School Boards and Education Production: Evidence from Randomized Ballot Order

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We examine the causal influence of educators elected to the school board on local education production. The key empirical challenge is that school board composition is endogenously determined through the electoral process. To overcome this, we develop a novel research design that leverages California's randomized assignment of the order that candidate names appear on election ballots. We find that an additional educator elected to the school board reduces charter schooling and increases teacher salaries in the school district relative to other board members. We interpret these findings as consistent with educator board members shifting bargaining in favor of teachers' unions.

Governance of public K-12 education in the U.S. is distinctively local and democratic: school districts are governed by boards, composed traditionally of lay members elected in non-partisan elections. Akin to corporate boards of directors, the policymaking and administrative responsibilities of school boards include strategic planning, financial oversight, recruitment of senior managers (e.g. the superintendent), and bargaining with teachers' unions. Although this governance structure suggests significant latitude to influence local education, there remains limited evidence on the causal impacts of school boards (Honingh et al., 2018).

Evidence is scarce because understanding the role of school boards is fraught with several empirical challenges. Although data exist that characterize teacher working conditions and student achievement, no administrative or public data source tracks the identity or characteristics of school board members over time. As a result, prior work on school board members frequently relies on case studies or surveys that are gathered at a single point in time (e.g. Land 2002; Grissom 2007). An additional empirical challenge is that school board composition is endogenously determined via the electoral process. For instance, trends in teacher salaries or student performance may generate responses in terms of who runs – and who voters elect – for the school board. This endogeneity of board composition

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implies that naive comparisons of school board characteristics with differences in education inputs or student outcomes are likely biased.

In this paper, we take a new empirical approach that is based on election records to study the influence of school boards on local education production. In particular, we examine the causal impacts of professional educators – identified as former classroom teachers, principals, superintendents, or other school administrators from election filings – as school board members. To empirically isolate exogenous variation in whether an additional educator is elected to the school board, we develop and implement a novel research design that leverages a feature of California elections: randomized assignment of the order that candidates appear on election ballots. While there are many dimensions of candidates for office for which this empirical strategy could be applied, this paper provides the first causal evidence that any characteristic of school board members affects either district resource allocation (including teacher salaries) or student success.

Our focus on educators is motivated by viewing school boards from a principal-agent perspective (John and Senbet, 1998; Adams, Hermalin and Weisbach, 2010). In this framework, board member human capital improves organizational performance by bridging information asymmetries (Arrow, 1963). Thus, school board members with backgrounds in education may bring specialized expertise to district leadership. For example, members who are formerly classroom teachers may have first-hand knowledge of the barriers to and constraints on student learning, and this expertise can translate into improved student performance by influencing school board decisions regarding teachers' working conditions in the district. Members' human capital has been shown to be empirically valuable in other board governance settings.[1]

On the other hand, pressure or interest groups may generate a misalignment between school board members' priorities and voters' interests via the electoral process (Becker, 1983; Toma, 1986; Rowley, Tollison and Tullock, 1988). Specifically, teachers' unions may compromise the independence of educators elected to the school board. Union membership among professional educators is historically widespread and teachers' unions spend substantial amounts of money to shape local school board elections (Hess and Leal, 2005; Moe, 2006). This raises the possibility that educators elected to the school board would shift collective bargaining towards union priorities, such as increased teacher pay or limitations on charter school growth. Unlike expertise, such rent-seeking may potentially be detrimental to education outcomes. A growing theoretical and empirical literature on the impact of teachers' unions on education highlights this possibility (Hoxby, 1996; Moe, 2009; Lovenheim, 2009; Lovenheim and Willén, 2019).

[1]Faleye, Hoitash and Hoitash (2018), for example, find that additional corporate board members with prior industry experience increase the firm's value. Wang, Xie and Zhu (2015) and Meyerinck, Oesch and Schmid (2016) also examine the value of industry experience. Related studies show that expertise, as measured by directors with CEO experience (Kang, Kim and Lu, 2018) and directors with experience in related industries (Dass et al., 2014), improve firm performance. Other work has examined the financial or legal skills of board members (e.g. Xie, Davidson and DaDalt 2003).

To pursue our analysis, we assemble a unique dataset that combines information about California school board members gleaned from election filings with publicly-available data on school districts and student outcomes. School board candidates self-identify their occupational background in election records. This allows us to empirically relate educators elected to the school board with changes in district education production. 18% of school board members in our sample are educators, a figure that closely matches representative survey data (Grissom, 2007). We then link these records with data on teacher salary schedules, district expenditures, and student learning outcomes, including end-of-grade standardized test scores and high school graduation. As local districts in California are primarily responsible for charter school authorization and oversight, we also link to data on charter schools. The resulting dataset reveals several descriptive patterns, including that average test scores are significantly lower in school districts served by a greater share of educators on the school board. These districts also tend to be larger and have more minority students.

The research design that we develop to estimate causal effects leverages California's randomized ballot order assignment. A well-established empirical phenomenon termed the "ballot order effect" shows that candidates listed at the top of the ballot gain an electoral advantage (Bain and Hecock, 1957; Koppell and Steen, 2004).[2] The insight of our empirical strategy is that random assignment of ballot order thus generates exogenous variation in the composition of the elected school board. 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 electoral advantage, when it is randomly conveyed on a candidate who is an educator, shifts the expected number of educators who are elected to the school board. This research design, which we subject to a variety of validity and placebo tests, allows us to provide causal evidence on how school board composition influences local education production.

We implement our research design to estimate the causal effects of an additional educator elected to the school board. We find that educators on school boards reduce charter schooling, as measured by both enrollment and the number of charters in the district. The point estimates suggest that the election of an educator leads to about one fewer charter school on average four years (i.e. a full board term) after the election. While there is little evidence of effects on some dimensions of teacher working conditions such as benefits, we find that – relative to board members with other professional backgrounds – educators raise teacher salaries. We find that an additional educator elected to the board leads to a teacher pay increase of approximately 2%. We also find that this increase applies

[2]A common explanation for the ballot order effect is a satisficing model with a cognitive cost of voting (Miller and Krosnick, 1998; Meredith and Salant, 2013). Accordingly, the ballot order effect tends 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).

across-the-board to different combinations of teaching experience and education levels, leading to an aggregate increase in the share of district expenditures spent on teacher salaries. Corresponding decreases in district spending towards services and capital outlays help to offset this salary increase.

We then turn to student outcomes and estimate causal effects on test scores and high school graduation rates. We find suggestive evidence that an additional educator elected to the school board reduces student performance. This is particularly evident for elementary level reading, but the treatment effect estimates are imprecise. While not statistically different from zero at conventional significance levels, the confidence intervals do rule out large positive treatment effects on student achievement (e.g. $\geq 0.05\sigma$). We do not find any meaningful impacts on high school graduation.

Our findings suggest that, despite raising teacher salaries, an additional educator elected to the board does not translate into improved outcomes for students. Rent-seeking models of political influence advance that this may be due to educators on school boards representing interests other than voters': those of teachers' unions. Our findings on increased teacher salaries and curbed charter growth are both consistent with predictions for greater union influence (Hoxby, 1996; Stoddard and Corcoran, 2007; Cowen and Strunk, 2015). To investigate the possibility these effects are due to alignment with union priorities, we examine survey responses of California school board members regarding their professional background and whether they were endorsed by a teachers' union (Grissom, 2007). Educators are 40% more likely to report being endorsed by unions relative to members from other professional backgrounds. Although a limitation of our empirical findings is that they remain ambiguous regarding whether educators reduce school quality, the supplementary survey data supports our conclusion that school boards are potentially an important causal channel through which teachers' unions exert influence.[3]

Our paper contributes to understanding the importance of schooling inputs to student learning. Much of this literature focuses on inputs at the school and teacher – rather than the district – levels (e.g. Rivkin, Hanushek and Kain 2005; Hanushek 2006; Chetty, Friedman and Rockoff 2014). Yet issues surrounding local control are gaining importance as recent reforms, such as the Every Student Succeeds Act, devolve authority to school districts. Prior work on school boards' role in local education is generally descriptive, focusing on minority representation (e.g. Meier and England 1984) or conflict (Grissom, 2010). An exception is Macartney and Singleton (2017), who rely on narrowly-decided elections to show that school boards causally impact how students are allocated across schools, with Democratic members tending to reduce racial segregation. We similarly identify board influence on school assignment via charters, but our paper is the first to

[3]Recent empirical work on teachers' unions suggests that negative consequences for students operate via reduced educational quality rather than lowered attainment (Lovenheim, 2009; Lovenheim and Willén, 2019).

bring causal evidence to bear on school boards' role in allocating district resources and their effectiveness at improving student learning.[4]

In addition, our paper connects with a wider literature on the effectiveness of board governance. Beyond corporate boards of directors, other applications include hospitals (Molinari et al., 1995) and central bank councils (Göhlmann and Vaubel, 2007). Previous studies suggest that the human capital and independence of board members are important for organizational performance. Our focus on public school districts relates to work that studies political representation and the quality of public good provision specifically (Pande, 2003; Ferreira and Gyourko, 2009, 2014; Beach and Jones, 2016, 2017; Logan, 2018; Beach et al., 2018).

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 I also presents descriptive patterns on the correlates of educator composition on school boards. We then detail our research design in Section II and present the results of our analysis in Section III. We discuss the interpretation of our findings in Section IV before concluding.

I. Background and Data

The almost 14,000 local public school districts across the U.S. vary substantially in characteristics such as size and demographics, but they share a common institutional feature: nearly all are governed by a school board comprised of elected members. School boards have several general responsibilities, which include strategic planning for the district, curricular decisions, community engagement, budgeting, and implementing federal and state programs and court orders (Hochschild, 2005; Maeroff, 2010). These responsibilities suggest several channels through which board decisions are likely to matter for the allocation of education inputs.

A key responsibility is recruiting and evaluating senior management: the superintendent (the school district's chief executive), central administrators, and school principals. In nearly all states, school boards collectively bargain with teachers' unions. Negotiations between boards and unions thereby influence salary schedules, benefits, work hours, and other parameters of teachers' employment. Boards' budgetary responsibility charges them with allocating district resources across schools, although voter approval is typically needed to change tax levies or sell bonds (Hess and Meeks, 2010). School boards also play a central role in the assignment of students to schools, a responsibility historically at the fore of school desegregation in the U.S. This role remains relevant both due to discretion over attendance zone boundaries (e.g. Macartney and Singleton 2017; Monarrez 2018) and because local school boards in many states authorize and monitor charter schools (Teske, Schneider and Cassese, 2005).

[4]Fischer (2020), which adopts our ballot order strategy, provides causal evidence that Hispanic representation on California school boards leads to increases in test scores at schools disproportionately serving Hispanic students.

While typical in most respects, school board governance in California has several attributes that distinguish it from other states. Under the 1975 Rodda Act, California school boards must collectively bargain with teachers at least once every three years. However, with 90% of California teachers full voting members of one of the two main unions (the California Teachers Association and the California Federation of Teachers), teachers' unions are perceived as especially influential.[5] Unlike most other states, school boards in California also effectively do not have the power to tax ever since Proposition 13 placed a ceiling on local property taxes (Loeb, 2001). Finally, while charter school authorization authority is vested in state-level bodies in many states, school boards are California's principal authorizers.[6] About 87% of charter schools in California are authorized by a school district, whereas only around half of charters nationally are locally authorized (Mumma, 2018).

A. Data Sources and Construction

We assemble the dataset by first constructing candidate rosters for California school board elections spanning nearly two decades. These rosters include each candidate's vote share, ballot position, electoral outcome, and occupational background. We then merge these records with panel data from school districts, including on teacher salaries and student outcomes.

School Board Elections and Ballot Order. — Data on school board elections and candidates for 1996-2015 come from the California Elections Data Archive (CEDA, 1996–2015).[7] We determine whether each candidate is an educator using occupational information provided in their ballot designations.[8] We identify educators as candidates who describe their primary occupation or profession as a teacher, educator, principal, superintendent, or school administrator. We exclude individuals who work in the education sector but do not focus on K-12 instruction, namely school employees such as counselors and custodians and those employed in postsecondary education.

[5]By comparison, approximately three-fourths of the nation's K-12 public school teachers are represented by a teachers' union (NCES, 2015–2016).

[6]Applications to open a charter school in California are generally submitted to local school districts, with appeals of denials taken up by county offices of education or the State Board of Education. Upon approval, charters must reapply for authorization every five years.

[7]CEDA data do not report uncontested elections. As a result, our candidate rosters are limited to members who ever participated in a contested race with at least two candidates.

[8]Ballot designations provide candidates with a three-word opportunity to describe their primary profession, vocation, or occupation to potential voters. By California law, the designation must correspond to the candidate's profession at the time of filing or, if retired from working, their principal occupation prior to retirement. 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.

We merge the candidate rosters with randomized alphabet information used to determine the order of candidates' names on election ballots in California elections from 1998-2015.[9] The randomized alphabet applies throughout candidates' last and first names. Crucially, the randomization takes place after the declaration of candidacy deadline, such that candidates cannot base their decisions to run on their ballot placement. We match election dates and use candidates' last and first names to determine ballot order.

We use the candidate rosters and election outcomes to construct yearly measures of school board composition in each district. Almost all members serve four year terms with staggered contests occurring every two years. 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 each given district-year to assemble the final school board roster. The rosters allow us to summarize the composition of each board over time, including the share of members who are educators.

School District Variables and Education Outcomes. — We link the school board rosters with district-level characteristics and outcome variables of interest. Information on district-level student enrollment and demographic composition come from the Common Core of Data (1998-2017) (NCES, 1998–2017). The Common Core of Data also identifies charter schools administered by each school district or local education agency. We use charter school status to construct the share of total district enrollment in charter schools and count the number of district-authorized charter schools in each school district.

Data on teacher working conditions during 2000-2019 come from the annual Salary and Benefits Schedule for the Certificated Bargaining Unit (Form J-90) (CA DOE, 2000–2019*c*). Form J-90 reports salaries corresponding to unique combinations of education level (column or lane) and years of experience (step). Common lanes include a BA degree with additional credit hours, while steps begin with the initial year of experience. An advantage of salary schedules – as opposed to summaries of expenditure on instruction or salaries – is that they are directly negotiated between the district and teachers' union. We collect salaries for the most common combinations, e.g. a Bachelor's degree with 60 credit hours at step 5.[10] We also gather other variables characterizing (collectively bargained) teacher working conditions from the data. These include the number of scheduled or required service days, district contributions to employee benefits, and teacher credential-based pay such as lump sums for having an MA. We compute class size

[9] We collected these from the California Secretary of State's office (CA Secretary of State, 1998–2015) for years after 2002 and from (Ho and Imai, 2008) for years prior.

