Expertise and Independence on Governing Boards: Evidence from School Districts*

Ying Shi
Stanford University†

John D. Singleton
University of Rochester‡

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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 or experience 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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†Stanford Center for Education Policy Analysis; 520 Galvez Mall, 421 CERAS Building, Stanford University, Stanford, CA 94305 (Email: yingsh@stanford.edu; Phone: 281-687-0926)

‡Singleton: Department of Economics, 280 Hutchinson Road, Box 270156, Rochester, NY 14627 (Email: john.singleton@rochester.edu; Phone: 303-549-3983)

1 Introduction

Governing boards are a common feature of many organizations, from nonprofits to private corporations. Typically viewing boards through a principal-agent framework, a growing literature emphasizes the importance of board expertise and independence for organizational performance (John and Senbet, 1998; Adams et al., 2010). Board members' expertise can reduce information asymmetries, while independence may improve monitoring and overcome potential conflicts of interest within organizations.

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 elected to the local school board – who we define to include former classroom teachers, principals, superintendents, or other school administrators – have on education production. In the United States, school board members are typically chosen by voters in local democratic elections and the responsibilities of school boards 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 one hand, school board members with backgrounds in education may bring valuable human capital or experience 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. Such expertise has been shown to be empirically valuable in other settings: Faleye et al. (2018), for example, find that additional corporate board members with prior employment experience in the industry increase the firm's value.

At the same time, school board members may be influenced by pressure or interest groups through the electoral process, leading to a misalignment with voters' interests (Becker, 1983; Toma, 1986; Rowley et al., 1988). Specifically, the independence of educators on 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. Such rent-seeking may potentially be to the detriment of education outcomes, offsetting any value of educators' expertise in education production. 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 for every school district. Candidates for school board 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 candidates for school board appear on election ballots. A well-established empirical phenomenon is 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, for example, indicate that more educators on the school board are associated with higher teacher salaries, lower charter enrollment, and lower standardized test scores in the district. These relationships are largely accounted for by significant differences across school districts in size and student composition, however. 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 test scores and educators is robust to several observed control variables, these comparisons are likely to be confounded by reverse causality or remaining unobserved factors.

To estimate the causal influence of educators that are elected to the local school board, we thus implement our empirical strategy of relying on randomized ballot order. The results reveal that educators causally increase teacher salaries relative to other board members: A 10 percentage point increase in the share of educators on the board causes an approximately 1% increase in pay, an effect that persists across the experience distribution. In addition, the results suggest that educators on the school board also 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 student test scores in math or reading.

Our results suggest that, despite raising teacher salaries, the expertise of board members who are professional educators does not translate into improved student outcomes. 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.[1]

[1]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

Our paper contributes to a broad literature estimating the contribution of schooling inputs to student learning. This literature primarily focuses on inputs at the school and teacher, rather than the district, level (Rivkin et al., 2005; Hanushek, 2006; Chetty et al., 2014). The limited prior work on school boards is largely descriptive (Land, 2002), focusing on minority representation or conflict, whereas we examine causal effects on learning outcomes.[2] 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). 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 to a wider literature on governing boards. Previous studies find that board member expertise and independence, as defined by the willingness to monitor, are important inputs for organizational performance (John and Senbet, 1998; Adams et al., 2010). Our paper thus contributes to a diverse empirical literature that examines the structure and composition of boards from this perspective.[3] Our focus on public school districts additionally relates to recent work that studies political representation and local public good provision (Pande, 2003; Ferreira and Gyourko, 2009, 2014; Beach and Jones, 2016, 2017; Logan, 2018; Beach et al., 2018). A ubiquitous and well-recognized 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 being the first to develop an instrumental variables approach based on the ballot order effects literature (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. 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

(Hoxby, 1996; Lovenheim, 2009; Lott and Kenny, 2013; Lovenheim and Willen, 2016). See Cowen and Strunk (2015) for a recent survey of this literature.

[2]For example, Meier and England (1984) examines 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.

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

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. Members are typically elected in non-partisan elections and oversee a wide range of activities and responsibilities in local public school districts. We construct a unique dataset that combines information about school board members in California, where almost all members serve four-term 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, whereby voters – the principals – elect board members to monitor teachers and administrators.

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 or experience 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. Principals and superintendents usually combine classroom knowledge with management experience. On the school board, such expertise may translate into improvements in student learning at the district-level by reducing information asymmetries.[4] Alternatively, the independence of educators may be distorted through the political process by pressure or interest groups (Becker, 1983; Rowley et al., 1988). Specifically, the independence of

[4] A literature focused on corporate settings, for example, shows that expertise is valuable for firm performance (Wang et al., 2015; Meyerinck et al., 2016; Faleye et al., 2018). Related studies show that expertise, as measured by directors with CEO experience in the industry (Kang et al., 2018) and directors with experience in related industries (Dass et al., 2014), improve firm performance. Other work examines the financial or legal skills of board members (e.g. Xie et al. 2003).

educators on school board may be compromised by the influence of teachers' unions, who devote a substantial amount of resources toward the election of preferred school board candidates (Hess and Leal, 2005; Moe, 2006). 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. Their responsibilities include but are not limited to hiring and evaluating superintendents, negotiating teacher salaries, establishing budget priorities, and making decisions regarding student allocation.[5] 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.

