The Electoral Power of Public Employees

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Abstract

Government employees vote at unusually high rates, but whether public employment causes mobilization or attracts the already engaged has long been contested. Linking school employment records to voter files in two states and using several causal research designs, we show that teachers in traditional public schools become substantially more likely to vote in low-turnout elections than comparable teachers who begin at charter schools—which are publicly funded but neither governed by elected boards nor supported by local property tax levies. These effects are largest when a levy for the district that employs them is on the ballot. We also find more modest spillovers among people who live with educators. Yet, contrary to conventional wisdom about election timing, this mobilized bloc is too small to decide most races, even in low-turnout, off-cycle elections. By casting doubt on this voter-composition channel, our findings suggest that public employees' electoral influence lies largely elsewhere.

Word Count: 9,919

Introduction

Government employees have long puzzled scholars of American politics. They vote at surprisingly high rates, a pattern that cannot be fully explained by demographic factors typically associated with political participation in the broader population (Brady, Verba and Schlozman 1995, Wolfinger and Rosenstone 1980). Although high levels of voter turnout are generally considered desirable, the normative implications of government employees' own political activity remain a matter of longstanding debate. Much depends on what mobilizes these workers to come to the polls, the considerations shaping their vote choices, and the institutional contexts in which these elections occur.

In their classic work *Who Votes?*, Wolfinger and Rosenstone (1980) devote nearly half a chapter to government employees, advancing several explanations for their unusually high turnout. One possibility combines selection and socialization: public-sector employment both attracts individuals who are already more civic-minded and further encourages these workers to identify with and advocate on behalf of the institutions they serve (e.g., Downs 1967, Perry and Wise 1990). As Wolfinger and Rosenstone argue, "Due to the nature of their work, many public employees are more concerned about the outcome of elections. Many deal on a daily basis with subjects that become campaign issues or would be affected by election outcomes" (pp. 95–96).

Yet the authors also raise a more troubling possibility—that government employees vote because they "owe their positions to political sponsors." In this account, public employees have an "undeniable and crucial stake in the election," since "if the wrong candidate wins, they will be out of a job, demoted, or transferred to the local equivalent of Siberia" (Wolfinger and Rosenstone 1980, 95-96). Wolfinger and Rosenstone subject these explanations to a series of empirical tests, but the evidence does not permit a clear assessment of their relative importance. Subsequent studies have attempted to determine whether public employees vote out of civic responsibility or self-interest, yet scholars have struggled to develop credible research designs capable of identifying the underlying drivers of turnout (see, e.g., Bennett and Orzechowski 1983, Corey and Garand 2002).

The stakes of this unresolved debate for American politics have grown considerably. When Wolfinger and Rosenstone began their research, public-sector unions were newly organized and few scholars explicitly considered how institutional rules, such as the timing of local elections, influence

the political power of organized interests. Since then, a substantial literature has argued that off-cycle elections—those not held in November of even-numbered years—systematically advantage organized public employees. Because turnout in these elections is exceedingly low (Hajnal and Lewis 2003, Kogan, Lavertu and Peskowitz 2018), some theorize that a relatively small but highly mobilized bloc of public employees can become pivotal (Anzia 2011; 2013, Berry 2009), inverting the conventional accountability relationship assumed in democratic theory. As Moe (2006) warned two decades ago, "In democratic systems, the political authorities are elected—and this simple fact means that even the most ordinary bureaucrats, by taking collective action in elections and other realms of politics, may be able to exercise *political* power in determining who their bosses are ...." (emphasis in original).

In this study, we bring new evidence to bear on these questions by linking a variety of administrative data, including public school employment records, with official voter files from two states. In Ohio, several institutional features—including the presence of publicly funded but privately operated charter schools and variation in the timing of local property tax levies—enable us to better isolate the effects of public-sector employment on voter turnout and decompose the underlying mechanisms influencing participation. In Iowa, we leverage a state law that shifted the timing of local school board elections and consolidated them with other races, examining how the reform affected turnout of different voter subgroups with varying employment stakes in the outcome of the elections.

Each of the identification strategies we employ—variation in the timing and type of job transitions, the presence vs. absence of levies in particular elections, and changes in election timing—comes with its own advantages and limitations. Yet each approach ultimately yields the same set of results: Public employees vote at higher rates, especially when they have personal employment stakes that hinge on the outcome of the elections.

To preview, we find that individuals in Ohio become significantly more likely to vote after obtaining teaching jobs in traditional public schools, far more than comparable teachers who begin at charter schools. Turnout does rise substantially among existing charter school teachers *after* they switch sectors and move into jobs in traditional public schools. In addition, public school teachers appear to be more likely to vote when they live and work in the same school district—and thus can affect the electoral fortunes of their bosses—and when the ballot includes property tax

measures affecting the resources available to their school districts. Non-teaching staff show smaller, less robust turnout effects, but respond to levies much as teachers do. These results point to *both* sector-wide mobilization *and* employee-specific self-interest in driving increased participation.

Perhaps our most novel finding is that these turnout effects extend beyond employees themselves. Other voters who live with educators are also more likely to participate. And as is the case with teachers themselves, these effects are most pronounced among voters who live in households of public school teachers rather than charter school teachers. These increases in turnout are particularly pronounced in elections when local tax levies appear on the ballot, indicating that the same self-interested motivations can spread through social networks and increase voting by non-employees.

Yet the ability of teachers to mobilize other voters appears to be quite limited. For example, we find no similar spillover effects among *former* household members—who live in the same household when teachers first begin their employment but move out afterward, even if they continue living in the same school districts.

These effects are not confined to local school-related elections alone. Although school board elections in Ohio take place in odd-year Novembers, we also examine the effects of public employment in even-year congressional and presidential elections. These even-year ballots do not include school board races, but we continue to find similarly elevated participation among public school employees, suggesting that mechanisms such as socialization and habit-formation beyond immediate occupational self-interest contribute to increased political mobilization.

To confirm the robustness of these findings, we collect and analyze additional data from a second state (Iowa), allowing us to examine a distinct institutional setting using an entirely different research design. Specifically, we exploit a reform that consolidated school board and other off-cycle local elections. Consistent with our Ohio results, adding school board races to previously very low-turnout contests disproportionately increased participation among public school employees and their household members—especially among educators who both lived and worked in the same school district.

Taken together, our results indicate that selection into public-sector employment among more politically engaged or civically minded citizens cannot alone explain higher rates of political participation among public employees. Instead, we find clear evidence of employment-related self-interest—although this mechanism cannot explain more general turnout increases among educators

and family members even when their own employment is unaffected by the election outcomes.

We conclude by considering the broader political consequences of these findings. Although public employees participate at disproportionately high rates in local elections, we show that voters with a direct employment stake in school board contests nevertheless constitute a surprisingly modest share of the electorate, even in otherwise very low-turnout elections. In the average Ohio school district, this "vested interest" bloc—teachers, non-teaching staff, and household members of both—accounts for less than 5 percent of voters in the school board election and is too small to affect the outcome of the majority of the relevant candidate races and tax referenda. The bloc's share of voters is only about one percentage point larger in the typical district's off-cycle election than in the presidential election, even though overall turnout is nearly three times higher in the presidential election. Taken together, our results make clear that election timing alone cannot explain the overwhelming electoral success of public employee unions in local elections documented in prior research. Understanding how public employees achieve such consistent electoral success despite representing only a modest share of the electorate thus remains an important unresolved puzzle.

In addition to the relevance of our findings for debates about the sources of public sector employee political power, our study has several broader implications for our understanding of American politics.

First, our findings speak to a large literature on political mobilization and policy feedback. Existing research shows that individuals who receive government benefits often become more politically engaged (Campbell 2003, Mettler 2007, Clinton and Sances 2018), yet such work rarely examines public employees themselves or considers government employment as a form of policy benefit (but see Flavin and Hartney 2015). In addition, prior research generally focuses on direct beneficiaries alone. By showing that living with an educator increases turnout, we document both how the political consequences of public-sector employment can be amplified through social networks and the limits of such spillovers.

Second, our results are relevant for understanding the broader political economy of school choice programs—including charter schools and private school vouchers. Existing research in this area asks how such programs affect political participation and, ultimately, public school governance (see, e.g., Cook et al. 2020, Mcilwain, Reckhow and Gordon 2025). There is some evidence of decreased

engagement in local politics among parents who opt their children out of traditional public schools. Our findings complement these results by providing evidence that school choice policies can also reduce turnout among *school employees* relative to individuals with comparable jobs in traditional public schools. The net effect of school choice programs on local school elections—at least the portion operating through changes in political participation—thus depends on the extent to which the policy and candidate preferences of parents and school personnel are aligned.

Ohio Institutional Setting and Data

Our Ohio analysis combines administrative employment data covering the Ohio public school system with historical snapshots of the state's voter file, and we complement these with hand-collected data on local school board elections and results from school tax referenda from the Ohio School Boards Association. We provide a brief overview of each source before proceeding to the analysis.

The administrative employment records are sourced from the Ohio Department of Education and Workforce. These records are organized by academic year and combine information about individual employees with details about each person's specific job assignment, allowing us to compare teachers and non-teaching staff. Although non-teaching staff include both blue-collar occupations, such as janitorial staff and bus drivers, and administrators, the public school sample has far more of the former than the latter. By contrast, charter school staff comprise far fewer blue-collar workers, perhaps because these schools contract out with third-party providers for many of these services. We account for these sectoral differences by applying weights in our analysis. The version of the data we use covers 20 school years, from 2005-06 through 2024-25, and includes hundreds of thousands of unique individuals.

An important feature of the Ohio context, which provides the basis for some of our research designs, is that public education provision is divided between two types of organizations. Most students attend—and most employees work for—traditional public schools. However, the state also has a sizable charter school sector that enrolls about one in ten students. Charter school enrollment is concentrated primarily around Ohio's large urban areas, a legacy of the enabling legislation that initially allowed charters to open in communities with the lowest-performing public schools (see Cook 2018). These geographic differences in the location of public and charter schools are captured

by various fixed effects in the estimating equations and so do not undermine our empirical strategies. The state employment data cover all school employees regardless of sector, with approximately 14 percent of the positions representing assignments at charter schools.

The Supplemental Appendix describes our pre-processing, which reconciles broken linkages and harmonizes the data, and explains our coding rules. To summarize, we fix each employee's position (teacher vs. not) the first time they are observed in the data. We also allow the sector of employment to change over time, as employees move from charter to public schools.[1] For a very small number of individuals[2] who are observed working in both sectors in a single year, we treat them as a public school employee, although our results are not sensitive to these choices.

The employment records are then linked to annual snapshots of the Ohio Secretary of State's voter files, which are available back to 2007 and obtained from private vendor L2. The voter files record historical turnout in elections, residential address (time-varying), and identify other household members who share the same address (also time-varying). These files require some harmonization as well, which we also describe in the Supplemental Appendix. In addition, for some auxiliary analyses, we add in commercially sourced demographic information on voter educational attainment, household composition, and housing tenure available in other L2 files.

Because both the employment and voter records include full names as well as year of birth, we can merge the two with a high degree of accuracy. We find a matching voter record for 95 percent of traditional public school employees and 87 percent of charter school employees (89 and 79 percent are linked one-to-one; see the Supplemental Appendix). The differential matching does not affect our results, which are robust to alternative assumptions about selection into matching using a bounding exercise in the spirit of Lee (2009) (reported in the Supplemental Appendix).

Before proceeding, we briefly address one important consideration related to the construction of our sample and the analytical approach. All of the results reported below condition on being registered to vote. This is necessary to locate an employee's residential address and to identify their household members. As noted by Nyhan, Skovron and Titiunik (2017), conditioning on registration status can introduce post-treatment bias in some applications if the probability of registration is itself impacted by the treatments of interest. This is less of a concern in our study because we

[1] Transitions in the opposite direction are much rarer.

[2] This number is well below 1 percent.

utilize a panel design that follows the same voters over time. By construction, the leverage in our analysis comes from voters already registered before being exposed to any of our "treatments" of interest.

Nevertheless, it is still possible that conditioning on registration may cause us to underestimate the total effects of interest, to the extent that the same mechanisms that increase turnout also cause someone to become registered.[3] We see some evidence of this—alternative modeling approaches that do not condition on registration (and treat someone as not voting in all elections prior to observed registration) generally produce larger estimates. However, these also show more severe violations of parallel trends and the unconditional results are more sensitive to assumptions about such violations. For those reasons, we do not prominently feature them in our exposition.

Motivating Examples

We begin with two illustrative examples that provide intuition for the sources of identifying variation we use in our full analysis. These examples also do a nice job of previewing many of the results that emerge from the more sophisticated difference-in-differences models introduced in the next section.

In Ohio, school board elections are intentionally held off-cycle, in November of odd years. Other municipal offices and certain judicial positions are also filled in these elections. Therefore, we begin by examining turnout in the November 2025 school board election cycle and contrasting it with turnout from the presidential election that took place exactly one year earlier.[4]

The top panel of Figure 1 presents the turnout breakdown for different subsets of voters. In the 2025 election, fewer than one-third of all registered voters cast a ballot. This is comparable to turnout in off-cycle local elections found in earlier studies (Hajnal and Lewis 2003, Kogan, Lavertu and Peskowitz 2018, Payson 2017). The first black bar in the figure presents turnout for voters without any direct employment connection to the public school system in the state. Subsequent black bars present comparable breakdowns for teachers and voters who live in the same household as teachers, with separate estimates by sector (public vs. charter) and, for public schools, residential location. The latter distinction separates teachers (and household members) who live and work in

[3]Since someone has turnout rates of zero prior to registration by definition, the resulting increase in voting is thus mechanically larger for individuals on this treatment margin.

[4]The Cleveland Metropolitan School District operates under a system of mayoral control and does not have an elected school board. We exclude its residents from the descriptive and compositional analyses.

the same school system—and where they are thus able to influence the election of their bosses—to teachers who work in one school district but are eligible to vote in another. A similar breakdown for non-teaching staff is presented in the Supplemental Appendix.

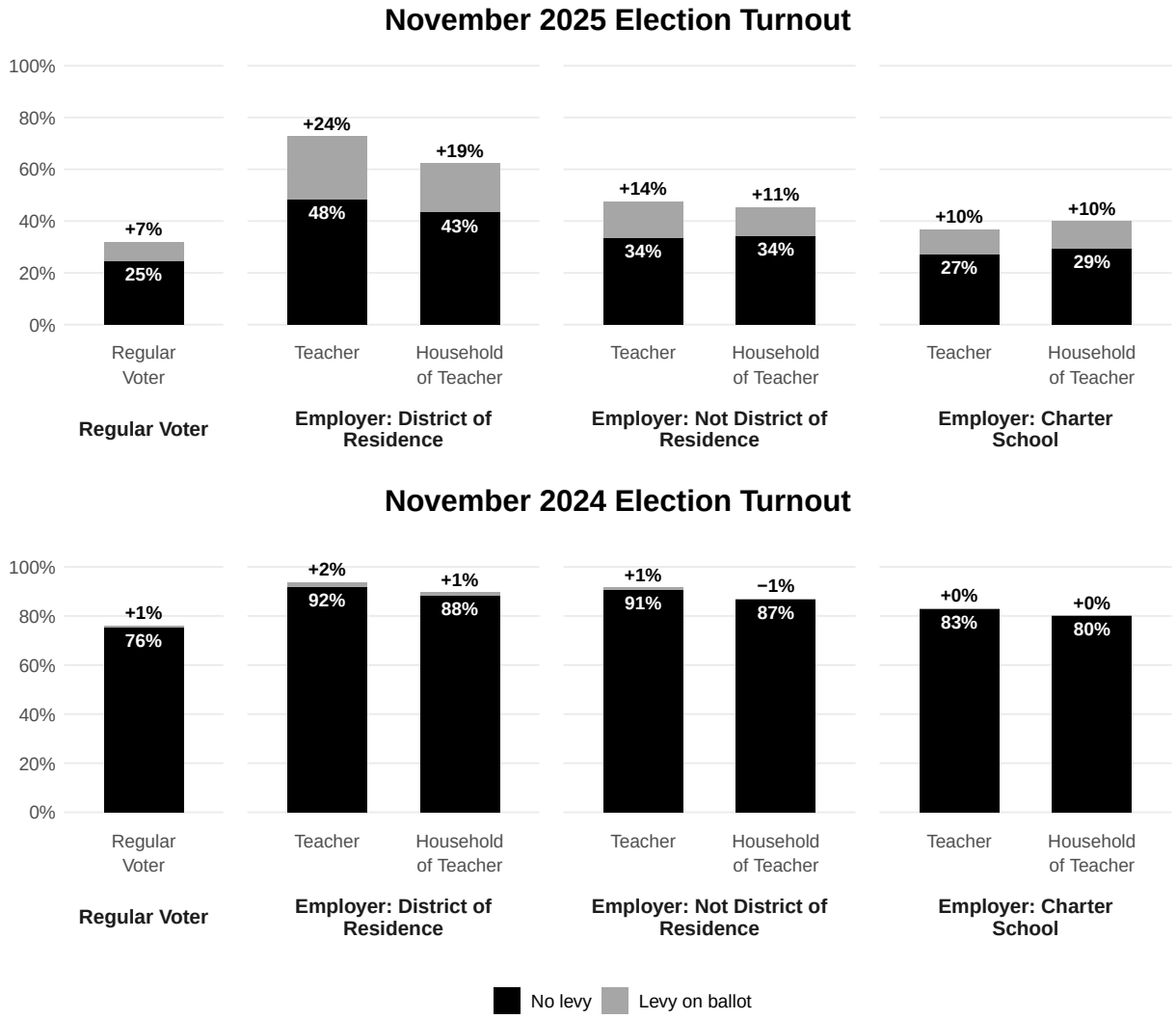

Figure 1: Turnout in Ohio November 2024 and 2025 Elections

Note: Black bars show turnout among registered voters whose school district of residence had no school levy on the November ballot, taken directly from the voter file. Gray bars show the estimated increase in turnout when the district of residence has a levy on the ballot, from a within-voter comparison with the previous election of the same type (2025 vs. 2021; 2024 vs. 2020). "Regular voters" are a random 10% sample of registered voters who are neither school employees nor household members of school employees.