[10] A Bachelor's degree with 60 credit hours is a common educational level among certified employees and is used as a reference category in the Form J-90. We impute salary if it is otherwise not explicitly listed on the district's schedule by using the column and/or step immediately below the missing educational and experience combination.

as pupils per teacher using full time equivalent teachers and student enrollment counts.[11]

District finance information from the California Department of Education (1996-2018) allow us to examine shares of overall expenditures allocated towards certified staff salaries, which includes teachers and all other staff requiring a teaching certification (CA DOE, 1996–2018). We likewise examine the allocation of district spending on classified salaries (non-teaching staff including paraprofessionals and those working in the business office, food service, maintenance, technology, and nursing), benefits, capital outlays and expenditure on miscellaneous services. We focus on allocation of spending rather than levels since school districts in California cannot appreciably affect overall expenditures. We also gather data on superintendent salaries from CA DOE (2000–2019*c*), which allow us to consider the consequences of educator representation on the school board for district leadership.

Finally, we collect data on student achievement and attainment outcomes as represented by test scores and high school graduation rates. School-by-grade-by-year averages of math and reading performance between 1998 and 2017 come from the California Department of Education (CA DOE, 1998–2013, 2015–2017*a*).[12] We normalize the score averages by year and grade according to the estimated student distribution among test takers.[13] We restrict attention to elementary and middle school scores because of issues related to tracking at the high school level (e.g. the available math scores are for Algebra I, which is taken at different grades by different students). Our graduation rate measure is constructed using 1997-2017 California Department of Education data (CA DOE, 1997–2017*b*). We divide the number of high school graduates in a given year by the average number of ninth-graders enrolled three years prior and the number of tenth-graders enrolled two years prior, in a manner akin to calculations of the Averaged Freshman Graduation Rate (AFGR).[14]

B. Data Summaries

Candidate and School Board Characteristics. — Table 1 summarizes candidate characteristics across 14,150 unique individuals in our sample of California school board elections. Half of this group won an election at least once between 1998 and 2015. 16% of candidates describe their primary vocation as an educator. Among those who ever won an election, educators comprise 19%, consistent with

[11] Note that we base our class size proxy on the number of full time equivalent teachers in the California staff demographics files and student enrollment data obtained from the Common Core of Data (NCES, 1998–2017).

[12] We also collect school-grade-year data from 1999-2019 by racial/ethnic group and school-year data by lunch status (CA DOE, 1998–2019*a*,-).

[13] One limitation of using these records is that they are missing for cell sizes smaller than ten for privacy reasons.

[14] We do not average over prior eighth-grade enrollment because of missing data among high school districts in California.

survey evidence on the occupational backgrounds of school board members.[15] 14% of candidates work in business, which we similarly identify from ballot designations.[16] In addition, one quarter of candidates are incumbents, a share that increases to over one-third among winning candidates. Finally, winning candidates appear an average of 7 years as board members in our data.

Table 1—: School Board Candidates

	Candidates	Winners
Ever won a contest	0.51	1.00
Educator	0.16	0.19
Businessperson	0.14	0.13
Incumbent	0.25	0.38
Tenure (years)	3.89	7.04
Total	14,150	7,268

Note: Sample includes unique candidates and their characteristics when first observed winning or participating in school board elections from 1998 - 2015. Sample excludes observations with missing ballot order information or district ID. Winners refer to candidates who ever won a school board election. Candidates who never won an election have 0 years of tenure.

Table 2 summarizes the characteristics of elected school boards. The average board in our sample has five members. The middle 50% of the distribution ranges from four to six members, which is consistent with board sizes across California of three, five, or seven individuals. The average share of educators on each school board is 18% and is zero at the 25th percentile. At the 75th percentile of the distribution, one third of the school board consists of educators, indicative that school boards in which educators hold a majority are a minority of the observations. Businesspeople comprise 12%, while the average share of board members who are incumbents is 58%.

Table 2—: School Board Characteristics

	Mean	Std. Dev.	25p	75p
Number of Members	4.84	1.58	4	6
Share of Board: Educators	0.18	0.21	0.00	0.33
Share of Board: Businesspeople	0.12	0.17	0.00	0.20
Share of Board: Incumbents	0.58	0.28	0.40	0.80

Note: N = 3,849 school board (district-election year) observations.

[15]The 2006 California District School Board Member Survey found that 17% of school board members are educators (Grissom, 2007)

[16]We identify businesspeople from the candidate roster as those who self-describe as an "executive," "businessman," "businesswoman," or "president." The category also includes chief financial officers and self-employed individuals.

District Inputs, Education Outcomes, and their Relationship with Board Composition. — Table 3 describes the set of district inputs and education outcomes that we use in our analyses. The first column reports averages over the sample. We then stratify by the share of educators on the school board in order to understand how the presence of educators relate to these variables. Specifically, we report and compare averages for four groups: school boards with no educators, boards with at least one but no more than a third of the board who are educators, boards with more than a third but not a majority educators, and majority-educator school boards.[17]

Table 3—: School District Variables and Education Outcomes by Educator Share of School Board

		Share of Board: Educators			
	Average	None	$0 < x \leq 1/3$	$1/3 < x \leq 1/2$	Majority
Panel A. School District Characteristcs					
Total Enrollment	9146	4731	9435	10978	17782
Share White	0.35	0.39	0.34	0.33	0.29
Share Black or Hispanic	0.51	0.47	0.52	0.52	0.58
Share Asian	0.09	0.08	0.09	0.10	0.09
Share FRP Lunch	0.55	0.54	0.54	0.56	0.57
Urban	0.59	0.45	0.62	0.66	0.79
Panel B. Charter Schooling					
Share of Enrollment	0.05	0.06	0.05	0.05	0.05
No. of Charters	1.14	0.69	1.02	1.26	2.44
Panel C. Teacher Working Conditions					
Service Days	184	184	184	183	183
MA Bonus Offered	0.61	0.70	0.62	0.59	0.39
Max Health Contribution	12392	11995	12480	12657	12810
Class Size	30.52	29.81	30.88	30.96	31.04
BA+60 Teacher Salary	64582	63410	64818	65233	66107
Panel D. School District Expenditures					
Share Certified Salaries	0.45	0.44	0.45	0.46	0.45
Share Classified Salaries	0.15	0.15	0.15	0.15	0.15
Share Benefits	0.19	0.19	0.19	0.19	0.19
Share Services	0.11	0.12	0.11	0.11	0.11
Share Capital Outlays	0.01	0.01	0.01	0.01	0.01
Superintendent Salary	171500	161122	171701	175297	191844
Panel E. Student Outcomes					
Reading Scores	0.00	0.08	-0.01	-0.08	-0.12
Math Scores	0.01	0.08	0.01	-0.06	-0.11
HS Graduation	0.88	0.90	0.88	0.87	0.86

Note: Table reports averages that correspond to the 2013-14 school year for the sample and by levels of educator representation on the school board.

The uppermost panel of Table 3 reports the means for several district characteristics. Average public school enrollment is over 9,100 students, around 35%

[17]We detrend the variables when computing the summary statistics reported in Table 3 and set the averages to those for the 2013-14 school year.

of whom are White and a slight majority are either Black or Hispanic. Districts with more educators on the school board tend to be larger with more minority students. Panel B on charter schooling patterns shows that average enrollment share does not vary substantially with the board share of educators, but districts represented by more educators tend to have more charters on average.

Panel C examines variables that characterize teachers' working conditions. On average, teachers are obligated to work 184 days per school year. 61% of districts provide a bonus for teachers for having a MA and the maximum district contribution to health benefits is approximately $12,400. Mean class size is just over 30 students. The average salary for teachers with a Bachelor's degree and 60 credit hours according to the district salary schedule is approximately $65,000. Overall, the table suggests limited variation in these variables according to the educator share of the school board. One exception to this relative uniformity is the likelihood that district offers an MA bonus, which decreases with a greater share of educators.

As reported in panel D, the average district spends 45% of its budget on certified salaries, with another 15% allocated to salaries for classified staff. Across all categories, expenditure shares vary little by board educator share. However, the table also shows that boards with more educators serve districts that pay their superintendents higher salaries. While the average district spends $172,000, a majority educator school board district spends 10% more on average.

Finally, the bottom panel of Table 3 presents summaries of student learning outcomes, including math and reading test scores for elementary and middle schools, and high school graduation. Each of these student outcomes exhibits a monotonic relationship with the educator share of the school board, with lower outcomes in districts served by boards with more educators. For example, students in districts served by majority educator school boards score about 0.11σ below the student mean.[18] High school graduation rates, on the other hand, suggest a weaker relationship with the share of educators on the school board.[19]

While Table 3 highlights several interesting patterns about educator representation on school boards, these relationships should not be regarded as causal. Student performance and educators may be inversely related, for example, if voters respond to lower test scores by disproportionately electing candidates with education experience to the board. Differences across boards along observable dimensions suggest important differences exist on unobserved ones as well, further highlighting the endogenous formation of school boards. With this empirical challenge in mind, the next section describes the research design that we implement in order to estimate causal effects of school boards.

[18] We construct district-year measures of test scores by first norming school-grade-year-level scores to the grade-by-year distribution. We then collapse across schools and grades to arrive at district-by-year values.

[19] This is despite considerable cross-sectional variation in graduation rates in the sample. For example, while the average graduation rate in 2016 was 88%, the 5th percentile of the distribution was 75% and the 95th percentile essentially 100%. The standard deviation was 10 points.

II. Research Design

This section details how we use California's randomization of ballot order to estimate causal effects of school boards on local education production. The novel insight of our research design is that the assignment of an educator to the first ballot position generates quasi-random variation in the expected number of educators that are elected to the school board.

A. *Setup and Overview*

Our research design aims to estimate the causal effects of an additional educator elected to the school board. School districts are governed by sequences of school boards, where each board is a unique combination of a district and election year (wherein the board is formed by contests for open seats). Note that staggered elections every two years, combined with members' four-year terms, means that boards will share specific members in common even though each cycle forms a distinct school board.[20]

The causal effects of interest concern the exogenous assignment of an additional educator to the school board: i.e. in the years following that member's election, how would education production in that school district differ from the counterfactual in which the marginal educator did not win? Conceptually, an additional educator could affect district inputs and outcomes through their own voting on issues, via the deliberative process, or by influencing the school board's agenda.[21] While the effects could manifest immediately and persist over time, it is also possible that they only become apparent in the longer run. In this regard, an important feature of our approach is that it examines the profile of effects over time.

To formally represent the causal effects, let b index school boards. We then denote by $Y_{b\tau}$ outcomes τ years following school board b's formation. With this notation, we can define a vector of causal effects by the following equation:

$$Y_{b\tau} = \beta_\tau T_b + \upsilon_{b\tau} \tag{1}$$

for $\tau > 0$. T_b is the number of educators elected to school board b. $\upsilon_{b\tau}$ represents all remaining observed and unobserved determinants of the outcome. $\beta_\tau = (dY_{b\tau})/(dT_b)$ is thus the effect of an additional educator on the district outcome τ periods after their election and the vector of β parameters characterizes the profile of the causal effects over time. Our research design aims to produce unbiased estimates of these effects.

[20] As an example, Los Angeles Unified 2010 is a conceptually distinct school board from Los Angeles Unified 2012 although the two boards share individual members.

[21] These potential channels of influence raise the question how the effects may depend on the presence of other educators on the school board. For instance, it may be that effects are pronounced when educators form a majority of board members. We highlight this question below and specifically test for heterogeneity in effects in Section 5.1.

Before proceeding to describe our empirical strategy, an aspect of the setting to flag is the role of elections that occur prior and subsequent to school board b's formation. For example, previous boards can independently affect outcomes during school board b's term and those actions are potentially correlated with T_b. This raises several issues: First, prior boards' actions can be viewed as omitted variables, so the empirical strategy must address this possible bias. Second, if an educator were elected in the previous election, the marginal educator elected to b is more likely to result in a majority share of educators on the board. This raises the question of heterogeneity in β and we devote a subsection later to assessing this (5.1). Another way in which the sequence of board elections may matter is if the marginal educator affects long-run district outcomes in part by changing future school boards' actions. This instead raises a question for the interpretation of β_τ for those τ that follow the next election cycle.[22] We examine the importance of this electoral mechanism for our findings in Section 5.2.

B. Empirical Strategy

The empirical challenge we face is that elements of $\upsilon_{b\tau}$ are likely to be unobserved and correlated with T_b and therefore confound naive estimates of equation (1). Our empirical strategy proposes an instrument for T_b based on randomized ballot ordering and candidates' backgrounds. Because candidates randomly listed at the top of the ballot gain an advantage, known as the "ballot order effect," the assignment of an educator to the first ballot position should generate quasi-random variation in the probability that an educator wins the election.[23] To implement this insight, we construct an indicator for whether an educator was randomly assigned to the top of the ballot for each electoral contest r for open seats on board b: $FirstEducator_{br}$. We call this variable the ballot order instrument.

Before describing the details of how we use the instrument, there are several issues to consider for this strategy to be viable and to yield internally valid estimates. The first issue is that an educator cannot be assigned to the top of the ballot if no educators enter the candidate pool in the first place. Likewise, an educator is always assigned to the top of the ballot when all of the candidates are educators. In both of these cases, random assignment of the instrument is not possible. In our sample, 48% of contests, representing 76% of school districts,

[22] We develop this point more formally in the Online Appendix.

[23] The importance of ballot order has been long recognized (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). Ballot order effects can be sizable for primaries, non-partisan races, or elections with low salience (Alvarez, Sinclair and Hasen, 2006; Ho and Imai, 2008; Pasek et al., 2014). Maeroff (2010, 128) 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."

permit random assignment. We term this subsample the "random assignment sample." An educator is assigned to the top ballot position 757 times in this sample, suggesting there is enough variation in the data to carry out the strategy.[24]

The second issue concerns the relevance of the instrument: does the random assignment of an educator to the top of the ballot actually lead to a significant shift in the likelihood that an educator wins? After replicating the ballot order effect in the next section, we test this directly in Section 4.1, finding a 26% increase in the number of election winners who are educators on average and a corresponding 2 percentage point increase in the share of the board who are educators.