An important input in our analysis is the working conditions of teachers. 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 seek improvements in working conditions involving pay, as stipulated by teacher salary schedules, or allocate district resources in a way they deem is especially beneficial for students.

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, for example, are the predominant authorizers of charter schools in California. With an active charter sector numbering 1,254 schools in 2016-2017, California relies heavily on boards as gatekeeper institutions for charter oversight.[6] Given a concern that charter schools may

[5]One of the main ways in which boards can impact district outcomes is through the selection and evaluation of superintendents. Since superintendents set achievement, budgetary, distributional, and related types of district priorities, their actions can meaningfully impact district outcomes. While this is an important area for research, presently we do not have the necessary superintendent data in California to conduct a thorough analysis.

[6]In almost all cases, charter petitions are submitted to local boards with appeals taken up by county boards of

generate significant fiscal impacts on the district (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 data 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 principal profession, vocation, or occupation to potential voters. These descriptions represent candidates' self-identified occupations, which by law correspond to professions during the year or immediately prior to the filing.[9] We categorize candidates' ballot designations into educators, businesspeople, or other professions. We identify educators as candidates who describe their primary occupation or profession as a teacher, educator, principal, superintendent, or school administrator. This excludes non-teaching employees working in education or those employed in postsecondary education. Business candidates are identified as those who self-describe as an

education or the State Board of Education. 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] California legislation stipulates that the designation must describe either the candidate's current profession or the profession the candidate held during the calendar year immediately preceding the candidate's filing. 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.

"executive," "businessman," "businesswoman," or "president." The category also includes chief financial officers and self-employed individuals.[10]

We then construct an annual panel of elected school board rosters using the election records. As contests for board seat are staggered, each board is comprised 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 the board-level characteristics 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 variables enter as control variables in the analysis. The Common Core of Data identifies charter schools in 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.[12]

In addition, we source information about teacher salary schedules from the annual Form J-90. These data provide the most comprehensive salary and benefit information in California in the traditional column and step format. Salary schedules enable us to determine compensation levels at a given level of education (column or lane) as well as by years of experience (step). To ensure comparability of teacher salaries across districts, we focus on specific education and experience combinations such as attaining a Bachelor's degree with 60 credit hours and 5 years of experience.[13] Our use of teacher salary schedules, as opposed to district summaries of expen-

[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 (Appendix Table A2). As such we can treat these categories as largely mutually exclusive

[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.

[12]Our panel documents charter school status beginning in 1998, the first year in which the National Center for Education Statistics began identifying charter schools in Common Core of Data nonfiscal surveys.

[13]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.

diture or teacher salaries, to measure teacher pay has several advantages, principally including 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.

Finally, we obtain standardized test score summaries from the California Department of Education as a measure of student outcomes. We 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 include normalize performance by year and grade (across all students in California) but use only test scores in traditional public schools (and therefore exclude charter schools) to construct 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, 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 17,974 unique individuals at the time they are observed in our sample of California school board elections. Half of this sample won an election at least once between 1996 and 2015. 16% of the candidate pool describes their primary vocation as an educator. Among those who ever won an election, educators comprise 18%, which is consistent with previous descriptive evidence on the occupational backgrounds of school board members.[14]

[14]The 18% is almost the same as the 17% estimated using occupational data provided by the 2006 California District School Board Member Survey covering 222 California school districts (Grissom, 2007). This share is lower than the 27% reported in a national survey of school board members (Hess and Meeks, 2010), though education is defined more broadly than only teachers and educators in that instance.

14% and 12% of candidates and election winners work in business, respectively. The limited range of keywords used to define businesspeople imply that these are likely underestimates of their true prevalence.[15] At the time we first observe these candidates, one-fifth were incumbents. This share increases to one-third among winning candidates. Finally, winners serve an average of 6.7 years as board members. This is somewhat lower than the mean of 2.2 terms or nearly 9 years reported in survey data (Grissom, 2007). Ou rnumbers are likely a lower bound because we do not observe elections prior to 1996 and those serving terms following uncontested elections.

Table 1: School Board Candidates

	All candidates	Winners
Educator	0.16	0.18
Businessperson	0.14	0.12
Incumbent	0.22	0.34
Ever won an election	0.50	1.00
Tenure (years)	3.47	6.67
Observations	17974	8965

Notes: Sample includes unique candidates and their characteristics when first observed in school board elections from 1996 - 2015. Winners refer to candidates who have ever won a school board election. Candidates who never won an election have 0 years of tenure.

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 19%, 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-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 also summarizes the dependent variables that we examine in our analysis. For teach-

[15]The latter estimate is substantially lower than the 23% reported in the 2006 California District School Board Member Survey (Grissom, 2007).