Consistent with Moe (2006), we observe significantly higher turnout among teachers—with important differences across sectors and residential locations. Teachers who lived and worked in the same public school district recorded turnout nearly double that of unaffiliated voters. Teachers employed by other public schools were significantly more likely to vote, although the magnitude

of the difference—approximately 9 percentage points—is less than half the size of the one we find among resident-employees (24 points).

Interestingly, we also see evidence that public school employment appears to boost the turnout of voters who live in the same household—especially when the employees (and household members) are eligible to vote in the school district of employment. By contrast, charter school teachers vote only slightly more often than unaffiliated voters (27 vs. 25 percent).

In Ohio, public schools are funded by both state dollars and local property taxes, with each source contributing roughly equal amounts in the aggregate. Local property tax dollars are authorized by voters through levies. Many of the levies are temporary, requiring regular renewal, and even the permanent ones are set in nominal (rather than inflation-adjusted terms), ensuring that districts must regularly return to the voters for reauthorization or increases (see Kogan, Lavertu and Peskowitz 2017 for details). An important feature of this system is that levies appear on the ballot in different elections in different districts due entirely to idiosyncratic factors. Cook, Lavertu and Miller (2021) provide evidence that the variation in the timing of local levies is plausibly exogenous, a fact that provides an additional source of identifying variation.[5]

The gray bars in Figure 1 show how having a levy on the ballot, in addition to other local races, affects turnout among different voters. Although the presence of levies boosts turnout across the board, the effect is larger among teachers of all kinds, larger even still among public school teachers, and greatest of all among teachers who live and work in the same school district. We find similar patterns among voters who live with teachers as well.

The bottom panel of the figure shifts our attention to the 2024 presidential election, where baseline turnout exceeded 75 percent. All teachers (and their household members) still voted at higher rates than unaffiliated members of the electorate in this election, although the differences are much smaller in absolute terms. In addition, the presence of levies seemed to have minimal (if any) impact on turnout across the board. The gap in turnout between teachers based on their residential location was less than one-tenth of the size in this on-cycle national election compared to the local elections the following year.

In the next section we combine these different sources of variation—in sector of employment,

[5]Levies may cover different combinations of operating and capital expenditures, and also vary depending on whether they represent renewal of existing funding or increases. We also take advantage of these differences in our analysis below.

occupation, residential location, and presence of school levies—to provide causal estimates of public-sector employment on political participation and decompose the relative importance of different mechanisms that give rise to these effects.

Ohio Difference-in-differences Estimates

In this section, we present results from a variety of difference-in-differences analyses that examine whether public-sector employment increases turnout and how these effects vary depending on the nexus between the outcome of local elections and individual voters' occupational self-interest. We describe the intuition behind each set of comparisons before presenting the results, but all of the event-study estimates use the Borusyak, Jaravel and Spiess (2024) imputation estimator, which is robust to staggered treatment timing. Briefly, the estimator fits individual and election fixed effects using only untreated observations (never-treated voters, and treated voters before their hire), uses them to impute each treated observation's untreated turnout, and averages the differences between observed and imputed turnout.

We prefer the Borusyak, Jaravel and Spiess (2024) estimator because it allows us to simultaneously consider different kinds of treatment (e.g., public vs. charter school employment), and because it can flexibly be adapted to include additional types of fixed effects (e.g., school-district-by-election). The Supplemental Appendix reports additional results using alternative control groups (discussed below) and several alternative estimators, and none of our key results depend on these modeling choices. The appendix also reports the sensitivity of our estimates to violations of the parallel trends assumption, which is necessary to interpret the results as causal, using the approach developed by Rambachan and Roth (2023).

One important note on the presentation of the results: As Roth (2026) shows, imputation estimators construct pre- and post-treatment coefficients asymmetrically, relative to different baselines. As a result, conventional event-study plots can appear to have hinge points at the time of treatment that do not represent actual treatment effects. Following his recommendation, we plot all coefficients—before and after treatment—relative to a common reference period.[6] The average treatment effects are reported relative to turnout pooled across all pre-treatment elections.

[6]The standard errors of the plotted coefficients account for estimation uncertainty in the reference period.

Timing of Employment Spells

The main empirical challenge in our setting is that turnout differentials between educators and regular voters may reflect the effects of working in the public sector, demographic differences between the two groups (some observable and some not), or a combination of the two. For example, almost 98 percent of the teachers in our sample have a college degree and nearly 65 percent have a graduate degree, either a master's or a doctorate. Educational differences alone would predict higher rates of political engagement (e.g., Brady, Verba and Schlozman 1995, Wolfinger and Rosenstone 1980), and teachers could differ from the broader population on other dimensions as well. Similar factors may also contribute to the observed differences across sectors. Specifically, charter school teachers in our sample are younger, less likely to have advanced degrees, and are paid lower salaries than public school teachers—all of which should produce higher rates of electoral participation among public school teachers.

To isolate the effects of school employment from pre-existing and demographic differences that might influence both turnout and selection into the education workforce, we begin by examining changes over time that coincide with when individual voters begin working in schools. This analysis is limited to the subset of individuals who begin their employment spells during the period covered by our sample. The samples include 53,278 traditional public and 11,197 charter school teachers, and 120,884 traditional public and 8,860 charter school non-teaching staff. Comparing only within-voters—by including individual-level fixed effects—also absorbs any time-invariant characteristics that may vary across individuals. (We return to potential time-varying confounds below.)

Figure 2 presents the event study results. The top two panels plot separate estimates depending on the sector of initial employment—traditional public school or charter—while the bottom panels show the difference between the two sectors. Overall, the results show that voters who become public school teachers see their turnout increase by roughly 6 percentage points after they start their jobs, without similar increases (and perhaps even decreases in the short-term) for teachers who instead find jobs in charter schools. Among non-teaching staff, we also see a significant increase that is also limited to those who begin employment in the traditional public schools. This effect is about a third of the size of the turnout increase observed for teachers, and is also less plausibly causal due to significant pre-trends.

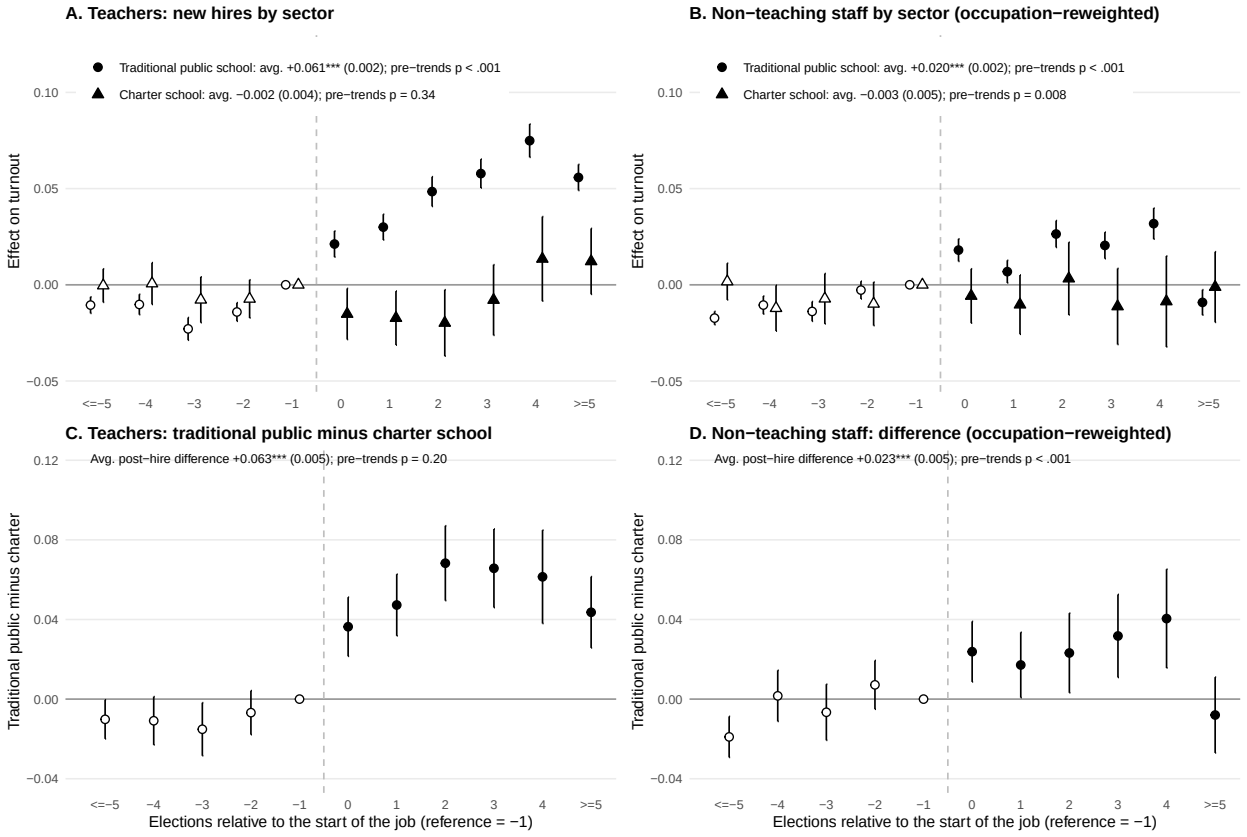

Figure 2: Effect of Starting School Employment on Odd-Year Turnout

Note: The figure plots event-study estimates of the effect of beginning employment in an Ohio public school on turnout in odd-year November general elections. Top panels report effects separately for employees whose first job is in a traditional public school (circles) or a charter school (triangles) and for teachers (A) and non-teaching staff (B). Panels C and D report the difference between the two sectors, separately for teachers and non-teaching staff. For non-teaching staff (Panels B and D), traditional public school employees are reweighted to match the occupational composition of charter school non-teaching staff, based on the position code in their first year of employment. The reported pre-trend p-values are from joint Wald tests that the pre-hire coefficients equal zero. Vertical bars show 95 percent confidence intervals based on standard errors clustered by individual.
Stars denote significance after a Holm adjustment (*** p < .001, ** p < .01, * p < .05)

Giving these estimates a causal interpretation requires assuming parallel trends—that charter and public school teacher turnout would have evolved similarly over time. As can be seen in Panel C of the figure, the lack of significant pre-trends is consistent with parallel trends. In addition, sensitivity estimates presented in the Supplemental Appendix show that the significant differences in the effects between public and charter school teachers can survive even substantial violations of this assumption, although the differences for non-teaching staff are less robust.

One remaining concern, however, is that the timing of when one begins employment in the public schools may be confounded by other contemporaneous life events—such as graduating from college or moving—that by definition vary over time and are not absorbed by the individual-level

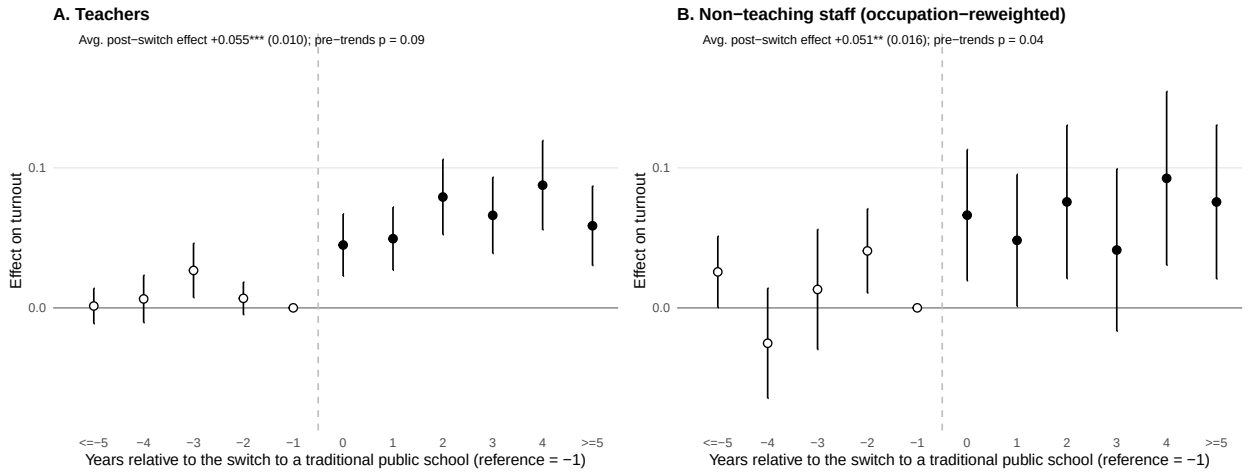

Figure 3: Effect of Switching Employment from Charter to Traditional Public School on Odd-Year Turnout

Note: The figure plots event-study estimates of the effect of moving from a charter school to a traditional public school on turnout in odd-year November general elections, separately for teachers (A) and non-teaching staff (B). In panel B, non-teaching switchers are reweighted so that the distribution of their occupation matches that of non-switching charter school staff. The reported pre-trend p-values are from joint Wald tests that the pre-switch coefficients equal zero. Vertical bars show 95 percent confidence intervals based on standard errors clustered by individual. The estimates draw on 4,927 teachers and 1,483 non-teaching staff who switch sectors.
Stars denote significance after a Holm adjustment (*** p < .001, ** p < .01, * p < .05)

fixed effects. Because charter school teachers are, on average, two years older than public school teachers when they begin their jobs, contrasting the two does not necessarily difference out these confounds.

We address this possibility in two ways. First, we compare new teachers to a set of clean controls—voters without any employment connection—who are exactly matched based on year of birth and district of residence at the time that employment begins. Second, we also reweight public school teachers so that they look exactly like charter school teachers, not just in terms of age but also other demographic characteristics we observe in the employment records, including age at time of hire, educational attainment, hire year, gender, race/ethnicity, prior turnout and registration, county, and residency in one of the urban "Big-8" Ohio school districts at the time of hire. Results from both sets of estimates are presented in the Supplemental Appendix and they are, if anything, somewhat larger than the ones we report here.

Our second empirical strategy is to zoom in only on charter school employees, and examine what happens to their turnout when they find jobs in traditional public schools. The results are reported in Figure 3 and they show significant increases in turnout on the order of 5 to 6 percentage

points after charter employees switch to working in traditional public schools. For teachers, this effect is nearly identical to our new-hire estimate from above. For non-teaching staff, however, it is considerably larger, although as before we see evidence of significant pre-trends among non-teaching staff.[7]

Residential Location and Self-Interest

The results in the previous section show that public school employees—most robustly, teachers—vote at higher rates than their charter school counterparts, and this gap cannot be explained by pre-existing differences. One alternative explanation is that public school employees have a more personal, self-interested stake in the outcome of these local elections because their future bosses are on the ballot. Of course, this is not the only possible explanation. Charter and public schools differ on a number of dimensions, perhaps most crucially in whether teachers who work in them are members of unions. While public school teachers are unionized, most charter school teachers are not.

To try to isolate the role of self-interest from other sectoral differences, we use a different design inspired by Moe (2006) and suggested by the descriptive data in Figure 1. Specifically, we limit our comparison to *within* public school employees—those who live and work in the same school district and those who don't. The intuition is that the employees have a personal stake in the outcome of the election that can be influenced by their own vote only when they reside in their district of employment. Other job characteristics—including, most crucially, unionization status—do not vary systematically with an individual's residential location.

Figure 4 presents two sets of estimates based on this comparison. The left panel focuses on new teachers. It compares those whose first job is in the district where they already live (about one in four new hires) with other new teachers who commute to a different district. Beginning with the first election after they begin their jobs, turnout among teachers who work where they live rises sharply relative to their commuting colleagues. The difference is largest in the year of hire (4.2 points) and remains positive for the next four years. The same 2.8-point gap appears in even-year elections. Turnout of the two groups moves together in the two years before hire, but further back

[7] About half of non-teaching staff who move from charter schools to work in traditional public schools also become teachers after the move. However, we find similar differences in the turnout change regardless of whether sector-switchers also change their positions.

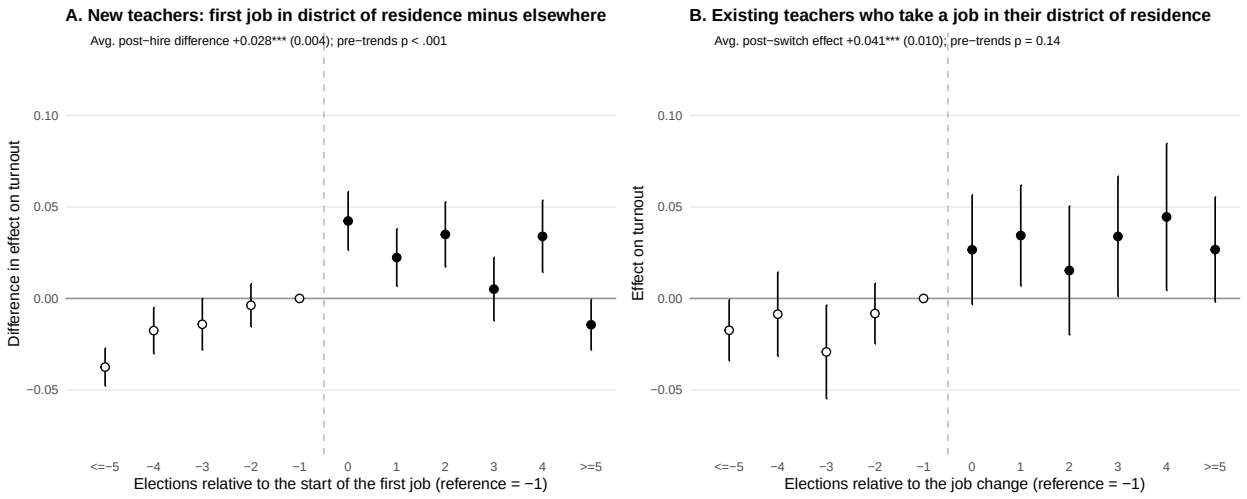

Figure 4: Effect of Finding Teaching Job in District of Residence vs. Commuting on Odd-Year Turnout

Note: The figure plots event-study estimates of the effect of working in one's district of residence on turnout in odd-year November general elections among traditional public school teachers. Panel A reports the difference in estimated effects between new teachers whose first job is in the school district where they reside and new teachers whose first job is elsewhere. Panel B reports the effect of taking a job in one's district of residence among existing traditional public school teachers. Residential location is measured in the last voter file before initial hire or, if the employee does not appear in one, the first subsequent file (Panel A), and in the last voter file before the job switch (Panel B). The reported pre-trend p-values are from joint Wald tests that the pre-event coefficients equal zero. Vertical bars show 95 percent confidence intervals based on standard errors clustered by individual. Panel A includes 13,130 teachers whose first job is in their district of residence and 40,118 whose first job is elsewhere, while Panel B includes 2,857 teachers who take a job in their district of residence and the teachers who do not. Stars denote significance after a Holm adjustment (*** $p < .001$, ** $p < .01$, * $p < .05$)

in-district teachers voted less than other new teachers, so the pre-hire coefficients trend upward. One interpretation is that these teachers simply settle into the community where they will later work, becoming more engaged in local politics, before they are hired there.