Finally, the most crucial issue concerns the validity of the ballot order instrument. Our identification assumption is based on conditional mean independence. Logically, randomization of ballot order implies that $FirstEducator_{br}$ is randomly assigned *conditional on* the share of educators in the candidate pool. We thus control for the candidate share of educators throughout the analysis. While conditional random assignment is not testable, it makes several strong predictions that we evaluate as validity checks and placebo tests. These provide supporting evidence that whether an educator was assigned to the first position is conditionally as good as random.

C. Empirical Specifications

We focus on estimating two sets of causal effects: (1) "event study" estimates that trace out the impact of the ballot order instrument over time; and (2) estimates of the treatment effects of an additional educator elected to the school board. We describe each of these in turn and detail the setup of the dataset as well as other specification choices.

The event study estimating equation is given by:

$$Y_{b\tau} = \pi_\tau FirstEducator_{br} + \Gamma_\tau W_{br} + v_{b\tau r} \tag{2}$$

where outcomes, such as educational inputs and student success, are a reduced-form function of the ballot order instrument, $FirstEducator_{br}$, and a vector of controls, W_{br} (whose effects are τ-specific). Importantly, the control set includes $\overline{Educator}_{br}$, the share of educators in the candidate pool of contest r. The parameters of interest are π_τ, representing the causal effect in period τ associated with a randomly-assigned educator at the top of the ballot. Causal inference is

[24] While we may obtain internally valid estimates from this sample, a potential external validity concern is whether those contests are representative. Table A1 compares the subsample where randomized assignment is possible with the sample of contests as a whole in terms of observed characteristics. The subsample districts are larger in size and have more minorities in addition to greater representation by educators on average, but are quite similar to the larger sample in terms of charter penetration, district expenditures, and student outcomes.

maintained under the assumption of conditional mean independence:

$$E[v_{b\tau r}|FirstEducator_{br}, \overline{Educator}_{br}] = E[v_{b\tau r}|\overline{Educator}_{br}] \tag{3}$$

This expectation says that, given the share of educators among the candidates in contest r, whether an educator is assigned to the top of the ballot is as good as random. This condition suggests several natural checks of our empirical strategy. Specifically, the instrument should be conditionally uncorrelated with predetermined election and school district characteristics – including election results prior to school board b's formation. We examine these validity checks in Section 3.4. In addition, the assumption implies that $\pi_\tau = 0$ for the periods before (and the year of) the board's formation (i.e. $\tau \leq 0$). We estimate these placebo causal effects as part of the event study.

For purposes of estimation, we pool data across multiple τ. This means that, for each school board b in the data, we stack outcome observations for the school district from three years *prior* to the board's formation ($\tau = -3$) through six years post-election ($\tau = 6$). We restrict the panels to six post-election periods because the set of boards for which additional years are available decreases substantially. We include four "pre-periods" (-3 to 0) for efficiency and to carry out the placebo tests.[25] We include any incomplete panels in the resulting dataset so long as we observe either salary or student outcome information at $\tau = 0$.[26] Similarly, when multiple electoral contests are held for the same school board, we stack and "duplicate" the observations. The school board b, contest identifier r, and the number of periods relative to the election year τ, uniquely identify observations in the resulting dataset.

In addition to the educator share among candidates, our preferred specification includes several other controls in W_{br}. The estimation sample includes all contests, including those which feature no educators or only educator candidates. We thus include indicator variables for these cases.[27] We also control for the number of open seats, its interaction with the educator share among candidates, and the number of contests in year t for each district to increase precision.[28] We decompose the error term in equation (2) as $v_{b\tau r} = \theta_j + \rho_t + \eta_\tau + \lambda_{t+\tau} + u_{b\tau r}$, with

[25]Note that period 0 is defined as when the school board election year and the spring of a school year coincide. For example, 2013-14 test scores are period 0 for the elections that occurred in 2014. Because elections occur after the end of that school year, this implies that period 0 is always a "pre-treatment" year.

[26]Stacking the data in this way means that the same calendar year outcome observation will be used for different school boards. For example, 2012 observations for Los Angeles Unified will be included in the panel for the school board elected in 2012 (as $\tau = 0$) as well as the board elected in 2010 (as $\tau = 2$) as well as for the board elected in 2008 (as $\tau = 4$).

[27]The instrument, $FirstEducator_{br}$, is zero by construction for contests in which no candidates or all candidates are educators, so such cases do not "identify" the causal effects but do improve the efficiency of the estimates. In the Appendix, we alternatively estimate effects on just the randomized assignment sample and show that this yields similar, though less precise, results.

[28]In addition to verifying that the ballot order instrument is conditionally uncorrelated with these variables in Section 3.4, we include results in the Appendix for models that do not include these covariates. The results are not sensitive to their inclusion.

separate district (θ_j), election year (ρ_t), period (η_τ), and calendar year fixed effects ($\lambda_{t+\tau}$).[29] Lastly, our preferred specification restricts Γ_τ; each control in W_{br} is interacted with pre- and post-elections intercepts and pre- and post-election linear trends.[30]

We also apply the ballot order instrument to estimate treatment effects of an additional educator elected to the school board via two-stage least squares. The first stage equation is given by:

$$T_b = \alpha FirstEducator_{br} + \Gamma W_{br} + \varepsilon_{br} \tag{4}$$

Rather than estimating ten parameters (four placebo effects and six post-election effects) for each outcome of interest, we estimate two parameter models that aim to summarize the treatment effects in a parsimonious fashion:

$$Y_{b\tau} = \beta_I T_b \mathbf{1}(\tau > 0) + \beta_S(\tau - 4) T_b \mathbf{1}(\tau > 0) + \Theta_\tau W_{br} + \theta_j + \rho_t + \eta_\tau + \lambda_{t+\tau} + e_{b\tau r} \tag{5}$$

β_I and β_S represent the "intercept" and "slope" of the post-election treatment effects, respectively. Note that the slope term equals zero when $\tau = 4$, such that the intercept β_I represents the average treatment effect of an additional educator *four years post-election*. We chose this normalization because four years is the expected term length of school board members. β_S characterizes the evolution of treatment effects over time as τ increases. $FirstEducator_{br}$ and its interaction with $\tau - 4$ are the excluded instruments. The estimation sample and covariates W_{br} included in estimating this model are the same as in equation (2).[31]

D. *Validity*

In this subsection, we present validity checks of the identifying assumption underlying our research design. To motivate these checks, let K_{br} represent a vector of observed variables corresponding to school board $b = b(j, t)$ that are determined *prior* to year t (i.e. $\tau \leq 0$), such as total enrollment. K_{br} also includes the results of prior school board elections along with the composition of school boards preceding b in the district.

If we decompose the residual of equation (2) into observed and unobserved determinants as $v_{b\tau r} = \Theta K_{br} + \bar{v}_{b\tau r}$, the conditional mean independence assumption

[29] A practical question raised by the inclusion of district fixed effects is the degree of within-district variation in $FirstEducator_{rjt}$. We directly test this in "first stage" regressions of board composition on the instrument, presented in Table 5. Appendix Table A2 shows that approximately half of districts in our sample had at least one electoral contest with a first-listed educator from 1998-2015. Figure A1 also visualizes common patterns of within-district variation in the ballot order instrument in the sample.

[30] Appendix Tables A5 to A8 show that all of the main findings are robust to this restriction.

[31] One difference with the event study equation (2), however, is that we apply the restriction that $\Theta_\tau = 0$ for $\tau \leq 0$. The event study, in contrast, allows for effects of controls prior to the election in order to carry out placebo tests.

implies the following:

$$E[K_{br}|FirstEducator_{br}, \overline{Educator}_{br}] = E[K_{br}|\overline{Educator}_{br}]$$

This says that the assignment of an educator to the top of the ballot should be uncorrelated with all observed pre-determined election, district, and school board variables (as long as the model controls for the share of educators in the candidate pool). This expression forms the basis for simple regression-based validity tests that condition only on the share of educators in the candidate pool and election year fixed effects.[32] Though these checks inherently cannot rule out correlation with unobservables, conditional independence from observed determinants is consistent with the empirical strategy isolating exogenous variation.

Table 4—: Validity: Electoral, District, and Board Characteristics

	Election Contest Characteristics				
	No. of Candidates in Contest (1)	No. of Open Seats in Contest (2)	No. of Open Seats on Board (3)	Share of Candidates: Businesspeople (4)	Share of Candidates: Incumbents (5)
Top of Ballot Educator	0.072 (0.096)	0.025 (0.036)	0.035 (0.038)	-0.06 (0.009)	-0.001 (0.010)

	School District Characteristics				
	Enrollment (1)	Share White (2)	Share Black or Hispanic (3)	Share Asian (4)	Share Free or Reduced Lunch (5)
Top of Ballot Educator	-951 (1,061)	-0.001 (0.013)	-0.002 (0.013)	0.000 (0.005)	-0.000 (0.012)

	School Board Characteristics				
			Share of Unexpired Seats:		
	Share of Educators On Board, *t-2* (1)	Share of Educators On Board, *t-4* (2)	Educators (3)	Businesspeople (4)	Incumbents (5)
Top of Ballot Educator	-0.006 (0.011)	0.018 (0.012)	-0.007 (0.007)	0.004 (0.005)	-0.001 (0.009)

Note: Robust standard errors are clustered at the district level. There are 2,165 contest observations in the random assignment sample. All specifications include the share of educators in the candidate pool, indicators for having no educators or all educators in the candidate pool, indicators for the number of contests per district-year, and separate district and election year fixed effects.

Table 4 examines the relationship between the ballot order instrument and election, district, and board characteristics. The estimates show that an educator assigned to the top of the ballot is statistically unrelated to the number of candidates in the contest (column 1) or the number of open seats in either the specific electoral contest or on the school board (columns 2 and 3). We also see that, conditional on the share of educators in the candidate pool, whether an educator is assigned to the top of the ballot is unrelated to the shares of candidates who

[32]We restrict the sample to the randomized assignment sample for these validity checks; alternative tests that include all contests but instead add indicators for either all or no educators in the pool yield the same findings.

have a background in business or who are incumbents. In addition to election attributes, we check whether the ballot order instrument predicts school district characteristics measured at the time of the election. The middle panel of Table 4 shows no association between the instrument and total public school enrollment or student demographics.

The validity checks in the bottom panel of Table 4 shows no association between the ballot order instrument and electoral outcomes *prior* to the focal school board's formation. Columns (1) and (2) find that the instrument is conditionally unrelated to the share of educators on the school board two and four years before the focal election. Similarly, whether an educator is assigned to the top of the ballot should have no relationship to the composition of previously-elected board members whose terms have not yet expired. As expected, columns (3) through (5) show that the instrument does not change the makeup of pre-existing board members.

We also check validity with respect to prior events. To do so, we regress the ballot order instrument on a) whether an educator was assigned to the top of the ballot in a previous election and b) the share of educators in the candidate pool in previous elections. Appendix Table A3 shows that top of the ballot assignment in a current election is unrelated to whether an educator was top-listed on the ballot two years ago and four years ago.

Finally, we test a direct implication of randomized ballot order: the probability that an educator is assigned to the top of the ballot should – on average – equal the share of candidates in the candidate pool. We use contest-level data to estimate a non-parametric regression of the ballot order instrument on the share of candidates who are educators. We then plot the estimates and associated confidence intervals by the decile of candidates who are educators. Appendix Figure A2 shows that the averages correspond closely with the associated candidate shares, displaying a strongly linear, 45 degree pattern over the support, consistent with ballot order randomization.

III. Results

A. *Evidence of Treatment*

We begin by providing evidence that the random assignment of an educator to the top of the ballot shifts school board composition. The viability of the ballot order instrument depends on whether ballot order significantly increases the number of educators elected to the school board.

Table 5 presents this evidence. Column (1) first tests for the ballot order effect at the candidate-level. We regress whether a candidate won the election on a top-of-the-ballot indicator while controlling for district and election year fixed effects. The results indicate that candidates randomly listed at the top of the ballot gain a 10.3 percentage point advantage relative to other ballot positions. The finding of a sizable ballot order effect is in keeping with results from prior literature. Depending on the electoral context, studies using vote shares as the dependent variable estimate the benefits of being listed first as increasing vote shares by less than 5 percent up to half of the baseline vote (Miller and Krosnick, 1998; Brockington, 2003; Ho and Imai, 2008). The magnitudes are often large enough to be decisive, with one study estimating a first candidate advantage on winning probability of nearly five percentage points (Meredith and Salant, 2013).

We then examine the implications of the ballot order effect when the advantage is randomly conveyed on an educator. We consider two outcomes: the number of election winners who are an educator (in column 2 of Table 5) and the share of all school board members that are educators (in column 3). The second group includes both the election winners and, because of staggered contests, existing members whose terms have not yet expired. The results in column (2) reveal that an educator randomly assigned to the top of the ballot increases the number of educators elected by 0.14 individuals. 0.54 educators win on average, so this effect is equivalent to a 26% increase. This model corresponds to the "first stage" of the treatment effect estimates we later examine.[33] The F-statistic for the ballot order instrument is over 24. Looking at the composition of the school board as a whole in column (3), a first-listed educator increases the proportion of educators by 2.3 percentage points. These results provide evidence that the random assignment of an educator to the top of the ballot will shift the seated board's composition.[34]

B. *Causal Effects*

The ballot order instrument's effect on the number of educators on the board enables us to isolate causal effects on charter schooling, school district inputs, and education outcomes. We begin by presenting "event study" results in this subsection before examining the robustness of causal estimates to alternative design and specification choices. In the last subsection, we present estimates of the treatment effects from an additional educator elected to the school board.

Event Study Estimates. — The estimates presented in this subsection correspond to equation (2). This event study-style specification facilitates testing for causal effects post-election and examining dynamics over time. The series of figures also enables visual inspection for significant intent-to-treat differences in periods -3 through 0 (placebo effects) or for trends in outcomes leading up to

[33]It is also possible to ask whether the ballot order instrument is associated with the number of educators elected in the previous election (as a placebo) or elected in subsequent election cycles (as evidence for electoral dynamics or feedback effects). We present these "event study" results in Section IVB, wherein we discuss the interpretation of the long-run causal effect estimates.

