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Expertise and Independence on Governing Boards: Evidence from School Districts

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Abstract

In this paper, we study the roles of expertise and independence on governing boards in the context of education. In particular, we examine the causal influence of professional educators elected to local school boards on education production. Educators may bring valuable human capital to school district leadership, thereby improving student learning. Alternatively, the independence of educators may be distorted by interest groups. The key empirical challenge is that school board composition is endogenously determined through the electoral process. To overcome this, we develop and implement a novel research design that exploits California's randomized assignment of the order that candidates appear on election ballots. The insight of our empirical strategy is that ballot order effects generate quasi-random variation in the elected school board's composition. This approach is made possible by a unique dataset that combines election information about California school board candidates with district-level data on education inputs and outcomes. The results reveal that educators on the school board causally increase teacher salaries and reduce district enrollment in charter schools relative to other board members. We do not find accompanying effects on student test scores. We interpret these findings as consistent with educators on school boards shifting bargaining in favor of teachers' unions.

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1 Introduction

Governing boards are a common feature of many organizations, from private corporations to non-profits. A large literature views boards from a principal-agent perspective: boards protect the interests of shareholders by monitoring and exercising control over the organization's managers (John and Senbet, 1998; Adams et al., 2010). In this framework, board independence is instrumental to internal governance. Likewise, the human capital or expertise of board members may bridge information asymmetries, contributing to organizational performance.

In this paper, we study the roles of expertise and independence on governing boards in the context of education. In particular, we examine the causal effects that professional educators – who we define to include former classroom teachers, principals, superintendents, or other school administrators – have on education production when elected to the local school board. In the United States, school board members are typically chosen by voters in local democratic elections and board responsibilities include strategic planning, selecting the superintendent, and bargaining with teachers' unions over pay and working conditions. While some research has examined associations between school board composition and district-level education variables (Land, 2002; Honingh et al., 2018), our paper is distinct in isolating causal effects of local school boards on district inputs and student learning.

The influence of educators on local school boards on education production is theoretically ambiguous: On the one hand, school board members with backgrounds in education may bring valuable human capital to school district leadership. For example, board members who are formerly classroom teachers may have first-hand knowledge of the barriers to and constraints on student learning. This expertise may translate into improved student performance at the district-level by influencing school board decisions regarding inputs, such as teachers' working conditions. Theoretically reducing agency costs, such expertise has been empirically shown to be valuable in other settings: Faleye et al. (2018), for example, find that additional corporate board members with prior industry experience increase the firm's value.[1]

At the same time, pressure or interest groups may influence school board members through

[1]Likewise focused on corporate settings, Wang et al. (2015) and Meyerinck et al. (2016) also examine the value of industry experience. Related studies show that expertise, as measured by directors with CEO experience (Kang et al., 2018) and directors with experience in related industries (Dass et al., 2014), improve firm performance. Other work has examined the financial or legal skills of board members (e.g. Xie et al. 2003).

the electoral process and lead to a misalignment with voters' interests (Becker, 1983; Toma, 1986; Rowley et al., 1988). Specifically, the independence of educators elected to the school board may be compromised by the influence of teachers' unions; Union membership among professional educators is historically widespread and teachers' unions spend substantial amounts of money to influence local school board elections (Hess and Leal, 2005; Moe, 2006). This raises the possibility that educators elected to the school board shift collective bargaining with the district towards union priorities. Unlike expertise, such rent-seeking may potentially be to the detriment of education outcomes. A large theoretical and empirical literature on the impacts of teachers' unions on education highlights this possibility (Hoxby, 1996; Moe, 2009; Cowen and Strunk, 2015).

To pursue our analysis, we assemble a unique dataset that combines election information about California school board members with district-level data on education inputs and outcomes. We use detailed election results collected from the California Elections Data Archive (CEDA) to construct a panel of school board rosters. Candidates for school boards self-identify their occupational background in the CEDA data, which allows us to empirically relate professional educators on the school board to school district variables. 18% of school board members in our sample are educators, a figure that closely matches representative survey data from California (Grissom, 2007). We then link these records with school district data on student enrollment, teacher salary schedules, as well as summaries of student performance on statewide standardized exams from the California Department of Education.

We develop a novel research design to overcome the key empirical challenge that board composition is endogenously determined through the electoral process. The research design exploits California's randomized assignment of the order that school board candidates appear on election ballots. A well-established empirical phenomenon shows that candidates listed at the top of the ballot gain an electoral advantage (Koppell and Steen, 2004). The insight of our empirical strategy is that random assignment generates plausibly exogenous variation in the composition of the elected board due to this ballot order effect. To implement this idea, we match school board election results with the corresponding randomized ballot ordering gathered from the California Secretary of State's office. These records allow us to replicate the finding that candidates assigned to the top of the ballot are more likely to win. We then show that this advantage, when it is randomly conveyed on a candidate who is an educator, in turn shifts the expected number of educators that

are elected to the school board. This research design, which we subject to a variety of validity and placebo tests, thus allows us to provide causal evidence for how school board composition influences educational outcomes.

We begin our analysis by descriptively examining the relationship between the share of educators on the local school board and district-level variables. Cross-sectional comparisons indicate that more educators on the school board are associated with fewer service days, better benefits and higher salaries for teachers, greater superintendent pay, and lower standardized test scores. These relationships are largely accounted for by significant differences across school districts in size and student composition. In particular, larger school districts tend to have both lower test scores and a greater proportion of educators on the school board. Although we find that the estimated negative relationship between educators and test scores is robust to several observed control variables, these adjusted comparisons are nonetheless likely to be confounded by reverse causality or remaining unobserved factors.

Thus we implement our empirical strategy of relying on randomized ballot order to estimate the causal influence of educators on local school boards. While we find little evidence for effects on other dimensions of teacher working conditions, the results reveal that educators causally increase teacher salaries relative to other board members. The estimates suggest that a 10 percentage point increase in the share of educators on the board causes an approximately 1% increase in pay. Further, we find that this salary increase generally applies across-the-board to experience and education levels. The results also indicate that educators on the school board shift district enrollment away from charter schools. Notably, we find that these effects on salaries and charter school enrollment are not accompanied by impacts on superintendant salary, a proxy for quality, or on student test scores in math or reading.

Our results suggest that the expertise of educators on boards does not translate into improved student outcomes despite raising teacher salaries. This may be because, consistent with rent-seeking models of political influence, educators on school boards represent interests other than voters': those of teachers. To investigate this, we examine survey responses of California school board members regarding their professional background and whether they were endorsed by a teachers' union (Grissom, 2007). Relative to members with other backgrounds, educators are 40% more likely to report being endorsed by unions. Our findings thus suggest that school boards are

an important causal mechanism behind teacher union influence on education.[2]

Our paper contributes to a broad literature estimating the contribution of schooling inputs to student learning. This literature has primarily focused on inputs at the school and teacher – rather than the district – levels (e.g. Rivkin et al. 2005; Hanushek 2006; Chetty et al. 2014). The limited prior work on school boards is largely descriptive, focusing on minority representation or conflict, whereas we examine causal effects on learning outcomes.[3] As a result, our work is relevant to ongoing debates regarding the role of school boards, and more generally of local control, in education policy (Howell, 2005; Hess and Meeks, 2010).[4] Issues surrounding local control are gaining importance as a number of recent education reforms such as the Every Student Succeeds Act devolve authority from the federal level back to districts.

Our paper also connects with a wider literature on governing boards.[5] Previous studies suggest that the human capital and independence of board members are important for organizational performance. For instance, Fauver and Fuerst (2006), drawing on German data, argue that representation of workers on corporate boards reduces information asymmetries. In addition, our focus on public school districts relates to recent work that studies political representation and public good provision in particular (Pande, 2003; Ferreira and Gyourko, 2009, 2014; Beach and Jones, 2016, 2017; Logan, 2018; Beach et al., 2018). A ubiquitous problem for empirical work that relates characteristics of board members to outcomes is that board composition is endogenously determined (Hambrick and Mason, 1984; Hermalin and Weisbach, 1998). Our research design overcomes this challenge by developing an empirical strategy based on ballot order effects (Koppell and Steen, 2004; Ho and Imai, 2008; Meredith and Salant, 2013).

The rest of this paper proceeds as follows: We describe the background and responsibilities of school boards in the United States as well as the construction of our dataset in the next section.

[2]The evidence on teachers' unions generally shows increases in intermediate inputs such as teacher salaries (e.g. Hoxby 1996; West and Mykerezi 2011; Brunner and Squires 2013), while results are more mixed for student outcomes (Hoxby, 1996; Lovenheim, 2009; Lott and Kenny, 2013; Lovenheim and Willen, 2016). See Cowen and Strunk (2015) for a recent survey of this literature.

[3]For example, Meier and England (1984) examine the association between minority representation and outcomes, while Grissom (2010) studies the predictors of intraboard conflict. Macartney and Singleton (2017) present evidence from narrowly-decided school board contests that boards causally influence student assignment to schools, but do not examine school boards' effect on student learning.

[4]Existing research shows that gains from local control in K-12 education varies by local context. See, for example, Laing et al. (2016) for evidence on how local socioeconomic status and management capacity matters for benefits derived from decentralization.

[5]Beyond corporate boards of directors, other applications include hospitals (Molinari et al., 1995) and central bank councils (Göhlmann and Vaubel, 2007).

Section 3 presents descriptive analysis of the relationship between educators on school boards and district variables. We then detail our research design in Section 4 and present the results of our analysis in Section 5. We discuss the interpretation of our findings before concluding in Section 6.

2 Background and Data

Locally-elected school boards are a distinctive feature of primary and secondary public education in the United States. Board members are typically elected in non-partisan elections and oversee a wide range of activities and responsibilities in public school districts. We construct a unique dataset that combines information about school board members in California, where almost all members serve four year terms with staggered contests occurring every two years, with data on school district inputs and education outcomes. This section expands on the motivation and variables of interest in our analysis, describes the sources and construction of the dataset, and presents summary statistics.

2.1 School Boards and Education Production

We study the influence that professional educators elected to school boards have on education production. This focus is motivated by viewing school boards as an internal governance mechanism in school districts. Within this framework, the effects of educators on school boards on district inputs and student learning are theoretically ambiguous: On the one hand, educators may bring important human capital to school district leadership. A former classroom teacher, for example, likely has first-hand knowledge regarding effective inputs for learning as well as of the barriers and constraints on education production. Former principals and superintendents may combine classroom knowledge with management experience. Paralleling findings in corporate settings, such expertise may translate into improvements in student learning at the district-level by reducing information asymmetries (Arrow, 1963). Alternatively, the independence of educators elected to the school board, key to their role as monitors on voters' behalf, may be distorted by pressure or interest groups (Becker, 1983; Rowley et al., 1988). Teachers' unions in particular devote a substantial amount of resources toward the election of preferred school board candidates and may compromise the independence of educators on the board (Hess and Leal, 2005; Moe, 2006). The

rents secured for teachers may come at the expense of education outcomes (Hoxby, 1996; Moe, 2009).

School board members can influence district policies and student learning via several channels. Board responsibilities include, but are not limited to, negotiating teacher salaries, hiring and evaluating superintendents, establishing budget priorities, and making decisions regarding student allocation. The scope of board responsibilities motivates us to examine both intermediate outcomes in the form of district-level education inputs as well as downstream effects on students as measured by performance on statewide standardized tests. We focus on inputs in order to understand their role in mediating student performance and education production more generally.

A primary focus of our analysis is the working conditions of teachers in the district. A prominent board responsibility is the collective bargaining process in which members negotiate with teachers' unions over contract dimensions such as salary schedules, instructional hours, and assignment and transfer policies. This role is especially salient in California, where nearly all school districts collectively bargain with unions at least once every three years under the 1975 Rodda Act. For example, educators elected to the school board may allocate district resources in ways they deem beneficial for students or seek improvements in overall pay, returns to additional experience and education, and benefits (as stipulated by the teacher salary schedule).