The right panel instead follows teachers already working in traditional public schools and estimates the effect of taking a new job in their district of residence. In this design, residence is fixed in the last voter file before the teacher's current spell of employment begins, and teachers already working in their home district at that point are excluded. Residence is therefore always measured before the job change, so the variation comes from teachers who change jobs, not from teachers who move into the district where they already work. For new teachers (the left panel), residence is measured in the last voter file before they are hired or, for those who do not yet appear in that file under their current voter record, in the first file after hire.

For new hires, those who begin working in their district of residence see their subsequent turnout increase by 2.8 percentage points, although it is difficult to separate this effect from the visible pre-

trend.[8] Among teachers already working in traditional public schools, those who take a new job in their district of residence see their odd-year turnout rise by a larger 4.1 percentage points relative to teachers who continue to work elsewhere. In this sample, we find no evidence of significant pre-trends, although this test admittedly has low statistical power.

Given these pre-trend limitations, we assess the sensitivity of both estimates using the methods of Rambachan and Roth (2023). Specifically, we ask how large post-period violations of parallel trends—relative to the largest change between adjacent pre-period coefficients—can be before the 95 percent confidence interval for the average effect over the first five years after hire includes zero. For new teachers, the estimate remains positive when post-period violations are as large as one quarter of the largest pre-period change ($\bar{M} = 0.25$). For existing teachers who switch districts, the confidence set includes zero even at $\bar{M} = 0.25$, the smallest value we consider. Neither average survives if the pre-period trend is extrapolated linearly into the post-period. The immediate effect for new teachers is more robust, as the difference at the first post-hire election remains positive for violations as large as the largest pre-period change ($\bar{M} = 1$) and under linear extrapolation. We view these overall results as suggestive of self-interest being an important mechanism. The levy analysis below provides separate—and more definitive—evidence that self-interest is indeed a key motivator of political participation for teachers.

In the interest of space, we present comparable results for non-teaching staff in the Supplemental Appendix. Overall, they are quite similar to our results for teachers, and subject to the same limitations about sensitivity to violations of parallel trends.

Household Spillovers

So far, our analysis has focused on the turnout of school employees themselves. In this section, we consider potential spillovers to other members of their households, whom we can identify based on shared addresses in the voter file.[9]

In our analysis, we separate voters who live with school employees into different clusters based on *when* the cohabitation occurs. Some voters are already members of an employee's household in

[8] Restricting the comparison to new teachers whose residence is observed before hire reduces the odd-year gap by about a third, to 1.9 percentage points, but it remains statistically significant ($p < .001$; see the Supplemental Appendix).

[9] We exclude cases where more than six voters are listed as living in the same household.

the year before they begin their employment and remain living with them afterward. Others join the household later, although we cannot observe the gap between when the two first met and when they moved in together. The final category includes voters who live with a school employee when they begin their job but subsequently move out.[10] Our main analysis focuses on the first and last category, since the expected timing of the spillover effects for them is unambiguous.[11]

To exclude parents and children, we restrict attention to household members born within ten years of the school employee. The untreated comparison group in this analysis is drawn from a 10 percent random sample of registered voters pooled across all voter files, purged of anyone who ever resides at the same address as a school employee. From this pool, we draw one control for every two household members within each birth cohort, so that the controls match the age distribution of household members. All treated household members are assigned the hire dates of the employee with whom they reside (or will eventually reside).

Figure 5 reports the results for voters who live with teachers at the time they begin their employment. The left panel includes separate estimates for household members of both traditional public and charter school teachers, and both groups show clear trends toward increased turnout that precede the hiring. For each group separately, it is difficult to disentangle any spillovers from these pre-trends. In the right panel, however, we focus on the *difference* between voters who live with teachers working in traditional schools and those working at charter schools. Prior to starting the positions, the groups move in parallel, with no visible pre-trend. After the hire date, turnout of voters living with traditional public school teachers increases by about 3.5 percentage points, roughly half the size of the effect we find among teachers themselves in Figure 2. These estimates are reasonably robust to violations of parallel trends. The gap between household members of traditional public and charter school teachers remains positive under linear extrapolation of any pre-hire differential trend and for post-hire violations up to one quarter of the largest pre-hire change ($\bar{M} = 0.25$; Rambachan and Roth 2023).

A comparable figure for non-teaching staff is reported in the appendix and follows the same

[10] It's possible for someone to move in after the employment spell starts and move out before it ends, but that is a relatively rare outcome in our data.

[11] Household members of traditional public and charter school teachers are equally likely to have moved out within two and four years of the hire (24 and 32 percent in both sectors), although over the full follow-up members of traditional public school teachers' households are 3.8 percentage points less likely ever to move out. Conditioning on household member type is therefore unlikely to introduce substantial post-treatment bias.

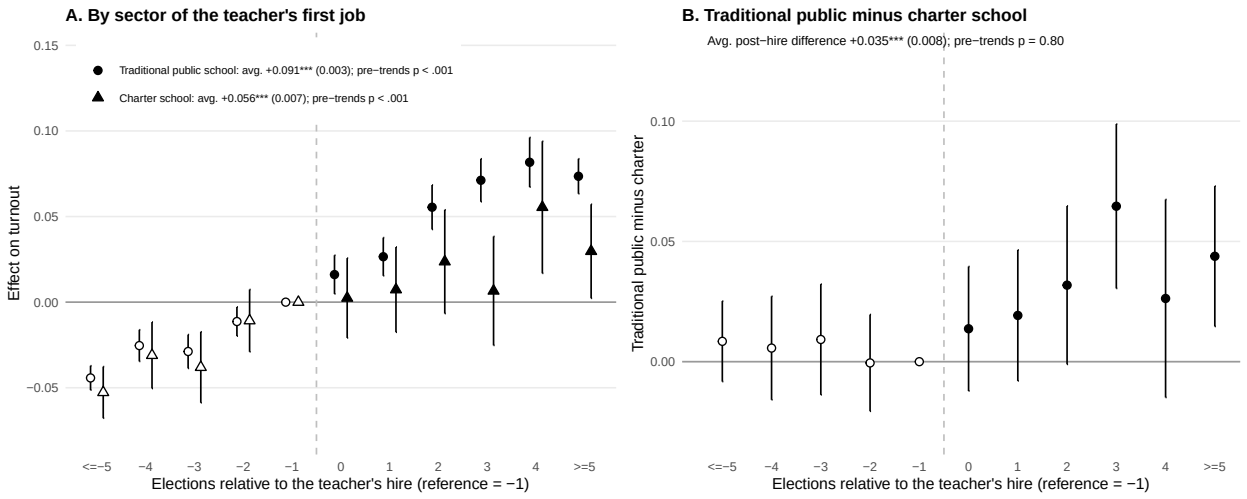

Figure 5: Effect of Household Member Obtaining Teaching Job on Odd-Year Turnout

Note: The figure plots event-study estimates of the effect of another member of a voter's household beginning employment as a teacher on the focal voter's turnout in odd-year November general elections. Household members are other registered voters at the teacher's residential address. The sample includes voters who live with the teacher at the time of hire and remain in the household in the teacher's most recent voter file, limited to voters born within ten years of the teacher. Panel A reports separate estimates for household members of teachers whose first job is in a traditional public school (circles) or a charter school (triangles). Panel B reports the difference between the two sectors, estimated jointly with a common set of "control" voters. These controls are registered voters drawn from a random sample of Ohio voters who never reside with a school employee, matched to household members on birth year. The reported pre-trend p-values are from joint Wald tests that the pre-hire coefficients equal zero. Vertical bars show 95 percent confidence intervals based on standard errors clustered by individual. The estimates draw on 17,831 voters who live with traditional public school teachers and 3,580 voters who live with charter school teachers. Stars denote significance after a Holm adjustment (*** p < .001, ** p < .01, * p < .05)

general pattern but with point estimates that are somewhat smaller.

By contrast, when we focus on household members who move out—and most do so within two years of their roommate starting a job as a teacher—we see no differences between those who previously lived with traditional public as opposed to charter school teachers. Turnout of voters who previously lived with traditional public school teachers declines slightly and insignificantly (-0.7 points), while that of voters who previously lived with charter school teachers drops by a statistically significant 1.8 points, although the difference between the two is not itself significant. In Figure 6, we separate these former household members based on their new residential location, comparing those who continue to live in (or move into) the same school district that employs their former roommate and those who move elsewhere. Perhaps surprisingly, we see no difference between these two groups. Put another way, although public school teachers vote at higher rates and appear able to increase the turnout of others who live with them, they fail to mobilize even their former roommates who remain eligible to vote in elections that can impact their district of employment.

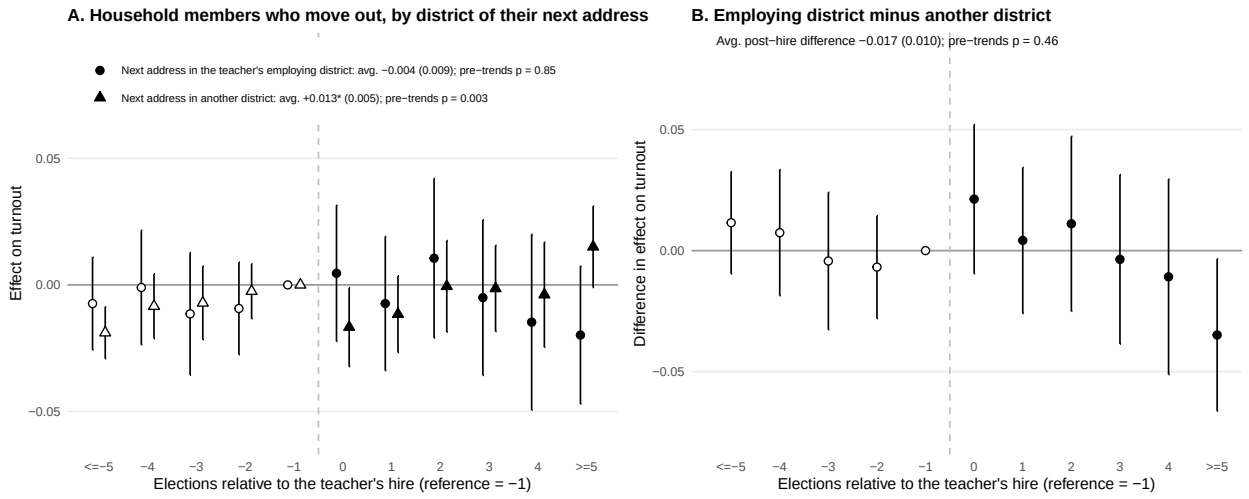

Figure 6: Effect of Voter Obtaining Teaching Job on Odd-Year Turnout of Former Roommates, by Subsequent Residential Location

Note: The figure plots event-study estimates of the effect of another member of a voter's household beginning employment as a teacher on the focal voter's turnout in odd-year November general elections. Household members are other registered voters at the teacher's residential address. The sample includes voters who live with a traditional public school teacher at the time of hire but are no longer in the household in the teacher's most recent voter file, limited to voters born within ten years of the teacher. Panel A reports separate estimates for voters whose next address after leaving the household is in the school district that employs the teacher in that year (circles) or in another school district (triangles). Voters with no subsequent voter record are excluded. Panel B reports the difference between the two groups, estimated jointly with a common set of control voters. The reported pre-trend p-values are from joint Wald tests that the plotted pre-hire coefficients equal zero. Vertical bars show 95 percent confidence intervals based on standard errors clustered by individual. The estimates draw on 2,660 voters whose next address is in the teacher's employing district and 7,671 voters whose next address is in another district. Stars denote significance after a Holm adjustment (*** p < .001, ** p < .01, * p < .05)

This seems inconsistent with an account in which teachers (and their unions) exercise political clout primarily by mobilizing total strangers.

Confirmatory Evidence from School District Tax Levies

We conclude this portion of the analysis with perhaps the cleanest test—leveraging variation across districts in the timing of their school levies. In particular, we ask whether the presence of levies increases turnout—and whether an individual voter's employment status and household membership amplify the increase. As above, we focus on within-voter variation, comparing effects before as opposed to after they begin their employment. In these specifications, we include individual-voter, residence-district-by-election-year, and group-by-event-time fixed effects, where groups are defined by sector, occupation, and whether the employee lives in the employing district. The latter fixed effects allow us to isolate the levy effects by removing each group's own turnout path around the

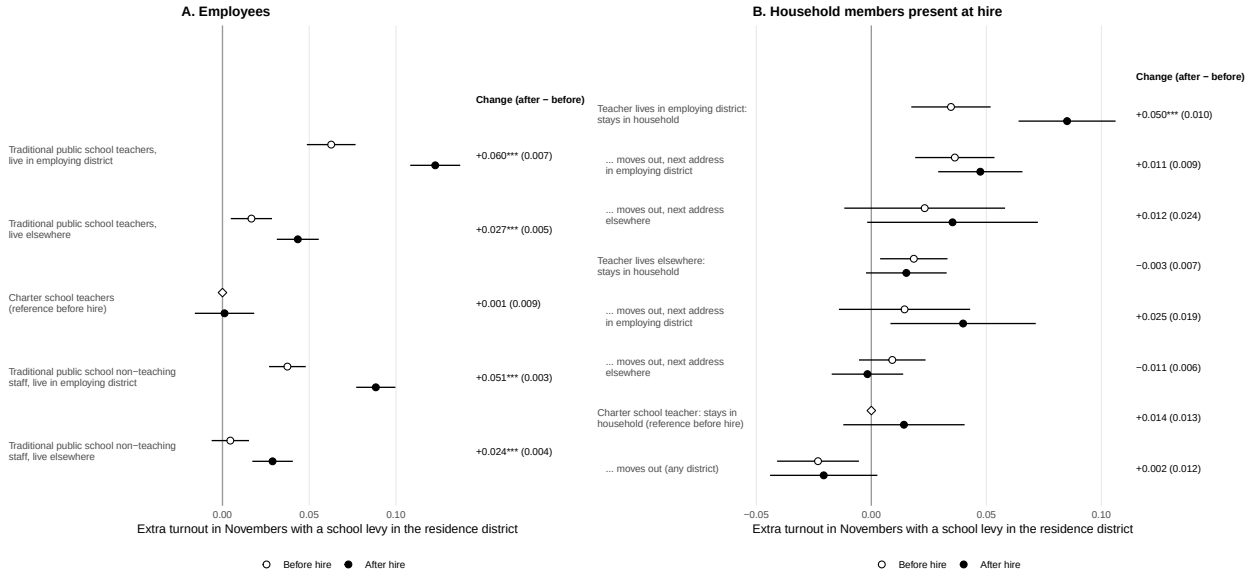

Figure 7: Effect of Having School Levy on the Ballot on Odd-Year November Turnout

Note: The figure plots the estimated effect on turnout of having a school levy on the odd-year November ballot in the voter's district of residence, separately for elections before (open circles) and after (filled circles) an employee's start year. Panel A reports estimates for new school employees, by sector and role and, for traditional public school employees, by whether they live in the district that employs them. Panel B reports estimates for household members who live with a new teacher at the time of hire, separately for members who remain in the household and members who later move out. For the latter, district of residence is based on their subsequent address. Estimates come from regressions of turnout on interactions of a levy indicator with group and with before- versus after-hire periods, controlling for individual fixed effects, residence-district-by-election-year fixed effects, and group-by-event-time fixed effects. Levy effects are measured relative to pre-hire baselines of charter school teachers (Panel A) and household members who stay with charter school teachers (Panel B), which are fixed at zero. The right-hand column reports the change in the levy effect from before to after start of employment. Horizontal bars show 95 percent confidence intervals based on standard errors clustered by individual.
Stars denote significance after a Holm adjustment (*** $p < .001$, ** $p < .01$, * $p < .05$)

start of their employment spells, accounting for any general increase in turnout from becoming a school employee and any pre-hire trend. Given the large number of estimates, we apply the Holm adjustment to account for multiple comparisons in flagging statistical significance.

The results, presented in Figure 7, reveal several important new findings. First, we see clear evidence of selection into public school employment—future teachers, and future non-teaching staff who live in the district that will hire them, already participate in local school levy elections at higher rates than their charter school counterparts even before they begin work. This gap is especially large for employees who live in the same district that later goes on to hire them. However, we see a significant increase in turnout after they begin their employment—especially when an employee works and votes in the same school district. In the right panel, we find similar results among voters who don't work in public schools but live with others who do. Turnout in levy elections

increases significantly, by nearly as much as among employees, but only for voters who live with educators employed in the local school district. We see no increase among household members of charter school teachers nor among those who live with teachers employed outside of their district of residence. And as before, the increase is observed only among household members who remain, not among former roommates who move out after a teacher begins their job.