[34]We consider the possibility that ballot order effects are not limited to just first-listed candidates in results presented in Table A4. We define educator candidates listed outside of the first slot, but still in a ballot position at or under the number of open seats, as "top tier" educators. When we augment the specifications with this additional variable, we find no significant effects for being a top tier educator candidate. In contrast, the coefficients on top-listed educator candidates remain almost identical.

Table 5—: Evidence of Treatment

	Won Election (1)	No. of Winners Who are Educators (2)	Share of Board: Educators (3)
Top of Ballot	0.103		
	(0.010)		
Top of Ballot Educator		0.141	0.023
		(0.029)	(0.008)
Observations	19240	4448	4448
F-statistics		24.21	7.90

Note: Robust standard errors are clustered at the district level. Column 1 uses observations at the candidate level and regresses an indicator for winning the electoral contest on an indicator for being at the top of the ballot. The specification includes separate district and election year fixed effects. Columns 2 and 3 use observations at the electoral contest level, and include separate district and election year fixed effects. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year.

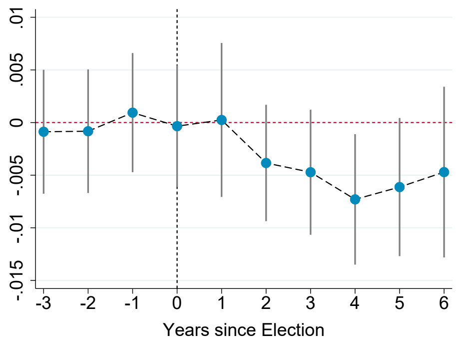
the election at period 0 (pre-trends).[35]

Figure 1 presents estimated effects (and associated confidence intervals) of the ballot order instrument on the share of district enrollment in charter schools and the number of charter schools in the school district. There is no evidence of an effect prior to the focal election at year 0 for either outcome. For charter enrollment, we see a decrease that emerges two years post-election with the effect peaking at four years post-election. In the case of the number of charters, however, the instrument initiates a negative trend approximately one year after the election that continues linearly through the six post-treatment years. These effects are consistent with top of the ballot educator-exposed boards actively reducing charter schooling or restricting the growth of charter schooling relative to non-exposed districts.

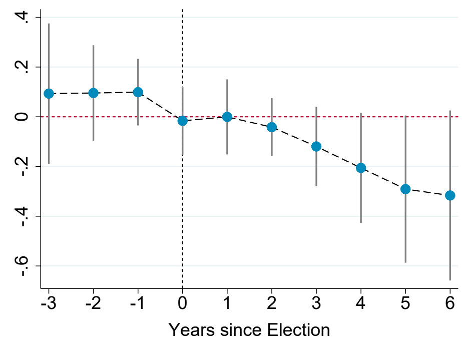
Figure 2 focuses on outcomes related to teacher working conditions. Specifically, we examine effects on the number of service days, whether the district offers an additional bonus to teachers with an MA, the generosity of the district's healthcare contribution, average class size, as well as teacher salaries.[36] Across the five outcomes, Figure 2 provides no evidence of effects prior to the election, consistent with the assumptions of our research design. The figure also indicates

[35]Note that the event-study specification estimates intent-to-treat differences at all periods in the sample (−3 through 6); these differences are plotted in the figures. This is different than figures that are often produced to examine pre-trends, which will sometimes normalize treatment-control differences to a specific (pre-intervention) year.

[36]Here, we focus on salaries corresponding to teachers with a Bachelor's degree and the equivalent of 60 credit hours (the column whose reporting is standardized). We expand our inquiry to examine heterogeneity in causal effects across experience and education level combinations in the treatment effects subsection. We also plot the event-study estimates for other salary schedule columns in Appendix Figure A3 for comparison.



(a) Share of Enrollment in Charter Schools



(b) No. of Charter Schools

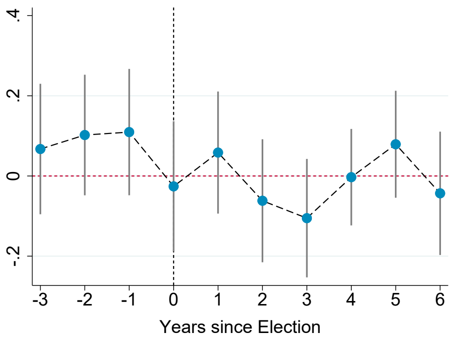
Figure 1. : Event-Study Causal Estimates - Charter Schools

Note: Coefficients correspond to interactions between the instrument and the number of years since the election. Error bars depict 95% confidence intervals. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The models furthermore include district, election year, years elapsed, and year fixed effects. Robust standard errors are clustered at the district-level.

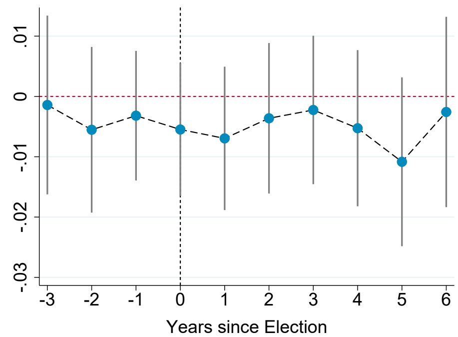
limited causal effects on teacher working conditions, with the exception of salaries. We see an increase in teacher salaries following the top of ballot assignment of an educator that appears to increase somewhat over time. The lag in the timing of salary effects is not unexpected given collective bargaining agreements are usually renegotiated every three years.

Figure 3 turns attention to the effects on district spending patterns. We examine the share of district expenditure allocated towards certified staff (teachers), classified staff, benefits, capital outlays, services and other operating expenses, all presented using the same scale to facilitate comparisons of effect size magnitudes. We also examine effects on the superintendent's salary. We do not find evidence of placebo causal effects prior to the focal election across any of the six outcomes. The share of post-election district expenditures allocated to certified staff increases, with statistically significant effects in years four and five post-election. This reflects the upward trend in teacher salaries captured earlier. Spending on capital outlays and services and other operating expenses both decrease over the same period, suggesting a shifting of resources away from these categories towards certified salaries. Superintendent salaries do not change, in contrast to the upward shift in teacher salaries as shown in the prior figure.

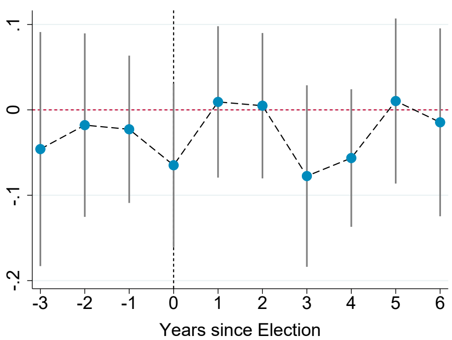
The previous results involve intermediate outcomes such as teacher working conditions and budget allocations that can in turn function as inputs into education production. Next we scrutinize the causal effects on student outcomes. Figure 4 examines effects on math and reading scores. We adapt equation (2) to



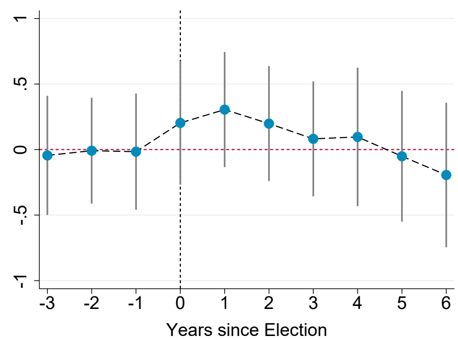
(a) Service Days



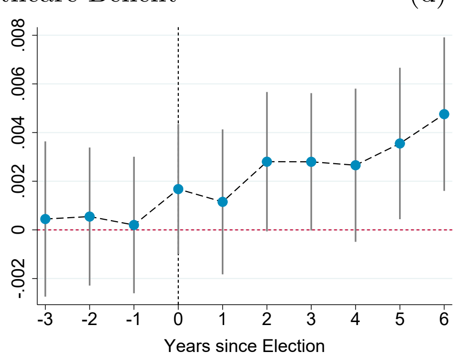
(b) MA Bonus Offered



(c) Log Max Healthcare Benefit



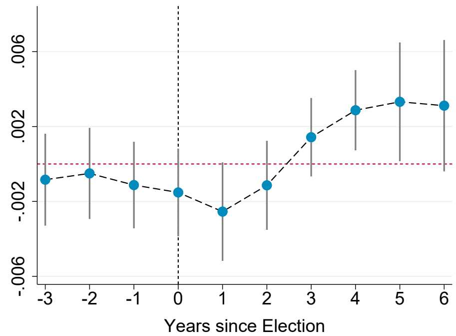
(d) Class size



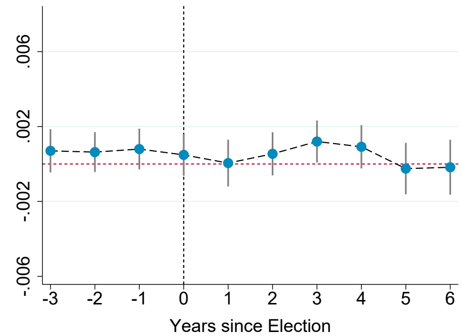
(e) Log Salary, BA+60

Figure 2. : Event-Study Causal Estimates - Teacher Working Conditions

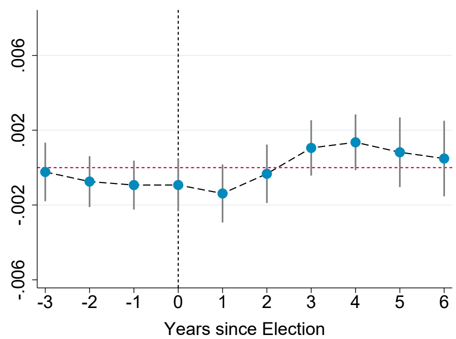
Note: Coefficients correspond to interactions between the instrument and the number of years since the election. Error bars depict 95% confidence intervals. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The models furthermore include district, election year, years elapsed, and year fixed effects. Robust standard errors are clustered at the district-level.



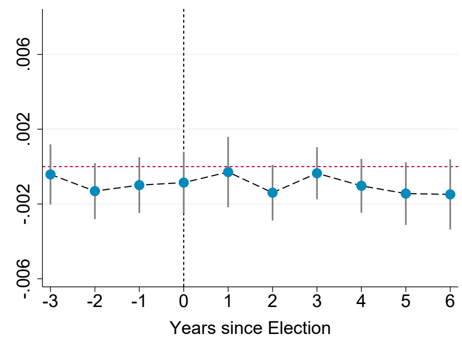
(a) Share of Exp.: Certified Salaries



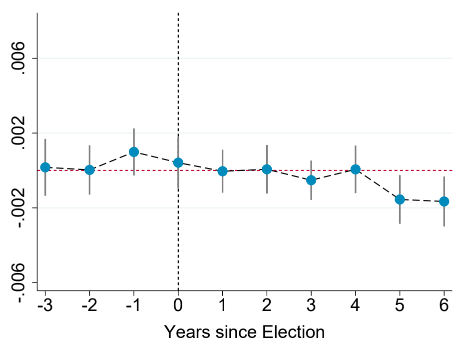
(b) Share of Exp.: Classified Salaries



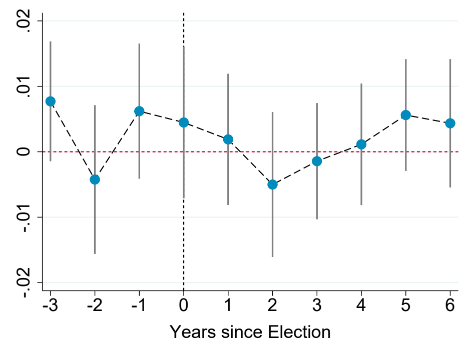
(c) Share of Exp.: Benefits



(d) Share of Exp.: Services & Other Expenses



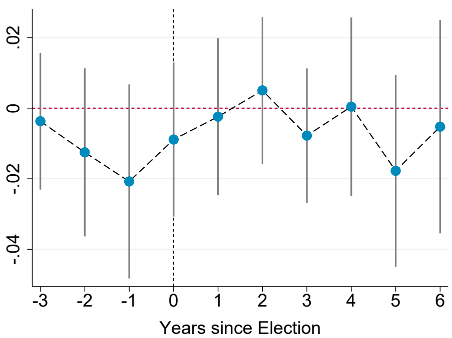
(e) Share of Exp.: Capital Outlays



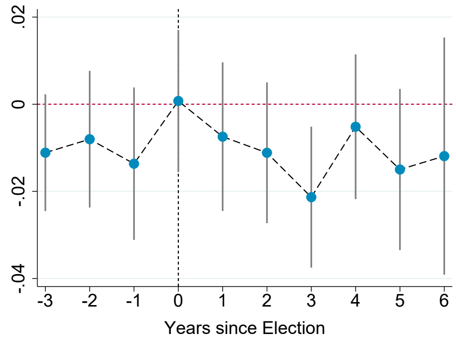
(f) Log Superintendent Salary

Figure 3. : Event-Study Causal Estimates - Expenditures

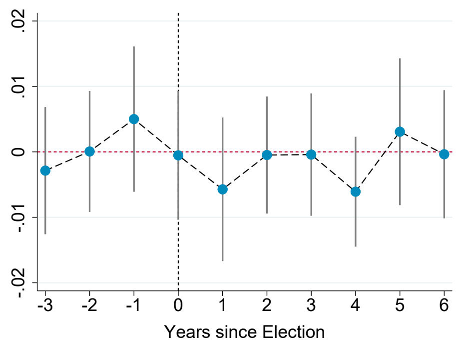
Note: Coefficients correspond to interactions between the instrument and the number of years since the election. Error bars depict 95% confidence intervals. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The models furthermore include district, election year, years elapsed, and year fixed effects. Robust standard errors are clustered at the district-level.



(a) Math Scores



(b) Reading Scores



(c) High School Graduation Rates

Figure 4. : Event-Study Causal Estimates - Student Outcomes

Note: Coefficients correspond to interactions between the instrument and the number of years since the election. Error bars depict 95% confidence intervals. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. All models furthermore include district, period, election year, and calendar year fixed effects. Robust standard errors are clustered at the district-level.

the school-by-grade-by-year level (the unit of observation for test scores), weighting each observed average by the number of test takers represented and including school and grade-by-year fixed effects in the estimation. We pool grade-level observations for the figures, but examine effects separately for elementary and middle schools in the next section. For both math and reading, we do not see evidence of placebo effects. There is also little evidence of causal effects on math. For reading scores, there is an initial downward trend in the three years after the focal election before the point estimates shift back towards zero. The average effect on elementary reading scores post-election is statistically significant.[37] Finally, we observe no effects prior to the election nor significant post-election effects on high school graduation.