In addition to their role in collective bargaining, school boards also select and evaluate the district's superintendent. Superintendents set achievement, budgetary, distributional, and related priorities as the chief executives or managers of school districts. As such, the ability of superintendents to exercise their responsibilities is an instrumental input to district direction, operations, and performance. We therefore examine the consequences of changing school boards on district leadership using a proxy measure for superintendent quality.

School boards also play a central role in allocating students to schools. Historically, this board responsibility is at the fore of school desegregation in the United States (Fraga et al., 2005; Reber, 2005; Cascio et al., 2008; Hanushek et al., 2009; Johnson, 2011) and remains important due to discretion over attendance zone boundaries (Macartney and Singleton, 2017; Monarrez, 2018). A somewhat overlooked related activity, however, is the availability of school choice in the district. School boards are the predominant authorizers of charter schools in California. In 2017-2018, 88% of the 336 entities overseeing the more than 1200 charter schools in California were local school

districts (Mumma, 2018).[6] Since charter schools may generate significant fiscal impacts on public school districts (Ladd and Singleton, 2018; Ridley and Terrier, 2018), this responsibility raises questions regarding school boards' incentives and may also have significant implications for overall student outcomes in the district (Teske et al., 2005).[7] We look at educators' influence over charter school enrollment in the district for this reason.

2.2 Data Sources

We assemble a unique dataset from multiple sources. First, we obtain information about school board contests and candidates over a period of two decades from the California Elections Data Archive (CEDA), a statewide database containing local election results.[8] The election records include district name, election date, and a list of candidates for each contest with their corresponding vote totals. Candidate characteristics in the data include full name, incumbency status, the type of term served, and election outcome.

We use information in the CEDA data to summarize the occupational background of each candidate for school board, including whether they worked as a professional educator. Occupation data comes from ballot designations which provide candidates with a three-word opportunity to describe their profession, vocation, or occupation to potential voters. These descriptions represent candidates' primary occupation. By California law, the designation must correspond to the candidate's profession at the time of filing or, if retired from working, their principal occupation prior to retirement.[9] We categorize candidates' ballot designations into educators, businesspeople, or other professions.[10] We identify educators as candidates who describe their primary occupation

[6]The remaining authorizers are 41 county offices of education and the State Board of Education. In almost all cases, charter petitions are submitted to local boards with appeals taken up by county boards of education or the State Board of Education. Upon approval, charters must reapply for authorization every five years. While some studies find that boards make for unsuitable authorizers because of political considerations (Palmer and Gau, 2003), others find few meaningful differences in effectiveness across authorizer types (Carlson et al., 2012).

[7]Influence on student learning may be through students that switch to charters or via spillover effects on students that remain in public schools. See Epple et al. (2016) for a survey of the evidence on charter school effectiveness and competitive impacts of charter schools.

[8]Since CEDA data does not report uncontested elections, our rosters are limited to those members who ever participated in a contested race with at least two candidates.

[9]To ensure the designation accurately portrays the candidate's true profession or vocation, the candidate must supply a Ballot Designation Worksheet providing the factual basis supporting their proposed designations, including a description of their work and contact information for current or former employers. Final word choice must be approved by election officials and can be challenged in court.

[10]Since ballot designations permit the use of up to three key words, it is possible that candidates are cross-listed. The prevalence of this is low; for instance, no more than 10% of educators on the board are listed as both an educator and businessperson. As such we can treat these categories as largely mutually exclusive.

or profession as a teacher, educator, principal, superintendent, or school administrator. In doing so we exclude individuals who work in the education sector but do not focus on K-12 instruction, namely school employees such as counselors and custodians and those employed in post-secondary education. We categorize businesspeople as those who self-describe as an "executive," "businessman," "businesswoman," or "president." The category also includes chief financial officers and self-employed individuals.

We then construct an annual panel of school board rosters using election records. As contests for board seat are staggered, each board consists of winning candidates in the most recent election and board members whose terms have not yet expired. To create the panel, we assume that members serving full terms remain for four years, while those serving short terms remain for the length of time until the next election in the data. These assumptions give us starting and end term dates for each elected board member, which are aggregated for a given district-year to create the final membership roster.[11] From there, we construct variables that summarize the school board's composition, including the share of all members who are educators. We also create the share of incumbents on each school board using a variable provided in the candidate-level records.

We merge the school board panel with district-level variables from a number of sources. From the Common Core of Data, we obtain data on student enrollment and composition by race and ethnicity, sex, and free and reduced price lunch status. These measures enter as control variables in the analysis. The Common Core of Data identifies charter schools administered by each district or local education agency. We use this charter school status to compute an outcome of interest: the share of total district enrollment in charter schools.

We gather information on teacher working conditions from the annual Salary and Benefits Schedule for the Certificated Bargaining Unit (Form J-90). This data source provides comprehensive salary and benefit information for all certified employees, along with variables on credential-based pay and days of service. As the traditional column and step salary schedule in Appendix Figure A1 shows, this source of data provides salaries corresponding to unique combinations of education level (column or lane) and years of experience (step). Common lanes include a BA degree with additional credit hours, while steps begin with the inital year of experience. To ensure

[11] Our predicted list can underestimate board size if an individual occupies a seat that was never contested, and overestimate board size if members step down or are removed before reaching the term limit.

comparability of teacher salaries across districts, we focus on specific education and experience pairs such as attaining a Bachelor's degree with 60 credit hours and 5 years of experience.[12] Our use of teacher salary schedules – as opposed to district summaries of expenditure on instruction or salaries – to measure teacher pay has several advantages, principally that the schedules are directly negotiated between the district and teachers' unions and are not confounded with the composition of the teacher workforce in the district.

In addition to teacher salaries, the collective bargaining process mandates other aspects of working conditions for certified employees. This includes the number of service days required of returning teachers, contributions to employee benefits, and teacher credential-based pay such as lump sums for having a Ph.D. We examine these outcomes as well as class size defined as pupils per teacher.[13] Since district spending on teacher salaries is a function of salaries for every education and experience profile and the number of teachers employed in each category, we also examine the share of overall expenditures going towards instruction as an outcome. This measure is based on district finance data provided by the California Department of Education. Lastly, we expand our scope beyond teachers to examine the consequences for superintendent quality as measured by salaries in the J-90 data.

The final set of outcomes is on student learning. We obtain standardized test score summaries from the California Department of Education, and use school-by-grade-by-year average math and reading scores between 1998 and 2017 to measure the average student performance for each district-year. We normalize performance by year and grade (across all students in California) but exclude charter schools to use only test scores in traditional public schools for constructing school district-level averages of student performance.

We merge these district-level variables with our school board panel to create the final sample. For this merge and our later analysis, we define a "school board" as a unique school district and election year combination. We then index school years subsequent to the election year for each school board as post-treatment periods, beginning with the election year as period 0. For example,

[12] If a district does not specify salaries for these combinations, we use the column and/or step immediately below these educational and experience thresholds. For example, if a district only reports Steps 20 and 30 for the BA+60 column, we would use the salary associated with Step 20 in place of Step 25.

[13] Collective bargaining agreements often specify maximum class size for teachers. This information is not available in the J-90, so instead we base our class size proxy on the number of full time equivalent teachers in the J-90 and student enrollment data.

one school board observed in our dataset is Los Angeles Unified (LAUSD) during the 2012 school board elections. Board members include candidates elected in 2012 and candidates elected prior whose terms have not yet expired. School years 2012-13 and 2013-14 correspond to periods 1 and 2 for this board, while 2011-12 represents period 0. In turn, LAUSD during the 2014 school board election cycle represents a distinct board, as the set of board members may have changed. Note that period 0 for the LAUSD 2014 school board is the same school year as for period 2 for LAUSD 2012.

2.3 Data Summaries

Table 1 summarizes candidate characteristics across 13,588 unique individuals in our sample of California school board elections. Half of this sample won an election at least once between 1998 and 2015. 17% of candidates describe their primary vocation as an educator. Among those who ever won an election, educators comprise 20%, which is consistent with previous descriptive evidence on the occupational backgrounds of school board members.[14] 14% and 13% of candidates and of election winners work in business, respectively. The limited range of keywords used to define businesspeople potentially underestimates their true prevalence.[15] Furthermore, one-quarter of candidates were incumbents while this share increases to over one-third among winning candidates. Finally, winning candidates appear an average of 7.1 years as board members.[16]

Table 2 shifts the unit of observation from candidates to school boards. As described in the data construction, each board represents a unique district-election year combination. The average board in our sample has nearly 5 members. The middle 50% of the distribution ranges from 4 to 6 members, which is consistent with board sizes across California of 3, 5, or 7 individuals. The average share of educators on each board is 18%, while businesspeople comprise 12%. The average share of board members who are incumbents is 58%, which is higher than the candidate-level snapshot taken at the time when the candidate was first observed because incumbency is time-

[14]The 2006 California District School Board Member Survey (covering 222 school districts) estimates that 17% of school board members are educators (Grissom, 2007). The reason for the slightly higher share may be attributable to the categorization of retirees. Approximately a third of the educators in our sample also self-describe as retired, but the 2006 survey has a separate category for retirees.

[15]23% of respondents to the 2006 California District School Board Member Survey report business as their occupation (Grissom, 2007).

[16]This is somewhat lower than the mean of 2.2 terms (or nearly 9 years) reported in survey data (Grissom, 2007). Our numbers are likely a lower bound because we do not observe elections prior to the mid-1990s or those serving terms following an uncontested election.

Table 1: School Board Candidates

	Candidates	Winners
Ever won a contest	0.51	1.00
Educator	0.17	0.20
Businessperson	0.14	0.13
Incumbent	0.25	0.38
Tenure (years)	3.89	7.07
Total	13,588	6,949

Notes: Sample includes unique candidates and their characteristics when first observed winning or participating in school board elections from 1998 - 2015. Winners refer to candidates who ever won a school board election. Candidates who never won an election have 0 years of tenure.

varying. The second panel in Table 2 shows student characteristics in associated school districts. On average the district enrolls nearly 9,000 students, of which 4% are African American, 43% are Hispanic, and 8% are Asian. Two-fifths of the student population are economically disadvantaged as measured by free and reduced lunch eligibility.

Table 2: School Board Characteristics

	Mean	Std. Dev.	25p	75p
Number of Members	4.86	1.59	4	6
Share of Board: Educators	0.18	0.21	0.00	0.33
Share of Board: Businesspeople	0.12	0.17	0.00	0.20
Share of Board: Incumbents	0.58	0.28	0.40	0.80
Total Enrollment	8805	20044	1174	10247
Share Black	0.04	0.06	0.01	0.05
Share Hisp	0.43	0.28	0.18	0.66
Share Asian	0.08	0.11	0.02	0.10
Share FRP Lunch	0.41	0.24	0.22	0.60

Notes: N = 3,672 school board (district-election year) observations. All panels provide characteristics and outcomes in school board election years (i.e. period 0).

Table 3 summarizes the dependent variables that we examine in our analysis. The top panel displays summaries for variables that characterize teachers' working conditions. These variables correspond to items potentially considered in the bargaining process between the school board and teachers' union. On average, teachers are obligated to work 184 service days per school year. 45% of districts provide a bonus for teachers for having a Ph.D. and the maximum district con-

tribution to health benefits is approximately $10,000. We also calculate the pupils per teacher for each district and year, which averages 26 students in our sample. Finally, the average salary for teachers with a Bachelor's degree and 60 credit hours with 5 years of experience according to the district salary schedule is about $53,000. There is notable variation around this average in the data as apparent from the standard deviation of $8,000.

Table 3: School District Inputs and Outcomes

	N	Mean	Std. Dev.	25p	75p
Service Days	10157	183.86	2.14	183	185
Ph.D Bonus Offered	10157	0.45	0.50	0.00	1.00
Max Health Contribution	9987	10108	4864	7065	12478
Pupils per Teacher	9519	26.27	98.43	17.61	25.47
Salary at BA+60, 5Y	10130	52799	8140	46971	57787
Teacher Salary Expenditure Share	11492	0.38	0.06	0.35	0.43
Superintendent Salary	9781	158679	48999	121500	190550
Charter Enrollment Share	10410	0.05	0.14	0.00	0.01
Math Scores	10222	0.05	0.80	-0.51	0.51
Reading Scores	10732	0.12	0.83	-0.47	0.64

Notes: Each observation represents a district-year.