To provide further evidence that self-interest contributes to these turnout effects, we also split the levy results by type, based on purpose (capital vs. operations) and funding source (renewal of existing levy vs. increase in funding). While we don't find consistent differences between levies that fund facilities as opposed to operations (including employee salaries), the turnout boost is significantly larger for levies that deliver additional tax revenues than for those that simply continue existing funding levels. One explanation for the gap is that levies to increase taxes are ex ante more competitive—only 37 percent pass, compared to 90 percent for renewals—producing more incentive for mobilization. This too is consistent with a self-interest explanation.

These findings confirm and underscore three key points from earlier analyses. First, selection into public school employment cannot explain the higher turnout we see in cross-sectional data. Employment itself plays a causal role. Second, self-interest—revealed by differences based on residential location—plays an important role, but is not the only mechanism producing greater engagement. And third, employment effects produce spillovers, but these appear to be fairly limited in scope—seen in current household members but not former roommates. If public employees are unable to mobilize their former roommates who remain eligible to vote in the same elections, it seems even less plausible that they can mobilize complete strangers.[12]

Do Mobilization Effects Persist in Even-Year Elections?

All of the results presented thus far have focused on odd-year elections, when baseline rates of turnout are quite low. In the Supplemental Appendix (Table A1), we present comparable estimates for even-year midterm and presidential elections.

To summarize these results, we continue to find significant increases in turnout following public school employment even in even-year elections. The magnitudes are somewhat smaller for employees

[12]We expect mobilization to depend far more on ex ante expectations about competitiveness than actual, ex post realizations reflected in observed election margins.

(for example, 4.9 vs. 6.3 points for the gap between new traditional public and charter school teachers) and similar for household members (4.5 vs. 3.5 points). Levy effects are also smaller in even years, most of all for household members.

Additional Evidence from an Iowa Natural Experiment

To complement our Ohio analysis, we now turn to a natural experiment in Iowa that provides an entirely different research design to validate our key findings. We begin by explaining the state law change that we leverage in the analysis.

Like Ohio, Iowa historically elected its local officials in the fall of odd years. However, unlike Ohio, Iowa separated its school board elections from other municipal office races, with school board elections appearing on the ballot in September of odd years and other local offices following in an entirely separate election two months later, in November. One consequence of this arrangement was that Iowans posted very low rates of participation in both types of elections.

Figure 8 provides data from the 2015 election cycle. That year, turnout in school board elections was just 7 percent. Teachers who lived in the district that employed them (and their household members) were far more likely to vote while teachers employed elsewhere were only modestly more likely to do so. Nevertheless, two-thirds of resident teachers stayed home in these September elections. Turnout in November municipal elections was somewhat higher, at 12 percent, but still exceptionally low by national standards. In November, when only municipal contests appeared on the ballot, resident teachers were only modestly more likely to vote than regular voters (15 vs. 12 percent), and teachers employed elsewhere were less likely to do so (9 percent).

In 2017, the Iowa legislature passed a significant electoral reform that consolidated local school board and municipal elections, requiring both to be held concurrently in November of odd-numbered years beginning in 2019. Although the bill received near-unanimous support from Republican state legislators, it was opposed by two-thirds of Democratic lawmakers. Media coverage and an interview the authors carried out with one of the bill's sponsors indicated that the partisan breakdown reflected an expectation that election consolidation would reduce the political power of teachers and their unions. (The Iowa State Education Association opposed the legislation for the same reason.)

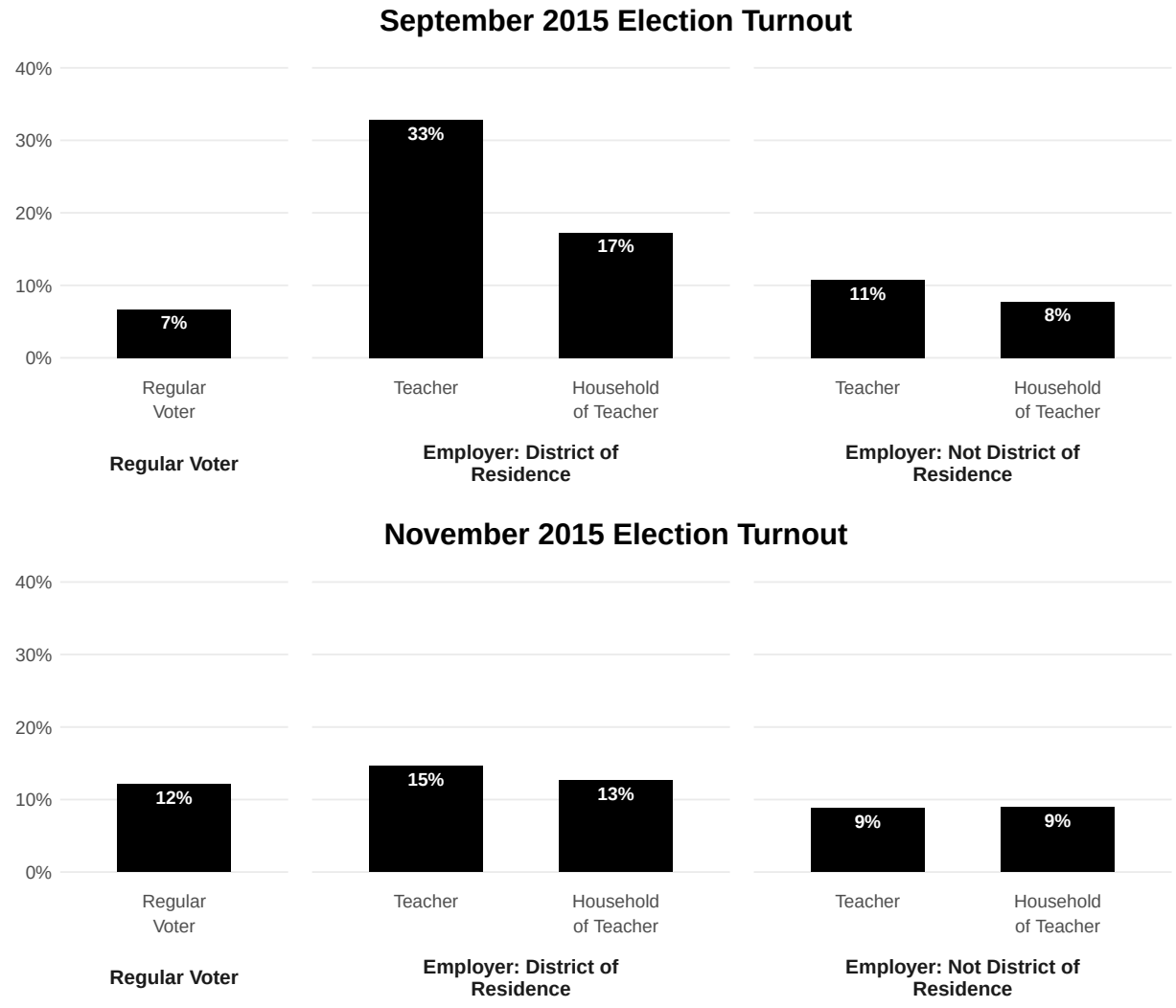

Figure 8: Turnout in 2015 Iowa Local Elections

Note: The figure reports turnout in the September 2015 school board elections (top) and the November 2015 municipal elections (bottom) among Iowa registered voters.

This change in timing motivates our analysis and provides an opportunity to replicate our Ohio results. Whereas in Ohio we examine the consequences of changes in employment status and levy timing while holding elections constant, in Iowa we can leverage changes in electoral rules while holding voters' employment status constant.

To construct our analysis panel, we obtained employment data from the Iowa Department of Education covering all school employees from the 2016-17 school year, during which the election timing legislation was passed. Using these data, we implement a similar difference-in-differences design that examines changes in turnout in odd-year November elections before vs. after school

board races are added to these ballots. In contrast to most of the Ohio analysis, this approach holds constant school employment (and household) status and instead varies the contests that appear on the ballot over time. We expect to see teachers (and their household members) increase their turnout in odd-year November elections once school board contests are added, above and beyond general increases in turnout as a result of consolidation observed among voters without any employment connection. In addition, we expect the increase to be particularly pronounced among teachers who live and work in the same school district, as well as among their household members, reflecting differences in employment-related self-interest.

As Figure 9 shows, that is precisely what we find. In 2019, the first year with a consolidated election, teachers who lived and worked in the same school district saw their turnout increase by 27 percentage points more than unaffiliated voters.[13] Their household members also experienced a turnout boost that is more than a third of the size. By contrast, teachers who lived outside their district of employment—and their household members—recorded much more modest turnout increases. In fact, the boost in turnout among teachers living and working in different districts was roughly comparable to the gains among *non-educators* who resided with educators living and working in the same district (compare the circles in the right panel to the triangles in the left panel). Of course, the only difference between the two groups was whether the outcome of the election would affect their employer.

Among non-teaching staff, we find strikingly similar patterns—although the absolute increase in turnout among each subgroup of voters is roughly half the size of that among teachers. These results are available in the Supplemental Appendix.

[13]We see a very modest, but nevertheless statistically significant, increase in 2017, the election after the bill is passed but before it takes effect.

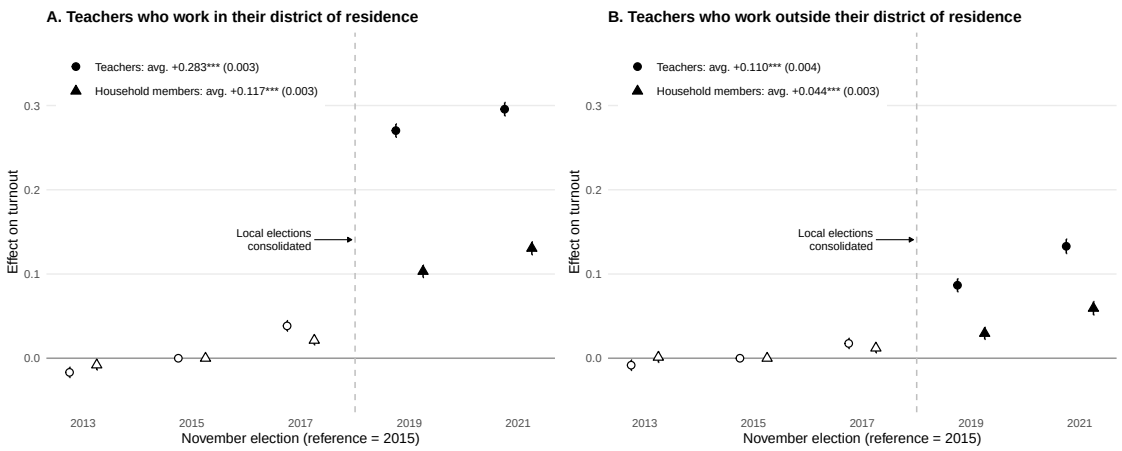

Figure 9: Differential Effect of Iowa Election Consolidation on Turnout in November Odd-Year Elections

Note: The figure plots difference-in-differences estimates of the change in turnout in odd-year November elections for teachers (circles) and their household members (triangles), relative to regular voters for teachers who work in their district of residence (Panel A) and teachers who work elsewhere (Panel B). Effects are measured relative to the 2015 baseline. The average reported for each series is the mean of the 2019 and 2021 estimates. Elections in 2013, 2015, and 2017 are municipal elections held separately from school board elections. Beginning in 2019, school board races appear on the same November ballot. The 2017 estimate captures turnout after the consolidation law passed but before it took effect. Estimates come from a regression with individual and school-district-of-residence-by-year fixed effects. Vertical bars show 95 percent confidence intervals based on standard errors clustered by individual. The estimates are based on 18,626 teachers who work in their district of residence and 20,354 of their household members, 14,251 teachers who work elsewhere and 15,733 of their household members, and 1,976,247 regular voters. Stars denote significance after a Holm adjustment (*** $p < .001$, ** $p < .01$, * $p < .05$)

Differential Turnout Does Not Explain Public Employee Political Power

We now consider the potential electoral consequences of school employee political participation and, motivated by Moe (2006) and Anzia (2013), examine how this is likely to depend on the timing of local school board elections. In these scholars' accounts, political power is exercised specifically by public employees who live and work in the same jurisdiction—the officials who effectively get to choose their bosses on Election Day. As we explain below, higher turnout among these voters is necessary but not sufficient to affect election outcomes. Much depends on the overall size of the voting bloc (Kogan, Lavertu and Peskowitz 2018). After all, a motivated group that turns out even at very high rates may nevertheless account for a small share of the overall electorate.

For exposition, we return to the Ohio November 2025 elections that opened our paper—the most recent low-turnout, off-cycle election in our sample. Using the statewide voter file, we identify all voters who cast a ballot in this election and identify the subset of these voters who work in their

school district of residence and their household members.[14] This includes both teachers and non-teaching staff and household members of each group. We then aggregate up to the school district level, calculating the share of voters in each district who also have an employment connection to that district. We exclude Cleveland, which has an appointed board, and two other school districts that did not have school board races on the ballot that year.

Figure 10 plots the distribution of these compositional measures for the remaining 608 Ohio public school districts in our sample. The left panel shows the share of employees and household members among the overall electorate in each district. If considered as a single voting bloc, this combined group accounted for just 4.1 percent of voters in the average school district. Thus, despite low overall turnout and much higher turnout among employees and their household members, more than 95 percent of voters in the typical district had no vested employment interest in the outcome of the election and did not live with someone who had such an interest. To be sure, there is some variation around these averages across the districts in our sample. However, only five districts had more than 10 percent of voters employed by the district or living with school employees, and in no district did the share exceed 11 percent of the electorate.

To calculate the share of school board races where this small voting bloc could *plausibly* be pivotal, we have collected and hand-coded November 2025 school board elections from the state's 88 counties. Nearly half of the races went uncontested, on par with the national average reported in Kogan, Lavertu and Peskowitz (2025), and the margin of victory in the vast majority of the remaining contests was larger than the size of the employee and household voting bloc. In only 22 percent of the school board races did the total number of school employees and household members exceed the margin of victory.[15] Similarly, of the 111 school levies on the ballot that November, only 15 percent were decided by a vote margin smaller than the school employee and household member voting bloc.

Next, we consider the extent to which low-turnout elections amplify the electoral power of public school employees (and their family members). To do so, we turn to the 2024 presidential election—held just one year earlier. Statewide turnout in 2024 exceeded 76 percent,[16] almost three

[14] We use employment status as measured during the 2024-25 school year.

[15] Including out-of-district school employees and household members would increase this to 32 percent.

[16] This is based on the 2025 voter file, which removed some inactive voters usually included in the state's official turnout calculation.

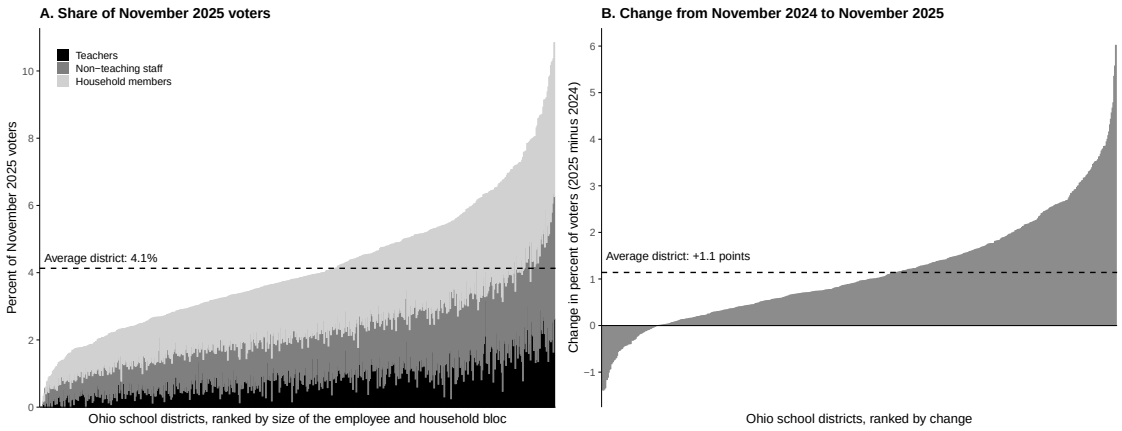

Figure 10: School Employees and Household Members as Share of Voters, 2024 and 2025 Election Cycles in Ohio

Note: Panel A shows the percent of November 2025 voters who are teachers, non-teaching staff, or household members of school employees for each Ohio school district, ranked by the combined share. Panel B shows the change in the combined share between the November 2024 and November 2025 elections, with districts ranked by the size of the change. School employees are those working in their district of residence and household members are other registered voters at the same residential address. Dashed lines show averages across districts. The figure includes 608 districts, excluding Cleveland, which has no elected school board, and two districts without a 2025 school board contest.

times the participation recorded in school board elections the following year. As before, we subset to voters who cast a ballot in 2024 and identify which of them had some employment connection to the local school district.

As can be seen in the right panel of Figure 10, election timing had a surprisingly small effect on the share of voters with an employment interest in the outcome of the local school board race. In the average Ohio school district, the share of voters who were public school teachers, non-teaching staff, or household members of either increased by only 1 percentage point in November 2025, when the school board races were decided, relative to the voter composition in the same district one year earlier, when the presidential race was at the top of the ticket. In no district did the share of employees or family members increase by more than 6 percentage points.

Although we cannot know what share of school board races would have been decided by margins small enough for the same voting bloc to be plausibly pivotal in 2024, during a high-turnout presidential election, since such races never appear on even-year ballots, we can do the calculation for school levies, which appeared in both election cycles. In fact, the fraction of levies decided by a vote margin smaller than the school employee and household member voting bloc was about the same in November 2024 as in 2025 (16 vs. 15 percent), even though this combined group made up

a smaller share of voters (2.8 vs. 4.4 percent in these districts).