Robustness. — We examine the robustness of our causal estimates to several alternative specification choices in this subsection. For purposes of brevity, Appendix Tables A5 to A8 report reduced-form intercept and slope coefficients that summarize effects over time as seen in event-study specifications. The intercept corresponds to the effect of the ballot order instrument on outcomes four years after the focal election while the slope describes the evolution of effects over time.

We begin by estimating the preferred intercept-slope model. Our preferred specification places restrictions on the controls and includes all electoral contests in the estimation sample – even those where randomized assignment of an educator to the top of the ballot is not possible. We examine robustness to these choices by first estimating a model that allows the effects of the controls over boards' tenure to be fully flexible. We then instead estimate effects on just the random assignment sample. The Appendix tables show effects that qualitatively match our main findings for each of these changes to the specification.

Next we consider estimates obtained from models that alter the control variable set, W_{br}. While the main estimates include controls for the number of seats available in the contest, its interaction with the share of educators in the candidate pool, and the number of contests held for seats on the board, we estimate a parsimonious model that controls only for the share of educators in the candidate pool.[38] Across the board, this model produces results that closely align with the main findings. In addition, we estimate specifications that control for the share of educators in the candidate pool in a more flexible fashion than do the main estimates. Rather than inputting the share of educators in the candidate pool linearly, we estimate models with quadratics and cubics of the number of candidates overall and the number of educator candidates (and their interaction). These results are nearly identical to our preferred specification.

Finally, we consider robustness of the estimates to controlling for lagged events. For each school board we create indicators for a top-of-the-ballot educator in an

[37]See Appendix Table A8.

[38]We also include indicators for having no educators or only educators in the candidate pool in the parsimonious model.

election that occurred from one up to five years prior. Likewise, we create variables summarizing the educator share of the candidate pool in prior elections. These variables effectively control for the quasi-random assignment of an educator to the top of the ballot in prior years. We also estimate models that exclude all boards from the sample in which an educator was top-listed in an election during the previous five years. Including these lagged controls produce results that are very similar to our main findings. The point estimates are likewise similar using the restricted sample, but the smaller sample reduces precision for several outcomes. For example, the restricted sample yields a 0.006 decline in the share of enrollment in charter schools four years post-election. This point estimate is similar to the 0.005 in our main specification, but it is not statistically significant.

Treatment Effects. — We estimate the effects of an additional educator elected to the school board in this subsection. These two-stage least squares estimates correspond to equation (5).

Table 6 reports estimated treatment effects on the share of district enrollment in charter schools and the number of charter schools in the district. Each additional educator elected to the school board causes a three percentage point decline in the charter enrollment share and 1.3 fewer charter schools in the district four years after the election. These effects are statistically different from zero at the 90% level. These are sizable point estimates in a state with an active charter sector serving at least one out of every ten public school students.

Table 6—: Treatment Effect Estimates - Charter Schools

	Share of Enrollment (1)	No. of Charters (2)
Additional Educator on School Board		
Effect 4-Years Post-Election	-0.032	-1.312
	(0.018)	(0.774)
Effect Slope	-0.006	-0.445
	(0.009)	(0.300)
Observations	40,975	40,975

Note: Robust standard errors are clustered at the district level. The sample is a stacked dataset of periods -3 through 6 for each school board. Models use an indicator for being a first-listed educator to instrument for the number of educators newly elected to the school board, with the intercept coefficient showing the causal effect of an additional educator four years post-election, and the slope coefficient showing the trend of effects post-election. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The models furthermore include district, election year, period, and year fixed effects.

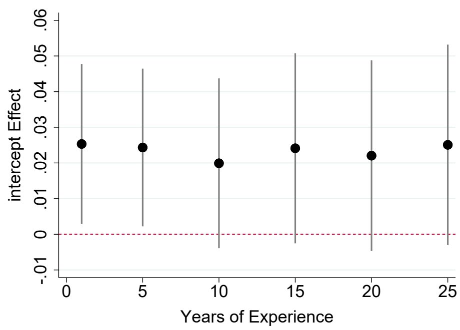
We consider treatment effects on the working conditions of teachers in Table 7. As with the reduced-form results, we find limited evidence of effects on service days, benefits, or class size. However, the results indicate that an additional educator on the school board leads to a 2% increase in salaries four years post-election. To put this magnitude in perspective, the average California teacher salary in 2013-14 was around $65,000 and teacher pay increased around 1% annually (in real terms) on average over the past twenty years. This finding motivates further examination of whether salary effects apply across-the-board or vary by education and experience. Figure 5 indicates that the causal increase in teacher salaries applies broadly across twenty-four of the most common education and experience combinations in the teacher salary schedule. Given the size of standard errors, we cannot reject the null hypothesis that causal effects are the same across cells. Additional educators on the school board lead to a general increase in teacher salaries in the district that is not concentrated among novice or experienced teachers.

Table 7—: Treatment Effect Estimates - Teacher Working Conditions

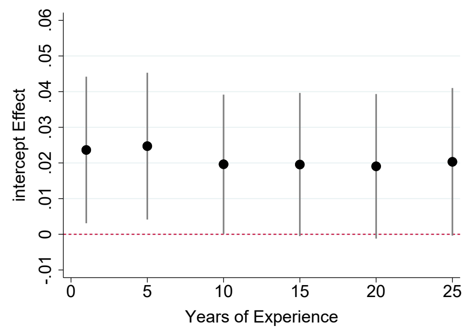
	Service Days (1)	M.A. Bonus Offered (2)	Log Max Health Benefit (3)	Class Size (4)	Log Salary: BA+60 (5)
Additional Educator on School Board					
Effect 4-Years Post-Election	-0.133	-0.037	-0.151	0.302	0.024
	(0.360)	(0.042)	(0.237)	(1.292)	(0.011)
Effect Slope	0.019	-0.000	-0.018	-0.699	0.004
	(0.217)	(0.017)	(0.135)	(0.541)	(0.004)
Observations	37,380	37,380	37,380	38,997	227,795

Note: Robust standard errors are clustered at the district level. The sample is a stacked dataset of periods -3 through 6 for each school board. Column 6 further stacks the dataset across six experience levels (1, 5, 10, 15, 20, and 25 years). Models use an indicator for being a first-listed educator to instrument for the number of educators newly elected to the school board, with the intercept coefficient showing the causal effect of an additional educator four years post-election, and the slope coefficient showing the trend of effects post-election. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The models furthermore include district, election year, period, and year fixed effects.

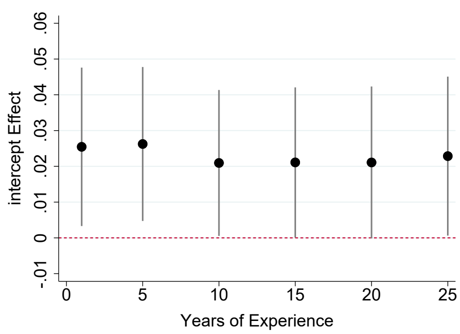
The increase in teacher salaries is accompanied by a shift in expenditures towards certified staff salaries. Table 8 shows that an additional educator on the board increases the share of spending on certified salaries by 1.3 percentage points four years post-treatment, and decreases spending on services and capital outlays by 0.7 and 0.6 points, respectively. The positive slope effect on certified staff salaries is partially offset by a decrease in the trend of spending allocations to capital outlays over this period. Notably, the significant coefficient on the slope term suggests a growing share of expenditures are allocated towards certified salaries in the post-election period, which does not appear to be driven by teacher salary increases (see slope term in Table 7). One likely explanation is the trajectory of teachers employed. Table A9 shows that even though there is no increase in full-time equivalent teachers four years post-election, the significant rise in the



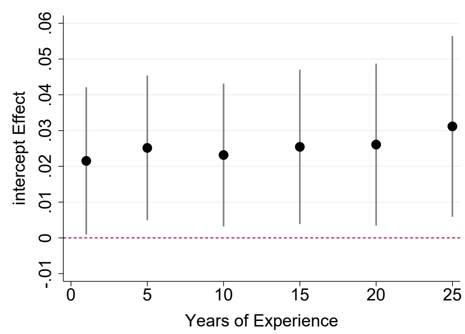
(a) BA+30



(b) BA+45



(c) BA+60



(d) BA+75

Figure 5. : Salary Effects 4-Years Post-Election by Education and Experience Level

Note: Coefficients show the causal effect of an additional educator on the school board four years post-election. Error bars depict 95% confidence intervals. The dataset is stacked across six experience levels (1, 5, 10, 15, 20, and 25 years) for each education level. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The models furthermore include district, election year, years elapsed, and year fixed effects. Robust standard errors are clustered at the district-level.

slope term mirrors that of spending on certified staff. Finally, we do not find evidence for impacts on superintendent salary, which is consistent with the event study results.

Table 8—: Treatment Effect Estimates - Expenditures

	Share of Expenditures On:					
	Certified Salaries (1)	Classified Salaries (2)	Benefits (3)	Services & Other Exp. (4)	Capital Outlays (5)	Log Supt. Salary (6)
Additional Educator on School Board						
Effect 4-Years Post-Election	0.013	0.002	0.004	-0.007	-0.006	0.011
	(0.008)	(0.004)	(0.005)	(0.004)	(0.003)	(0.026)
Effect Slope	0.010	-0.001	0.003	-0.001	-0.003	0.012
	(0.004)	(0.001)	(0.002)	(0.002)	(0.002)	(0.014)
Observations	43,569	43,569	43,569	43,569	43,569	36,791

Note: Robust standard errors are clustered at the district level. The sample is a stacked dataset of periods -3 through 6 for each school board. Models use an indicator for being a first-listed educator to instrument for the number of educators newly elected to the school board, with the intercept coefficient showing the causal effect of an additional educator four years post-election, and the slope coefficient showing the trend of effects post-election. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The models furthermore include district, election year, period, and year fixed effects.

Table 9 presents estimates for the effect of additional educators on student achievement and attainment.[39] Despite negative coefficients across elementary and middle school test scores four-years after the focal election, point estimates for the treatment effect are all insignificant. However, we can rule out a large positive effect (i.e. $\geq 0.05\sigma$) on reading from the 95% confidence interval. We similarly find no effects of an educator elected to the school board on high school graduation.

IV. Interpretation and Mechanisms

In this section, we consider the interpretation of our empirical findings by investigating the role of several mechanisms. We first explore the heterogeneity of causal effects according to how many educators are on the school board. We then examine the extent to which dynamics that impact future electoral outcomes contribute to the longer-run effects. Lastly, we discuss what the findings reveal about the influence of teachers' unions on school boards.

[39]These effects are estimated by adapting equation (5) to the school-by-grade-by-year level (the unit of observation for test scores). The estimation again weights the averages by the number of test takers and includes school and grade-by-year fixed effects.

Table 9—: Treatment Effect Estimates - Student Outcomes

	Elementary		Middle		
	Math	Reading	Math	Reading	HS Graduation
	(1)	(2)	(3)	(4)	(5)
Additional Educator on School Board					
Effect 4-Years Post-Election	-0.076	-0.170	-0.109	-0.106	-0.009
	(0.103)	(0.113)	(0.096)	(0.073)	(0.021)
Effect Slope	-0.049	0.008	0.024	0.026	0.008
	(0.068)	(0.053)	(0.059)	(0.036)	(0.012)
Observations	1,263,925	1,263,574	488,559	503,140	25,934

Note: Robust standard errors are clustered at the district level. The sample is a stacked dataset of periods -3 through 6 for each school board. Models use an indicator for being a first-listed educator to instrument for the number of educators newly elected to the school board, with the intercept coefficient showing the causal effect of an additional educator four years post-election, and the slope coefficient showing the trend of effects post-election. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The models also include district, election year, period, and year fixed effects.

A. Heterogeneity in School Board Composition

While our empirical approach identifies the average effect of electing an additional educator to the school board, it is plausible that this marginal effect depends on how many other educators also hold seats on the board. Heterogeneous effects may derive from the additional educator shifting the identity of the median board "voter" for a given issue and influencing board decisions through deliberations and agenda-setting. To examine these possibilities, we adapt our slope-intercept 2SLS specification to allow for heterogeneity according to whether one of the non-contested board seats is currently held by an educator.

The results in Appendix Tables A11 to A14 suggest that the presence of a pre-existing educator on the board is of limited importance. On the one hand, the negative effects on charter school enrollment appear to be amplified when at least one of the board seats not up for election in the current cycle is held by an educator. However, the interaction effect in this case and across other variables such as teacher pay are not statistically significant. The two exceptions to this pattern are for spending on services and capital outlays: while the marginally-elected educator tends to reduce district spending in these categories, those effects are actually somewhat attenuated when the board already includes another educator.

B. Electoral Dynamics

Our empirical approach traces out the consequences of the quasi-random election of an additional educator to the board on subsequent outcomes. As more formally discussed in the Appendix, one channel through which these effects may manifest over the longer-run is via electoral dynamics: the election of an additional educator impacts the board's composition during their term, but it also may have consequences for what happens in subsequent elections. Dynamics may arise from incumbency effects or because the behavior of candidates, voters, or other parties is endogenously affected (e.g. more educators run for seats).

We adapt a version of our event-study specification, equation (2), to examine impacts on *electoral* outcomes and characteristics over time. For each distinct school board $j-t$, we group elections into four categories based on their timing relative to t: elections that occur *prior* to t, the focal elections at t, the *next* elections (held one to three years hence), and elections subsequent to next (three plus years later). As before, prior impacts serve as a placebo test. We estimate effects of the ballot order instrument on the total number of educators who win seats on the board, the share of educators in the candidate pool, and the aggregate share of educators on the school board.

Table 10—: Reduced-Form Causal Estimates – Political Dynamics

	Total No. of Educator Winners (1)	Share of Educators in Candidate Pool (2)	Share of Educators on Board (3)
Top of Ballot Educator			
Effect on prior elections	-0.021	-0.011	-0.009
	(0.034)	(0.009)	(0.008)
Effect on this election	0.127		0.021
	(0.029)		(0.008)
Effect on next election	-0.088	-0.013	0.014
	(0.038)	(0.010)	(0.008)
Effect on elections after next	0.039	0.034	-0.006
	(0.040)	(0.011)	(0.010)
Observations	12,651	8,154	12,651

Note: Robust standard errors are clustered at the district level. The sample is a stacked dataset including elections from three years prior up to five years subsequent for each school board, except column (2) which excludes the year of this election. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The models also include district, election year, period, and year fixed effects.