Table 2: Board-Level Characteristics

	Mean	Std. Dev.	25p	75p
Board size and composition:				
Number of Members	4.86	1.59	4	6
Share of Board: Educators	0.19	0.21	0.00	0.29
Share of Board: Businesspeople	0.12	0.17	0.00	0.20
Share of Board: Incumbents	0.58	0.28	0.40	0.80
Student enrollment:				
Total Enrollment	8805	20044	1174	10247
Share Black	0.04	0.06	0.01	0.05
Share Hispanic	0.43	0.28	0.18	0.66
Share Asian	0.08	0.11	0.02	0.10
Share FRL	0.41	0.24	0.22	0.60
Educational outcomes:				
Salary: Step 5	51406	6878	46551	55780
Salary: Step 25	70835	10878	63313	78218
Charter Enrollment Share	0.04	0.12	0.00	0.01
Standardized Math Scores	0.05	0.78	-0.50	0.51
Standardized Reading Scores	0.12	0.82	-0.47	0.65

Notes: School boards are defined as district-year combinations. All panels provide characteristics and outcomes in school board election years (i.e. period 0). The sample size is 3,672 for the top two panels. Salary measures are given for teachers with a BA and 60 credit hours, with years of experience corresponding to steps in the traditional column and step schedule. The sample sizes for teacher salary outcomes range from 2,867 - 2,874, and 3,120-3,293 for reading and math scores.

ers with a Bachelor's degree and 60 additional credit hours, their average salary increases from $51,000 to $71,000 as they advance from 5 to 25 years of experience. In addition to negotiating over teacher salary schedules and working conditions, the board responsibility also extends into domains such as the authorization of charter schools.[16] Charters take up 4% of overall student enrollment in the district. To examine how these district-level inputs subsequently influence productivity, we focus on student achievement in math and reading as outcomes of interest. 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

The summary statistics highlighted in the preceeding section indicate substantial variation across districts in composition of the school board, characteristics, and education 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 outcome for school board $j-t$ representing school district j at election year t, while τ indexes periods relative to t. For this analysis, we pool the two immediate years following the election year t during which no subsequent elections are held in the estimation sample (i.e. $\tau \in \{1, 2\}$). As T_{jt} is the share of educators on school board $j-t$, β represents the coefficient of interest in equation (1). 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 and therefore prior to any actions or interventions by the $j-t$ school board.

Table 3 presents results from estimating equation (1). We estimate two specifications for each

[16] In California, local school districts, county offices of education, and the State Board of Education are the only entities able to authorize charter schools. Upon approval, charter organizations must reapply to renew their charters once every five years.

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. The second specification includes district covariates Θ_{jt0} to estimate conditional associations. Results in the upper panel of Table 3 reveal that more educators on the school board is correlated with higher salaries, particularly for more experienced teachers. The share of educators does not appear to be statistically related with charter enrollment. School districts with boards with more educators also show substantially lower student performance on statewide exams.

Table 3: OLS - Effect of Board Composition

	Log Salary:		Charter Enrollment	Test Scores:	
	Step 5	Step 25	Share	Math	Reading
	(1)	(2)	(3)	(4)	(5)
Share of Board: Educators	0.0471^{***}	0.0732^{***}	-0.0207^{**}	-0.2890^{***}	-0.2980^{***}
	(0.0093)	(0.0112)	(0.0096)	(0.0596)	(0.0609)
Observations	6,109	6,124	6,811	6,458	6,808
Share of Board: Educators With Controls	-0.0090	-0.0045	-0.0119	-0.1460^{***}	-0.1010^{***}
	(0.0075)	(0.0095)	(0.0102)	(0.0360)	(0.0309)
Observations	6,109	6,124	6,811	6,458	6,808

Notes: All models regress outcomes for the first two years of treatment on the share of the board that are educators. All specifications include period indicators and election year fixed effects, and cluster robust standard errors at the school board level. Additional covariates include the shares of the student population who are black, Hispanic, Asian, and eligible for free and reduced lunch, district size quintiles, and district type. *** p<0.01, ** p<0.05, * p<0.1

The bottom panel of Table 3 then controls for observable differences across districts in size and student composition. The results reveal a number of interesting patterns. For one, while the raw correlations suggest that educators on the school board may pay teachers more, the reversed signs and insignificant coefficients indicate that this relationship does not hold when additional covariates are considered. The association between educators on the board and lower student performance is robust despite becoming smaller in magnitude when controls are added. The point estimate corresponds to a substantial 0.015σ decrease in math and 0.010σ decrease in reading test scores associated with a 10 percentage point increase in the share of educators.

Taken on their face, these results appear inconsistent with the view that expertise may improve student outcomes, as more educators are associated with lower student performance. With controls, the apparent reduction in student performance is not association with any difference

in teacher pay, so the results are also ambiguous regarding the role of rent-seeking by teachers' unions, who are predicted to raise salaries. However, it is important not to infer causal relationships from these empirical associations. While the specification in the second row controls for some observable differences across school districts, the estimates are likely confounded by reverse causality or omitted variables leading school board composition to form endogenously. Student performance and educators may be inversely related, for example, because voters may respond to lower test scores by disproportionately electing professionals with education experience to the board. Similarly, where teacher salaries are low, teachers may organize through the union to elect sympathetic candidates to the school board, which may plausibly include former teachers.

Table 4: Board Composition and District Characteristics

	Total Enrollment	Share White	Share Black/Hisp	Share FRL
Share of Board: Educators	17954***	−0.136***	0.119***	0.083***
	(2593)	(0.0192)	(0.0193)	(0.0166)

Notes: The sample includes 4,830 school board elections. District enrollment and composition variables are regressed on the share of the board that are educators. All models include county and election year fixed effects. All standard errors are clustered at the board level. *** p<0.01, ** p<0.05, * p<0.1

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 3. Table 4 shows strong associations between educator representation and markers of disadvantage. School boards with more educators also 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 test score outcomes. We therefore develop and implement a novel research design to estimate the causal effects of board composition.