Figure 11 summarizes the differences between turnout and voter composition between these two elections for the average Ohio school district. In addition to overall turnout and our staffing and household measure, we use the L2 commercial data to identify likely parents of school-age children, combining the reported presence of children with the ages of the household's adults, and whether each voter owns or rents. Consistent with Kogan, Lavertu and Peskowitz (2018), election timing has the largest impact on voter age, with the share of seniors increasing nearly 15 percentage points between the high-turnout presidential and low-turnout school board election. This increase is an order of magnitude larger than the increase in the fraction of voters with an employment interest in the school district. The share of parents falls by about 3 points (25.7 to 22.9 percent). In addition, the share of homeowners increases by more than 6 percentage points in the off-cycle elections. Fischel (2001) argues that these "homevoters" also have their own "vested interest" in local education, because both school quality and the property taxes that fund schools are capitalized into the prices of homes (see also Kogan 2025, Chapter 8). The interests of these voters in increasing test scores and tax efficiency are unlikely to be perfectly aligned with the policy priorities of school employees—and in many instances, likely put the two groups into direct conflict. Given these simultaneous, multidimensional changes in voter composition, it is far from obvious that off-cycle elections should necessarily be expected to increase the political and electoral influence of school employees.

We provide comparable estimates for Iowa elections in the Supplemental Appendix. Given the much lower baseline rate of turnout prior to the consolidation reforms, school employees and their household members represented a larger voting bloc in September 2015 school board elections—accounting for about 13.4 percent of voters in the average district.

To quantify the share of races where this group of voters could *plausibly* be pivotal—an upper bound, implausibly assuming perfect bloc voting—we combine Iowa school board election data from Kogan, Lavertu and Peskowitz (2025) with the original data collection on Ohio's November 2025 school board election contests.

In both states, roughly half of school board races went uncontested (50 percent in Ohio and 55 percent in Iowa), and most of the remaining contests were decided by margins larger than the share of the electorate represented by school employees and their household members. As a result,

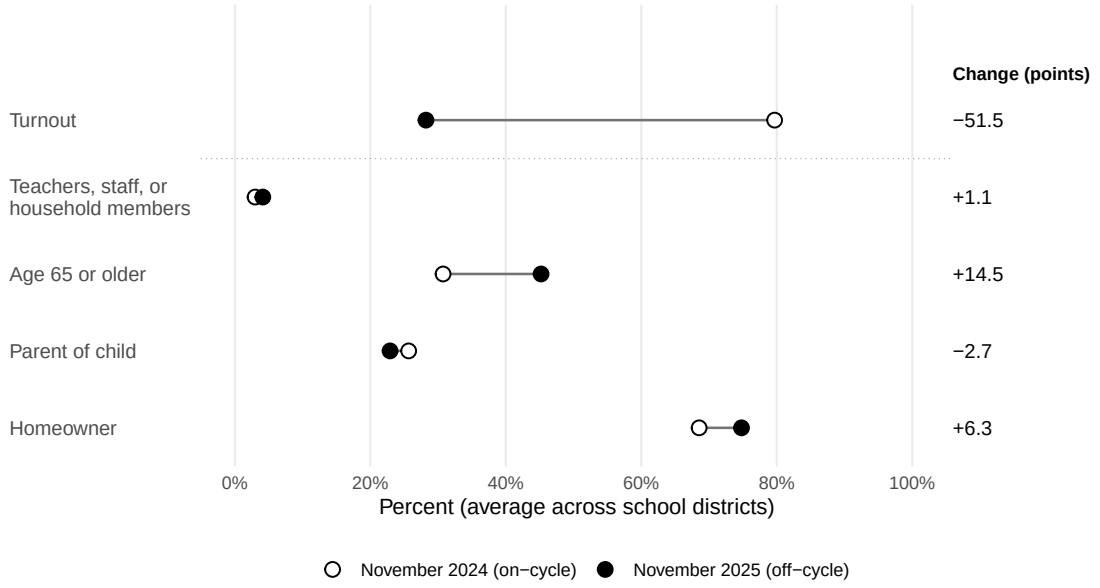

Figure 11: Change in Voter Composition Between November 2024 and 2025 Elections in Ohio

Note: The figure compares turnout and the composition of the electorate in the November 2024 (on-cycle) and November 2025 (off-cycle) elections. Each point is the average across 608 Ohio school districts of the share of voters with the listed characteristic; turnout is the share of registered voters who voted. The change between the two elections, in percentage points, is shown at the right. Teachers, staff, and household members are school employees working in their district of residence and other registered voters at the same residential address. Age, parental status, and homeownership come from L2 voter file data. Cleveland and two districts without a 2025 school board contest are excluded.

it was mathematically impossible for this bloc to have affected the outcome of about four in five races (78 percent in Ohio and 79 percent in Iowa). Election timing matters more in Iowa than in Ohio, but in neither state does it dramatically increase school employees' political influence through raw voter numbers alone. Had the electorate looked like that of the most proximate presidential election, the share of races the bloc could have decided would have fallen from 22 to 16 percent in Ohio and from 21 to 11 percent in Iowa. Given the typical margin of victory in local school board elections, district employees and their family members are simply too small a voting bloc to alter the outcome of the vast majority of school board contests in these states, and election timing does not appear to dramatically moderate their influence, at least not through the voter composition channel.

Reconciling Our Findings with Prior Research

Before concluding, we briefly compare the totality of our results with findings from prior studies and offer some possible explanations to reconcile what may at first blush appear to be inconsistencies.

Overall, we confirm that public school employment has a causal impact on political participation that is particularly pronounced when educators work, live, and vote in the same school district, confirming an observation first noted by Moe (2006). The magnitude of our causal estimates is somewhat smaller than the cross-sectional differences in turnout he finds in his small sample of Southern California school districts.

Importantly, however, we show that these *individual* level differences in participation do not (necessarily) scale up to imply that public school employees *in the aggregate* represent a sizable electoral bloc. Put another way, substantially higher turnout at the individual level need not—and in the case of public school employees, does not—translate to substantially greater electoral influence. This insight applies to other studies on policy feedback, which tend to focus on individual-level turnout differences and based on such effects draw conclusions about overall political power and policy influence.

Most closely related to our study is Geys and Sørensen (2022), who use Norwegian registry data to examine differences in turnout among workers from different sectors. Like us, these authors find that local government employees vote at significantly higher rates when they live and work in the same jurisdictions. Unlike our findings, however, they show that local government employees in Norway who live in a different jurisdiction are actually somewhat *less* likely to cast a ballot compared to individuals employed in the private sector (see Geys and Sørensen 2022, Table A5). This may be due to differences across the two political contexts. Unlike the candidate-centered U.S. system, Norway uses proportional representation across all levels of government. Turnout in Norwegian local elections is dramatically higher—indeed, higher than in Ohio midterm elections.

Perhaps the biggest challenge is reconciling the implications of our results with the literature on election timing. Our finding that school employees represent a relatively small part of the electorate regardless of when elections are held and the conclusion that timing has a much more dramatic impact on voter age echoes several earlier studies using alternative data sources and empirical strategies (Kogan, Lavertu and Peskowitz 2018, Hajnal, Kogan and Markarian 2022). Yet this

raises the question of why some researchers find that timing appears to affect policy outcomes in ways that imply increased public employee influence in low-turnout, off-cycle elections (see, e.g., Anzia 2011, Dynes, Hartney and Hayes 2021).[17]

One explanation is that policy outcomes depend not only on the balance of *actual* political power but also on the *perceptions* of elected officials. If public officials *believe* that low-turnout elections amplify the power of public employees, they may act on this belief even if it is ultimately erroneous. One piece of evidence consistent with this explanation comes from Anzia (2012), who studies an election timing reform in Texas. She finds that districts experiencing a consolidation in the timing of their school board elections with other races subsequently recorded slower teacher salary growth. Notably, she found that the salary changes were nearly instantaneous, before a single election had been held under the new system—"there was a negative effect in the years between the announcement of the election timing change and the first on-cycle election" (p. 240)—implying that incumbents responded *in anticipation* of the change.

The belief that school (and other government) employees dominate low-turnout, off-cycle elections represents the conventional wisdom among many educational policymakers. In the Ohio context that we study, for example, the conservative-leaning education reform group the Thomas B. Fordham Institute has long advocated moving school board elections to even years with the goal of reducing teachers' union influence. Over the past decade, they have cited Moe (2006) in at least three commentaries to make the case for this change. As we noted above, Iowa Republicans offered a similar rationale in shifting the timing of their school board elections. One possibility, then, is that election timing affects policy outcomes because elected officials act on the belief that timing amplifies the political influence of public employees. Even if this belief is ultimately misplaced, it becomes self-fulfilling. The relationship between timing and policy outcomes thus reflects the perceptions of these elected officials and not necessarily the reality.

Conclusion

Why do public employees vote at unusually high rates, and what are the political consequences of their elevated turnout? Using administrative data linking public school employees to voter records

[17] Although the evidence on this point is mixed (see, e.g., Berry and Gersen 2011).

in two states, we provide some of the first causal evidence in the American context that public-sector employment itself increases political participation. Specifically, we show (1) that obtaining employment in the public sector increases political participation, both in local and even-year elections; (2) that these effects are largest when employees are eligible to vote in the jurisdictions where they work, consistent with occupational self-interest playing a decisive role; but (3) despite these large individual-level differences in turnout, the aggregate impacts on voter composition, public employee political power, and likely election outcomes appear to be modest.

We conclude by acknowledging several limitations and potential objections to these findings. First, it is important to recognize that public employment in the Iowa and Ohio contexts we examine is a bundled treatment, which involves both collective bargaining and an elected superior. This remains true in the comparison to charter schools, which differ from Ohio public schools on both dimensions. Flavin and Hartney (2015) provide persuasive evidence that mandatory collective bargaining laws play an important role in mobilizing teachers, and we cannot decompose the effects we find into the influence of unions as opposed to other forms of occupational socialization that may occur through employment in the public sector. While collective bargaining cannot explain differences in turnout we observe between otherwise identical employees (and household members) based on their residential location, it is still possible that the dynamics we document may not generalize to all institutional contexts. For example, it is possible that our results may not generalize to the handful of states that forbid public sector collective bargaining.

Finally, although our results suggest that *voter composition* is unlikely to be the main channel through which election timing influences public policy, at least at the turnout levels we observe, we acknowledge this is not the only argument made in support of the claim that off-cycle elections increase the power of organized public employees. For example, Anzia (2013) also argues groups with a fixed amount of resources to spend on mobilization or persuasion could reach a larger share of the electorate when overall turnout is lower. With fewer voters, the same amount of door-knocking and direct mail expenditures could go a longer way.

More broadly, our findings leave scholars with an important paradox that warrants further inquiry. Public-sector employment clearly mobilizes political participation, and these effects extend beyond employees themselves to members of their households. Yet the resulting changes in voter composition are far too modest to explain the remarkable electoral success of teachers' unions and

other organized public employee interests in local elections. Future research should therefore look beyond voter composition and turnout levels and investigate other potential mechanisms—including candidate recruitment, endorsements, and persuasion—through which organized public employees may exert political influence in democratic institutions.

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Supplemental Appendix for "The Electoral Power of Public Employees"

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A Construction of the Analysis Datasets **A1**

B Additional Results **A11**

C Sensitivity Analyses **A17**

A Construction of the Analysis Datasets

This appendix describes how we assemble the Ohio analysis datasets from the raw employment records and voter files. We describe the employment records first, then the voter files, the algorithm for linking the two, and the measurement of residence, households, and turnout. We conclude with a brief discussion of the Iowa data, which largely follows the same process.

Ohio Employment Records

The employment records come from the Ohio Department of Education and Workforce and cover the 20 school years from 2005-06 through 2024-25. Each row is a single job assignment, so the same employee can appear several times in the same school year when they hold more than one position, work in more than one building, or change jobs during the year. Each row reports the employee's name, birth year, demographic characteristics, highest degree, salary, the position code of the assignment, the building and district, and a state staff identifier.

We match each school year to November elections. Employees generally begin work in August, so we treat August 15 as the start date and, as an example, match the 2020-21 school year to the November 2020 election. An employee hired in that year is therefore first treated in the November 2020 election.

Sector of employment. We classify every job assignment based on the type of employer. In Ohio, charter schools are known as "community schools," and we use both terms interchangeably here. Because the state's publicly available school directory lists only schools open today, we identify closed charter schools (such as the Electronic Classroom of Tomorrow) by the structure of their state identifiers. Assignments in city, local, and exempted village districts are coded as traditional public. These include five local districts that have since been dissolved and are missing from the current directory. We drop assignments at educational service centers, independent STEM schools, joint vocational school districts and career centers, state schools (such as the schools for the deaf and blind), and other state agencies, as well as public employees assigned to private schools (mostly to provide federally required special education services to their students). We also drop extracurricular positions (position codes 801 through 899, which cover coaches, advisors, and athletic trainers). The

state began reporting these positions systematically only in 2014-15, when the number of coaching assignments rose from 6,508 to 44,233.

We then collapse the retained assignments to one record per employee and year. An employee is coded as a teacher in a given year if at least one assignment is a teaching position. An employee-year is coded as traditional public if at least one retained assignment is in a traditional public school district, and as charter only if every retained assignment is in a charter school. Employees whose only assignments are at the excluded employers or in extracurricular positions never enter the employee samples. We nevertheless exclude them, along with any voter who could be one of them, from all comparison groups.

Following employees across identifier changes. The state data include an alphanumeric staff identifier that may change over time. Many employees appear under two or more identifiers, typically one that ends in one year and another, with the same name and birth year, that begins in the next. Among linkage keys formed from last name, first name, middle initial, and birth year that are held by exactly two identifiers, 27,560 pairs have employment spells that do not overlap, and 22,477 of these have no gap year between them. New identifiers are especially common in 2006-07, 2007-08, and 2024-25. Left uncorrected, these breaks would create spurious "new" employees and leave the same voter linked to several staff records.

We therefore reconcile changes in these identifiers so they point to the same person using two rules. Under the first rule, two identifiers belong to the same person if they share last name, first name, middle initial, and birth year and their years of employment do not overlap. When more than two identifiers share a key, we add them to a person in order of their position start date only as long as the person's years remain non-overlapping. Under the second rule, two identifiers belong to the same person if each links to exactly one voter record and it is the same record, whatever their years of employment. We block any merge under the first rule that would combine two different, uniquely linked voters into the same employee. These rules reduce 694,635 staff identifiers to 607,553 unique people, whom we refer to simply as employees.

New employees and sector switchers. Our main designs study employees at the start of their careers in Ohio schools. Anyone observed in the first year of the data (2005-06) may have been

hired earlier, so we exclude them from the samples of new employees. We classify new employees by their role in their first year. The new-teacher sample includes employees whose first job is a teaching position, and the new non-teaching staff sample includes those whose first job is not. Employees who begin in a non-teaching role and later teach therefore remain in the non-teaching sample. Figure A1 shows the size of each sample and how its members move between the two sectors. Of the 100,287 new teachers, 79,287 begin in a traditional public school and 21,000 in a charter school. Moves from traditional public to charter schools are rare (3.2 percent of those who start in traditional public schools), while 39 percent of new charter school teachers later work in a traditional public school. Almost all of these switchers (93 percent) stay in the traditional public sector. These 8,106 switchers make up the sample for the sector switcher design. As in the rest of the analysis, an employee's position is fixed before the switch, so a charter school teacher whose first traditional public job is a non-teaching position is considered a teacher switcher in the analysis.

Treatment status follows employment. A new employee is treated from the November election at the start of their first school year through the last school year in which they appear anywhere in the employment data, including years at the excluded employers (explained earlier) and in extracurricular positions. Years in which the employee is missing from the data but later returns remain treated. Elections after the last year of any school employment are censored. In the comparisons of new traditional public and charter school employees, each employee is assigned to the sector in which they begin, and elections from years in which they work in the other sector are excluded from the analysis. A charter school teacher who later moves to a traditional public school therefore contributes only her charter years to this comparison. Her later years identify the separate switcher design. For employees who move from charter to traditional public schools, the years before they begin their initial charter employment are excluded, so that the charter years serve as a clean pre-treatment baseline, and the traditional public years as the post-treatment period.

