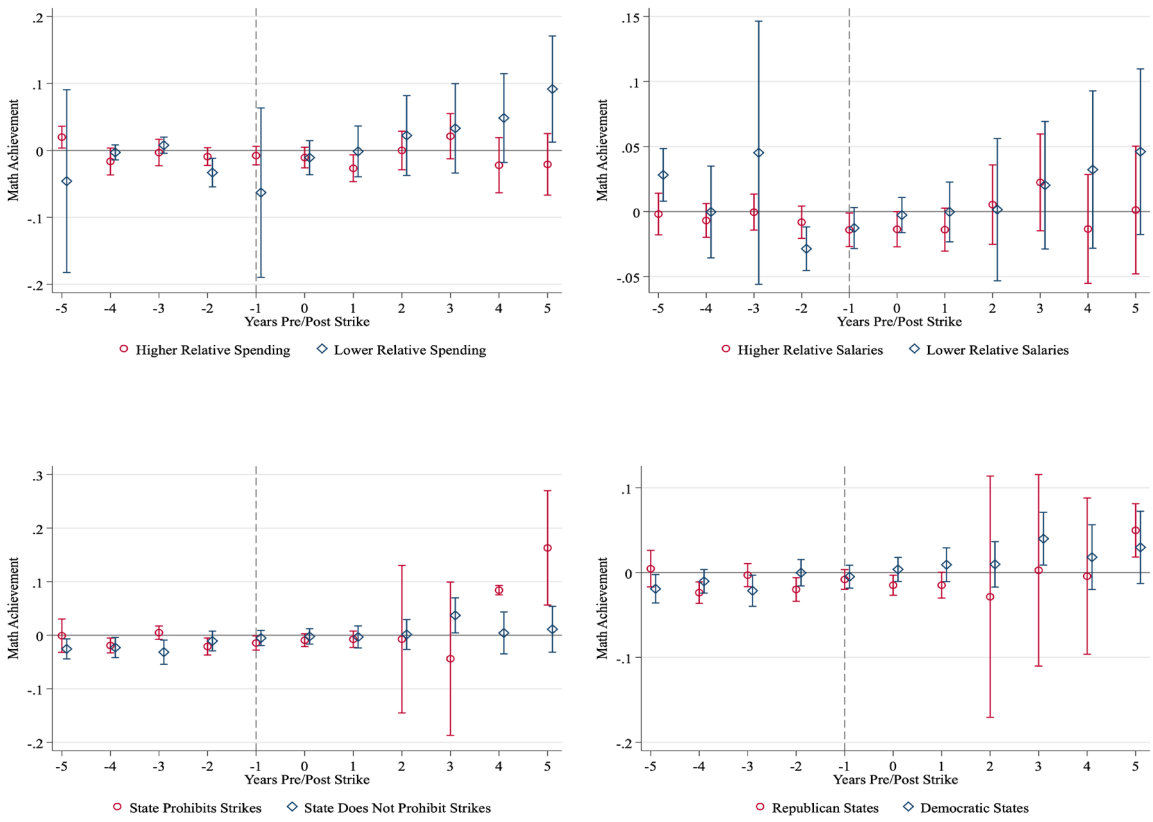


Figure B12. Heterogeneity Analysis for Math Achievement

Notes: See notes on Figures 9-12 in the main text.