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 causes a boost in the probability of a candidate winning the election (Koppell and Steen, 2004; Ho and Imai, 2008; Meredith and Salant, 2013; Pasek et al., 2014).[17] 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 costs. Accordingly, ballot 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.[18] The probability that candidate i in school board election contest r in district j wins the contest can be expressed as:

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

[17] 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."

[18] For ease of exposition, we characterize ballot order effects as simply a top of the ballot advantage.

where $First_{jri}$ is an indicator for whether candidate i is listed at the top of the ballot in the contest. i indexes candidates in contest r. $Educator_{jri}$ is an indicator variable for whether i is a professional educator while ϵ_{jri} contains all other variables that determine electoral success.[19] 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.[20] 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 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, $\bar{\epsilon}_{jr}$.[21]

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

[19] Note that we do not assume that this ϵ is uncorrelated with being an educator.

[20] Recall that, in general, school board contests may have multiple winners.

[21] 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 $\epsilon_{jri} Educator_{jri}$.

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.[22] 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.[23]

We compile a database of randomized alphabet drawings for elections from 1996-2015 from press releases from the California Secretary of State's office. The full list of randomized alphabets is shown in Table A1.[24] We determine the ballot order by matching election dates and applying the alphabet throughout candidates' last and first names. Using multiple sample ballots that we collected online, we then verify that the order coincides with actual candidate ordering on the election ballot.

4.3 Empirical Specification

We are interested in the treatment effects of school board composition on district-level inpouts 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 and T_{jt} is the share of educators on the school board. β represents the treatment effect of interest. 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

[22] 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.

[23] 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.

[24] Alphabet orderings up to 2003 are corroborated using Ho and Imai (2008).

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 a set of 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):

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

This specification, in which district input and education outcome variables are directly related to our instrument, thus 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 $\tau = 0$ (i.e. the year of the election) in the estimation sample for each school board:

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

The causal effect as represented by π turns "on" for post-election periods, 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 treatment effects via two-stage least squares using the ballot order instrument for interpretive purposes. Our base model is just identified with the share of educators on the board as the only endogenous regressor. Yet there are reasons to believe that ballot order effects depend on electoral context. We therefore use additional variation from electoral records: the total number of seats contested for each school board, which we interact with our instrument. The

[25] We cluster standard errors at the board level for all of the estimates for this reason.

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

intuition is that the first candidate advantage may be amplified when there are more contested elections, signaling a more competitive cycle for the board. We estimate an overidentified model that takes advantage of this first-stage heterogeneity in addition to the just identified specification that uses only the ballot order instrument.

The validity of our research design rests on the assumption that our instruments are valid given the candidate pool composition embedded in W_{jt0r}: $E[u_{jt\tau}, FirstEducator_{jtr}|\overline{Educator}_{jtr}] = 0$. 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 in years *prior* to the election.

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.

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 5 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. We examine two outcomes: the candidate's vote share and whether they won the contest. Column (1), which controls for district and year fixed effects, reveals that candidates randomly listed at the top of the ballot gain 5.5 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 the exact number of candidates, open seats, and other electoral attributes. Translated into winning probability, the base and augmented models show a 10.5 and 8.1 percentage point first candidate advantage (columns (3) and (4)) relative to all other ballot order positions, respectively.

Table 5: Estimates of Ballot Order Effects

	Vote Share		Winner	
	(1)	(2)	(3)	(4)
Top of Ballot	0.055***	0.018***	0.105***	0.081***
	(0.002)	(0.002)	(0.010)	(0.010)
Controls	N	Y	N	Y

Notes: Sample includes candidate observations with non-missing ballot order and district data in school board elections from 1996 - 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. Standard errors are clustered at the school board level. Sample size is 22,189 across all specifications. *** p<0.01, ** p<0.05, * p<0.1

The ballot order effects we estimate are broadly consistent with results from the prior literature. Studies 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.

To examine its implications for the composition of the elected school board, we next apply the ballot order advantage to candidates who are educators. To do this, we estimate first-stage equation (5) for two measures of composition: 1) the share of contested seats on the board obtained 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 three 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. The third overidentified model includes the interaction between

the instrument and the total number of contested seats in the first stage.

Table 6: Evidence of Treatment

	Share of Cont. Seats: Educators			Share of Board: Educators		
	(1)	(2)	(3)	(4)	(5)	(6)
Top of Ballot Educator	0.071***	0.067***	0.121***	0.022***	0.029***	0.096***
	(0.015)	(0.015)	(0.044)	(0.008)	(0.007)	(0.025)
Top of Ballot Educator X			-0.020			-0.025***
Total No. of Cont. Seats			(0.015)			(0.008)
Controls	N	Y	Y	N	Y	Y
District FE	Y	Y	Y	Y	Y	Y
F-statistics	21.44	19.88	10.43	7.07	16.54	10.48

Notes: 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, and 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 type indicators. All models furthermore include election year and district fixed effects. Standard errors are clustered at the school board level. Sample size is 4,830 across all specifications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6 provides evidence that the ballot order instrument causally shifts the share of educators on the school board. Having an educator at the top of the ballot increases the share of educators among winners for contested seats by 6.7 percentage points after controlling for election- and district-level attributes. A more intuitive interpretation uses the full school board inclusive of members whose terms are still ongoing. In this case, a first-listed educator increases the proportion of educators by 2.9 percentage points. A negative coefficient on the interaction term in column (6) suggests that the increase in educator share induced by the instrument is larger when there are fewer contested seats. Each winner takes up a larger share of the board by construction.