Ohio Voter Files

We link the employees to 18 annual snapshots of the statewide voter file. The most recent is a statewide file released after the November 2025 general election, which records turnout in that election. The other 17 are historical Secretary of State snapshots, one per year, dated June 2007, July 2010, and then once a year from April 2011 through May 2025. All of the historical snapshots

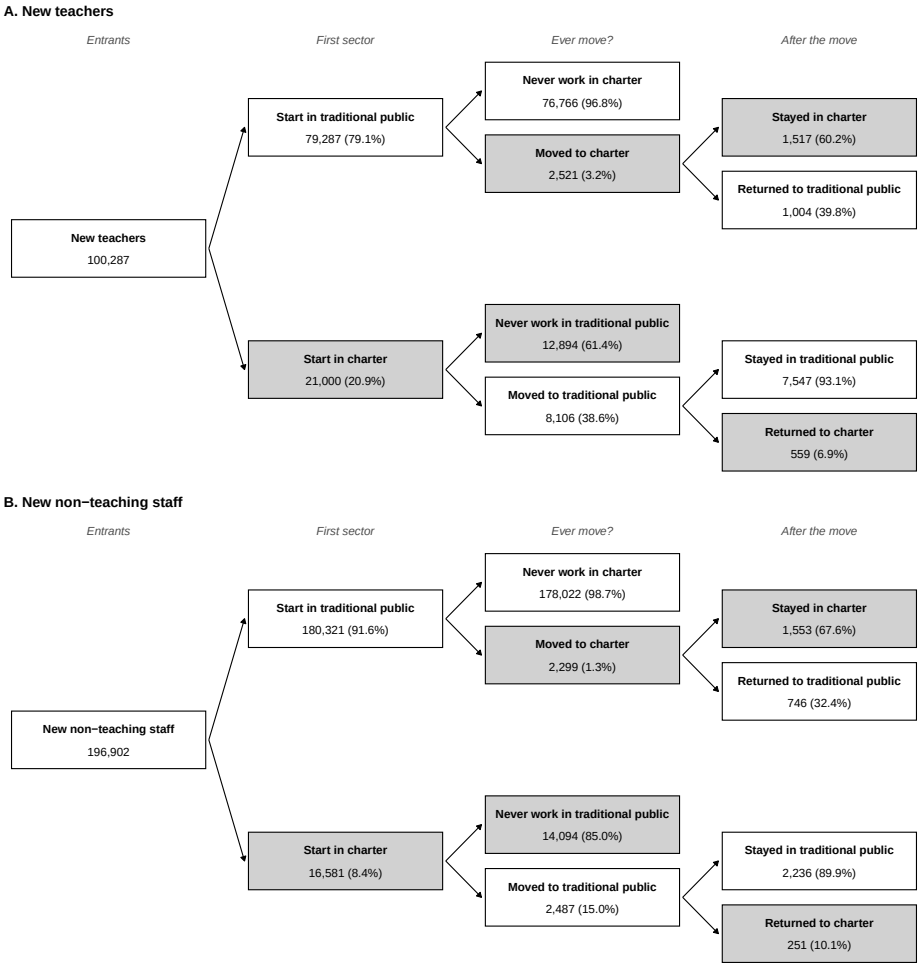

Figure A1: Movement of New Ohio School Employees between Traditional Public and Charter Schools

Note: The figure shows the first sector of employment of new Ohio school employees and whether they later move to the other sector. Panel A includes new teachers, whose first job in the employment data is a teaching position, and classifies each year by the sector of the teaching assignments. Panel B includes new non-teaching staff, whose first job is not a teaching position, and classifies each year by the sector of employment in any role. New employees are those first observed between 2006-07 and 2024-25. Anyone observed in 2005-06 is excluded. A year is coded as traditional public if any assignment is in a traditional public school district and as charter only if all assignments are in charter schools. Assignments at educational service centers, STEM schools, career centers, state schools, and nonpublic schools, and extracurricular positions, are excluded. Exits from Ohio schools are not shown.

were taken before the November election of their year. We use only files that carry the Secretary of State's statewide voter identifier, which allows us to follow the same registration record from file to file. No usable files are available for the period between June 2007 and July 2010.

The snapshots differ in format. We harmonize all of them to a common set of fields, including name, birth year, registration date, residential address, county, school district, and the full history of November general elections.

Following voters across identifier changes. Within a registration record, the state identifier is stable, and each file carries the record's full turnout history. A voter who re-registers after moving to another county, however, often receives a new identifier. The state carries over the earlier vote history to the new record, but the registration date frequently resets. Because we link employees starting with the most recent file, the voter identifier we find is the one the person held most recently, and for many employees that record did not yet exist when they were hired. For 36 percent of new employees (and 49 percent of new traditional public school teachers), the current record is absent from the last file before their hire date, even though most of these employees have votes recorded before they were hired.

To recover their earlier records, we begin by identifying possible matches by searching the last file before the hire date statewide for records with the same first name and birth year and comparing each candidate with the employee's current record. (Last names are less reliable due to name changes after marriages.) Because the state carries the vote history over to the new record, the history before the hire date (general, primary, and special elections) helps uniquely identify voters even as their identifiers change. We link voters only if their last name matches a last name the employee has used in the voter or employment records in any year, and only if it is the unique best candidate. Linking proceeds in the following order: Potential matches whose vote history is identical to the current record's rank first, followed by potential matches with the same middle initial whose history is a subset of the current record's, and then by potential matches with the same middle initial and registration date. Ties at the best level are left unmatched, as are 3,998 employees whose only possible match has an identical vote history but a different last name. This procedure recovers the earlier record for 23,120 of the 73,842 employees we try to link back in time (31 percent). In 85.5 percent of the successful matches, the earlier record is in a different county, and in 99.6 percent the earlier identifier is the lower number in the state voter identifier field, consistent with new identifiers issued after cross-county moves. We use the same procedure to link to records held before a charter school employee's move to a traditional public school and before the start of an existing teacher's employment spell in the district-switcher design.

Registration and turnout. Turnout comes from the November general election history on the person's record in the latest file in which they appear. Because each file records the full history, we

observe the full voting history for a person missing from an intermediate voter file. Elections after a person drops out of the voter file are treated as missing rather than as abstentions. This applies equally to employees, their household members, and the comparison voters.

The registration date for the same voter is not constant across files. Comparing the 2007 and post-election files, it is identical for 77.5 percent of records, later in the newer file for 21 percent (mostly re-registrations), and earlier for 1.5 percent (mostly corrections). We therefore use the earliest registration date that appears on any of the person's voter file records. About 3 percent of records carry a placeholder date of January 1, 1900, which we ignore whenever any file shows a real date. A person counts as registered for a November election if their registration date falls on or before October 6 of that year (the approximate registration deadline) or if they have a recorded vote in that election or an earlier November election. The second condition keeps voters who re-registered after a move, and whose registration date was reset, in the panel for prior elections. All of our turnout estimates condition on registration using this rule. The panels include November elections from 2000 or the year a person turns 18, whichever is later, through 2025.

Linking Employees to Voter Records

We normalize names by converting them to upper case and keeping only letters, and we take the middle initial from the first letter of the second given name. Names change over the 20 years of the employment data, mostly through marriage and corrections, so we keep every distinct spelling a person has in any year, every distinct birth year recorded for them, and each part of a hyphenated or two-word last name. A voter record is a candidate match for an employee if it has exactly the same birth year and last name and a first name that matches exactly, matches through a table of common nicknames, or has a Jaro-Winkler similarity of at least 0.90. Because we search with every version of an employee's name, an employee can match several voter records, some through an exact match and others only through a nickname, a fuzzy match, or part of a hyphenated last name. We keep only the strongest matches, in which full last names take priority over parts of hyphenated names and exact first names over nicknames and fuzzy matches, and resolve any remaining ties as described below.

We search the voter files in reverse chronological order, so that an employee who has left Ohio or died is still found in the last file in which they were registered. Figure A2 reports the source

voter files where employees are found. Most are matched to the most recent file, but the historical files add substantially, especially for employees who left the Ohio schools early in the period (Panel B). Overall, 95.8 percent of traditional public school teachers and 94.0 percent of traditional public school non-teaching staff have at least one voter file match, compared with 86.6 percent of charter school teachers and 87.1 percent of charter school non-teaching staff.

When an employee has more than one possible match, we keep the matches where the middle initial agrees with the employee's (if any do) and then the candidate with the highest name similarity score. Employees with two or more equally good matches remain unlinked. We also require that each voter record be linked to only one employee. When two employees claim the same voter, we assign them to the employee with the strongest match, and leave exact ties unlinked. We do not use geography to break ties, because a rule that preferred candidates living in the employing district would mechanically raise the share of employees who live where they work. Under these rules, 88.1 percent of employees are linked to exactly one voter record. Another 33,222 employees remain unlinked because of ties between possible matches and 2,481 because another employee had a better match to the same voter. Nearly all links (97.6 percent) match on the exact first name. Among the samples of new employees used in the event studies, 89.8 percent of new traditional public school teachers and 81.8 percent of new charter school teachers are linked. The bounding exercise reported below addresses this difference in match rates.

Every voter record that is a possible match for any staff identifier, under any name variant and in any file, is excluded from the comparison groups, whether or not it is ultimately linked. This keeps unlinked or ambiguously linked school employees out of the samples of "regular" voters to prevent contaminating the estimates.

Residence and School Districts

School district of residence. The voter files report each voter's school district, but in older files 21 counties leave the field blank for some or all years. For the most recent file, we use the reported district, supplemented by geocoded assignments in the counties that do not report one. For older files, we first translate the reported district name into the state district identifier. Where the name is missing, we assign the district recorded for the same address in the most recent file, then the district of the same street and ZIP code, and finally the district that covers most of the ZIP code.

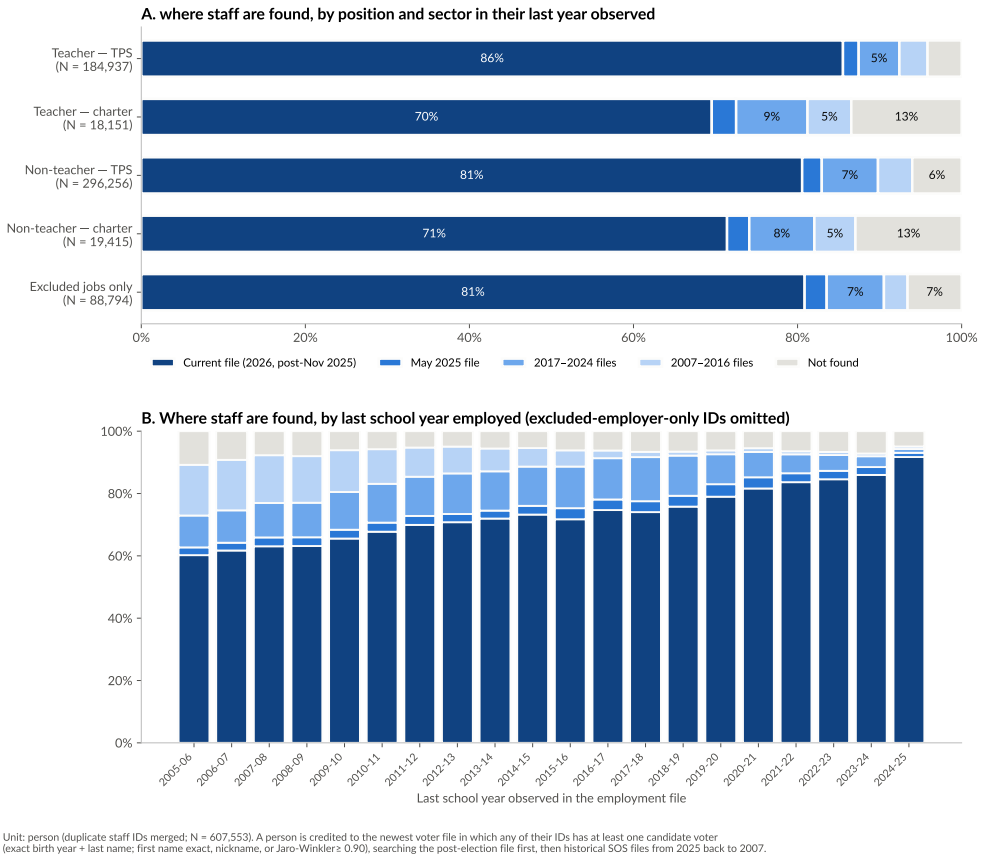

Figure A2: Ohio School Employees Found in the Voter Files, by Voter-File Snapshot

Note: The figure reports the share of Ohio school employees with at least one possible voter-file match, by the most recent voter file in which they are found. A voter record is a possible match if it has the same birth year and last name and a first name that matches exactly, through a nickname, or with a Jaro-Winkler similarity of at least 0.90. Panel A classifies employees by their position and sector in the last school year they are observed. Panel B classifies employees by that last school year and omits employees who hold only excluded positions (at educational service centers, STEM schools, career centers, state schools, and private schools, or in extracurricular positions). The figure includes 607,553 employees, of whom 184,937 are traditional public school teachers, 18,151 charter school teachers, 296,256 traditional public school non-teaching staff, 19,415 charter school non-teaching staff, and 88,794 employees who hold only excluded positions.

Where both are available, the district assigned from the address or street agrees with the reported district name for 99.3 to 99.5 percent of records. After these steps, fewer than 0.4 percent of records in any file after 2007 (and 1.9 percent in 2007) lack a district. We geocode the remaining employee addresses with no district or only a ZIP-code assignment directly, which leaves 505 employees with no district or only a ZIP-code assignment.

Residence at hire. We measure a new employee's residence in the latest snapshot dated on or before September 30 of the hire year in which the employee appears. When the employee does not appear in any such snapshot, we use the first later snapshot in which they appear. Only employees who

were registered before hire contribute to the estimates, however, because the imputation estimator requires at least one pre-hire election in which the employee was registered. Because there is no usable file between June 2007 and July 2010, residence for employees hired in 2008 and 2009 comes from the June 2007 file, up to two years before the hire date, and employees hired in 2006 have no earlier file at all. An employee is coded as working in their district of residence if at least one of their traditional public school assignments in the first year is in that district. For 41 percent of the new traditional public school teachers who identify the odd-year estimates, residence comes from a snapshot taken after the hire date, mostly because their current voter record did not yet exist (see above). Restricting the residency analysis to addresses observed before hire reduces the odd-year same-minus-other-district gap by about a third, from 0.028 to 0.019, but the gap remains statistically significant ($p < .001$). The residency effect therefore does not depend on addresses measured after hire.

For charter school employees who move to a traditional public school, residence is measured in the last snapshot before the sector switch. In the design that follows existing traditional public school teachers, residence is fixed in the last voter file before the start of the teacher's spell of consecutive years in traditional public schools, and teachers already working in their district of residence at that point are excluded. In both cases, residence is therefore measured before the job change that defines treatment.

Households. We define a household as the set of registered voters at the same normalized street address and five-digit ZIP code in the same voter file snapshot. Addresses with more than six registered voters are dropped, to avoid treating apartment buildings or group quarters recorded without unit numbers as households. Household members exclude every linked employee and every voter who is a candidate match for any staff identifier, so no household member is a school employee. Because we observe each employee's address in every snapshot, we can follow household composition over time. Members who live with the employee at hire and are still at the employee's address in the employee's latest file are classified as members who stay. Members who live with the employee at hire but not in the latest file are classified as members who move out, and voters who first join the household after the hire as new members. In the main household analyses, we keep members born within ten years of the employee, which removes most parents and adult children and leaves

mostly spouses and partners. For members who move out, the destination is the school district matched to that voter's address in the first subsequent voter file in which they appear.

Untreated Comparison Voters

Our main sector comparisons use other, not-yet-treated new employees as the comparison group. Whenever a design also uses voters outside the school workforce, they come from a fixed 10 percent random sample of registered voters drawn from all 18 files. The sample excludes any voter who is a candidate match for any staff identifier, any household member of a new employee, and anyone who ever shares an address with a new employee in any file. Because the sample is drawn from every file rather than only the most recent one, it includes voters who later die or leave the state. For these groups, elections after their last file appearance are treated as missing in the same way as for the employees. The sample includes about 1.47 million voters. In the household designs, comparison voters are matched to household members on birth year. The regular voters in the descriptive figures are drawn from the same sample.

School Levies

The levy data come from the Ohio School Boards Association and cover 7,752 school tax measures from 2003 through 2026, including the vote counts for and against each issue. We match measures to school districts by district name and county. We use only issues on the November general election ballot.

Iowa

The Iowa data differ in three ways. First, the employment data cover a single school year (2016-17), when the consolidation law was passed, and include only traditional public school employees. Second, we link employees to a single voter file, the August 2018 Secretary of State file, using the same name rules and one-to-one matching as in Ohio. We link 84.1 percent of the 82,222 school employees and 86.4 percent of teachers. Employees whose only position is coaching are not counted as employees. Turnout in the November 2019 and 2021 elections, which postdate the 2018 file, comes from the April 2022 Secretary of State file, linked by the state voter identifier. A voter counts as registered for an election if their earliest registration date in the two files falls on

or before Election Day or if they are observed as having voted in that election. Third, because employment, residence, and households are each observed once, we hold them fixed at their 2016-17 and 2018 values throughout, rather than following them over time as in Ohio. The school district of residence comes from the district code in the voter file, which we translate to the district names in the employment data, and households are defined as registered voters at the same address just as in Ohio, dropping addresses with more than six registered voters.

B Additional Results

This section reports the results for non-teaching staff referenced in the main text, even-year estimates, and additional figures for Iowa.

Table A1: Post-treatment average effects in odd- and even-year November elections

	Odd years	Even years	Odd − even
A. New hires, traditional public minus charter school			
Teachers	0.063***	0.049***	0.015*
	(0.005)	(0.004)	(0.006)
Non-teaching staff (reweighted)	0.023***	0.025***	−0.002
	(0.005)	(0.004)	(0.006)
B. Charter to traditional public school job changers			
Teachers	0.055***	0.041***	0.013
	(0.010)	(0.009)	(0.013)
Non-teaching staff (reweighted)	0.051**	0.050***	0.001
	(0.016)	(0.014)	(0.022)
C. Working in the district of residence, traditional public school teachers			
New teachers: residence minus elsewhere	0.028***	0.028***	0.001
	(0.004)	(0.004)	(0.006)
Existing teachers moving into residence district	0.041***	0.021**	0.019
	(0.010)	(0.007)	(0.012)
D. Household members who stay, traditional public minus charter school			
Members born within ten years of the teacher	0.035***	0.045***	−0.011
	(0.008)	(0.007)	(0.010)

Notes: Entries are average post-treatment effects on turnout among registered voters from the imputation estimator of Borusyak, Jaravel and Spiess (2024), with standard errors clustered by individual in parentheses, estimated separately for odd- and even-year November general elections. Panels A, C (first row), and D report differences between groups, as in the corresponding figures. Non-teaching staff are reweighted by occupation, as in the corresponding figures. The odd − even column reports the difference between the two estimates, with a standard error that treats them as independent. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$ (two-sided, unadjusted for multiple comparisons).