Table 10 examines the dynamic political implications of random assignment of an educator to the top of the ballot. Column (1) finds that the ballot order instrument increases the number of educators who win the current election by 0.13, which is consistent with our first stage results. However, the instrument leads to a 0.09 *reduction* in the number of educators that win the next election, while not inducing changes in the number of educator winners in previous elections (as expected) or in elections beyond the next cycle.

Does this potentially represent electoral "backlash" against educator candidates? Column (2) looks at the share of educators in the candidate pool in the next election, finding that educators are as in abundance as before. Rather, educators are less likely to win the elections immediately following the current one. Column (3) shows the aggregate consequences: after two cycles of elections (typically four years on), educators comprise no greater share of the school board than they did prior to the current electoral cycle. An important implication deriving from these political dynamics is that the causal effects of electing an additional educator would be even larger in the absence of the reduced likelihood an educator wins the next election. In other words, the causal effects that we estimate are actually somewhat attenuated in the long-run by the endogenous electoral response to the marginal educator's election.

C. The Role of Teachers' Unions

We focus on educators on the school board because these members may be positioned to bring relevant human capital to district leadership. For instance, educators may have additional information about education production and can apply this expertise towards improving student outcomes. While we find impacts on some inputs, the lack of evidence for student gains raises the question why educators' expertise does not appear to translate into improved education quality.

One possibility is that, although democratically-accountable to voters, educators on school boards tend to instead advance the priorities of teachers' unions. A large literature on rent-seeking models emphasizes that influence from pressure or interest groups may distort policymaking (Becker, 1983; Rowley, Tollison and Tullock, 1988). In the education context, unions optimize outcomes for teachers by negotiating for improved compensation and working conditions (Moe, 2009), potentially at the cost of lower education quality (Hoxby, 1996; Lovenheim and Willén, 2019; Cook, Lavertu and Miller, 2020). Teachers' unions are able to advocate for their membership base via collective bargaining as well as the electoral process by spending substantial resources to elect candidates favorably disposed to union interests (Hess and Leal, 2005; Moe, 2006).

In this subsection, we analyze supplemental survey data on whether educators are more likely to be union-endorsed than other school board members, consistent with educators shifting district policymaking towards union priorities. We then analyze our results within the larger context of the existing literature on the causal impacts of teachers' unions. While inconclusive regarding effects on education quality, our findings suggest that school boards may be an important causal channel through which teachers' unions influence local education production.

Educators and Teachers' Union Alignment. — Are educator school board members more likely to be union endorsed? To answer this, we draw upon unique survey data from California school board members. 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 that they received in their most recent election (Grissom, 2006).[40] Member responses to the survey allow us to examine the empirical relationship between professional experience as an educator and alignment with union priorities.

Table 11—: Educators and Union Endorsement

	Union Endorsed: Board Member	Union Endorsed: 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

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

We first examine the association at the individual board member-level of having an educator background with receiving union endorsement. Column (1) in Table 11 reveals that former educators on the school board are over 40 percent more likely than board members from other professions to receive union endorsement. Second, we examine the association at the school board-level. Column (2) shows that an additional educator on a modal five-member board raises the share of union-endorsed members by over 4 percentage points, or 20% of the baseline level. The California District School Board Survey evidence thus points to a strong positive association between professional experience in education and alignment with union priorities.[41]

The Effects of Teachers' Unions. — The pattern of our causal findings is broadly consistent with predictions from rent-seeking models of union influence and with prior empirical evidence. For example, previous research estimates that unionization raises teacher salaries by 4-5% (e.g. Hoxby 1996; West and Mykerezi 2011), a magnitude that is equivalent to our estimated effect of two additional educators on the school board. The reallocation of expenditures toward certified staff salaries is consistent with salary increases as a core union objective.

[40] Full details of the survey methodology and and descriptive details are provided in Grissom (2007).

[41] There is also recent survey evidence that educators on school boards hold policy views that are more aligned with teachers' unions. Hartney (2020) reports that whether a board member is an educator strongly predicts support for teacher pay increases (as well as lack of support for performance pay and charter schools) even after conditioning on demographics and partisan ideology.

Our results also document a causal reduction in charter schooling, echoing prior research showing that greater union strength predicts less support for legislation favoring the charter sector (Stoddard and Corcoran, 2007).

We find no causal effects of an additional educator on the school board on high school graduation. This echoes null effects on high school dropouts found by Lovenheim (2009) and largely supported by Lovenheim and Willén (2019), although the literature is mixed on this front; Hoxby (1996) finds increased dropout rates using an alternative measure of unionization.[42] Our causal estimates for academic achievement are also in line with most empirical studies on unions, which similarly document insignificant or modestly sized negative test score effects from unionization or greater union strength (Cowen and Strunk, 2015).[43]

A recent paper, Lovenheim and Willén (2019), argues that the primary mechanism through which unions negatively affect student outcomes in the long-run is through lower schooling quality. They use cohort-based differences-in-difference event study models to document only modest reductions in educational attainment and measurable decreases in non-cognitive skills as a result of exposure to duty-to-bargain laws. One question this raises in interpreting our findings is whether the effects on student achievement we estimate are due to reduced productivity of school inputs. While we lack student-level data to estimate effects on school quality directly, we can instead examine whether the election of an educator changes the composition of public school students. To do so, we pool school-grade-year data on student demographics and school-year data on free and reduced price lunch status.

Table A10 shows that an additional educator raises the share of non-white students in public schools by 5 to 7 percentage points four years post-election. There is also a positive 2 percentage point effect on the share of students eligible for free and reduced lunch. All of the estimated effects are statistically insignificant, however. Nonetheless, the changes in student test scores we estimate likely stem in part from an increase in the disadvantaged student composition. We thus view our results as largely ambiguous regarding whether educators reduce education quality. However, there are several limitations of this conclusion, including lack of precision in the estimates, that point to directions for future work.

V. Conclusion

A major focus in economics is identifying and quantifying the importance of various inputs to the production of human capital (Becker, 1993). However, little

[42] Both papers explicitly address the endogeneity of unionization by relying on the passage of state laws that facilitate collective bargaining, although differences remain in approach and context. Hoxby (1996) uses a Census of Government unionization measure, while Lovenheim (2009) measures unionization through certification dates. The latter restricts to three midwestern states while the former includes all states.

[43] Lott and Kenny (2013), for instance, show that student proficiency rates are lower in states with stronger unions.

work to date has quantified the role of locally-elected school boards in education production, despite their significant responsibilities that include collective bargaining and leadership recruitment and evaluation. We address this gap by developing a new empirical approach based on randomized ballot order to estimate the causal influence of professional educators elected to school boards. We apply this strategy, which can be adapted to other dimensions of candidates for office, to provide the first evidence regarding local school boards' causal influence on district resource allocation and student performance.

Relative to other board members, we find that educators elected to school boards reduce charter schooling in the district and increase teacher salaries. An additional educator leads to one fewer charter school and around 2% increase in pay on average. We find that this salary increase generally applies across-the-board and is partly financed by a corresponding reduction in district expenditures towards capital outlays and miscellaneous services. We do not find effects on other variables characterizing teacher working conditions, such as class size, or on superintendent salary. We similarly find no evidence of impacts on high school graduation rates. The estimated treatment effects on student performance are negative, but are not statistically different from zero.

These findings suggest that educators' professional expertise on boards does not translate into improvements in student learning. This may be because the objectives of educators elected to the school board are misaligned with voters' due to the electoral influence of teachers' unions. Our results are consistent with a rent-seeking framework, in which representation of union interests predicts higher teacher salaries and potentially negative effects on student performance (Hoxby, 1996; Moe, 2006). Evidence from school board member survey data from California indicate a strong positive association between professional experience in education and alignment with union priorities. Although the effect of educators on education quality remains inconclusive due to data limitations, our findings suggest that school boards may be an important causal mechanism for the influence of teachers' unions on local education (Cowen and Strunk, 2015).

Our work points to several avenues for future research. A valuable next step would be to collect and analyze candidate-level records of union endorsement. These records would facilitate separating out the influence of educators' human capital on education production from their possible alignment with teachers' unions. Likewise, shifting from aggregate school-level to administrative student records would enable disentangling impacts on student sorting from their effects on education quality. Future work should also focus on broader dimensions of student skills and behavior, such as socioemotional attributes and civic engagement. Finally, our ballot order-based instrument provides a new approach to inferring how the characteristics of elected officials causally affect outcomes.

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Online Appendix
"School Boards and Education Production: Evidence from Randomized Ballot Order"

By Ying Shi and John D. Singleton

December 2021

I. Tables and Figures

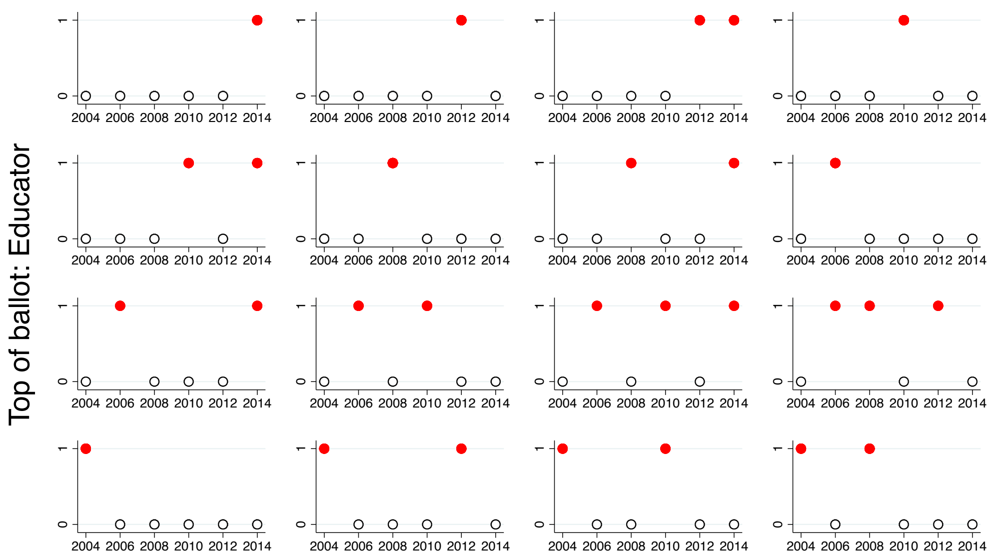

Figure A1. : Within-District Variation in Having Educator at Top of Ballot, 2004-2014 Elections

Note: The graph shows the sixteen most common patterns for even-year elections in 2004-2014. Red dots denote having an educator candidate at the top of the ballot in the district for a given year, while white dots denote having a non-educator candidate at the top.

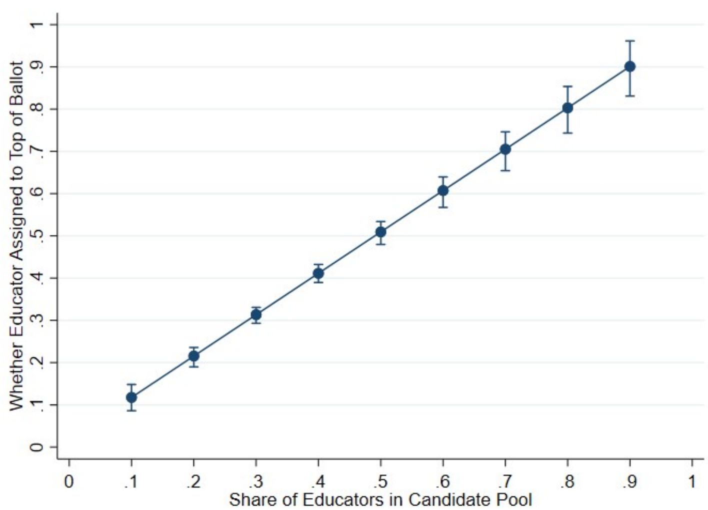

Figure A2. : Test of Randomized Assignment to Top of Ballot

Note: Figure reports empirical means for whether an educator is assigned to the top of the ballot (and associated confidence intervals) by share of educators in the candidate pool across election contests. Predictions are obtained from a non-parametric regression with Epanechnikov kernel and bandwidth chosen by cross-validation. Confidence intervals are calculated by bootstrap (200 draws).

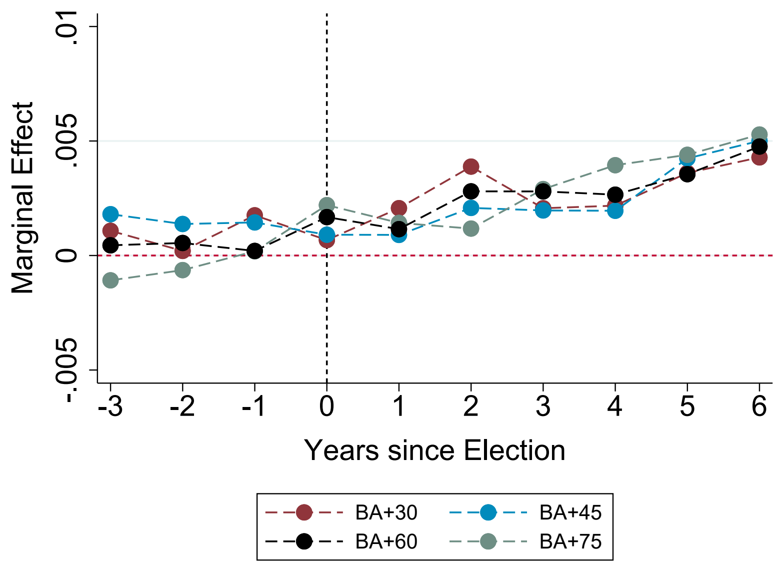

Figure A3. : Event-Study Causal Estimates - Log Salary by Column

Note: The dataset is stacked across six experience levels (1, 5, 10, 15, 20, and 25 years) for each education level. Coefficients correspond to interactions between the instrument and the number of years since the election. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The models furthermore include district, election year, years elapsed, and year fixed effects.