Table 6 also reports F-statistics for each dependent variable ("treatment") and corresponding instruments. For our preferred specification in column (5) that instruments for the share of educators in the school board, 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). When we rely on variation in the number of contested seats, the associated F-statistic is over 10. We estimate treatment effects via two-stage least squares for both the just-identified and overidentified models.

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 reduced form estimates before turning to two-stage least squares estimates of treatment effects.

5.2.1 Reduced-Form Estimates

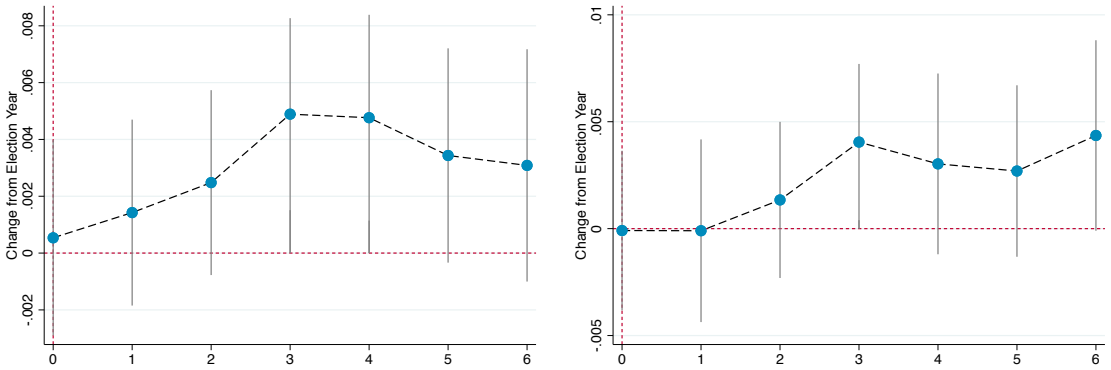
We first examine the reduced-form effects of our top of the ballot instrument through an "event study"-style specification that estimates the causal effect for each period τ. To do this, we adapt equation (7) as:

$$Y_{jt\tau} = \pi_\tau FirstEducator_{jtr} + \kappa W_{jt0r} \times \mathbf{1}(\tau > 0) + \theta_j + u_{jtr} \tag{8}$$

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 in turn enables a time profile of reduced-form causal effects. We estimate π_0 as part of the event analysis, which serves as a placebo test because the ballot order instrument should have no relationship with the outcome variables in the year of the election. We include $\tau = 0, ..., 6$ in the estimation sample for each school board and estimate equation (8) for each dependent variable. The 6-year post-treatment window provides sufficient time for changes to take effect. In particular, collective bargaining agreements are negotiated at least once every three years and some teacher salary effects may only be realized after the current window. Input changes may similarly take time to manifest in test scores.

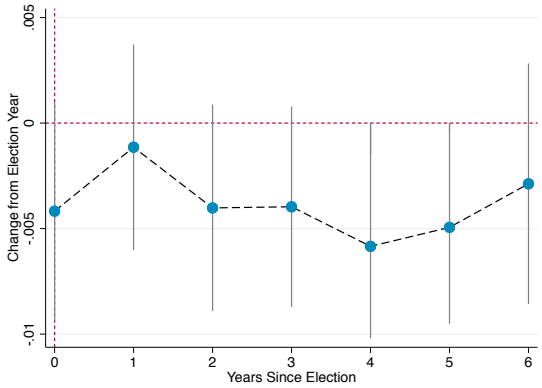
The results plotted in Figure 1 suggest significant causal effects of ballot order instrument on a number of outcomes. For instance, panel (a) reveals that whether an educator is listed at the top of the ballot causally raises salaries for teachers with 5 years of experience, with effects increasing over time. Panel (b) reveals a similar pattern for teachers with 25 years of experience. The ballot order instrument also appears to reduce the share of students enrolled in charter schools in district, as shown in panel (c). On the other hand, the figure indicates little effects on student test scores. Finally, across all specifications we find no evidence of an effect in period 0, the year of the election, consistent with the assumptions of our empirical strategy.