The lower panel of Table 3 expands the district-level variables we examine to collective bargaining outcomes, charter enrollment shares, and student test scores. On average, 38% of district expenditure goes to teacher salaries. We look at the superintendant's salary as a proxy for quality; on average, superintendants are paid nearly $160,000, though the data exhibits considerable variation in this value across observations. Charters schools comprise 5% of overall student enrollment on average, although no students attend charters for districts at the 25th percentile. We focus on student achievement in math and reading as outcomes of interest to examine educational productivity. Standardized test scores show that observations included in the final sample have smaller variance with the middle half of the math distribution falling between -0.5 and 0.5 standard deviation (σ).

3 Descriptive Analysis

Summary statistics indicate substantial variation across districts in school board composition, enrollment shares, and outcome variables. In this section, we draw upon the merged panel dataset to

examine how the share of educators on the school board is related to district inputs and education outcomes. To do this, we estimate regressions of the form:

$$Y_{jt\tau} = \beta T_{jt} + \rho \Theta_{jt0} + \epsilon_{jt\tau} \tag{1}$$

In this equation, $Y_{jt\tau}$ is the district-level outcome variable for school board $j-t$ in district j at election year t, while τ indexes periods relative to t. For this analysis, we pool the immediate (up to two) years following the election year t during which no subsequent election is held. The estimation sample thus consists of school district-year observations for which we successfully match the share of the school board who are educators with each respective dependent variable. As T_{jt} is the share of educators on school board $j-t$, β represents the coefficient of interest in equation (1).

We estimate two specifications for each outcome variable. The first only controls for year fixed effects and can thereby be viewed as estimating base correlations between the share of educators and the outcome variable. These descriptive associations are presented in Table 4. In the second specification shown in Table 5, we estimate conditional correlations of the share of educators on a board with various outcomes by including district-level covariates, Θ_{jt0}. The controls include the size and the prevalence of minority or economically disadvantaged students in the school district and thereby adjust for these observed differences across school districts. The control variables are dated at $\tau = 0$, the year of the school board's election which takes place prior to any actions or interventions by the $j-t$ school board.

Table 4 reveals that the share of educators on the school board is significantly correlated with a number of teacher inputs and district outcomes. For example, the upper panel of the table shows that more educators are associated with fewer teacher service days, a greater likelihood that a Ph.D bonus is offered, larger health benefits, and higher teacher salaries. The point estimate in column (5) suggests that a 10 percentage point increase in the share of educators on the school board is associated with around a 4% increase in negotiated teacher salaries. As shown in the bottom panel of Table 4, the educator share is also associated with higher superintendent salaries, though it does not appear to be statistically related with charter enrollment. Notably, school districts with more educators on the board show substantially lower student performance on statewide exams.

Table 4: Descriptive Associations - Teacher Inputs and District Outcomes

	Service Days (1)	Ph.D Bonus Offered (2)	Log Max Health Benefit (3)	Pupils per Teacher (4)	Log Salary: BA+60, step 5 (5)
Share of Board: Educators	-0.565**	0.352***	0.338***	1.574	0.044***
	(0.232)	(0.072)	(0.186)	(5.650)	(0.016)
Observations	6,203	6,203	6,085	6,334	6,186
	Teacher Salary Share (6)	Log Superint. Salary (7)	Charter Share (8)	Math Scores (9)	Reading Scores (10)
Share of Board: Educators	0.008	0.298***	-0.020	-0.295***	-0.304**
	(0.006)	(0.035)	(0.014)	(0.101)	(0.107)
Observations	7,238	6,010	6,797	6,443	6,793

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are clustered at the district level. All models regress outcomes on the share of the board that are educators. Models furthermore include year fixed effects.

Table 5 presents results corresponding to estimating equation (1) that control for observable differences across districts in size and student composition. Across all of the dependent variables, we observe little association with the share of educations on the school board after conditioning on observed district characteristics. For example, while the correlation in Table 4 suggests that educators on the school board may raise teacher pay, the reversed sign and insignificant coefficient in column (5) of Table 5 indicates that this relationship does not hold when additional covariates are considered. An important exception to this pattern, however, is student achievement. As columns (9) and (10) of Table 5 show, school districts with more educators on the school board show substantially lower student performance on statewide exams even after adjusting for district covariates. The point estimates correspond to a 0.015σ decrease in math and a 0.010σ decrease in reading test scores associated with a 10 percentage point increase in the share of educators.

Taken on their face, the finding that more educators are associated with lower student performance is inconsistent with the view that the human capital of educators may improve student learning. At the same time, the apparent reduction in student performance is not associated with any difference in teacher pay and other working condition variables once accounting for observed differences. Although interesting from a descriptive point of view, it is important not to infer causal relationships from these empirical associations. Estimates are likely confounded by reverse causality or omitted variables leading school board composition to form endogenously. Student

Table 5: Adjusted Associations - Teacher Inputs and District Outcomes

	Service Days (1)	Ph.D Bonus Offered (2)	Log Max Health Benefit (3)	Pupils per Teacher (4)	Log Salary: BA+60, step 5 (5)
Share of Board: Educators	-0.431*	0.093	0.087	-3.636	-0.011
	(0.236)	(0.069)	(0.195)	(4.951)	(0.012)
Observations	6,203	6,203	6,085	6,334	6,186
	Teacher Salary Share (6)	Log Superint. Salary (7)	Charter Share (8)	Math Scores (9)	Reading Scores (10)
Share of Board: Educators	-0.005	0.019	-0.011	-0.153***	-0.111**
	(0.005)	(0.018)	(0.015)	(0.054)	(0.047)
Observations	7,238	6,010	6,797	6,443	6,793

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are clustered at the district level. All models regress outcomes on the share of the board that are educators. Models furthermore include separate year and period fixed effects, and district covariates covering the shares of the student population who are black, Hispanic, Asian, and eligible for free and reduced lunch, district size quintiles, and district type.

performance and educators may be inversely related, for example, if voters respond to lower test scores by disproportionately electing professionals with education experience to the board.[17]

To highlight the likelihood that these associations may be confounded, we examine how the share of educators is related to observed district characteristics used as controls in the second row of Table 5. Table 6 shows strong associations between educator representation and markers of student disadvantage. School boards with more educators have substantially higher enrollment as well as more minority and free and reduced lunch eligible students. These differences along observable dimensions suggest important differences exist along unobserved ones as well. Moreover, because student performance is inversely related to the minority share and free or reduced lunch shares, it is likely such unobserved factors bias the comparison towards greater differences in student performance. We therefore develop and implement a novel research design, detailed in the next section, to estimate the causal effects of board composition.

[17]Similarly, where teacher salaries are low, teachers may organize through the union to elect sympathetic candidates such as former teachers to the school board.

Table 6: Descriptive Associations - Board Composition and District Characteristics

	Total Enrollment	Share White	Share Black/Hisp	Share FRL
Share of Board: Educators	18645***	-0.161***	0.136***	0.038
	(3640)	(0.037)	(0.039)	(0.031)

Notes: Sample size is 3,672 school board observations (save for total enrollment missing in two cases). *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are clustered at the district level. All models regress district enrollment and composition variables on the share of the school board that are educators.

4 Research Design

In this section, we outline our empirical strategy before summarizing the randomized ballot order data that we collect from the California Secretary of State's office. We implement the strategy within an instrumental variables framework, which is discussed alongside the empirical specifications that we estimate.

4.1 Empirical Strategy

Our research design is based around an institutional feature of California school board elections: randomized ballot order. The key insight that we apply is that the combination of ballot order effects and school board candidates' pre-determined characteristics generates quasi-random variation in the probability that a candidate with a particular attribute wins the contest.

The ballot order effect refers to the empirical phenomenon that being listed at the top of the ballot boosts the probability of a candidate winning the election (Koppell and Steen, 2004; Ho and Imai, 2008; Meredith and Salant, 2013; Pasek et al., 2014).[18] A common theoretical explanation for this effect is a satisficing model with a cognitive cost of voting (Miller and Krosnick, 1998; Meredith and Salant, 2013). A voter evaluates candidates according to ballot order and selects the first candidate meeting a minimum threshold for quality net of search costs. Accordingly, ballot

[18]The importance of ballot order has been long recognized by political scientists (Gold, 1952; Bain and Hecock, 1957). Early evidence on this subject was dominated by observational studies and laboratory experiments (Miller and Krosnick, 1998). In the 2000s, researchers began deriving credible causal estimates from natural experiments (Ho and Imai, 2006). While effects are largely minimal or null in US-based general elections, results show sizable effects for primaries, non-partisan races, or elections with low salience (Koppell and Steen, 2004; Alvarez et al., 2006; Ho and Imai, 2008). The ballot order effect is not a phenomenon limited to the academic literature. Maeroff (2010) quotes a candidate as being "delighted when my name came out first, giving me the top position on the ballot. What a fortunate piece of luck. I was as lucky as a jockey who gets the rail position in the Kentucky Derby. The names of candidates are often unknown or barely familiar to voters in school board elections and so for those who mark ballots arbitrarily from top to bottom my name would appear first."

order effects tend to be pronounced in local, non-partisan elections such as school board contests where party labels conveying information about candidates are not available (Ho and Imai, 2008).

Random ballot order assignment ensures that the variation in the probability that an educator wins the election induced by ballot order effects is exogenous given the candidate pool. To formalize this intuition, we begin with a setup that embeds: 1) ballot order effects, and 2) randomized ballot order.[19] The probability that candidate i in school board election contest r in district j wins the contest can be expressed as:

$$Win_{jri} = \alpha First_{jri} + \gamma Educator_{jri} + \epsilon_{jri} \tag{2}$$

where $First_{jri}$ is an indicator for whether candidate i is listed at the top of the ballot in contest r. $Educator_{jri}$ is an indicator variable for whether candidate i is a professional educator while ϵ_{jri} contains all other variables that determine electoral success.[20] This equation is analogous to those estimated in the ballot order literature. A top of the ballot advantage is expressed as $\alpha > 0$ in equation (2). The randomized ballot order meanwhile implies that $First_{jri}$ is independent of $Educator_{jri}$ and ϵ_{jri}.

The insight of our research design is to consider what the top of the ballot advantage, when conveyed on a candidate who is an educator, implies for the total number of winners of the contest that are educators.[21] The total is given by $\#Educators_{jr} = \sum_i Win_{jri} Educator_{jri}$. We mutiply both sides of equation (2) by $Educator_{jri}$ and aggregate over i. This yields:

$$\#Educators_{jr} = \tilde{\alpha} FirstEducator_{jr} + \tilde{\gamma} \overline{Educator}_{jr} + \bar{\epsilon}_{jr} \tag{3}$$

In this expression, the number of educators that win election to the school board depends on three factors: 1) the share of educators among the candidate pool in contest r, denoted by $\overline{Educator}_{jr}$; 2) other electoral determinants, $\bar{\epsilon}_{jr}$; and 3) whether the candidate assigned to the top of the ballot is an educator, which we denote by $FirstEducator_{jr}$.