Table A1—: Characteristics of Full Sample and Randomized Assignment Sample

	Full Sample	Subsample	
	Mean	Mean	SE
Panel A. School District Characteristcs			
Total Enrollment	9736	12041	(1005)
Share White	0.41	0.38	(0.01)
Share Black or Hispanic	0.47	0.49	(0.01)
Share Asian	0.08	0.09	(0.01)
Share FRP Lunch	0.51	0.52	(0.01)
Urban	0.59	0.68	(0.02)
Panel B. Charter Schooling			
Share of Enrollment	0.04	0.04	(0.00)
No. of Charters	1.00	1.17	(0.19)
Panel C. Teacher Working Conditions			
Service Days	184	184	(0.08)
MA Bonus Offered	0.58	0.52	(0.02)
Max Health Contribution	9149	9609	(164)
Pupils per Teacher	27.46	27.78	(0.23)
BA+60 Teacher Salary	59063	60530	(344)
Panel D. School District Expenditures			
Certified Salaries	0.47	0.47	(0.00)
Classified Salaries	0.15	0.15	(0.00)
Benefits	0.18	0.18	(0.00)
Services & Other Exp.	0.10	0.10	(0.00)
Capital Outlay	0.01	0.01	(0.00)
Superintendent Salary	142648	153316	(2095)
Panel E. Student Outcomes			
Reading Scores	0.07	0.06	(0.03)
Math Scores	-0.01	-0.02	(0.03)
HS Graduation	0.79	0.79	(0.01)
Panel F. Election and Board Characteristics			
No. of Open Seats in Contest	2.12	2.28	(0.03)
No. of Candidates in Contest	4.10	4.67	(0.07)
Share of Candidates: Educators	0.17	0.34	(0.00)
Share of Board: Educators	0.19	0.30	(0.01)
No. of Contests	4550	2165	
No. of School Districts	859	652	

Note: Table reports averages for sample of all electoral contests and for randomized assignment subsample (contests in which at least one educator is an educator but not all candidates are educators). Third column reports district clustered standard errors of the subsample means.

Table A2—: Within-District Variation in Ballot Order Instrument

No. of Election Years With First-Listed Educator	No. of districts (1)	Pct. (2)	Cum. Pct. (3)
0	431	50.17	50.17
1	240	27.94	78.11
2	107	12.46	90.57
3	43	5.01	95.58
4 or more	38	4.42	100.00
Total	859	100.00	

Note: Table shows the number of election years in which each of the 859 districts in our sample had an educator at the top of their electoral ballot. Election years span 1998-2015.

Table A3—: Validity: Prior Events

	Top of Ballot Educator (current contest)				
	(1)	(2)	(3)	(4)	(5)
Share of Educators in Candidate Pool	0.977	1.041	1.051	0.998	1.012
	(0.062)	(0.073)	(0.073)	(0.085)	(0.090)
Top of Ballot Educator, *t-2*		-0.039	-0.027	-0.009	0.010
		(0.026)	(0.027)	(0.031)	(0.034)
Share of Educators in Candidate Pool, *t-2*			-0.063		-0.099
			(0.061)		(0.074)
Top of Ballot Educator, *t-4*				0.048	0.047
				(0.030)	(0.030)
Share of Educators in Candidate Pool, *t-4*					0.014
					(0.076)
Observations	2,165	1,522	1,522	1,075	1,075

Note: Robust standard errors are clustered at the district level. Sample in column (1) includes all contests in which the candidate pool is neither only educators or without any educators. Columns (2) and (3) are contests in which an election two years prior is also observed; (4) and (5) contests in which elections two and four years prior are observed.

Table A4—: Evidence of Treatment: Down-Ballot Effects

	No. of Winners Who are Educators		Share of Board: Educators	
	(1)	(2)	(3)	(4)
Top of Ballot Educator	0.141	0.139	0.023	0.023
	(0.029)	(0.031)	(0.008)	(0.009)
Other Top Tier Educator		-0.007		-0.003
		(0.029)		(0.007)
Observations	4448	4448	4448	4448
F-statistics	24.21	12.50	7.895	4.191

Note: Robust standard errors are clustered at the district level. Other top tier educators are defined as educators who occupy a ballot position that is 1) not at the top and 2) at or below the number of open seats. For instance, an educator who is second on the ballot in an electoral contest with two open seats would fall into this category, but an educator at the top or third on the ballot would not. All specifications include separate district, election year, and year fixed effects. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year.

Table A5—: Robustness: Causal Estimates - Charter Schools

Top of Ballot Educator	Share of Enrollment (1)	No. of Charters (2)
Main Model		
Effect 4-Years Post-Election	-0.005	-0.200
	(0.003)	(0.110)
Effect Slope	-0.001	-0.069
	(0.001)	(0.041)
Observations	40,975	40,975
Flexible Model		
Effect 4-Years Post-Election	-0.005	-0.200
	(0.003)	(0.110)
Effect Slope	-0.001	-0.069
	(0.001)	(0.041)
Observations	40,975	40,975
Main Model - Random Assignment Sample		
Effect 4-Years Post-Election	-0.006	-0.222
	(0.003)	(0.122)
Effect Slope	-0.001	-0.071
	(0.001)	(0.042)
Observations	19,478	19,478
Parsimonious Model		
Effect 4-Years Post-Election	-0.005	-0.193
	(0.003)	(0.105)
Effect Slope	-0.001	-0.062
	(0.001)	(0.040)
Observations	40,975	40,975
Main Model - Quadratic Controls for No. of Candidates and Educator Candidates		
Effect 4-Years Post-Election	-0.005	-0.178
	(0.003)	(0.098)
Effect Slope	-0.001	-0.059
	(0.001)	(0.036)
Observations	40,975	40,975
Main Model - Cubic Controls for No. of Candidates and Educator Candidates		
Effect 4-Years Post-Election	-0.005	-0.189
	(0.003)	(0.104)
Effect Slope	-0.001	-0.064
	(0.001)	(0.039)
Observations	40,975	40,975
Main Model - Controls for Lags of Events		
Effect 4-Years Post-Election	-0.005	-0.211
	(0.003)	(0.121)
Effect Slope	-0.001	-0.074
	(0.001)	(0.042)

Table A5—: Robustness: Causal Estimates - Charter Schools

	Share of Enrollment	No. of Charters
Top of Ballot Educator	(1)	(2)
Observations	40,975	40,975
Main Model - Excludes Boards with Recent Top-Listed Educator		
Effect 4-Years Post-Election	-0.006	-0.166
	(0.004)	(0.143)
Effect Slope	-0.001	-0.053
	(0.001)	(0.059)
Observations	31,224	31,224

Note: Robust standard errors are clustered at the district level. The sample is a stacked dataset of periods -3 through 6 for each school board. Covariates in the main specification include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. For each control variable we estimate a level effect, an interaction with a post-election indicator, and an interaction with both the post-election indicator and the period (linear trend). The flexible model interacts all control variables listed above with post-election period intercepts. The parsimonious model restricts the set of control variables to the share of educators in the candidate pool, indicators for having no educators or all educators in the candidate pool. The random assignment sample excludes electoral contests that have no educator candidates or all educator candidates. The next two specifications control for the quadratics (cubics) of the number of candidates and candidates who are educators, and their interaction, instead of a linear share of educators. The next specification controls for lags of the instrument and share of educators in the candidate pool going back five years. The final specification restricts to school boards that have not had a top-of-the-ballot educator in the previous five years. All models include district, period, election year, and calendar year fixed effects.

Table A6—: Robustness: Causal Estimates - Teacher Working Conditions

Top of Ballot Educator	Service Days (1)	M.A. Bonus Offered (2)	Log Max Health Benefit (3)	Class Size (4)	Log Salary: BA+60 (5)
Main Model					
Effect 4-Years Post-Election	-0.017	-0.005	-0.020	0.035	0.003
	(0.046)	(0.005)	(0.030)	(0.195)	(0.001)
Effect Slope	0.002	-0.000	-0.003	-0.091	0.001
	(0.025)	(0.002)	(0.015)	(0.072)	(0.000)
Observations	37,380	37,380	37,380	38,997	227,795
Flexible Model					
Effect 4-Years Post-Election	-0.016	-0.005	-0.019	0.031	0.003
	(0.046)	(0.005)	(0.030)	(0.195)	(0.001)
Effect Slope	0.002	-0.000	-0.003	-0.094	0.001
	(0.025)	(0.002)	(0.015)	(0.072)	(0.000)
Observations	37,380	37,380	37,380	38,997	227,795
Main Model - Random Assignment Sample					
Effect 4-Years Post-Election	-0.040	-0.006	-0.015	0.051	0.003
	(0.049)	(0.005)	(0.031)	(0.191)	(0.001)
Effect Slope	0.003	-0.000	-0.004	-0.093	0.000
	(0.024)	(0.002)	(0.015)	(0.070)	(0.000)
Observations	18,448	18,448	18,448	18,866	112,149
Parsimonious Model					
Effect 4-Years Post-Election	-0.018	-0.005	-0.019	0.034	0.003
	(0.047)	(0.005)	(0.030)	(0.196)	(0.001)
Effect Slope	-0.001	-0.000	-0.002	-0.093	0.001
	(0.025)	(0.002)	(0.015)	(0.072)	(0.000)
Observations	37,380	37,380	37,380	38,997	227,795
Main Model - Quadratic Controls for No. of Candidates and Educator Candidates					
Effect 4-Years Post-Election	-0.018	-0.005	-0.014	0.065	0.003
	(0.045)	(0.005)	(0.030)	(0.196)	(0.001)
Effect Slope	-0.001	-0.000	0.001	-0.095	0.001
	(0.025)	(0.002)	(0.015)	(0.072)	(0.000)
Observations	37,380	37,380	37,380	38,997	227,795
Main Model - Cubic Controls for No. of Candidates and Educator Candidates					
Effect 4-Years Post-Election	-0.018	-0.005	-0.012	0.049	0.003
	(0.046)	(0.005)	(0.030)	(0.194)	(0.001)
Effect Slope	-0.001	-0.000	-0.000	-0.094	0.001
	(0.024)	(0.002)	(0.015)	(0.071)	(0.000)
Observations	37,380	37,380	37,380	38,997	227,795
Main Model - Controls for Lags of Events					
Effect 4-Years Post-Election	-0.011	-0.006	-0.017	-0.005	0.004
	(0.050)	(0.006)	(0.033)	(0.211)	(0.001)
Effect Slope	0.007	-0.000	-0.003	-0.094	0.001

Table A6—: Robustness: Causal Estimates - Teacher Working Conditions

Top of Ballot Educator	Service Days (1)	M.A. Bonus Offered (2)	Log Max Health Benefit (3)	Class Size (4)	Log Salary: BA+60 (5)
	(0.025)	(0.002)	(0.016)	(0.073)	(0.000)
Observations	37,380	37,380	37,380	38,997	227,795
Main Model - Excludes Boards with Recent Top-Listed Educator					
Effect 4-Years Post-Election	-0.073	-0.006	-0.000	-0.100	0.004
	(0.058)	(0.007)	(0.048)	(0.247)	(0.002)
Effect Slope	0.001	0.000	0.015	-0.105	0.000
	(0.032)	(0.002)	(0.020)	(0.084)	(0.001)
Observations	27,897	27,897	27,897	29,473	170,080

Note: Robust standard errors are clustered at the district level. The sample is a stacked dataset of periods -3 through 6 for each school board. Covariates for the main specification include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The random assignment sample excludes electoral contests that have no educator candidates or all educator candidates. The flexible model interacts all control variables listed above with post-election period intercepts. The parsimonious model controls for the share of educators in the candidate pool, indicators for having no educators or all educators in the candidate pool. The next two specifications control for the quadratics (cubics) of the number of candidates and candidates who are educators, and their interaction, instead of a linear share of educators. The next specification controls for lags of the instrument and share of educators in the candidate pool going back five years. The final specification restricts to school boards that have not had a top-of-the-ballot educator in the previous five years. All models include district, period, election year, and calendar year fixed effects.

Table A7—: Robustness: Causal Estimates - Expenditures

	Share of Expenditures On:					
	Certified Staff	Classified Staff	Benefits	Services & Other Exp.	Capital Outlays	Log Supt. Salary
Top of Ballot Educator	(1)	(2)	(3)	(4)	(5)	(6)
Main Model						
Effect 4-Years Post-Election	0.002	0.000	0.001	-0.001	-0.001	0.002
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.003)
Effect Slope	0.001	-0.000	0.000	-0.000	-0.000	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)
Observations	43,569	43,569	43,569	43,569	43,569	36,791
Flexible Model						
Effect 4-Years Post-Election	0.002	0.000	0.001	-0.001	-0.001	0.002
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.003)
Effect Slope	0.001	-0.000	0.000	-0.000	-0.000	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)
Observations	43,569	43,569	43,569	43,569	43,569	36,791
Main Model - Random Assignment Sample						
Effect 4-Years Post-Election	0.002	0.000	0.001	-0.001	-0.001	0.001
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.003)
Effect Slope	0.001	-0.000	0.000	-0.000	-0.000	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)
Observations	20,742	20,742	20,742	20,742	20,742	18,384
Parsimonious Model						
Effect 4-Years Post-Election	0.002	0.000	0.001	-0.001	-0.001	0.002
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.003)
Effect Slope	0.001	-0.000	0.000	-0.000	-0.000	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)
Observations	43,569	43,569	43,569	43,569	43,569	36,791
Main Model - Quadratic Controls for No. of Candidates and Educator Candidates						
Effect 4-Years Post-Election	0.002	0.000	0.001	-0.001	-0.001	0.002
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.003)
Effect Slope	0.001	-0.000	0.000	-0.000	-0.000	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)
Observations	43,569	43,569	43,569	43,569	43,569	36,791
Main Model - Cubic Controls for No. of Candidates and Educator Candidates						
Effect 4-Years Post-Election	0.002	0.000	0.001	-0.001	-0.001	0.002
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.003)
Effect Slope	0.001	-0.000	0.000	-0.000	-0.000	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)
Observations	43,569	43,569	43,569	43,569	43,569	36,791
Main Model - Controls for Lags of Events						
Effect 4-Years Post-Election	0.002	0.000	0.001	-0.001	-0.001	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.004)

Table A7—: Robustness: Causal Estimates - Expenditures

	Share of Expenditures On:					
	Certified Staff	Classified Staff	Benefits	Services & Other Exp.	Capital Outlays	Log Supt. Salary
Top of Ballot Educator	(1)	(2)	(3)	(4)	(5)	(6)
Effect Slope	0.001	-0.000	0.000	-0.000	-0.000	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)
Observations	43,569	43,569	43,569	43,569	43,569	36,791
Main Model - Excludes Boards with Recent Top-Listed Educator						
Effect 4-Years Post-Election	0.003	0.000	0.002	-0.002	-0.001	0.005
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)
Effect Slope	0.001	0.000	0.001	-0.000	-0.000	0.003
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)
Observations	33,128	33,128	33,128	33,128	33,128	27,242

Note: Robust standard errors are clustered at the district level. The sample is a stacked dataset of periods -3 through 6 for each school board. Covariates for the main specification include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The random assignment sample excludes electoral contests that have no educator candidates or all educator candidates. The flexible model interacts all control variables listed above with post-election period intercepts. The parsimonious model controls for the share of educators in the candidate pool, indicators for having no educators or all educators in the candidate pool. The next two specifications control for the quadratics (cubics) of the number of candidates and candidates who are educators, and their interaction, instead of a linear share of educators. The next specification controls for lags of the instrument and share of educators in the candidate pool going back five years. The final specification restricts to school boards that have not had a top-of-the-ballot educator in the previous five years. All models include district, period, election year, and calendar year fixed effects.