To assess the overall statistical significance of the causal impacts, we estimate pooled, constant

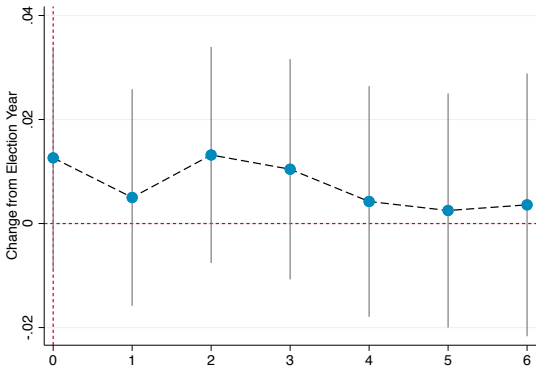


(a) Log Salary at Step 5

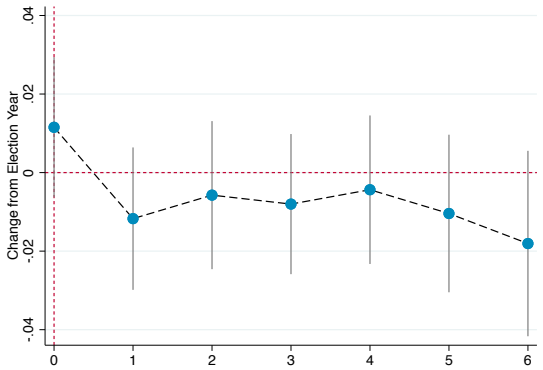
(b) Log Salary at Step 25



(c) Charter Enrollment Share



(d) Math Scores



(e) Reading Scores

Figure 1: Reduced-Form Effects by Years Elapsed

Note: The sample includes periods 0-6 for each school board. Coefficients correspond to interactions between the instrument and the number of elapsed years. In the model, election covariates (quadratic of the share of candidates who are former educators and incumbents, their interactions, the number of available seats and candidates for each race, and the total number of available seats at the district level for a given year) are interacted with a treatment indicator for years 1-6, while district covariates (shares of the student population who are black, Hispanic, Asian, and eligible for free and reduced lunch, district size quintiles, and district type dummies) flexibly interact with indicators for years elapsed. The model also includes separate election year and district fixed effects. Robust standard errors are clustered at the school board level.

reduced-form effects following (7). We estimate this equation separately for the $\tau = 0, ..., 2$ and $\tau = 0, ..., 6$ windows. An advantage of limiting to two years is that variation in outcomes during this condensed window can only be traced back to shifts in initial board composition, not to subsequent electoral outcomes. We supplement these results by extending treatment to six years, which cover multiple cycles in a staggered electoral system. Findings are interpreted as inclusive of both the direct impact of educator share on outcomes of interest and the indirect impact on future electoral outcomes. This longer view accommodates impacts taking longer to realize. Consistent with this motivation is that some of the effects appear to grow over time in the event study results.

Table 7 reports the pooled reduced-form results by post-treatment window. In the 2 years immediately after the election, the first candidate advantage increases the salaries of teachers with 5 years of experience. While the coefficient maintains the same size for those with 25 years of experience, it is insignificant due to slightly inflated standard errors. When we extend the window to 6 years post-treatment, we find significant and similarly sized reduced-form increases in salaries across the experience distribution. In addition to higher salaries, the ballot order instrument also lowers the share of students in the district who are enrolled in charter schools during the 6 years after the initial election. We do not find any corresponding changes in math or reading scores.

5.2.2 Treatment Effect Estimates

To translate our reduced-form results into the causal effect of additional educators on the school board, we estimate two-stage least squares specifications analogous to the reduced-form specification (7):

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

This specification includes district fixed effects, $\tilde{\theta_j}$, and relies on within-district variation in T_{jt} induced by our instrument for identification. For each outcome variable, we present results from a just-identified model that only uses the top of the ballot instrument for identification and a specification that exploits first-stage heterogeneity via interactions with the number of contested seats on the school board. As with the pooled reduced-form results, we estimate results for both the two and six year post-treatment windows.

The upper panel of Table 8 presents the two-stage least squares results for the immediate

Table 7: Reduced-Form Estimates

	Log Salary:		Charter Enrollment	Test Scores:	
	Step 5	Step 25	Share	Math	Reading
	(1)	(2)	(3)	(4)	(5)
Years 1-2 Post-Treatment:					
Top of Ballot	0.0024*	0.0023	−0.0027	0.0074	−0.0109
Educator	(0.0015)	(0.0018)	(0.0023)	(0.0106)	(0.0097)
Observations	11,800	11,823	13,789	12,535	13,289
Years 1-6 Post-Treatment:					
Top of Ballot	0.0034***	0.0025**	−0.0036**	0.0070	−0.0092
Educator	(0.0011)	(0.0012)	(0.0017)	(0.0084)	(0.0078)
Observations	26,089	26,144	28,187	26,948	28,633

Notes: The top panel is a stacked dataset that examines outcomes 1-2 years post-treatment, while the bottom panel examines outcomes 1-6 years post-treatment. All samples include period 0, and covariates are interacted with a treatment indicator for year 1 onwards. The table reports coefficients corresponding to the ballot order instrument in the reduced form specification. Log salary outcomes are for teachers with a BA degree and equivalent of 60 credit hours earned. All specifications include a quadratic of the share of candidates who are former educators and incumbents, and their interactions, 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, and 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 dummies. All models furthermore include election year and district fixed effects. Robust standard errors are clustered at the school board level. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Treatment Effect Estimates