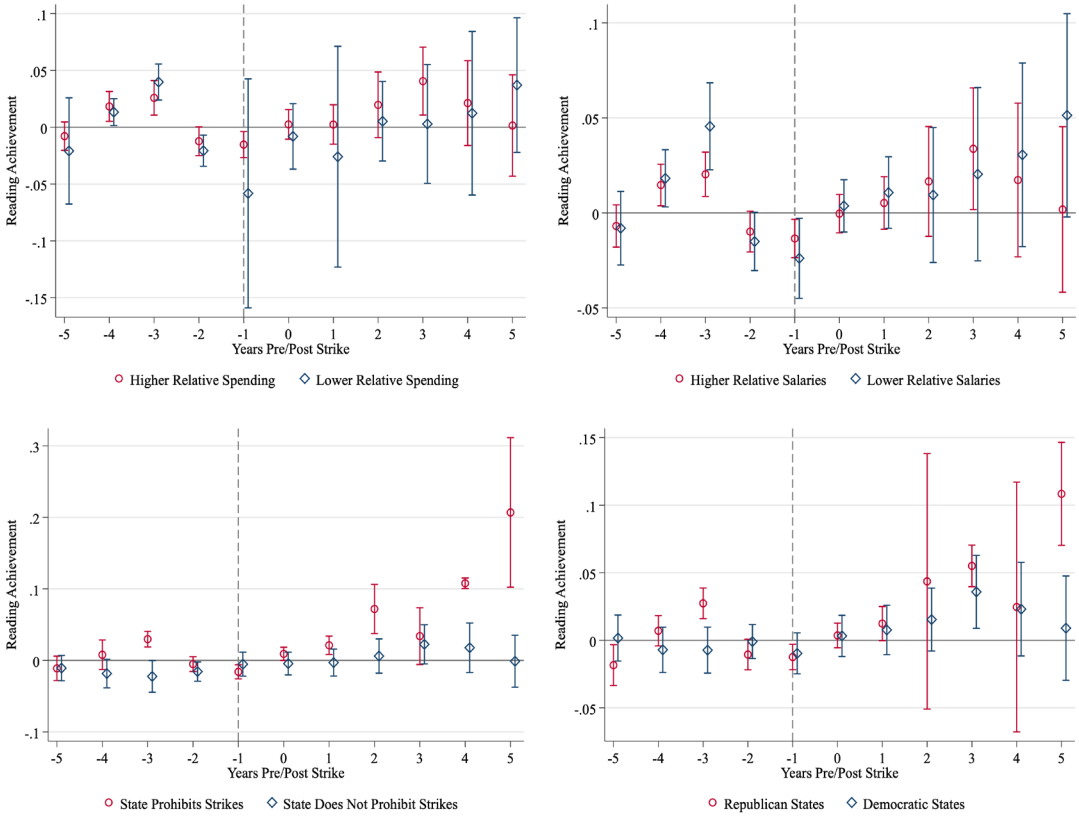


Figure B13. Heterogeneity Analysis for Reading Achievement

Notes: See notes on Figures 9-12 in the main text.