$FirstEducator_{jr}$ is the core instrument our empirical strategy is built around: Because $\alpha > 0$ in equation (2), $FirstEducator_{jr}$ has a causal impact on the total number of winners that are

[19] For ease of exposition, we characterize ballot order effects as simply a top of the ballot advantage.
[20] Note that we do not assume that this ϵ is uncorrelated with being an educator.
[21] Recall that, in general, school board contests may have multiple winners.

educators, i.e. $\tilde{\alpha} > 0$. This impact in turn has causal implications for the composition of the elected school board. Moreover, the randomized ballot order ensures that, conditional on the fraction of candidates in the contest who are educators, $\overline{Educator}_{jr}$, whether a candidate who is an educator is assigned to the top of the ballot is unrelated to the other determinants of electoral outcomes, $\bar{\epsilon}_{jr}$.[22]

4.2 Randomized Ballot Order in California

We gather ballot order data from California to implement our empirical strategy. California began randomizing alphabets to determine candidate ordering on ballots in 1975 (California Election Code Section 13112). The randomization is conducted by the Secretary of State's office on the 82nd day before an election and the resulting alphabet applies throughout candidates' last and first names.[23] Importantly, candidates for school board must file a declaration of candidacy between 113 to 88 days before the election date. As such, the alphabet drawing always takes place after the election entry deadline, so candidates cannot base their decisions to run on their ballot placement.[24]

We compile a database of randomized alphabet drawings for elections from 1998-2015 using press releases from the California Secretary of State's office. We determine the ballot order by matching election dates and applying the alphabet throughout candidates' last and first names. Appendix Figure A2 shows a sample ballot from the November 4, 2014 general election. The school board part of the ballot allows up to three votes corresponding to all available seats. We can verify that the actual ordering of candidates on this ballot coincides with our predicted candidate order using the alphabet ordering in Appendix Table A1. We repeat this exercise with multiple election ballots to ensure that predicted ballot order is accurate across districts and years.

[22] This follows from observing that independence of $First_{jri}$ implies that, conditional on $Educator_{jri}$, whether an educator is top of the ballot should be uncorrelated with $\epsilon_{jri} Educator_{jri}$.

[23] While for statewide offices and U.S. Congressional elections, candidate ordering rotates across Assemblies, all nonpartisan races such as school board elections taking place on major election dates abide by the same randomized alphabet ordering.

[24] A related consideration is the possibility of changed campaigning tactics in response to candidates' assigned ballot order. For instance, those at the top of the ballot may scale back their campaigns because they believe the reduced effort will be offset by their relative order advantage, while those near the bottom of the ballot may increase their effort. Even if these behavioral changes are present, their influence is limited to the time frame of 82 days between the alphabet drawing and election date. Furthermore, the hypothesized direction of these responses would just attenuate the ballot order effect.

4.3 Empirical Specification

We are interested in the treatment effects of school board composition on district-level inputs and education outcomes:

$$Y_{jt\tau} = \beta T_{jt} + \theta W_{jt0} + \upsilon_{jt\tau} \tag{4}$$

As before, $Y_{jt\tau}$ is an outcome variable for school board $j{-}t$ in post-election period τ. T_{jt} is the share of educators on the school board, such that β represents the treatment effect of interest. W_{jt0} includes covariates at time t of the election (and hence subscripted by 0). The empirical challenge, discussed in our descriptive analysis, is that naive estimates of equation (4) are likely to be confounded, such as by reverse causality or omitted variables.

We use the randomized ballot order instrument to overcome this challenge. To do this, we construct $FirstEducator_{jtr}$ – whether the candidate assigned to the top of the ballot in contest r is an educator – from the ballot order data for each contest. We then estimate first-stage regressions that examine how the instrument shifts the elected school board's composition where T_{jt} is pooled across all contests for each board $j{-}t$:[25]

$$T_{jt} = \alpha FirstEducator_{jtr} + \Gamma W_{jt0r} + \varepsilon_{jtr} \tag{5}$$

W_{jt0r} in this equation represents election and district covariates observed during the election year, which importantly includes the share of educators in the candidate pool for electoral contest r. To estimate causal effects, we begin with reduced-form specifications that combine equations (5) and (4):

$$Y_{jt\tau} = \pi FirstEducator_{jtr} + \kappa W_{jt0r} + u_{jt\tau r} \tag{6}$$

This specification directly relates district input and education outcome variables to our instrument for post-treatment periods ($\tau > 0$). Thus it has the advantage of cleanly uncovering causal effects while maintaining agnosticism regarding the exact channel through which the ballot order instrument affects education. For estimation, however, we augment equation (6) to also leverage panel variation. We do this by including district fixed effects in the equation and including period $\tau = 0$

[25] We cluster standard errors at the district level for all of the estimates.

(i.e. the year of the election) in the estimation sample for each school board:

$$Y_{jt\tau} = \pi FirstEducator_{jtr} \times \mathbf{1}(\tau > 0) + \kappa W_{jt0r} + \theta_j + u_{jt\tau r} \tag{7}$$

The causal effect as represented by π turns "on" for post-election periods ($\tau > 0$), while the election year outcomes contribute to identification of the district fixed effects, θ_j. These fixed effects absorb any time-invariant unobserved differences across districts, relying just on changes within-district associated with the instrument to identify causal effects.[26] Notably, the assumption of no causal effect in the election year, implicit in equation (7), is a placebo test of our empirical strategy. We also estimate analogous treatment effects via two-stage least squares using the ballot order instrument for interpretive purposes.[27]

The validity of our research design rests on the assumption that our instruments are valid given the candidate pool composition embedded in W_{jt0r}: $E[u_{jt\tau r}, FirstEducator_{jtr} | \overline{Educator}_{jtr}] = 0$. While not directly testable, we perform a number of checks to support this assumption. In particular, we examine whether our top of the ballot indicators are associated with any observed electoral or district covariates after conditioning on the candidate pool composition. We also examine whether the instruments appear to shift the composition of the school board or the outcomes variables *prior* to the treatment.

5 Results

We report results in three parts. First, we provide evidence that the ballot order instrument shifts school board composition by increasing the share of educators on the elected board. We then present reduced-form and two-stage least squares estimates of causal effects on district inputs and education outcomes. We also compare the reduced-form estimates for educators with estimates for incumbents on the school board. Validity checks and placebo tests follow.

[26] This specification is similar to those estimated by Beach and Jones (2017) and Cellini et al. (2010).

[27] While for ease of exposition, equation (7) restricts κ to be constant for all τ, we relax this in the estimation and also include time and period effects.

5.1 Evidence of Treatment

The viability of our ballot order instrument, the assignment of an educator to the top of the ballot, depends on whether ballot order can significantly shift the composition of elected school boards. We begin by replicating the ballot order effects estimated in the prior literature, before examining their implications for board composition and district outcomes.

Table 7 reports ballot order effects in our sample of California school board contests. The results are obtained from candidate-level regressions of electoral success on an indicator for being top of the ballot while controlling for district and year fixed effects. We examine two outcomes: the candidate's vote share and whether they won the contest. Column (1) reveals that candidates randomly listed at the top of the ballot gain 5.4 percentage points in vote shares relative to other candidates. Since vote share is a function of candidate pool size and other electoral features, this first candidate advantage attenuates to a significant and sizable 1.8 percentage points after we control for election attributes. Translated into winning probability, the base and augmented models show a 10.8 and 8.4 percentage point first candidate advantage (columns (3) and (4)) relative to all other ballot order positions, respectively.[28]

Table 7: Ballot Order Effects

	Vote Share		Win Contest	
	(1)	(2)	(3)	(4)
Top of Ballot	0.054***	0.018***	0.108***	0.084***
	(0.003)	(0.002)	(0.011)	(0.011)
Controls	N	Y	N	Y
Mean	0.22		0.47	

Notes: Sample size is 20,331 candidate-electoral contest observations. *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the district level. Sample includes candidate observations with non-missing ballot order and district data in school board elections from 1998 - 2015. All specifications include separate election year and district fixed effects. Control specifications additionally include a quadratic of the share of candidates who are former educators and incumbents, and their interactions, indicators for the number of open seats and candidates for each race, and the total number of open seats at the district level for a given year.

The ballot order effects we estimate are consistent with results from the prior literature. Stud-

[28]We further investigate whether the first order advantage differentially affects candidates from different occupational backgrounds. Specifically, we interact the top of the ballot indicator with whether a candidate is an educator. Table A2 shows that no additional advantages in winning probability are conferred on educator candidates randomly assigned to the top of the ballot.

ies using vote shares as the dependent variable estimate the ballot order effect as between 1-5% for Ohio elections (Miller and Krosnick, 1998; Brockington, 2003) and 2-3 percentage points for California primaries (Ho and Imai, 2008). Our results on winning probabilities are also comparable to the 5 percentage point first candidate advantage estimated by Meredith and Salant (2013) using all nonpartisan elections in California.

We next apply the ballot order advantage to candidates who are educators to examine its implications for the composition of the elected school board. We estimate first-stage equation (5) for two measures of composition: 1) the share of contested seats on the board that are won by educators, and 2) the share of all school board members that are educators. The second group includes both election winners and, because of staggered contests, members whose terms have not yet expired. We estimate two specifications for each measure. The first only controls for the share of educators in the candidate pool. The second model is augmented with additional election and district covariates.

Table 8: Evidence of Treatment

	Share of Cont. Seats: Educators		Share of Board: Educators	
	(1)	(2)	(3)	(4)
Top of Ballot Educator	0.070***	0.066***	0.022**	0.029***
	(0.017)	(0.017)	(0.009)	(0.007)
Controls	N	Y	N	Y
F-statistics	17.63	15.94	6.16	16.08

Notes: Sample size is 4,830 electoral contests across all specifications. *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the district level. The base model includes a quadratic of the share of candidates who are former educators. Additional controls include a quadratic of the share of candidates who are incumbents, the interaction between the shares of educators and incumbents, indicators for the number of available seats, candidates for each race, and the total number of contested seats at the district level for a given year, the proportions of the board who are educators or incumbents and not up for election in the current cycle, shares of the student population who are black, Hispanic, Asian, and eligible for free and reduced lunch, district size quintiles, district type indicators, and district type-specific trends. All models include election year and district fixed effects.

Table 8 provides evidence that the ballot order instrument causally shifts the share of educators on the school board. As shown in column (2), an educator assigned to the top of the ballot increases the share of educators among election winners by nearly 7 percentage points after controlling for election- and district-level attributes. Looking at the composition of the school board as a whole, a first-listed educator increases the proportion of educators by 2.9 percentage points under the

preferred specification in column (4). Table 8 also reports F-statistics for each dependent variable ("treatment") to asses the power of the ballot order instrument. For column (4), the F-statistic is over 16. We can reject the hypothesis that the maximum relative bias is at least 10% under a test with 5% significance level (Stock and Yogo, 2005).

5.2 Causal Effects

The ballot order instrument's effect on the number of educators on the board enables us to isolate causal effects on district inputs and education outcomes. We begin by presenting "event study" and pooled reduced-form estimates before turning to treatment effect results.

5.2.1 Reduced-Form "Event Study" Estimates

We first examine the reduced-form effects of our top of the ballot instrument through an "event study"-style specification that estimates the causal effect for each period τ. To do this, we adapt equation (7) as:

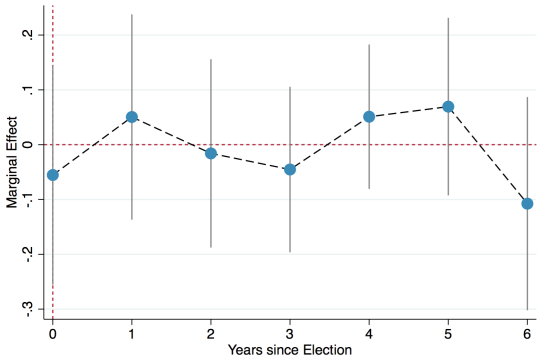
$$Y_{jt\tau} = \pi_\tau FirstEducator_{jtr} + \kappa_\tau W_{jt0r} + \theta_j + u_{jt\tau r} \tag{8}$$

The indexing of π_τ by τ allows us to estimate the effect of first candidate advantage on outcome $Y_{jt\tau}$ for each year post-election. This specification thus estimates a time profile of reduced-form causal effects. We also estimate π_0 as part of this analysis, which serves as a placebo test because the ballot order instrument should have no relationship to the outcome variables in the year of the election. We include $\tau = 0, ..., 6$ in the estimation sample for each school board.[29] The 6-year post-treatment window provides time for changes to take effect. In particular, collective bargaining agreements are negotiated at least once every three years, so effects may not be realized immediately. Input changes may similarly take time to manifest in test scores and other outcomes.[30]

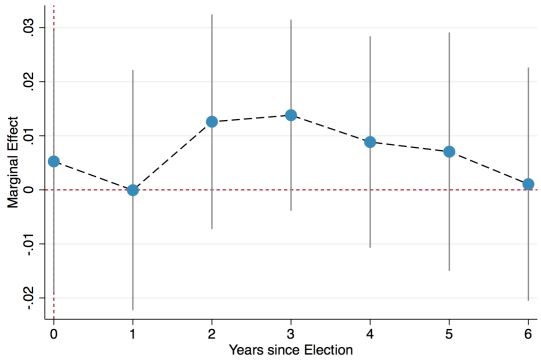
We visually present the results of the reduced-form event study specification by plotting estimates over time (and corresponding error bars). Figure 1 plots estimates corresponding to the five outcomes on teacher working conditions. One initial takeaway from the figures as a whole is that

[29] While not represented explicitly, we include election year (t) and period (τ) fixed effects as well as district type-specific secular trends throughout in the estimation of causal effects. We also specify κ_τ as the combination of a period 0 indicator and a trend for the post-treatment periods.