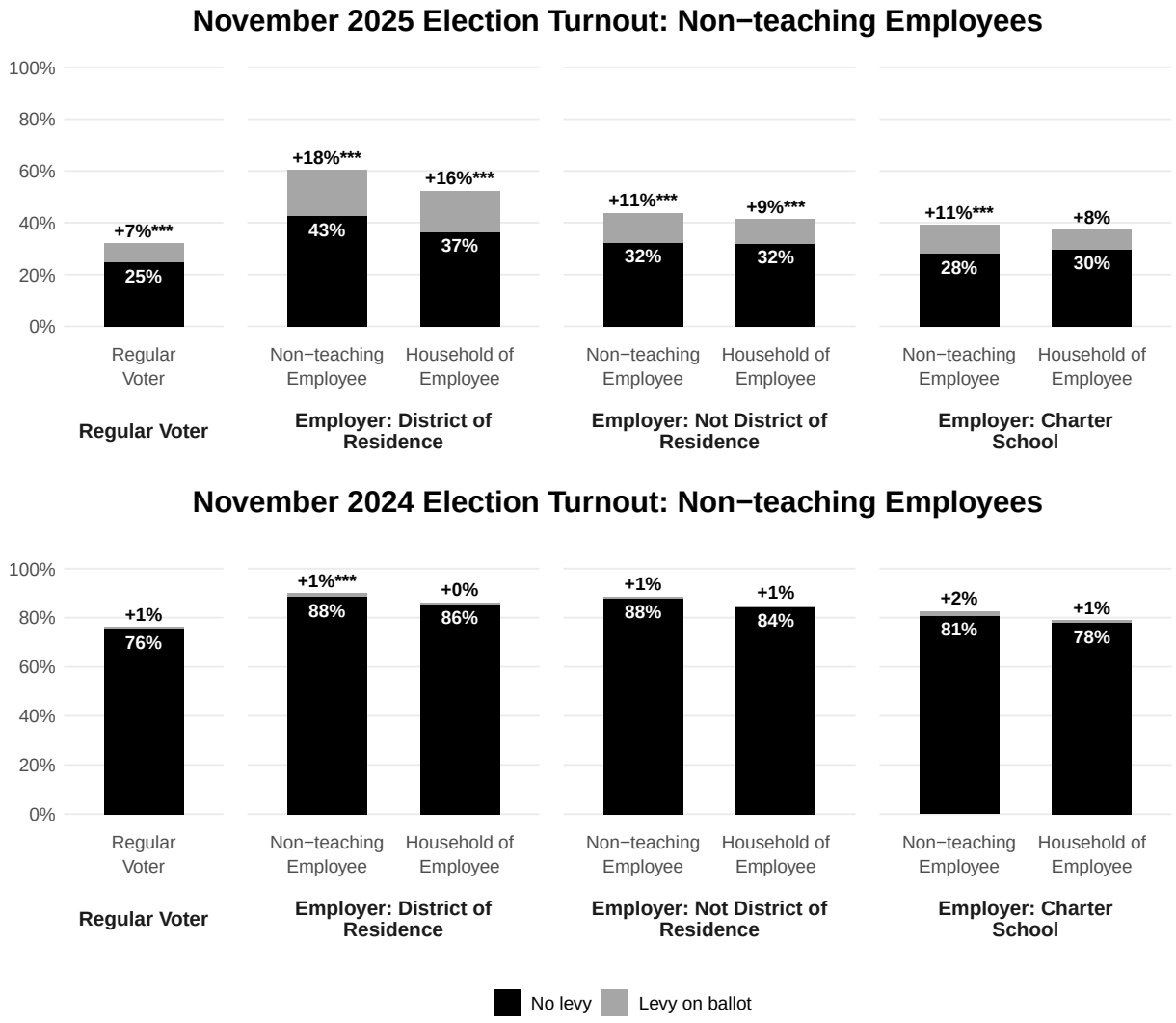

Figure A3: Turnout in Ohio November 2024 and 2025 Elections, Non-Teaching Staff

Note: Black bars show turnout among registered voters whose school district of residence had no school levy on the November ballot, taken directly from the voter file. Gray bars show the estimated increase in turnout when the district of residence has a levy on the ballot, from a within-voter comparison with the previous election of the same type (2025 vs. 2021; 2024 vs. 2020). Employees are school employees with no teaching assignment in the 2024-25 school year, classified by sector and, for traditional public schools, by whether they work in their district of residence. "Regular voters" are a random 10% sample of registered voters who are neither school employees nor household members of school employees.
Stars denote significance after a Holm adjustment (*** $p < .001$, ** $p < .01$, * $p < .05$)

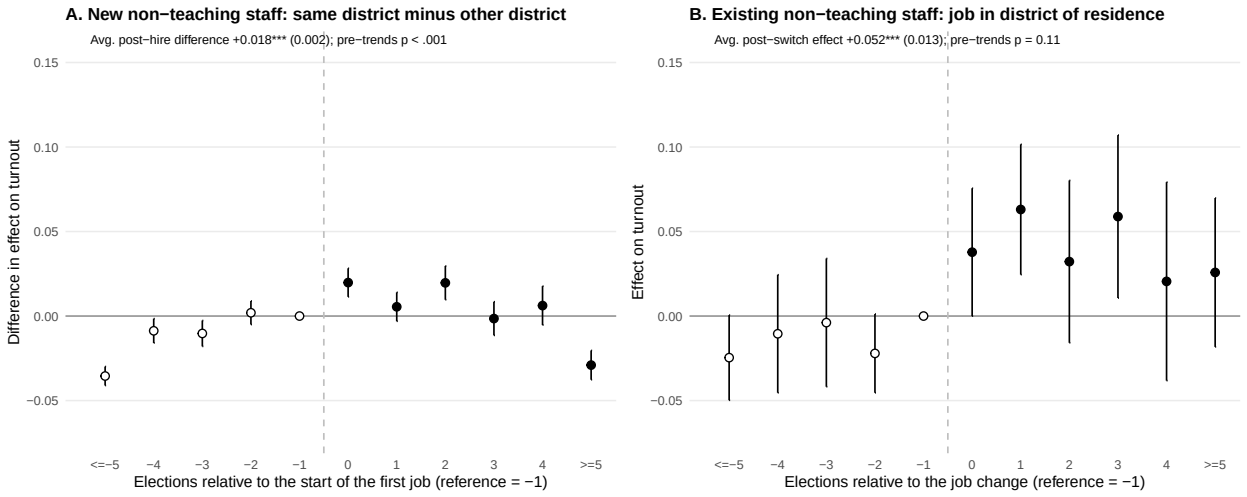

Figure A4: Effect of Finding Non-Teaching Job in District of Residence vs. Commuting on Odd-Year Turnout

Note: The figure plots event-study estimates of the effect of working in one's district of residence on turnout in odd-year November general elections among traditional public school non-teaching staff. Panel A reports the difference in estimated effects between new non-teaching staff whose first job is in the school district where they reside and new non-teaching staff whose first job is elsewhere. Panel B reports the effect of taking a job in one's district of residence among existing traditional public school non-teaching staff. Residential location is measured in the last voter file before initial hire or, if the employee does not appear in one, the first subsequent file (Panel A), and in the last voter file before the job switch (Panel B). The reported pre-trend p-values are from joint Wald tests that the pre-event coefficients equal zero. Vertical bars show 95 percent confidence intervals based on standard errors clustered by individual. Panel A includes 64,051 non-teaching staff whose first job is in their district of residence and 56,788 whose first job is elsewhere, while Panel B includes 1,390 non-teaching staff who take a job in their district of residence and the non-teaching staff who do not.
Stars denote significance after a Holm adjustment (*** $p < .001$, ** $p < .01$, * $p < .05$)

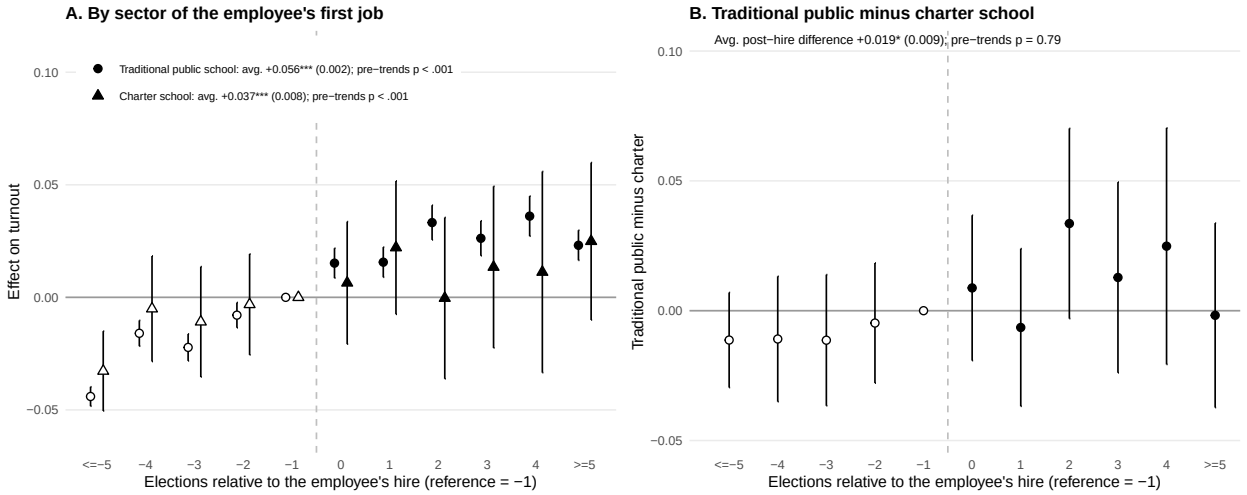

Figure A5: Effect of Household Member Obtaining Non-Teaching School Job on Odd-Year Turnout

Note: The figure plots event-study estimates of the effect of another member of a voter's household beginning employment as a non-teaching school employee on the focal voter's turnout in odd-year November general elections. Household members are other registered voters at the employee's residential address. The sample includes voters who live with the employee at the time of hire and remain in the household in the employee's most recent voter file, limited to voters born within ten years of the employee. Panel A reports separate estimates for household members of non-teaching staff whose first job is in a traditional public school (circles) or a charter school (triangles). Panel B reports the difference between the two sectors, estimated jointly with a common set of "control" voters. These controls are registered voters drawn from a random sample of Ohio voters who never reside with a school employee, matched to household members on birth year. The reported pre-trend p-values are from joint Wald tests that the pre-hire coefficients equal zero. Vertical bars show 95 percent confidence intervals based on standard errors clustered by individual. The estimates draw on 50,159 voters who live with traditional public school non-teaching staff and 2,499 voters who live with charter school non-teaching staff.
Stars denote significance after a Holm adjustment (*** p < .001, ** p < .01, * p < .05)

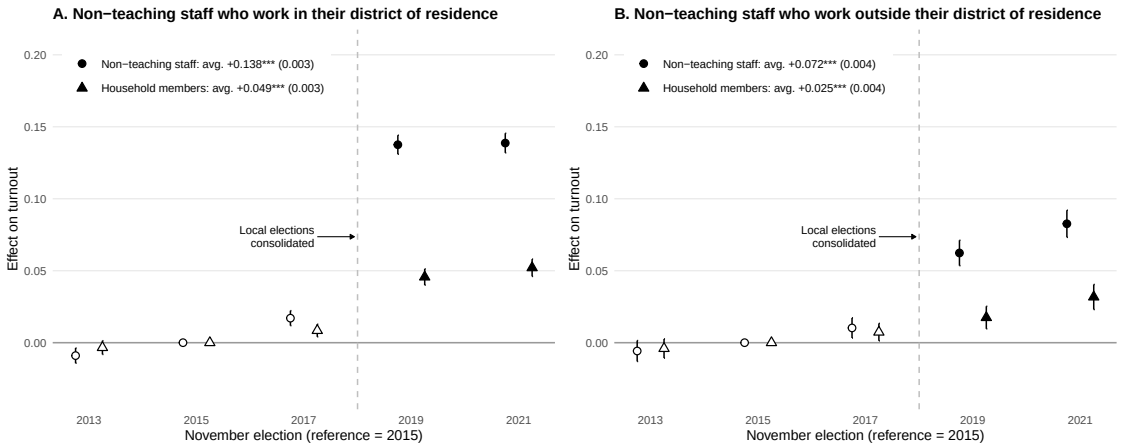

Figure A6: Differential Effect of Iowa Election Consolidation on Turnout in November Odd-Year Elections, Non-Teaching Staff

Note: The figure plots difference-in-differences estimates of the change in turnout in odd-year November elections for non-teaching staff (circles) and their household members (triangles), relative to regular voters for non-teaching staff who work in their district of residence (Panel A) and non-teaching staff who work elsewhere (Panel B). Non-teaching staff are 2016-17 school employees with no teaching assignment, and household members are registered voters at addresses with no linked teacher. Effects are measured relative to the 2015 baseline. The average reported for each series is the mean of the 2019 and 2021 estimates. Elections in 2013, 2015, and 2017 are municipal elections held separately from school board elections. Beginning in 2019, school board races appear on the same November ballot. The 2017 estimate captures turnout after the consolidation law passed but before it took effect. Estimates come from a regression with individual and school-district-of-residence-by-year fixed effects. Vertical bars show 95 percent confidence intervals based on standard errors clustered by individual. The estimates are based on 23,719 non-teaching staff who work in their district of residence and 28,514 of their household members, 10,874 non-teaching staff who work elsewhere and 12,122 of their household members, and 1,976,247 regular voters. Stars denote significance after a Holm adjustment (*** $p < .001$, ** $p < .01$, * $p < .05$)

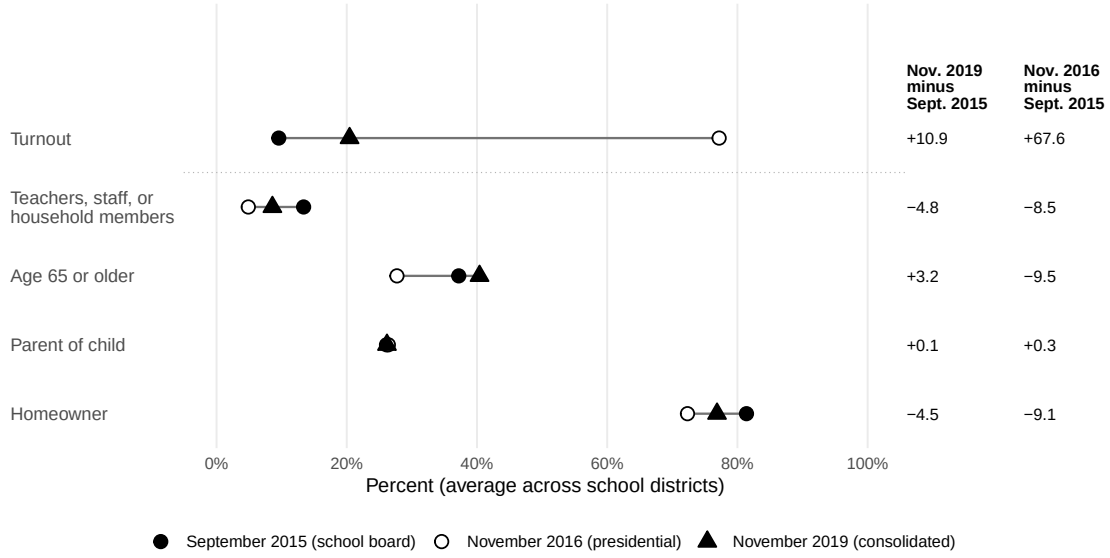

Figure A7: Voter Composition in the September 2015, November 2016, and November 2019 Elections in Iowa

Note: The figure compares turnout and the composition of the electorate in the September 2015 (off-cycle) school board election, the November 2016 (on-cycle) presidential election, and the November 2019 election, the first in which school board races appeared on the same ballot as municipal contests. Each point is the average across 333 Iowa school districts of the share of voters with the listed characteristic. Turnout is the share of registered voters who voted. The changes from September 2015 to November 2019 and from September 2015 to November 2016, in percentage points, are shown at the right. Teachers, staff, and household members are 2016-17 school employees working in their district of residence and other registered voters at the same residential address. Age comes from the 2018 voter file, and parental status and homeownership come from 2018 L2 voter file data. Because the Iowa L2 data lack the household information used in Ohio, parents are identified with an approximation of the Ohio procedure.

C Sensitivity Analyses

Bounding Analysis for Differential Match Rates

We link new traditional public school employees to the voter file at a higher rate than new charter school employees. Among new teachers, 89.8 percent of those who begin in a traditional public school are linked one-to-one to a voter record, compared with 81.8 percent of those who begin in a charter school. Among new non-teaching staff, the rates are 87.0 and 77.7 percent. Although we account for any baseline differences using individual fixed effects, this does not rule out possible heterogeneity in the estimates. If the employees we fail to link differ in *how their turnout responds* to employment, the differential match rate could bias the comparison between the two sectors. For example, this would happen if charter school employees we link are a more positively selected group, because some of the unlinked employees are people who never register in Ohio.

We assess this concern with trimming bounds in the spirit of Lee (2009). If traditional public school employees were linked only as often as charter school employees, a share $q = 1 - r_C/r_T$ of the linked traditional public school employees would be missing, where r_T and r_C are the two match rates. This share is 8.9 percent for teachers and 10.7 percent for non-teaching staff. We do not know which employees would be missing, so we consider the two most extreme cases. For each linked employee, we compute the average of their individual post-hire effects from the imputation estimator. We then drop the share q of traditional public school employees with the largest average effects, which gives the lower bound on the traditional public school effect, or the share q with the smallest average effects, which gives the upper bound. Subtracting the charter school estimate gives bounds on the difference between the sectors. We report confidence intervals for the difference following Imbens and Manski (2004), extending the bounds by 1.645 times the standard error of the untrimmed difference.

The bounds are by design conservative, because they assume that the unlinked employees are exactly those with the most extreme effects. We therefore also ask how large the effect among unlinked charter school employees would have to be to erase the difference between the sectors in the full population of new employees. If unlinked traditional public school employees have the same average effect as linked ones (τ_T), the population difference is zero when unlinked charter school employees have an average effect of $u_C = (\tau_T - r_C\tau_C)/(1 - r_C)$, where τ_C is the effect among linked

charter school employees. If unlinked traditional public school employees instead have no effect at all, the required effect is $u_C = (r_T\tau_T - r_C\tau_C)/(1 - r_C)$.