	Log Salary:		Charter Enrollment	Test Scores:	
	Step 5	Step 25	Share	Math	Reading
	(1)	(2)	(3)	(4)	(5)
Years 1-2 Post-Treatment:					
Just-identified	0.0869	0.0827	−0.0995	0.2950	−0.4020
	(0.0561)	(0.0659)	(0.0877)	(0.4300)	(0.3730)
Overidentified	0.0904**	0.1110**	−0.0282	−0.2020	−0.3980
	(0.0387)	(0.0499)	(0.0679)	(0.3340)	(0.2800)
Observations	11,800	11,823	13,789	12,535	13,289
Years 1-6 Post-Treatment:					
Just-identified	0.1210**	0.0905*	−0.1260*	0.2640	−0.3310
	(0.0481)	(0.0480)	(0.0659)	(0.3190)	(0.2870)
Overidentified	0.0882***	0.0624*	−0.0433	−0.0171	−0.3790
	(0.0336)	(0.0341)	(0.0488)	(0.2410)	(0.2330)
Observations	26,089	26,144	28,187	26,948	28,633

Notes: The endogenous variable of interest is the share of educators on the school board. The top panel refers to years 1-2 during the window after treatment, while the bottom panel refers to years 1-6. All covariates are interacted with a treatment indicator for year 1 onwards. The just-identified model only uses the top of ballot instrument, while over-identified model instruments for school board composition using both the ballot order instrument and its interaction with the total number of contested seats. All specifications include a quadratic of the share of candidates who are former educators and incumbents, their interactions, 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, and the proportions of the board who are businesspeople 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 dummies. All models furthermore include election year and district fixed effects. Robust standard errors are clustered at the school board level. *** p<0.01, ** p<0.05, * p<0.1

two years following the election. The just-identified estimates suggest a positive causal effect on teacher salaries, consistent with the reduced form results. However, these effects are estimated imprecisely. The additional power from the interactions reduces the variance, indicating positive and statistically significant effects on teacher pay. The point estimates for 5 and 25 years of experience indicate that a 10 percentage point increase in the share of educators on the board raises teacher salaries by about 1%.[27] The two year post-election results cannot reject zero impacts on charter enrollment or test scores.

The bottom panel of Table 8 extends the period to six years post-election to allow for a longer horizon for impacts to be realized. These results are largely consistent with the shorter window, including causal effects on teacher salaries of similar magnitude as before. There are no significant effects on math or reading scores in the longer term, but the results suggest a longer-run impact on charter enrollment. The point estimate of the just identified model 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, though this estimate is significant at only the 90% confidence level.

5.3 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. In this section, we further investigate the category of non-educators to understand how the impact of educators compares to other groups. Doing so informs an understanding of how education expertise on school boards translates to district outcomes in ways that are distinct from or similar to other kinds of experience. We focus on incumbents because they comprise a larger share of the sample relative to other categories of candidates that we can construct using occupational data from the ballot designation. We examine whether incumbents' on-the-job human capital translates to measurable shifts in outcomes.

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 the prior findings. In contrast to the influence of educators on salaries and charter enrollment, however, having incumbents in the

[27] In results not shown here, we verify that this result persists across educational and experience levels.

first ballot order position does not statistically affect any of the five outcomes in either the 2-year or 6-year pooled samples. The juxtaposition of outcomes suggests that the influence of educators on school boards is thus distinct from effects of incumbent board members.

5.4 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 9, 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 9 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 9: 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 Educator	0.042	0.009	0.014	-0.009
	(0.088)	(0.035)	(0.039)	(0.006)
R-squared	0.508	0.617	0.452	0.443

Notes: 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, and separate election year and district fixed effects. Standard errors are clustered at the school board level. There are 4,830 observations in the sample. *** p<0.01, ** p<0.05, * p<0.1

A further validity check regresses the instrument on the shares of other occupational and experience groups in the candidate pool and school board. 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.

Table 10: 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)	(0.012)	(0.005)	(0.010)
R-squared	0.322	0.285	0.381	0.386

Notes: 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, and separate election year and district fixed effects. Standard errors are clustered at the school board level. There are 4,830 observations in the sample. *** p<0.01, ** p<0.05, * p<0.1

In addition to election attributes, we also examine the relationship between the ballot order instrument and district characteristics. Table 11 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 credence to the assumption that the assignment process is random and not driven by any district-level attributes.

Table 11: Validity: District Characteristics

	Total Enrollment	Share Black	Share Hispanic	Share Asian	Share FRPL
	(1)	(2)	(3)	(4)	(5)
Top of Ballot	23.62	0.001	-0.001	0.000	-0.001
Educator	(83.68)	(0.001)	(0.002)	(0.001)	(0.003)
R-squared	0.997	0.955	0.984	0.980	0.944

Notes: 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 include election year and district fixed effects. Standard errors are clustered at the school board level. There are 4,737 observations for column 1 and 4,739 observations for the remaining columns. *** p<0.01, ** p<0.05, * p<0.1

Finally, we perform placebo tests to estimate causal effects using the ballot order instrument in period 0, the year of the election. As the treatment has not had time to take effect, we expect no

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

5.5 Interpretation

In this section, 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 industry-specific human capital and 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 voters and of school board members are aligned in this goal. In this setting, this potential alignment is enforced through the democratic process in which board members are chosen by voters to represent their interests. 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 thus compromise their independence, potentially offsetting 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), sug-

gesting that educators seeking election to the school board may also prioritize union interests.[28] 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.[29]

To determine whether educators on school boards may be disproportionately aligned with union interests, we therefore draw upon unique survey data on California school board members. The responses of members to the survey allow us to examine the empirical relationship between being a former educator and alignment with union priorities. Data from the 2006 California District School Board Survey contains responses from 567 school board members regarding their prior occupation and, importantly, any kinds of union support they received in their most recent election.[30]

We use the survey responses to evaluate the association between educators on the school board 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 12 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.