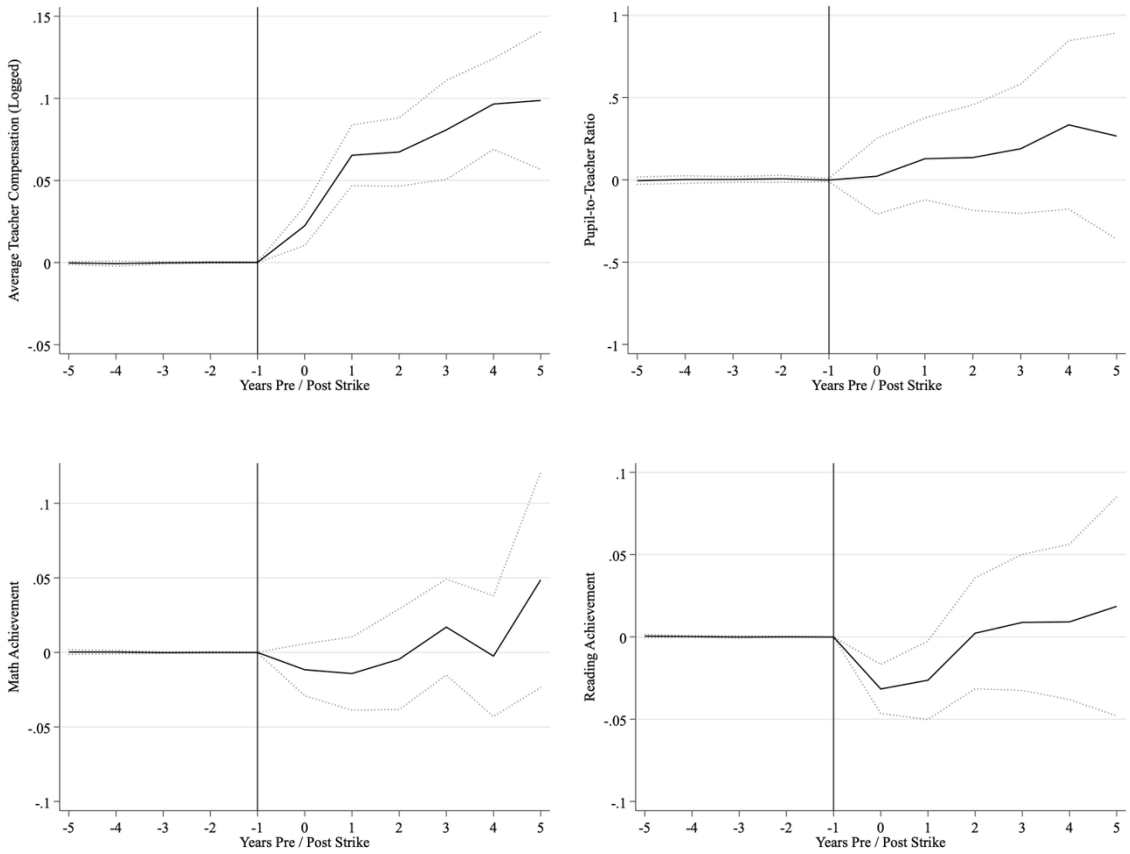


Figure B14. Synthetic Differences-in-Differences with State-by-Year Fixed Effects, Individual Strikes Only

Notes: For each analysis, we use the synthetic DiD specification recommended by Arkhangelsky et al. (2021) to estimate an average treatment effect across distinct group-time estimates. We include district and state-by-year fixed effects. We restrict the sample to individual strikes because of the low (and sometimes 0) number of comparison groups within state-years for coordinated strikes. Dotted line indicates the 95% confidence interval. Data are from the authors' compilation, as well as the NCES and the SEDA 5.0.

Appendix C. Teacher Strikes & the Law

Background.—Teacher strikes have been a persistent feature of American labor movements at least since the late 1920s. One of the earliest recorded instances of such strikes occurred in Wilkes-Barre, Pennsylvania, in October of 1929, when a group of 146 educators went on strike due to a lack of pay for six months (*The New York Times* 1929). In the following decades, teachers across the United States organized and went on strike in increasing numbers, demanding better working conditions, higher pay, and remediation of gender discrimination (Perrillo 2012). Despite the exclusion of public sector employees from the 1935 National Labor Relations Act, teachers began to secure collective bargaining rights on a state-by-state basis in the 1960s and 1970s. Public school teachers often secured the right to collectively bargain as part of compromise legislation that granted these rights in exchange for the prohibition of strikes (Paglayan 2019). Today, collective bargaining between teachers' unions and districts is mandatory or permitted in 43 states, but teacher strikes are illegal in most states (37). Despite these prohibitions, the majority of the public supports teachers having the right to strike (Henderson et al. 2019).