Table A2 reports the results. For teachers in odd-year elections, the difference between the sectors is 6.3 points among registered voters. Even after trimming the traditional public school sample in the least favorable way, the difference remains positive (1.4 points), and its 95 percent confidence interval excludes zero. Erasing the difference would require unlinked charter school teachers to experience an increase in turnout of 31 to 35 points from charter employment, more than five times the effect we estimate for linked traditional public school teachers and far larger than any effect we observe for any group. The bounds are wider in even-year elections, where the lower bound falls just below zero. For non-teaching staff, whose difference between the sectors is smaller, the bounds include zero in every specification. Differential matching therefore cannot account for the larger turnout effects we find for traditional public school teachers.

Table A2: Bounds on the Difference between Traditional Public and Charter School Employees, Accounting for Differential Matching

	Odd years	Even years	Odd years, matched controls
A. Teachers ($r_T = .898$, $r_C = .818$, trimmed share $q = .089$)			
Traditional public effect	0.061	0.036	0.100
Charter effect	−0.002	−0.013	0.023
Difference	0.063	0.049	0.077
	(0.005)	(0.004)	(0.005)
Lee bounds	[0.014, 0.132]	[−0.003, 0.091]	[0.023, 0.143]
95% confidence set	[0.007, 0.140]	[−0.010, 0.098]	[0.015, 0.151]
Break-even unlinked charter effect			
unlinked traditional public = linked	0.346	0.255	0.444
unlinked traditional public = 0	0.312	0.235	0.388
B. Non-teaching staff ($r_T = .870$, $r_C = .777$, trimmed share $q = .107$)			
Traditional public effect	0.009	0.020	0.050
Charter effect	−0.003	−0.001	0.029
Difference	0.012	0.021	0.021
	(0.005)	(0.004)	(0.005)
Lee bounds	[−0.050, 0.081]	[−0.034, 0.069]	[−0.038, 0.089]
95% confidence set	[−0.057, 0.088]	[−0.041, 0.076]	[−0.045, 0.096]
Break-even unlinked charter effect			
unlinked traditional public = linked	0.049	0.093	0.125
unlinked traditional public = 0	0.044	0.082	0.096

Notes: Entries are average post-hire effects on turnout in November general elections for new employees, estimated with the imputation estimator of Borusyak, Jaravel and Spiess (2024), with standard errors of the difference clustered by individual in parentheses. All estimates condition on registration. The last column compares new employees with comparison voters matched on birth year and school district of residence at hire. r_T and r_C are the shares of new traditional public and charter school employees linked one-to-one to a voter record. Lee bounds drop the share $q = 1 - r_C/r_T$ of linked traditional public school employees with the largest (lower bound) or smallest (upper bound) average individual post-hire effects. Confidence intervals for the difference extend the bounds by 1.645 standard errors of the untrimmed difference, which covers the true difference with 95 percent probability when the bounds are wide relative to sampling error (Imbens and Manski 2004). The break-even rows report the average effect among unlinked charter school employees at which the difference in the full population of new employees would be zero, assuming that unlinked traditional public school employees have the same average effect as linked ones or no effect.

Clean Controls in Place of Not-Yet-Treated Design

The new hire analysis in the manuscript compares school employees with other, not-yet-treated school employees. New hires are compared with future employees who are hired later, and charter school employees who move to a traditional public school are compared with charter school employees who never move. These comparisons hold constant whatever leads people to work in schools, but they also mean that employees' effects are measured against a group that will itself be treated or is already employed in a school and is potentially confounded by simultaneous life-cycle events. Table A3 compares these estimates with estimates that instead use "clean" comparison

voters—registered voters from a random sample of Ohio voters who are never school employees or household members of school employees. Each employee is exactly matched to two such voters with the same birth year (to address life-cycle event timing) and either the same school district of residence at hire or the same prior turnout in the three most recent previous November elections. The table also reports estimates in which new traditional public school employees are reweighted to resemble new charter school employees in the same role on age at hire, hire year, gender, race and ethnicity, highest degree, registration and turnout in the three November elections before hire, county of residence, and residence in one of Ohio's eight largest urban districts. After reweighting, no characteristic differs between the two groups by more than 0.08 standard deviations.

Against clean comparison voters, every group's estimated effect is larger, which suggests that the turnout of people who take school jobs rises over time relative to otherwise similar voters. This appears to be true regardless of the sector of employment. In odd-year elections, the effect for new traditional public school teachers rises from 6.1 to about 10 points, and the effect for new charter school teachers from essentially zero to about 3 points. The difference between the two sectors, however, barely changes (6.3 points in the figures in the manuscript and 7.0 to 7.1 points with clean voters). Reweighting increases it to 8.3 points, although the reweighted difference fails its pre-trend test ($p < .01$). The difference is similarly stable in even-year elections (4.9 points using our preferred estimate and 5.1 to 5.2 points with clean voters). For charter school employees who move to a traditional public school, the effect roughly doubles against clean voters (from 5.5 to 9.6–10.8 points for teachers in odd years). Overall, the choice of comparison group shifts the level of all the estimates but leaves the difference between traditional public and charter school employees, our primary quantity of interest, essentially unchanged.

Table A3: Average post-treatment effects with other school employees and with clean comparison voters

	Odd years				Even years			
	Other employees		Clean voters, matched		Other employees		Clean voters, matched	
	Un-weighted	Re-weighted	District	Prior turnout	Un-weighted	Re-weighted	District	Prior turnout
A. New teachers								
Traditional public school	0.061***	0.080***	0.099***	0.102***	0.036***	0.055***	0.062***	0.064***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]
Charter school	−0.002	−0.002	0.029***	0.031***	−0.013***	−0.013***	0.011**	0.012**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
	[0.34]	[0.34]	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]
Difference	0.063***	0.083***	0.070***	0.071***	0.049***	0.068***	0.051***	0.052***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
	[0.20]	[<0.01]	[0.27]	[0.25]	[<0.01]	[<0.01]	[<0.01]	[<0.01]
B. New non-teaching staff								
Traditional public school	0.009***	0.050***	0.049***	0.051***	0.020***	0.040***	0.046***	0.048***
	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]
Charter school	−0.003	−0.003	0.031***	0.033***	−0.001	−0.001	0.020***	0.021***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
	[0.01]	[0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]
Difference	0.012*	0.053***	0.018***	0.018***	0.021***	0.041***	0.026***	0.027***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)	(0.004)	(0.004)
	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[0.04]	[<0.01]	[<0.01]
C. Charter to traditional public school job changers								
Teachers	0.055***	—	0.096***	0.108***	0.041***	—	0.097***	0.100***
	(0.010)		(0.007)	(0.007)	(0.009)		(0.006)	(0.006)
	[0.09]		[0.01]	[0.01]	[0.55]		[<0.01]	[<0.01]
Non-teaching staff	0.021	—	0.068***	0.078***	0.010	—	0.067***	0.069***
	(0.015)		(0.013)	(0.013)	(0.013)		(0.011)	(0.011)
	[<0.01]		[0.01]	[0.02]	[0.35]		[0.83]	[0.81]

Notes: Entries are average post-treatment effects on turnout among registered voters in November general elections, estimated with the imputation estimator of Borusyak, Jaravel and Spiess (2024) with election-year fixed effects and standard errors clustered by individual in parentheses and pre-trend p-values in brackets (joint Wald tests that the pre-treatment coefficients are equal). "Other employees" columns use the comparisons in the event-study figures in the body of the manuscript. In the "reweighted" columns, new traditional public school employees are reweighted to resemble new charter school employees of the same role (odds weights from a logit of charter employment on age at hire, hire year, gender, race and ethnicity, highest degree, registration and turnout in the three November elections before hire, county of residence, and residence in one of the eight largest urban districts, trimmed at the 99th percentile). The remaining columns compare each group with registered voters from a random sample of Ohio voters who are never school employees or household members of school employees, matched two to one on birth year and either the school district of residence at hire (at the job change in Panel C) or turnout in the three previous November elections. Non-teaching staff are not reweighted by occupation, so their estimates differ slightly from the figures. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$ (two-sided, unadjusted for multiple comparisons).

Alternative Staggered DiD Estimators

Table A4 compares the imputation estimates reported in the manuscript with two alternative heterogeneity-robust estimators: the interaction-weighted estimator of Sun and Abraham (2021) and stacked difference-in-differences with the corrective weights of Wing, Freedman and Hollingsworth (2024). The main conclusions hold across estimators. The size of the residency effects, however,

depends on the estimator, ranging from 0.011 to 0.050 for new teachers and from 0.031 to 0.063 for existing teachers.

The differences reflect the comparison baseline and post-treatment horizon used by each method. The imputation estimator measures each treated voter's post-hire turnout against a counterfactual built from all of that voter's pre-hire elections and from all not-yet-treated and never-treated observations. Sun and Abraham (2021) instead measures effects relative to a single reference election, the last one before hire, and uses only one comparison cohort (see Roth 2026). Where never-treated comparison voters exist (Panels B and D and existing teachers in Panel C), Sun and Abraham (2021) and imputation estimates differ by less than 0.01 and never significantly. The larger gaps arise in the new-hire sample, where everyone is eventually hired. There, the only comparison group available to Sun and Abraham (2021) is the most recently hired cohort, observed only in the years leading up to its own hire, a period in which turnout tends to rise as future employees move and register. A rising comparison series lowers the estimated effects, especially for non-teaching staff (-0.033) and for the residency contrast (-0.017).

Stacking, by contrast, uses clean not-yet-treated and never-treated comparisons but averages effects over only the first three post-hire elections for cohorts whose full window falls within the panel. Its estimates are therefore higher where effects emerge faster and lower where they build up over time. The residency gap for new teachers is largest in the first post-hire election and fades thereafter, so stacking yields a larger estimate (0.050 versus 0.028). Effects on household members grow with time, so stacking yields a smaller one (0.023 versus 0.035). The one estimate that does not survive stacking, for non-teaching staff who move from charter to traditional public schools, rests on fewer than 1,000 treated staff in six cohorts and is imprecise under every estimator.

Table A4: Odd-year post-treatment average effects by estimator

	Imputation (BJS)	Sun–Abraham	Sun–Abraham − BJS	Stacked DiD	Stacked − BJS
A. New hires, traditional public minus charter school					
Teachers	0.063^{***}	0.054^{***}	-0.010^{**}	0.061^{***}	-0.003
	(0.005)	(0.005)	(0.003)	(0.005)	(0.004)
Non-teaching staff (reweighted)	0.023^{***}	-0.010	-0.033^{***}	0.026^{***}	0.003
	(0.005)	(0.005)	(0.004)	(0.005)	(0.004)
B. Charter to traditional public school job changers					
Teachers	0.055^{***}	0.060^{***}	0.005	0.042^{***}	-0.012
	(0.010)	(0.010)	(0.005)	(0.011)	(0.008)
Non-teaching staff (reweighted)	0.051^{**}	0.042^{**}	-0.009	0.002	-0.049^{**}
	(0.016)	(0.016)	(0.011)	(0.020)	(0.015)
C. Working in the district of residence, traditional public school teachers					
New teachers: residence minus elsewhere	0.028^{***}	0.011^{*}	-0.017^{***}	0.050^{***}	0.022^{***}
	(0.004)	(0.005)	(0.003)	(0.005)	(0.004)
Existing teachers moving to residence district	0.041^{***}	0.031^{**}	-0.010	0.063^{***}	0.022^{*}
	(0.010)	(0.010)	(0.005)	(0.012)	(0.011)
D. Household members who stay, traditional public minus charter school					
Members born within ten years	0.035^{***}	0.042^{***}	0.007	0.023^{**}	-0.012^{*}
	(0.008)	(0.009)	(0.006)	(0.008)	(0.006)

Notes: Entries are average post-treatment effects on turnout among registered voters in odd-year November general elections, with standard errors clustered by individual in parentheses. The first column reproduces the imputation estimates of Borusyak, Jaravel and Spiess (2024) reported in the event-study figures (average over all post-treatment elections, relative to each voter's average pre-treatment turnout). The Sun–Abraham column uses the interaction-weighted estimator of Sun and Abraham (2021): a regression with individual and election-year fixed effects (birth-year-by-election-year fixed effects in Panel D) and a full set of hire-cohort-by-event-time indicators, relative to the last election before hire (one year before hire for even-year hires and two years before for odd-year hires, since odd-year November elections occur every other year), averaged over all post-treatment cohort-by-event-time cells with weights proportional to their numbers of observations. The comparison cohort is never-treated individuals in Panels B and D and the second row of Panel C and, because every member of the new-hire sample is eventually hired, the last cohort hired (2024–25) in Panel A and the first row of Panel C. The stacked column uses the stacked difference-in-differences estimator with the corrective weights of Wing, Freedman and Hollingsworth (2024): one sub-experiment per hire cohort with three pre- and three post-treatment odd-year elections, compared with never-treated individuals and individuals not yet hired by the end of the window, so it averages effects over the first three post-treatment elections only, for cohorts whose window lies inside the panel. Non-teaching staff are reweighted by occupation, as in the corresponding figures. Differences between groups are estimated with a common set of comparison individuals. Standard errors of the differences between estimators come from joint individual-level influence functions. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$ (two-sided, unadjusted for multiple comparisons).

Violations of Parallel Trends

Figure A8 reports the sensitivity of the imputation estimates to violations of parallel trends, following Rambachan and Roth (2023). The target is the average of the post-hire coefficients plotted in the event-study figures. Panel A bounds post-hire violations by $\bar{M}$ times the largest change between consecutive pre-hire coefficients. Panel B allows a differential linear trend whose slope may change by up to M per period.

The core results for teachers are the most robust. The gap between new traditional public and

charter school teachers remains positive for post-hire violations up to three-quarters as large as the largest pre-hire change ($\bar{M} = 0.75$) and under linear extrapolation of any pre-hire differential trend (95% CI $[0.036, 0.067]$). The effect on teachers who move from charter to traditional public schools survives $\bar{M} = 0.25$ and changes in slope of up to 0.0025 per period, and the gap between household members of traditional public and charter school teachers survives $\bar{M} = 0.25$ and linear extrapolation ($[0.012, 0.067]$). The residency contrasts are more sensitive to possible violations. Together, these results suggest that the jump in turnout at hire is not an artifact of pre-existing trends, while the persistence of the residency gap in later elections is harder to separate from these trends. Because odd-year November elections occur every other year, adjacent pre-hire coefficients are estimated from different hire cohorts, which makes the pre-hire changes that calibrate $\bar{M}$ noisier and the relative-magnitudes bounds inherently conservative.

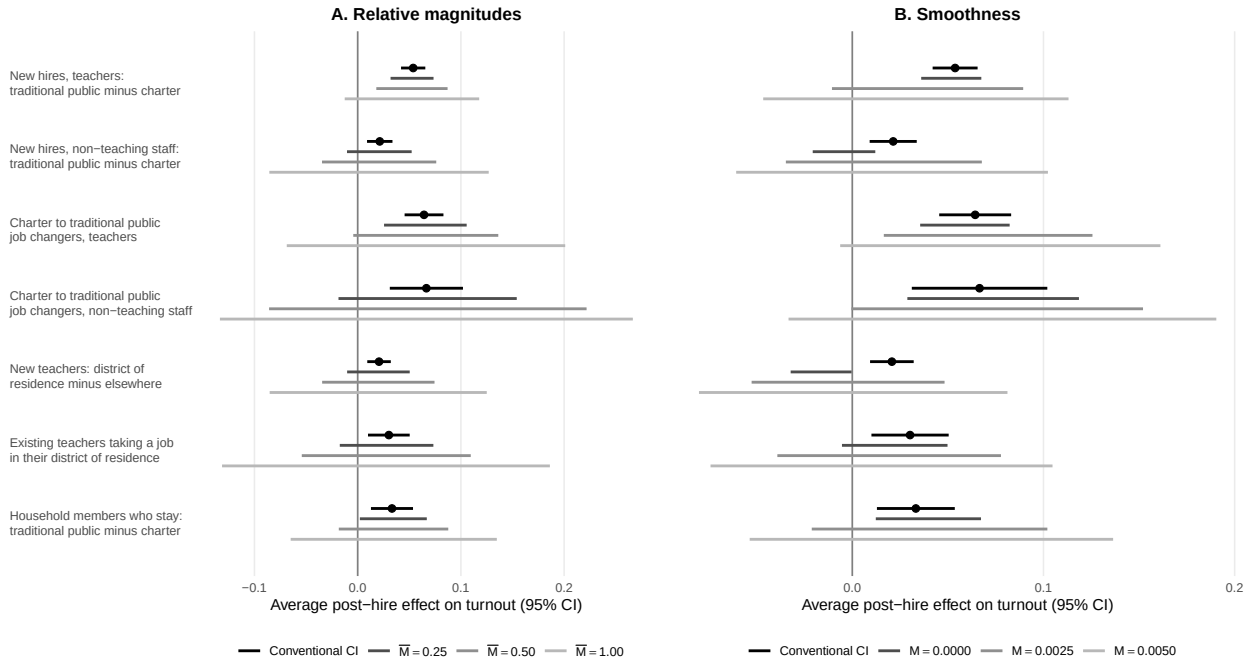

Figure A8: Sensitivity of odd-year effects to violations of parallel trends

Note: The figure reports 95 percent confidence intervals for the average post-hire effect in the odd-year event-study figures under the sensitivity analysis of Rambachan and Roth (2023). The target is the average of the post-hire coefficients (elections zero through four and five or more years after hire), each measured relative to the last election before hire. Black lines show conventional intervals and point estimates. Grey lines show robust intervals, lighter for weaker restrictions. Panel A bounds post-hire violations of parallel trends by $\bar{M}$ times the largest change between consecutive pre-hire coefficients (conditional-hybrid intervals). Panel B allows the slope of a differential trend to change by at most M per period (fixed-length intervals). $M = 0$ corresponds to linear extrapolation of the pre-hire trend. Estimates for non-teaching staff are reweighted by occupation, as in the corresponding figures.

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