The survey evidence thus indicates a strong positive association between professional expe-

[28] 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.

[29] 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).

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

Table 12: 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: 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. Standard errors are clustered at the school board level. *** p<0.01, ** p<0.05, * p<0.1

rience 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 influence, 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 (Land, 2002; Honingh et al., 2018). 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 (Koppell and Steen, 2004; Ho and Imai, 2008; Meredith and Salant, 2013), allows us to provide causal evidence for how school boards influence district inputs and education outcomes.

The results demonstrate that educators on school boards causally increase teacher salaries across the experience distribution relative to other board members. We find that salary increases maintain an upward trend post-election and are usually significant by the third year, consistent with 3-year collective bargaining cycles. In addition, we find that educators on school boards reduce the share of students in charter schools in the district. However, we find no evidence that they have a significant effect on either student math or reading scores relative to other board members.

These findings suggest that educators' professional expertise on boards does not translate to 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, our approach estimates the net effects of expertise, as embodied in educators' human capital, and union-alignment on district outcomes. A valuable next step would be to isolate the independent influence of expertise on education production. In addition, our novel identification strategy may be extended to examine the roles of gender, race and ethnicity, political partisanship, and other dimensions of school boards on distributional and student outcomes. Related directions for future work include the political economy elements of school board composition and the implications of electoral changes for follow-on elections. 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

Table A1: Alphabet ordering: 1996 - 2015

Election date	Ordering																								
3/26/96	G	E	F	C	Y	P	D	B	Z	I	V	A	U	S	M	L	H	K	N	T	O	J	Q	R	X	W
11/5/96	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
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: Summary Statistics - School Board Candidates

	Education	Business	Incumbent
All candidates			
Educator	1.00	0.07	0.05
Businessperson	0.07	1.00	0.04
Incumbent	0.06	0.06	1.00
Observations	2796	2450	3877
Candidates who ever won			
Educator	1.00	0.10	0.05
Businessperson	0.07	1.00	0.04
Incumbent	0.10	0.11	1.00
Observations	1627	1077	3059

Notes: Sample includes unique candidates and their characteristics when first observed in school board elections from 1996 - 2015.

Table A3: Reduced-Form Placebo Estimates

	Log Salary:		Charter Enrollment	Test Scores:	
	Step 5	Step 25	Share	Math	Reading
	(1)	(2)	(3)	(4)	(5)
Top of Ballot Educator	0.0021	0.0023	−0.0022	0.0015	0.0001
	(0.0016)	(0.0019)	(0.0024)	(0.0134)	(0.0104)
Observations	3,658	3,665	4,737	3,994	4,242

Notes: The sample examines only outcomes occurring during the same year as school board elections. Point estimates correspond to the ballot order instrument in the reduced form specification Log salary outcomes are for teachers with a BA degree and equivalent of 60 credit hours earned. All specifications include a quadratic of the share of candidates who are former educators and incumbents, and their interactions, the number of available seats and candidates for each race, the total number of available seats at the district level for a given year, shares of the student population who are black, Hispanic, Asian, and eligible for free and reduced lunch, district size quintiles, and district type dummies. All models furthermore include election year and district fixed effects. Robust standard errors are clustered at the school board level. *** p<0.01, ** p<0.05, * p<0.1

Table A4: Reduced Form Estimates - Educators vs. Incumbents

	Log Salary:		Charter Enrollment	Test Scores:	
	Step 5	Step 25	Share	Math	Reading
	(1)	(2)	(3)	(4)	(5)
Years 1-2 Post-Treatment:					
Top of Ballot Educator	0.0025*	0.0023	−0.0028	0.0088	−0.0098
	(0.0015)	(0.0018)	(0.0023)	(0.0107)	(0.0098)
Top of Ballot Incumbent	0.0009	−0.0006	−0.0005	0.0116	0.0101
	(0.0012)	(0.0013)	(0.0021)	(0.0088)	(0.0073)
Observations	11,800	11,823	13,789	12,535	13,289
Years 1-6 Post-Treatment:					
Top of Ballot Educator	0.0034***	0.0025**	−0.0035**	0.0076	−0.0082
	(0.0011)	(0.0012)	(0.0017)	(0.0085)	(0.0078)
Top of Ballot Incumbent	0.0010	0.0004	0.0008	0.0049	0.0092
	(0.0009)	(0.0010)	(0.0018)	(0.0065)	(0.0058)
Observations	26,089	26,144	28,187	26,948	28,633

Notes: The top panel is a stacked dataset that examines outcomes 1-2 years post-treatment, while the bottom panel examines outcomes 1-6 years post-treatment. All samples include period 0, and covariates are interacted with a treatment indicator for year 1 onwards. The table reports coefficients corresponding to the ballot order instrument in the reduced form specification. Log salary outcomes are for teachers with a BA degree and equivalent of 60 credit hours earned. All specifications include a quadratic of the share of candidates who are former educators and incumbents, and their interactions, 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, and 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 dummies. All models furthermore include election year and district fixed effects. Robust standard errors are clustered at the school board level. *** p<0.01, ** p<0.05, * p<0.1