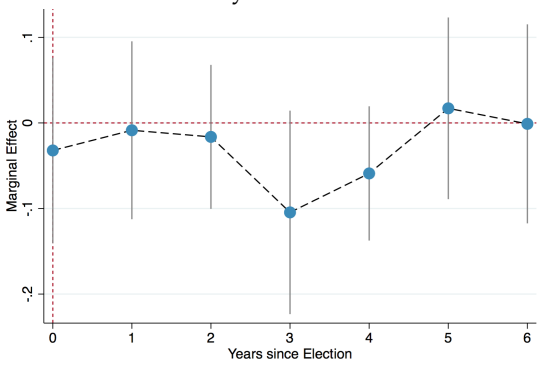
[30] Note that the composition of the school board is likely to change due to subsequent elections over the post-treatment window. The findings should thus be interpreted as a combination of the direct impact of the marginally-elected education on outcomes of interest and any indirect impact via subsequent electoral results.



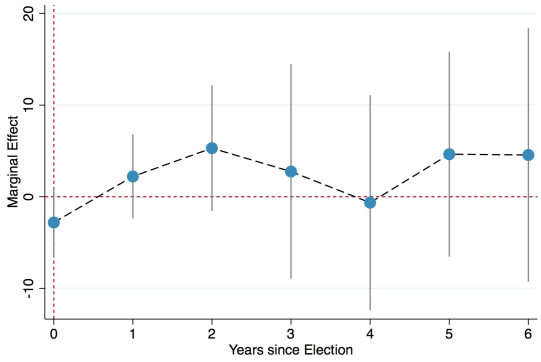
(a) Days of Service



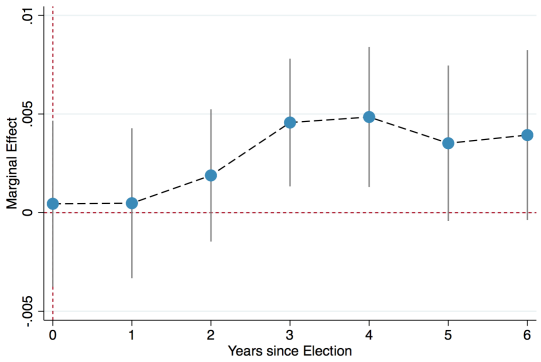
(b) Ph.D Bonus Offered



(c) Log Max Healthcare Benefit



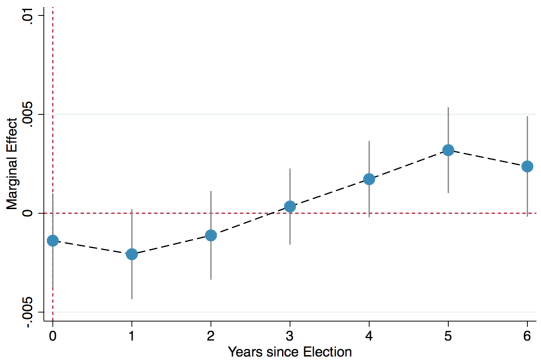
(d) Pupils per Teacher



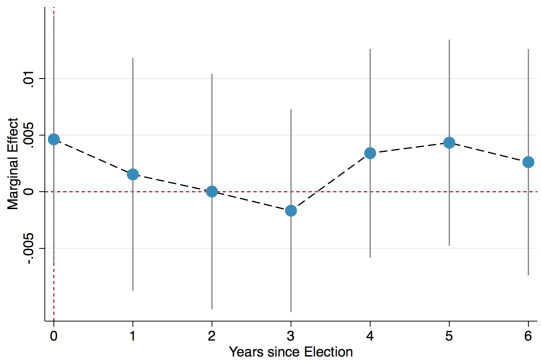
(e) Log Salary at BA+60, 5 Years of Experience

Figure 1: Event-Study Causal Estimates - Teacher Inputs

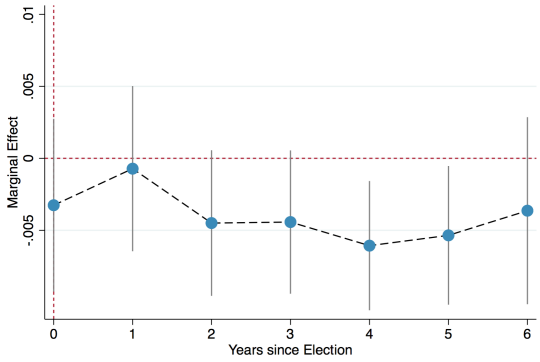
Note: Robust standard errors are clustered at the district-level. Coefficients correspond to interactions between the instrument and the number of elapsed years. Model covariates include a quadratic of the share of candidates who are incumbents, the interaction between the shares of educators and incumbents, indicators for the number of available seats, candidates for each race, and the total number of contested seats at the district level for a given year, the proportions of the board who are educators or incumbents and not up for election in the current cycle, shares of the student population who are black, Hispanic, Asian, and eligible for free and reduced lunch, district size quintiles, and district type indicators. These covariates are separately interacted with an indicator for period 0 and a trend for post-treatment periods. Models furthermore include indicators for missing salary or test score outcomes for a given period, period and election year fixed effects, district type-specific trends, and district fixed effects.



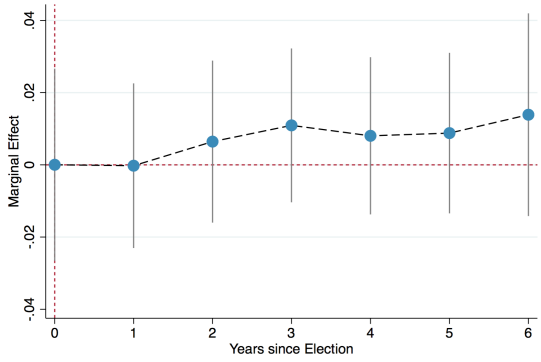
(a) Share of Expenditures on Teacher Salaries



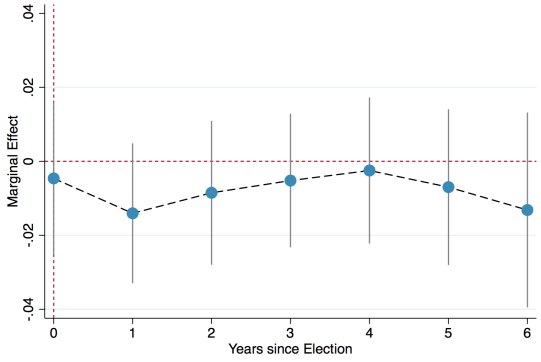
(b) Log Superintendent Salary



(c) Share of Enrollment in Charter Schools



(d) Math Scores



(e) Reading Scores

Figure 2: Event-Study Causal Estimates - District Outcomes

Note: Robust standard errors are clustered at the district-level. Coefficients correspond to interactions between the instrument and the number of elapsed years. Model covariates include a quadratic of the share of candidates who are incumbents, the interaction between the shares of educators and incumbents, indicators for the number of available seats, candidates for each race, and the total number of contested seats at the district level for a given year, the proportions of the board who are educators or incumbents and not up for election in the current cycle, shares of the student population who are black, Hispanic, Asian, and eligible for free and reduced lunch, district size quintiles, and district type indicators. These covariates are separately interacted with an indicator for period 0 and a trend for post-treatment periods. Models furthermore include indicators for missing salary or test score outcomes for a given period, period and election year fixed effects, district type-specific trends, and district fixed effects.

we do not find an effect at period $\tau = 0$ that can be rejected as different from zero for any of the dependent variables. The results thus pass this placebo test. The figures also do not show evidence of post-election effects on days of service, whether the district offers a Ph.D bonus, healthcare benefits, or pupils per teacher. On the other hand, we do find evidence of impacts on negotiated teacher salaries as shown in panel (e). While not different from zero through two post-election years, the figure shows a statistically significant increase in the salary of teachers starting in the third year after the initial election. The uptick in salaries over the first three years is consistent in timing with the once-every-three-year cadence of renegotiating collective bargaining agreements. Effects then persist through the end of the period studied. The event-study results thus indicate that an educator assigned to the top of the ballot leads to a causal increase in teacher salaries, but has no effect on the remaining four teacher inputs.

Figure 2 presents the time profile of estimates for the five district-level outputs in our analysis. As with the teacher inputs, we reassuringly do not find evidence for impacts in the placebo year. In addition to validating our research design, panel (a) shows some evidence that the rise in negotiated teacher salaries tracks into an increase over the longer-run in the share of district expenditure on teacher salaries. Panel (b) indicates no significant effects on the salaries of district superintendents. Panel (c) estimates of the effects on charter school enrollment suggest that an educator assigned to the top of the ballot leads to a causal reduction in the share of district enrollment in charters. Point estimates are statistically different from zero in periods five and six. Finally, panels (d) and (e) of Figure 2 examine reduced-form effects on student achievement in math and reading. We find no evidence of a significant effect on either test following the assignment of an educator to the top of the ballot.

5.3 Reduced-form Estimates

We present pooled reduced-form estimates following (7) in this subsection to assess the overall statistical significance of the causal impacts. We estimate the equation pooling $\tau = 0, ..., 6$ while restricting the effect to be constant for post-treatment periods and, consistent with the results of the placebos presented in the prior subsection, assuming no impact in $\tau = 0$.