The formation of two large, federated teachers' unions, the National Education Association (NEA) and the American Federation of Teachers (AFT), played a critical role in the rapid expansion of teachers' labor representation between 1970 and 1990. Figure C1 shows longitudinal membership counts for the NEA and AFT over the past century from archival and present-day NEA handbooks and original data provided to us from the AFT.[14] From 1960 to 1990, NEA membership increased from 750,000 to 2.1 million, and AFT membership increased 13-fold from 59,000 to 750,000. Since 1990, the pace of membership gains has slowed, and in some years, membership has declined. Teachers' union density has more consistently declined since the 1990s. According to federal data, 85% of public school teachers reported that they were union members in 1990, a figure that fell to 79% in 1999 and then to 68% by 2020.[15]

Strike Processes.—In states where strikes are legal, union leadership generally proposes the strike to the union membership. Union members then vote, and if approved, the union will set a date for the strike and notify the local school district. If the district and union still have not come to an agreement by the set date, then the teachers in that district are authorized to go on strike, often with some restrictions on the length of the strike. In the state of Illinois, for example, the general procedure for a teacher strike is determined by state law (the Illinois Educational Labor Relations Act), which allows for teacher strikes under certain circumstances (*Act 5* 1998). Teachers must be represented by an exclusive bargaining representative (i.e., a union) and must not have an active contract. The district and union must have attempted to resolve their differences through mediation. The teachers' union must have held a vote to authorize a strike in which three-fourths of union members supported the strike. The union must have provided the school district, regional superintendent, and the Illinois Educational Labor Relations Board with notice of its intent to strike at least 10 days in advance of the strike. The length of the strike can vary, but the strike will typically end when a settlement is reached or the school district is able to get a court injunction.

Illegality, however, is not enough to deter teacher strikes (Paglayan 2019). Teachers have engaged in various forms of work stoppages in many states where striking is not permitted. For example, thousands of teachers prominently chose to participate in work stoppages in 2018 and

[14] Membership counts are not mutually exclusive because the AFT and NEA have merged in several states.

[15] Source: Schools and Staffing Survey; National Teacher and Principal Survey

2019 under the banner of "#RedforEd," despite strikes being illegal in nearly every one of the states that experienced such strikes (e.g., Arizona, Kentucky, Oklahoma, North Carolina, and West Virginia). The processes for illegal teacher strikes differ even more widely across states and districts than those for legal strikes. In states where striking is illegal, teachers' unions can effectively strike when teachers collectively walk out of their classrooms (a "walk-out"). Another option is a "sick out," in which teachers call in sick *en masse*. In some instances, as seen during the 2018 and 2019 strikes, teachers may stage coordinated, statewide strikes targeting state legislatures, framed as a form of political expression protected as free speech. Teachers may also choose to go on an illegal strike even in a state where striking is permitted by state law. Illustrating this tactic, some California teachers have organized so-called "wildcat strikes," i.e., strikes that occur without the support of union leadership and do not adhere to the state's processes for legal strikes.

In illegal teacher strikes, organizers often attempt to mobilize such a large-scale collective action that imposing legal penalties becomes unmanageable or impractical. Teachers and their unions can sometimes negotiate away penalties in the context of their collectively bargained agreements (e.g., agreeing that striking teachers will not be fined), or via other less formal agreements ending a teacher strike if collective bargaining is not in place. In 2023, teachers in Newton, Massachusetts went on strike for two weeks with the Massachusetts Teacher Association stepping in to pay the fines for their illegal action. These practices are reflective of the maxim among labor organizers that "there is no such thing as an illegal strike, only an unsuccessful one" (Reddy 2021).