Table A8—: Robustness: Causal Estimates - Student Outcomes

	Elementary		Middle		
	Math	Reading	Math	Reading	HS Graduation
Top of Ballot Educator	(1)	(2)	(3)	(4)	(5)
Main Model					
Effect 4-Years Post-Election	-0.011	-0.020	-0.014	-0.014	-0.001
	(0.012)	(0.010)	(0.010)	(0.008)	(0.003)
Effect Slope	-0.007	-0.003	-0.001	-0.001	0.001
	(0.005)	(0.003)	(0.004)	(0.003)	(0.001)
Observations	1,263,925	1,263,574	488,559	503,140	25,934
Flexible Model					
Effect 4-Years Post-Election	-0.010	-0.020	-0.013	-0.013	-0.001
	(0.012)	(0.009)	(0.010)	(0.008)	(0.003)
Effect Slope	-0.004	-0.001	0.000	0.001	0.001
	(0.004)	(0.004)	(0.004)	(0.003)	(0.001)
Observations	1,263,925	1,263,574	488,559	503,140	25,934
Main Model - Random Assignment Sample					
Effect 4-Years Post-Election	-0.014	-0.023	-0.018	-0.018	-0.002
	(0.013)	(0.010)	(0.010)	(0.009)	(0.003)
Effect Slope	-0.004	-0.001	0.001	0.001	0.001
	(0.004)	(0.003)	(0.004)	(0.003)	(0.001)
Observations	710,366	710,224	271,134	278,245	13,188
Parsimonious Model					
Effect 4-Years Post-Election	-0.007	-0.019	-0.013	-0.014	-0.001
	(0.013)	(0.010)	(0.010)	(0.008)	(0.003)
Effect Slope	-0.004	-0.002	-0.001	0.000	0.001
	(0.004)	(0.004)	(0.004)	(0.003)	(0.001)
Observations	1,263,925	1,263,574	488,559	503,140	25,934
Main Model - Quadratic Controls for No. of Candidates and Educator Candidates					
Effect 4-Years Post-Election	-0.012	-0.021	-0.013	-0.014	-0.001
	(0.012)	(0.009)	(0.010)	(0.008)	(0.003)
Effect Slope	-0.005	-0.001	0.000	0.001	0.001
	(0.004)	(0.004)	(0.004)	(0.003)	(0.001)
Observations	1,263,925	1,263,574	488,559	503,140	25,934
Main Model - Cubic Controls for No. of Candidates and Educator Candidates					
Effect 4-Years Post-Election	-0.011	-0.020	-0.013	-0.014	-0.001
	(0.012)	(0.010)	(0.010)	(0.008)	(0.003)
Effect Slope	-0.005	-0.001	-0.000	0.000	0.001
	(0.004)	(0.004)	(0.004)	(0.003)	(0.001)
Observations	1,263,925	1,263,574	488,559	503,140	25,934
Main Model - Controls for Lags of Events					
Effect 4-Years Post-Election	-0.011	-0.022	-0.014	-0.014	-0.001
	(0.014)	(0.010)	(0.011)	(0.008)	(0.003)
Effect Slope	-0.005	-0.002	0.000	0.000	0.001

Table A8—: Robustness: Causal Estimates - Student Outcomes

	Elementary		Middle		
	Math	Reading	Math	Reading	HS Graduation
Top of Ballot Educator	(1)	(2)	(3)	(4)	(5)
	(0.005)	(0.003)	(0.005)	(0.003)	(0.001)
Observations	1,263,925	1,263,574	488,559	503,140	25,934
Main Model - Excludes Boards with Recent Top-Listed Educator					
Effect 4-Years Post-Election	-0.008	-0.018	-0.014	-0.012	-0.006
	(0.014)	(0.014)	(0.013)	(0.011)	(0.005)
Effect Slope	0.002	0.003	0.008	0.003	0.002
	(0.005)	(0.004)	(0.006)	(0.004)	(0.002)
Observations	746,800	746,550	278,201	286,876	13,188

Note: Robust standard errors are clustered at the district level. The sample is a stacked dataset of periods -3 through 6 for each school board. Covariates for the main specification include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The random assignment sample excludes electoral contests that have no educator candidates or all educator candidates. The flexible model interacts all control variables listed above with post-election period intercepts. The parsimonious model controls for the share of educators in the candidate pool, indicators for having no educators or all educators in the candidate pool. The next two specifications control for the quadratics (cubics) of the number of candidates and candidates who are educators, and their interaction, instead of a linear share of educators. The next specification controls for lags of the instrument and share of educators in the candidate pool going back five years. The final specification restricts to school boards that have not had a top-of-the-ballot educator in the previous five years. All models include district, period, election year, and calendar year fixed effects.

Table A9—: Treatment Effects Estimates - Teacher Employment

	Log FTE Teachers (1)
Additional Educator on School Board	
Effect 4-Years Post-Election	-0.015
	(0.055)
Effect Slope	0.049
	(0.025)
Observations	40,983

Note: Robust standard errors are clustered at the district level. The sample is a stacked dataset of periods -3 through 6 for each school board. Coefficients show the causal effect of the instrument four years post-election, as well as the slope of the causal effect post-election. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The models furthermore include district, period, election year, and calendar year fixed effects.

Table A10—: Treatment Effect Estimates - Student Demographic Composition

	Share of Non-White Students:		
	Elementary (1)	Middle (2)	Share of FRL Students: (3)
Additional Educator on School Board			
Effect 4-Years Post-Election	0.067	0.046	0.021
	(0.051)	(0.030)	(0.028)
Effect Slope	0.008	0.011	-0.002
	(0.010)	(0.009)	(0.006)
Observations	2,130,574	564,729	628,372

Note: Robust standard errors are clustered at the district level. The sample is a stacked dataset of periods -3 through 6 for each school board. Models use an indicator for being a first-listed educator to instrument for the number of educators newly elected to the school board, with the intercept coefficient showing the causal effect of an additional educator four years post-election, and the slope coefficient showing the trend of effects post-election. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The models also include district, election year, period, and year fixed effects.

Table A11—: Treatment Effect Heterogeneity - Charter Schools

	Share of Enrollment (1)	No. of Charters (2)
Additional Educator on School Board		
Effect 4-Years Post-Election	-0.029	-1.299
	(0.020)	(0.793)
$\times \geq 1$ Curr. Educator	-0.009	0.007
	(0.007)	(0.248)
Observations	40,975	40,975

Note: Robust standard errors are clustered at the district level. The sample is a stacked dataset of periods -3 through 6 for each school board. $\times \geq 1$ Curr. Educator equals 1 if a seat not up for election in the current cycle is occupied by an educator (and 0 otherwise). The first coefficient shows the causal effect of the instrument four years post-election for a board without any current educators. The second coefficient captures the differential treatment effect four years post-election for a board with at least one current educator. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The models furthermore include district, election year, period, and year fixed effects.

Table A12—: Treatment Effect Heterogeneity - Teacher Working Conditions

	Service Days (1)	M.A. Bonus Offered (2)	Log Max Health Benefit (3)	Class Size (4)	Log Salary: BA+60 (5)
Additional Educator					
Effect 4-Years Post-Election	-0.110	-0.030	-0.189	0.540	0.027
	(0.383)	(0.046)	(0.270)	(1.399)	(0.012)
$\times \geq 1$ Curr. Educator	-0.071	-0.018	0.087	-0.577	-0.006
	(0.130)	(0.015)	(0.110)	(0.564)	(0.004)
Observations	37,380	37,380	37,380	38,997	227,795

Note: Robust standard errors are clustered at the district level. The sample is a stacked dataset of periods -3 through 6 for each school board. Column 6 further stacks the dataset across six experience levels (1, 5, 10, 15, 20, and 25 years). Coefficients show the causal effect of the instrument four years post-election, as well as the slope of the causal effect post-election. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The model furthermore includes district, election year, period, and year fixed effects.

Table A13—: Treatment Effect Heterogeneity - Expenditures

	Share of Expenditures On:					
	Certified Salaries (1)	Classified Salaries (2)	Benefits (3)	Services & Other Exp. (4)	Capital Outlays (5)	Log Supt. Salary (6)
Additional Educator						
Effect 4-Years Post-Election	0.012	0.002	0.004	-0.009	-0.007	0.011
	(0.008)	(0.004)	(0.005)	(0.005)	(0.003)	(0.028)
$\times \geq 1$ Curr. Educator	0.003	0.001	0.000	0.003	0.002	0.004
	(0.003)	(0.001)	(0.002)	(0.002)	(0.001)	(0.011)
Observations	43,569	43,569	43,569	43,569	43,569	36,791

Note: Robust standard errors are clustered at the district level. The sample is a stacked dataset of periods -3 through 6 for each school board. Coefficients show the causal effect of the instrument four years post-election, as well as the slope of the causal effect post-election. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The models furthermore include district, election year, period, and year fixed effects.

Table A14—: Treatment Effect Heterogeneity - Student Outcomes

	Elementary		Middle		
	Math (1)	Reading (2)	Math (3)	Reading (4)	HS Graduation (5)
Additional Educator					
Effect 4-Years Post-Election	-0.073	-0.166	-0.103	-0.097	-0.011
	(1.000)	(0.107)	(0.092)	(0.071)	(0.023)
$\times \geq 1$ Curr. Educator	-0.009	-0.009	-0.017	-0.022	0.008
	(0.053)	(0.052)	(0.043)	(0.035)	(0.010)
Observations	1,263,925	1,263,574	488,559	503,140	25,934

Note: Robust standard errors are clustered at the district level. The sample is a stacked dataset of periods -3 through 6 for each school board. Coefficients show the causal effect of the instrument four years post-election, as well as the slope of the causal effect post-election. Covariates include the number of open seats, share of educators in the candidate pool, their interaction, indicators for having no educators or all educators in the candidate pool, and indicators for the number of contests per district-year. The models furthermore include district, election year, period, and year fixed effects.

II. Overlapping Elections, Causal Effects, and Electoral Dynamics

Our research design focuses on using the ballot order instrument to estimate the causal effects of an additional educator elected to the school board over time. Drawing on the framework of Cellini, Ferreira and Rothstein (2010), this section expands on the interpretation of these effects in recognition that 1) outcomes are likely to depend on current and prior boards' actions; and 2) current board composition will also depend on results of prior elections (e.g. because of staggered terms).

To assist the exposition, we use separate district j and election year t indices here in lieu of school board identifiers (b). τ indexes periods relative to election year t as before. An outcome Y_{jt} for a given school board can be expressed as a function of the composition of the current board as well as of those boards that preceded it:

$$Y_{jt} = \sum_{\tau=0}^{\infty} \theta_\tau T_{j,t-\tau} + u_{jt} \tag{A.1}$$

T_{jt} is the number of educators elected to the district j school board in year t. For the modal school district, elections are held every two years with members serving term lengths of four years. For non-election years, $T_{jt} = 0$ by definition. This equation in principle allows the decisions of all previous school boards to influence the outcome. This setup accommodates staggered elections: the school board immediately prior is likely to share members in common with the board elected at t. However, the setup also allows that the decisions of boards for which all members' terms have expired by t may continue to matter (e.g. because of path-dependence in collective bargaining or because education input changes may not immediately translate into effects on learning).

The causal effects we estimate correspond to the thought experiment of randomly assigning an educator to the board at time t and tracing out its consequences for outcomes. These effects are represented in equation (1) (re-written without the b notation this time):

$$Y_{j,t+\tau} = \beta_\tau T_{jt} + u_{j,t+\tau} \tag{A.2}$$

Note that equations (A.1) and (A.2) are linked via the following identity:

$$\begin{aligned} \beta_\tau &= \frac{dY_{jt}}{dT_{j,t-\tau}} \\ &= \theta_\tau + \sum_{h=1}^{\tau} \theta_{\tau-h} \frac{dT_{j,t-\tau+h}}{dT_{j,t-\tau}} \end{aligned}$$

where $(dT_{j,t-\tau+h})/(dT_{j,t-\tau})$ represents the effect of a change in the number of educators elected in a given year on the number who are elected h periods subsequent.

This equation formalizes that the causal effects represented by β_τ include both the "partial" effect of an additional educator as well as the cumulative impact of intermediate changes in the school board's composition. In Cellini, Ferreira and Rothstein (2010)'s framework, the β_τ parameters are "intent to treat" causal effects. θ_τ, in contrast, corresponds to the effect of exogenously changing the board composition on the outcome in period τ, holding the board's composition in the years between t and τ fixed. These notions of causal effect are thus by definition the same in post-election periods prior to the next election year (typically $\tau = 1$ and 2 in our setting), but diverge when electoral dynamics are present (i.e. when $(dT_{j,t-\tau+h})/(dT_{j,t-\tau}) \neq 0$). We examine these electoral dynamics directly and discuss their implications for interpreting our findings in Section IV.B.

REFERENCES

Cellini, Stephanie Riegg, Fernando Ferreira, and Jesse Rothstein. 2010. "The Value of School Facility Investments: Evidence from a Dynamic Regression Discontinuity Design." *The Quarterly Journal of Economics*, 125(1): 215–261.