Table 9 reports pooled reduced-form results for the five teacher input variables that are broadly consistent with the event-study figures. For service days, Ph.D bonus, and health benefits, we do

Table 9: Reduced-Form Causal Estimates - Teacher Inputs

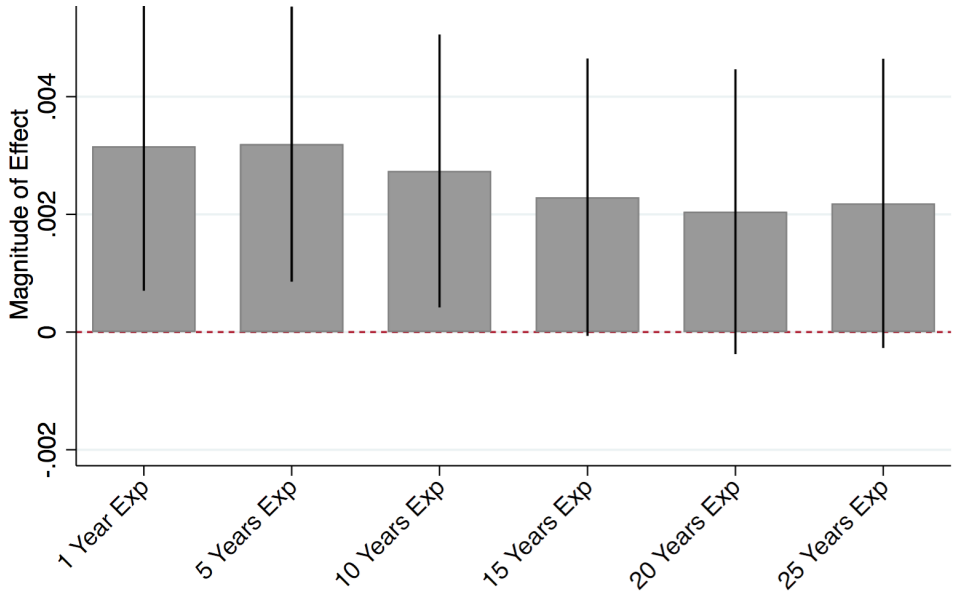
	Service Days (1)	Ph.D Bonus Offered (2)	Log Max Health Benefit (3)	Pupils per Teacher (4)	Log Salary: BA+60, step 5 (5)
Top of Ballot Educator	0.0030	0.0073	-0.0302	3.198*	0.0032***
	(0.0447)	(0.0062)	(0.0274)	(1.800)	(0.0012)
Observations	28,423	28,423	27,915	26,680	28,362

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are clustered at the district level. The sample is a stacked dataset including periods 0-6 that examines outcomes 1-6 years post-treatment. Coefficients correspond to the ballot order instrument in the reduced-form specification. Model covariates include a quadratic of the share of candidates who are incumbents, the interaction between the shares of educators and incumbents, indicators for the number of available seats, candidates for each race, and the total number of contested seats at the district level for a given year, the proportions of the board who are educators or incumbents and not up for election in the current cycle, shares of the student population who are black, Hispanic, Asian, and eligible for free and reduced lunch, district size quintiles, and district type indicators. These covariates are separately interacted with an indicator for period 0 and a trend for post-treatment periods. Models furthermore include indicators for missing salary or test score outcomes for a given period, period and election year fixed effects, district type-specific trends, and district fixed effects.

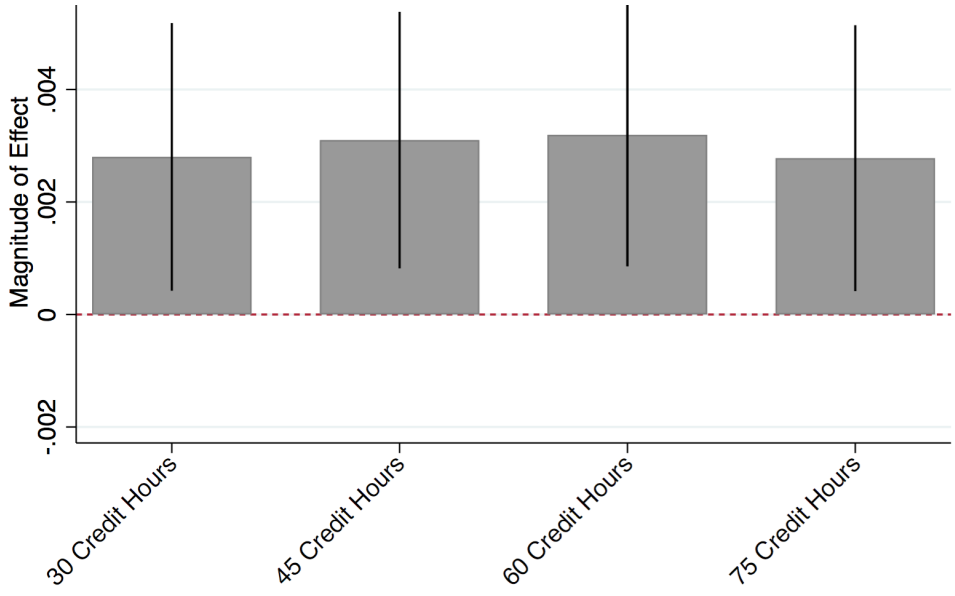
not find any evidence of an effect stemming from the assignment of an educator to the top of the ballot. For pupils per teacher, presented in column (4), the pooled results suggest a slight increase, suggesting larger class sizes, but this result is not statistically different from zero at conventional thresholds. Column (5) indicates a causal (and statistically significant) increase in teacher salaries that is due to an educator at the top of the ballot.

In our primary analysis, we examine effects on negotiated salaries for teachers with a Bachelor's degree, 60 credit hours, and 5 years of experience. Evidence of causal influence motivates a further examination of whether salary effects apply across-the-board or vary with education and experience. Figure 3 displays period-specific coefficients from estimating equation (7) on teacher salaries at various combinations of experience and education levels. The results show that the causal increase in teacher salaries generally applies to different degree and credit attainment and experience levels. While the upper panel of the table show reduced precision when estimating impacts at 20 and 25 years of experience, we cannot reject that the point estimates do not differ from the effect size at 5 years of experience. The assignment of an educator to the top of the ballot thus leads to a general increase in teacher salaries in the district.

Table 10 presents pooled reduced-form estimates of effects on district outcomes. We do not find effects on the share of expenditure on teacher salaries when pooling post-election periods. This



(a) Log Salary Effects by Experience



(b) Log Salary Effects by Education

Figure 3: Causal Estimates - Teacher Salaries by Experience and Education

Notes: Robust standard errors are clustered at the district level. The sample is a stacked dataset including periods 0-6 that examines outcomes 1-6 years post-treatment. Coefficients correspond to interactions between the instrument and a treatment indicator. Each bar shows results from a separate model. Model covariates include a quadratic of the share of candidates who are incumbents, the interaction between the shares of educators and incumbents, indicators for the number of available seats, candidates for each race, and the total number of contested seats at the district-level for a given year, the proportions of the board who are educators or incumbents and not up for election in the current cycle, shares of the student population who are black, Hispanic, Asian, and eligible for free and reduced lunch, district size quintiles, and district type indicators. These covariates are separately interacted with an indicator for period 0 and a trend for post-treatment periods. Models furthermore include indicators for missing salary or test score outcomes for a given period, period and election year fixed effects, district type-specific trends, and district fixed effects.

Table 10: Reduced-Form Causal Estimates - District Outcomes

	Teacher Salary Share (1)	Log Superint. Salary (2)	Charter Share (3)	Math Scores (4)	Reading Scores (5)
Top of Ballot Educator	0.0006	0.0015	-0.0038**	0.0075	-0.0083
	(0.0007)	(0.0033)	(0.0018)	(0.0085)	(0.0079)
Observations	30,772	27,624	28,187	26,948	28,633

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are clustered at the district level. The sample is a stacked dataset including periods 0-6 that examines outcomes 1-6 years post-treatment. Coefficients correspond to the ballot order instrument in the reduced form specification. Model covariates include a quadratic of the share of candidates who are incumbents, the interaction between the shares of educators and incumbents, indicators for the number of available seats, candidates for each race, and the total number of contested seats at the district level for a given year, the proportions of the board who are educators or incumbents and not up for election in the current cycle, shares of the student population who are black, Hispanic, Asian, and eligible for free and reduced lunch, district size quintiles, and district type indicators. These covariates are separately interacted with an indicator for period 0 and a trend for post-treatment periods. Models furthermore include indicators for missing salary or test score outcomes for a given period, period and election year fixed effects, district type-specific trends, and district fixed effects.

contrast with the event-study results because, as shown in panel (a) of Figure 2, teacher salary share appears to decrease before increasing over the longer-run. Consistent with the event-study, we do not find evidence for impacts on superintendent salary, but the results indicate that the assignment of an educator to the top of the ballot reduces charter school enrollment. The final columns of Table 10 are consistent with Figure 2 in finding no meaningful impact on math and reading scores.

5.3.1 Treatment Effect Estimates

To facilitate the interpretation of reduced-form results, we present corresponding treatment effect estimates in this subsection. They derive from two-stage least squares specifications analogous to the reduced-form specification (7):

$$Y_{jt\tau} = \beta T_{jt} \times \mathbf{1}(\tau > 0) + \rho_\tau \Theta_{jt0r} + \tilde{\theta_j} + \epsilon_{jt\tau r} \tag{9}$$

where T_{jt} is the share of school board $j-t$ who are educators and β is the treatment effect of interest. This specification includes district fixed effects, $\tilde{\theta_j}$, and relies on within-district variation in T_{jt} induced by our instrument for identification. The estimation sample includes periods $\tau = 0, ..., 6$.

Table 11: Treatment Effect Estimates - Teacher Inputs

	Service Days (1)	Ph.D Bonus Offered (2)	Log Max Health Benefit (3)	Pupils per Teacher (4)	Log Salary: BA+60, step 5 (5)
Share of Board: Educators	0.107	0.259	-1.151	114.80	0.114**
	(1.596)	(0.228)	(1.071)	(70.54)	(0.050)
Observations	28,423	28,423	27,915	26,680	28,362

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are clustered at the district level. The sample is a stacked dataset including periods 0-6 that examines outcomes 1-6 years post-treatment. Models use an indicator for being a first-listed educator to instrument for the share of school boards who are educators. Model covariates include a quadratic of the share of candidates who are incumbents, the interaction between the shares of educators and incumbents, indicators for the number of available seats, candidates for each race, and the total number of contested seats at the district level for a given year, the proportions of the board who are educators or incumbents and not up for election in the current cycle, shares of the student population who are black, Hispanic, Asian, and eligible for free and reduced lunch, district size quintiles, and district type indicators. These covariates are separately interacted with an indicator for period 0 and a trend for post-treatment periods. Models furthermore include indicators for missing salary or test score outcomes for a given period, period and election year fixed effects, district type-specific trends, and district fixed effects.

Table 11 presents treatment effect estimates on teacher inputs, while Table 12 presents estimates on district outcomes. Column (5) of Table 11 displays the results for teacher salaries, the variable for which we identified reduced-form impacts in the prior subections. The results indicate that a 10 percentage point increase in the share of educators on the school board raises teacher salaries in the district by about 1.1%. Table 12 similarly places a treatment effect intrepretation on the charter enrollment results. The point estimate, displayed in column (3), indicates that a 10 percentage point shift towards more educators on school boards leads to a 1.3 percentage point decrease in the share of students enrolled in charter schools.

5.4 Causal Effects of Educators vs. Other Groups

Our results identify the causal effects of educators on the school board relative to members with other professional backgrounds. Our focus on educators is motivated by the possibility that their sector-specific human capital is valuable. In this subsection, however, we consider another dimension of human capital: on-the-job experience on the school board. We thus extend our empirical strategy of leveraging ballot order variation to examine the causal influence of incumbent board members. This exercise informs an understanding of how educational expertise on school boards translates into district outcomes in ways that may be distinct from or similar to other kinds of

Table 12: Treatment Effect Estimates - District Outcomes

	Teacher Salary Share (1)	Log Superint. Salary (2)	Charter Share (3)	Math Scores (4)	Reading Scores (5)
Share of Board: Educators	0.020	0.054	-0.133*	0.283	-0.300
	(0.026)	(0.122)	(0.070)	(0.325)	(0.289)
Observations	30,772	27,624	28,187	26,948	28,633

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are clustered at the district level. The sample is a stacked dataset including periods 0-6 that examines outcomes 1-6 years post-treatment. Models use an indicator for being a first-listed educator to instrument for the share of school boards who are educators. Model covariates include a quadratic of the share of candidates who are incumbents, the interaction between the shares of educators and incumbents, indicators for the number of available seats, candidates for each race, and the total number of contested seats at the district level for a given year, the proportions of the board who are educators or incumbents and not up for election in the current cycle, shares of the student population who are black, Hispanic, Asian, and eligible for free and reduced lunch, district size quintiles, and district type indicators. These covariates are separately interacted with an indicator for period 0 and a trend for post-treatment periods. Models furthermore include indicators for missing salary or test score outcomes for a given period, period and election year fixed effects, district type-specific trends, and district fixed effects.

experience.