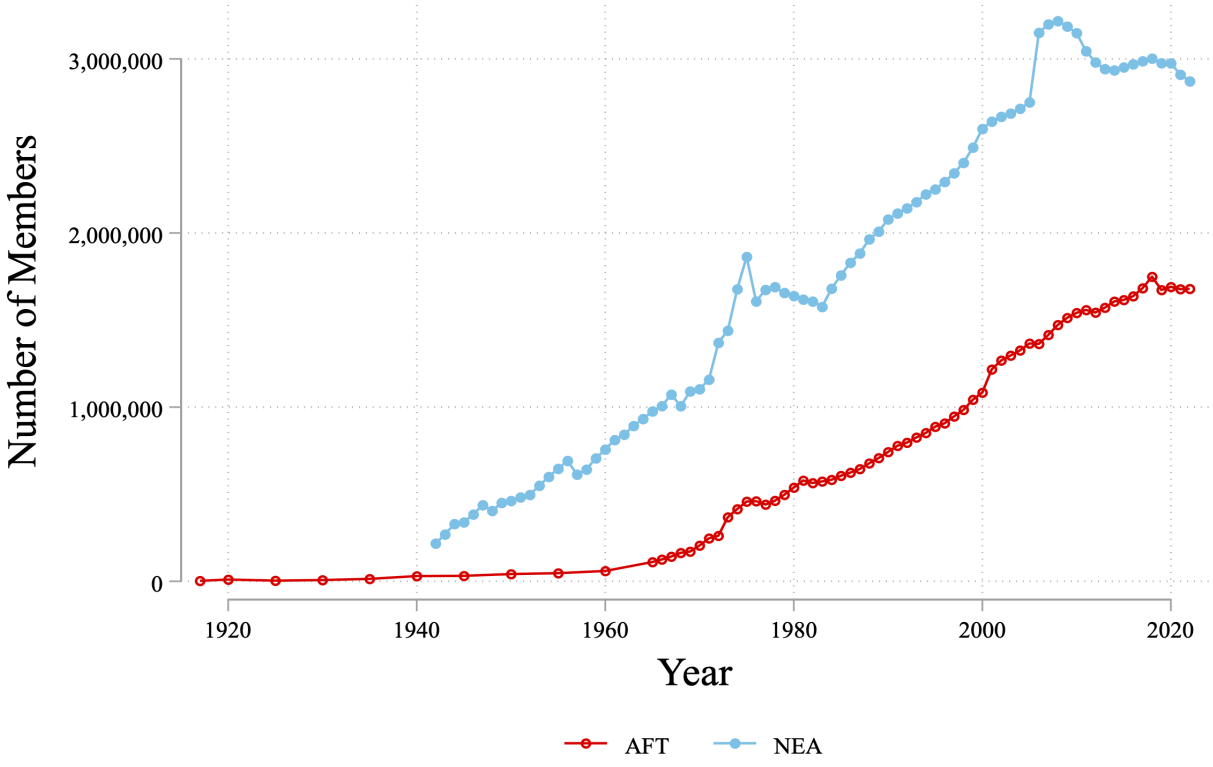


Figure C1: Number of Teachers' Union Members Over Time

Notes: Data are sourced from archival and present-day NEA handbooks and original data provided to us from the AFT. Membership counts are not mutually exclusive because the AFT and NEA have merged in several states.

References

Act 5. 1998. https://www.ilga.gov/legislation/ilcs/documents/011500050K13.htm.

Caughey, Devin, and Christopher Warshaw. 2018. "Policy Preferences and Policy Change: Dynamic Responsiveness in the American States, 1936–2014." *The American Political Science Review* 112 (2): 249–66. http://dx.doi.org/10.1017/S0003055417000533.

Henderson, Michael B., David M. Houston, Paul E. Peterson, and Martin R. West. 2019. "Public Support Grows for Higher Teacher Pay and Expanded School Choice." *Education Next* (blog). August 20, 2019. https://www.educationnext.org/school-choice-trump-era-results-2019-education-next-poll/.

Paglayan, Agustina S. 2019. "Public-Sector Unions and the Size of Government." *American Journal of Political Science* 63 (1): 21–36. https://doi.org/10.1111/ajps.12388.

Perrillo, Jonna. 2012. *Uncivil Rights: Teachers, Unions, and Race in the Battle for School Equity*. University of Chicago Press.

Reddy, Diana S. 2021. "'There Is No Such Thing as an Illegal Strike': Reconceptualizing the Strike in Law and Political Economy." *The Yale Law Journal Forum* 130:421.

The New York Times. 1929. "TEACHERS VOTE TO STRIKE.," October 24, 1929.