Appendix Table A4 presents estimates from reduced-form specifications that embed two ballot advantage instruments: top of the ballot educator and top of the ballot incumbent. The estimates for educators at the top of the ballot are very close in magnitude to prior findings. In contrast to the influence of educators on salaries and charter enrollment, however, having incumbents in the first ballot order position does not significantly affect teacher salaries or charter share. However, column (2) of Table A4 shows that first order advantage among incumbents reduces the likelihood that the district offers a Ph.D bonus to teachers. The juxtaposition of outcomes suggests that the influence of educators on school boards is distinct from the effects of school board experience.

5.5 Validity and Placebo Tests

Our research design rests on the assumption that the ballot order instrument is exogenous given the candidate pool. While this assumption cannot be directly tested, we present a number of validity checks and placebo tests in this section to support causal inference.

We first examine the relationship between the ballot order instrument and observed electoral characteristics. These results, presented in Table 13, show that an educator assigned to the top of the ballot is statistically unrelated to the number of candidates in the race or the number of

contested seats in either the specific electoral race or district. The last column of Table 13 presents a placebo test motivated by the evidence of treatment results: whether an educator is assigned to the top of the ballot should have no relationship to the share of active board members who are educators but are not up for re-election yet in this cycle. As expected, we find that the instrument does not change the makeup of board members not involved in the present election.

Table 13: Validity - Electoral Characteristics

	No. of Candidates in Contest	No. of Cont. Seats in Contest	No. of Cont. Seats on Board	Share of Board: Educators Elected Prior
	(1)	(2)	(3)	(4)
Top of Ballot	0.040	0.008	0.013	-0.009
Educator	(0.086)	(0.034)	(0.039)	(0.007)

Notes: Sample size is 4,840 electoral contests across all specifications. *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the district level. All specifications include a quadratic of the proportion of candidates who are educators, shares of the student population who are black, Hispanic, Asian, and eligible for free and reduced lunch, enrollment size quintiles, district type indicators, indicators for missing salary or test score outcomes for a given period, and separate election year and district fixed effects.

Additional validity checks examine the association between the instrument and the shares of other occupational and experience groups in the candidate pool and on the school board. These are presented in Table 13. We find that, conditional on the share of educators in the candidate pool, the instrument is unrelated to the shares of candidates who have a background in business or who are incumbents. This implicitly tests the possibility that non-education candidates may exit the contest in response to missing the first ballot order position. Moreover, the ballot order instrument is unrelated to the share of businesspeople and incumbents among active board members who are not up for re-election in the current cycle.

In addition to election attributes, we also examine the relationship between the ballot order instrument and characteristics of the school district. Table 15 reveals that the top of the ballot assignment among educators is not associated with public school enrollment, the proportion of students in a given racial or ethnicity group, or the share of students eligible for free and reduced price lunch. This lends additional credence to the assumption that the assignment process is random and not driven by any district-level attributes.

Finally, we perform placebo tests to estimate causal effects using the ballot order instrument in period 0, the year of the election, when we expect no association between the instrument and

Table 14: Validity - Cross-Group Characteristics

	Share of Candidates:		Share of Board: Elected Prior	
	Businesspeople	Incumbents	Businesspeople	Incumbents
	(1)	(2)	(3)	(4)
Top of Ballot	-0.010	-0.002	0.004	0.009
Educator	(0.009)	(0.011)	(0.004)	(0.009)

Notes: Sample size is 4,840 electoral contests across all specifications. *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the district level. All specifications include a quadratic of the proportion of candidates who are former educators, shares of the student population who are black, Hispanic, Asian, and eligible for free and reduced lunch, enrollment size quintiles, district type indicators, indicators for missing salary or test score outcomes for a given period, and separate election year and district fixed effects.

outcomes in this period. While our event study-inspired specifications produce insignificant effects for period 0, we also estimate reduced-form effects for just period 0 and report the results in Appendix Table A3. We find no statistical relationship with the treatment or instrument in period 0 across these outcomes, consistent with our research design.

Table 15: Validity - District Characteristics

	Total Enrollment	Share Black	Share Hispanic	Share Asian	Share FRPL
	(1)	(2)	(3)	(4)	(5)
Top of Ballot	26.11	0.001	-0.001	0.000	-0.001
Educator	(102.4)	(0.001)	(0.002)	(0.001)	(0.003)

Notes: Sample size is 4,737 electoral contests in column (1) and 4,739 for the remaining columns. *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the district level. All specifications include a quadratic of the proportion of candidates who are former educators or incumbents, the interactions between these shares, indicators for the number of available seats, candidates for each race, as well as the total number of available seats at the district level for a given year. All models furthermore include indicators for missing salary or test score outcomes for a given period, and election year and district fixed effects.

5.6 Interpretation

We consider the interpretation of our empirical findings in the context of the value of expertise and independence on governing boards for organizational performance. On the one hand, the investigation into the causal influence of educators on school boards was prompted by the possibility that educators may bring important human capital or expertise to school district leadership. Such expertise may translate into improved student learning at the district-level.

However, this motivation rests on an important implicit assumption: that the objectives of vot-

ers and of school board members are aligned in this goal. In this setting, the democratic process by which voters choose board members reinforces this potential alignment. As a large literature emphasizes, however, this alignment of interests may be distorted by the influence of pressure or interest groups (Becker, 1983; Rowley et al., 1988). In rent-seeking models, unions optimize outcomes for teachers by negotiating for better compensation and working conditions (Moe, 2009), potentially at the cost of lower education outcomes (Hoxby, 1996). If such a distortion were disproportionately applied to educators elected to the school board, it may compromise their independence and offset the value of educators' expertise in education production.

There are a number of reasons to believe that educators on the school board may be influenced by the interests of teachers' unions in California. In the first place, teachers' unions spend substantial resources on the election of preferred candidates in school board contests (Hess and Leal, 2005; Moe, 2006). In addition, union membership by teachers is nearly universal in California. Approximately 90% of teachers are full voting members of one of two main unions in the state, the California Teachers Association (CTA) or the California Federation of Teachers (CFT), suggesting that educators seeking election to the school board may also prioritize union interests.[31] Moreover, the pattern of our results – showing causal increases in teacher salaries and imprecise effects on test scores – is broadly consistent with prior evidence on the effects of teachers' unions (Cowen and Strunk, 2015). For instance, unionization is estimated to raise teacher salaries by 4-5% (e.g. Hoxby 1996; West and Mykerezi 2011), which is roughly equivalent to our estimate of the effect size of shifting from no educators to approximately half of the school board. Research has also shown that greater union strength predicts less support for legislation favoring the charter sector (Stoddard and Corcoran, 2007), suggesting that the shift away from charter schools into traditional public schools we estimate is consistent with union efforts to curb the growth of the charter schooling.[32]

To determine whether educators on school boards may be disproportionately aligned with union interests, we draw upon unique survey data on California school board members. Member responses to the survey allow us to examine the empirical relationship between professional

[31]The CTA and CFT are state affiliates of the National Education Association and the American Federation of Teachers, respectively. According to 2016 Labor Organization Annual Reports filed with the Department of Labor (Form LM-2), the CFT had 95,198 members and fee payers, of which 84,804 were members.

[32]Some studies, however, find evidence of negative effects of teachers' unions on student outcomes (e.g. Hoxby 1996; Lott and Kenny 2013; Lovenheim and Willen 2016).

experience as an educator and alignment with union priorities. Data from the 2006 California District School Board Survey contain responses from 567 school board members regarding their prior occupation and, importantly, any kinds of union support they received in their most recent election.[33]

We use the survey responses to evaluate the association between educators and alignment with union priorities in two ways. We first examine the association at the board member level of a background as an educator with receiving union endorsement. Column (1) in Table 16 reveals that former educators on the school board are over 40 percent more likely than board members from other professions to receive union endorsement. We also examine the association at the school board level. As shown in column (2), a 10 percentage point increase in the share of educators on the school board raises the share that is union endorsed by 2 points, which is 10% of the baseline level.

Table 16: Educators and Union Endorsement

	Union Endorsed:	
	Board Member	Share of Board
	(1)	(2)
Educator	0.106**	
	(0.050)	
Share of Board: Educators		0.222**
		(0.106)
Constant	0.247***	0.199***
	(0.024)	(0.027)
Observations	567	205

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the school board level. Both the individual and school board samples derive from the 2006 California District School Board Member Survey. 567 individuals spanning 205 unique school boards responded to questions on occupational background and union support. The survey asked individuals to choose from a set of occupational categories, such that Educator is defined as those who selected education.

The survey evidence thus indicates a strong positive association between professional experience in education and alignment with union priorities. This has two sets of implications for interpreting our findings. For one, it suggests that teachers' unions may distort the alignment of voters' and school board members' interests, compromising educators' independence. This in-

[33]These data are described in greater detail in Grissom (2007).

fluence, because it is applied to educators, may in turn reduce the effective value of educational expertise. The second implication is that our findings are consistent with educators on school boards shifting collective bargaining in favor of teachers' unions. This suggests that school boards are an important causal channel through which teachers' unions influence local education production. In this way, our findings complement existing descriptive evidence of union influence on school boards (e.g. Strunk and Grissom 2010; Strunk 2011) and evidence of teachers' unions' impacts that rely on policy changes and certification elections (e.g. Hoxby 1996; Lovenheim 2009).

6 Conclusion

A major focus in economics is identifying and quantifying the importance of various inputs to the production of human capital, which is key to facilitating growth, economic development, and social mobility (Becker, 1993). That focus has generally been placed on classrooms and schools, with a large literature that studies the effects of teacher quality in particular (Hanushek, 2006; Chetty et al., 2014). However, despite their significant district-level responsibilities such as collective bargaining and leadership recruitment and evaluation, little work to date has examined the role of locally-elected school boards in education production. We address this gap by studying the causal influence of professional educators elected to school boards on district inputs and education outcomes.

To pursue our analysis, we construct a unique dataset that allows us to empirically relate characteristics of California school board members to district-level inputs and outcomes. The key empirical challenge is that school board composition is endogenously determined through the electoral process. We therefore develop and implement a novel research design that exploits California's randomized assignment of the order that school board candidates appear on election ballots. The insight of our empirical strategy is that, due to ballot order effects, random assignment generates plausibly exogenous variation in the composition of the elected board. To implement this idea, we match the election results with the corresponding randomized ballot ordering from the California Secretary of State's office to show that the top-of-the-ballot advantage, when randomly conveyed on a candidate who is an educator, shifts the expected number of educators on the school board. This research design, which builds upon and extends the literature on ballot

order effects, allows us to provide causal evidence for how school boards influence district inputs and education outcomes.

The results demonstrate that, relative to other board members, educators on school boards causally increase teacher salaries. In our event-study results, we find that salaries trend upward post-election and are significant by the third year of treatment, consistent with 3-year collective bargaining cycles. We find that this salary increase generally applies across-the-board to all experience and education levels. We do not find effects on other teacher input variables, such as whether the district offers a Ph.D bonus. In addition, we find that educators on school boards reduce the share of students attending charter schools in the district. However, we find no evidence that they have a significant effects on either superindendent salary or student exam performance relative to other board members.

These findings suggest that educators' professional expertise on boards does not translate into meaningful improvements in student learning at the district-level. This may be because the objectives of educators that are elected to the school board may be misaligned with voters' due to the electoral influence of teachers' unions. In a rent-seeking framework (Hoxby, 1996; Moe, 2006), representation of such interests predicts higher teacher salaries, consistent with our results, and potentially negative effects on student performance. The latter may therefore offset any benefits of expertise from educators on the school board. To investigate this possibility, we draw upon California school board survey data to examine whether educator board members are more likely than members from other backgrounds to be endorsed by a teachers' union. The survey evidence indicates a strong positive association between professional experience in education and alignment with union priorities. Our findings are thus consistent with school boards as an important causal mechanism for the influence of teachers' unions on local education (Cowen and Strunk, 2015).

Our findings point to several avenues for future research. For one, estimates on educator representation yield the combined influence of educators' human capital and union-alignment on outcomes. A valuable next step would be to isolate the independent effect of expertise on education production. Our novel identification strategy can also be applied towards an examination of how gender, race and ethnicity, and political partisanship composition on school boards affects distributional and student outcomes. Related directions for future work include the political

economy elements of school board composition and the implications of electoral outcomes. Finally, we are constrained by data availability to only measuring learning via student test scores. Future work should therefore also focus on broader dimensions of skills and behavior, such as socioemotional attributes and civic engagement.

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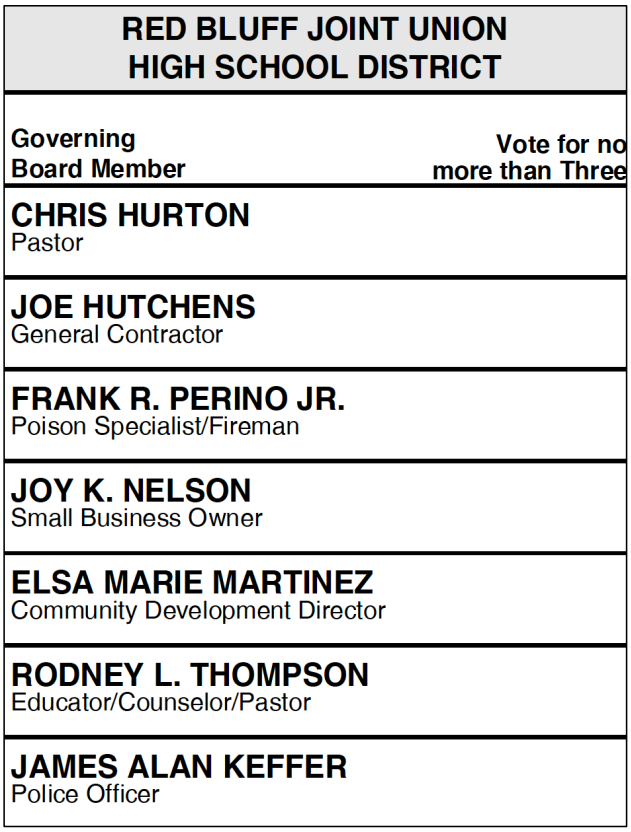
APPENDIX

Effective 7/01/2016 SAN MATEO-FOSTER CITY SCHOOL DISTRICT *FOR DISTRIBUTION*

CERTIFICATED SALARY
Teachers, Librarians, Nurses, Counselors
SCHEDULE 2016 - 2017 (186 days)

RANGE		I	II	III	IV
		BA 44 or less	BA + 45	BA + 60	BA + 75
STEP					
1		51,070	51,243	51,749	52,419
2		52,495	53,579	54,320	55,251
3		54,499	55,915	56,889	58,081
4		56,548	58,253	59,460	60,913
5		58,597	60,589	62,031	63,744
6		60,644	62,927	64,600	66,576
7		62,692	65,266	67,170	69,408
8		64,741	67,604	69,740	72,238
9		66,789	69,939	72,309	75,070
10		68,838	72,278	75,524	77,900
11		70,887	74,616	78,045	80,732
12		72,934	76,952	80,616	83,563
13		72,934	76,952	80,616	86,395
14		72,934	76,952	80,616	87,204
15		72,934	76,952	80,616	88,012
16		72,934	76,952	80,616	88,817
17		72,934	76,952	80,616	89,627
18		72,934	76,952	80,616	90,436
19		72,934	76,952	80,616	91,244
20		72,934	76,952	80,616	92,053
21		72,934	76,952	80,616	92,861
22+		72,934	76,952	80,616	96,042

Figure A1: Sample salary schedule



RED BLUFF JOINT UNION HIGH SCHOOL DISTRICT

Governing Board Member — **Vote for no more than Three**

CHRIS HURTON
Pastor

JOE HUTCHENS
General Contractor

FRANK R. PERINO JR.
Poison Specialist/Fireman

JOY K. NELSON
Small Business Owner

ELSA MARIE MARTINEZ
Community Development Director

RODNEY L. THOMPSON
Educator/Counselor/Pastor

JAMES ALAN KEFFER
Police Officer

Figure A2: Sample ballot from November 4, 2014 election

Table A1: Alphabet ordering: 1998 - 2015

Election date	Ordering																								
6/2/98	L	W	U	J	X	K	C	N	D	O	Q	A	P	T	Z	R	Y	F	E	V	B	H	G	I	M	S
11/3/98	W	K	D	N	V	A	G	P	Y	C	Z	I	S	T	L	J	X	Q	O	F	H	R	B	U	M	E
3/7/00	O	P	C	Y	I	H	X	Z	V	R	S	Q	E	K	L	G	D	W	J	U	T	M	B	F	A	N
11/7/00	I	T	F	G	J	S	W	R	N	M	K	U	Y	L	D	C	Q	A	H	X	O	E	B	V	P	Z
3/5/02	W	I	Z	C	O	M	A	Q	U	K	X	E	B	Y	N	P	T	R	L	V	S	J	H	D	F	G
11/5/02	H	M	V	P	E	B	Q	U	G	N	D	K	X	Z	J	A	W	Y	C	O	S	F	I	T	R	L
3/2/04	V	A	X	E	U	I	G	S	L	C	T	K	F	W	P	O	B	N	Y	R	Z	D	H	M	J	Q
11/2/04	J	M	Z	R	N	L	P	Q	O	H	I	G	X	D	F	K	E	S	C	W	T	U	A	B	V	Y
11/8/05	G	K	X	H	N	C	S	P	V	R	T	B	L	A	O	M	I	D	E	Z	J	F	Y	W	Q	U
6/6/06	Z	D	E	L	O	A	C	R	H	N	G	K	X	V	P	B	U	J	I	T	F	Q	Y	S	W	M
11/7/06	G	O	H	D	U	J	B	M	C	I	E	N	X	Z	W	R	L	Y	F	Q	A	P	T	S	K	V
11/6/07	D	F	X	K	Z	L	R	E	Q	T	U	B	S	I	P	J	N	V	H	W	O	G	A	Y	M	C
6/3/08	H	E	A	N	O	V	P	J	U	L	S	M	X	B	C	T	I	K	R	Q	D	Y	F	W	G	Z
11/4/08	R	X	M	W	S	J	L	H	A	Z	I	D	F	Y	G	V	C	K	N	E	O	P	U	Q	B	T
11/3/09	T	H	C	Z	O	G	I	A	P	W	K	F	D	R	Q	Y	L	N	J	V	E	U	B	S	M	X
6/8/10	Y	B	N	F	T	S	W	L	P	Z	V	X	Q	A	I	O	J	R	G	D	C	U	M	K	H	E
11/2/10	R	T	Y	C	W	O	K	G	B	E	J	V	L	F	S	P	Q	Z	N	M	I	A	U	X	D	H
11/8/11	F	Q	Y	K	O	C	H	U	T	G	B	I	S	A	V	W	E	X	L	Z	N	J	R	M	D	P
6/5/12	U	N	A	D	I	V	X	W	Q	G	O	Z	L	T	R	K	S	J	H	M	C	B	F	P	Y	E
11/6/12	I	X	C	A	O	U	Z	S	W	H	K	T	D	F	Q	V	G	M	R	J	L	Y	E	B	P	N
11/5/13	G	W	C	O	K	H	Z	A	T	S	V	Y	E	F	Q	U	D	N	M	X	B	I	R	P	L	J
6/3/14	R	O	Y	W	B	M	C	K	V	T	F	U	Q	P	I	H	D	A	J	N	E	X	G	S	Z	L
11/4/14	H	S	R	P	O	L	V	J	U	N	G	B	C	Q	A	M	D	E	X	Z	T	Y	W	F	K	I
11/3/15	J	Y	E	P	A	U	S	Q	B	H	T	R	K	N	L	X	F	D	O	G	M	W	I	Z	C	V

Notes: Randomized alphabets up to 2003 are corroborated using Ho and Imai (2008), while remaining alphabets come from the California Secretary of State's office.

Table A2: Ballot Order Effect Heterogeneity

	Win Contest		
	(1)	(2)	(3)
Top of Ballot	0.084***	0.083***	0.080***
	(0.011)	(0.011)	(0.011)
Top of Ballot × Educator			0.016
			(0.023)
Educator		0.160***	0.156***
		(0.015)	(0.017)
Mean		0.466	

Sample size is 20,331 candidate-electoral contest observations. *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the district level. Sample includes candidate observations with non-missing ballot order and district data in school board elections from 1998 - 2015. All specifications include separate election year and district fixed effects as well as a quadratic of the share of candidates who are former educators and incumbents, and their interactions, indicators for the number of open seats and candidates for each race, and the total number of open seats at the district level for a given year.

Table A3: Reduced-Form Placebo Estimates

	Service Days (1)	Ph.D Bonus Offered (2)	Log Max Health Benefit (3)	Pupils per Teacher (4)	Log Salary: BA+60, step 5 (5)
Top of Ballot Educator	−0.006	0.007	0.071	−1.864	0.002
	(0.104)	(0.013)	(0.061)	(1.886)	(0.002)
Observations	3,665	3,665	3,600	4,258	3,658
	Teacher Salary Share (6)	Log Superint. Salary (7)	Charter Share (8)	Math Scores (9)	Reading Scores (10)
Top of Ballot Educator	−0.001	0.008	−0.003	0.001	0.001
	(0.001)	(0.006)	(0.002)	(0.014)	(0.010)
Observations	5,174	3,531	5,316	3,994	4,243

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are clustered at the district level. The sample includes only outcomes occurring during election years ($\tau = 0$). Coefficients correspond to the ballot order instrument in the reduced form specification. Model covariates include a quadratic of the share of candidates who are incumbents, the interaction between the shares of educators and incumbents, indicators for the number of available seats, candidates for each race, and the total number of contested seats at the district level for a given year, the proportions of the board who are educators or incumbents and not up for election in the current cycle, shares of the student population who are black, Hispanic, Asian, and eligible for free and reduced lunch, district size quintiles, and district type indicators. Models furthermore include indicators for missing salary or test score outcomes for a given period, period and election year fixed effects, district type-specific trends, and district fixed effects.

Table A4: Reduced Form Estimates - Educators vs. Incumbents

	Service Days (1)	Ph.D Bonus Offered (2)	Log Max Health Benefit (3)	Pupils per Teacher (4)	Log Salary: BA+60, step 5 (5)
Top of Ballot Educator	−0.025	0.003	−0.031	4.543	0.003**
	(0.048)	(0.007)	(0.028)	(3.072)	(0.001)
Top of Ballot Incumbent	0.036	−0.010**	0.041	−4.593*	0.001
	(0.036)	(0.005)	(0.030)	(2.476)	(0.001)
Observations	28,423	28,423	27,915	26,680	28,362
	Teacher Salary Share (6)	Log Superint. Salary (7)	Charter Share (8)	Math Scores (9)	Reading Scores (10)
Top of Ballot Educator	0.001	0.001	−0.004**	0.005	−0.007
	(0.001)	(0.003)	(0.002)	(0.009)	(0.008)
Top of Ballot Incumbent	0.001	−0.001	0.001	0.006	0.010
	(0.001)	(0.003)	(0.002)	(0.007)	(0.007)
Observations	30,772	27,624	28,187	26,948	28,633

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are clustered at the district level. The sample is a stacked dataset including periods 0-6 that examines outcomes 1-6 years post-treatment. Coefficients correspond to the ballot order instrument in the reduced form specification. Model covariates include a quadratic of the share of candidates who are incumbents, the interaction between the shares of educators and incumbents, indicators for the number of available seats, candidates for each race, and the total number of contested seats at the district level for a given year, the proportions of the board who are educators or incumbents and not up for election in the current cycle, shares of the student population who are black, Hispanic, Asian, and eligible for free and reduced lunch, district size quintiles, and district type indicators. These covariates are separately interacted with an indicator for period 0 and a trend for post-treatment periods. Models furthermore include indicators for missing salary or test score outcomes for a given period, period and election year fixed effects, district type-specific trends, and district fixed effects.