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Why Does Education Increase Voting? Evidence from Boston's Charter Schools*

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Abstract

Americans with more education vote more, but we know little about whether this effect on civic participation arises from educational *quality* or *quantity*. Using admissions lotteries at Boston charter schools, we find that charter attendance boosts voter participation, substantially increasing voting in the first presidential election after a student turns 18 by six percentage points from a baseline of 35 percent. This effect operates through increased turnout, as there is no increase in registration. Rich data enable us to explore multiple potential channels of this voting impact. Our evidence suggests that charters increase voting by increasing noncognitive skills. (JEL: D72, H75, I21)

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1 Introduction

People with more years of education vote at higher rates: over the last thirty years, voter turnout has been at least ten percentage points higher for college graduates than for high-school graduates and even higher relative to those who do not finish high school (McDonald 2019, based on the Current Population Survey [CPS]). This disproportionate electoral participation is one factor contributing to a political and policy process that responds to the preferences of and benefits elites (Gilens and Page, 2014). Some evidence exploiting exogenous variation in years of schooling shows that the relationship between years of education and civic participation is causal (Dee, 2004; Milligan et al., 2004; Sondheimer and Green, 2010; Oreopoulos and Salvanes, 2011; Dinesen et al., 2016); however, other studies find no change in voting among those with more education, implying that the association between voting behavior and education may be a proxy for other factors that contribute to both education and civic participation (Berinsky and Lenz, 2011).[1] Even given an established causal link between education quantity and voting, we know little about why education increases voting and whether educational quality matters.

To understand both whether and how education influences civic participation, we examine the case of Boston's charter schools. Attendance at Boston charter schools has been found to boost pass rates on high-school exit exams, test scores, and college attendance (Angrist et al., 2016), so we have a clear case of educational improvement. However, these academic gains may not translate into increased civic participation if education is a correlate of voting rather than a causal factor driving it. Charter schools are a particularly interesting setting in which to investigate voter participation because charters are often criticized for shifting both students and funding away from traditional public school systems—an institutional feature that could lead charter students and their families to consider themselves separate from their communities and potentially depress their voting rates.

In this paper, we study applicants to Boston charter schools from the projected high-school

[1]We focus on evidence from the United States that comes from causal identification strategies beyond matching. As Sondheimer and Green (2010) write, "literally thousands of cross-sectional surveys have indicated that turnout rates climb with years of formal schooling."

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classes of 2006–2017 who were old enough to vote in the 2016 or prior elections. We generate causal estimates based on the charter school lotteries, confirming that the academic benefits of charter attendance found in Angrist et al. (2016) persist in our larger sample, which includes more schools and more years of lottery data. We then match student data to voter files from Massachusetts and nearby states to observe party registration and turnout in the elections from 2008 to 2020.

Looking at the first presidential election after the students turned 18, we see that charter school attendance substantially increased voter turnout. Specifically, it lifted voter rates by six percentage points from a baseline of 35 percent, representing a 17 percent increase. Girls increased their voting in this election to a much greater extent than boys. Boston charter school attendance also boosted the share of presidential elections in which students participated to 45 percent from the 40 percent among students in the comparison group. Charter school students participated at a similarly higher rate in the second presidential election in which they could participate but at an equal rate in the third election. The immediate boost to electoral participation in the first presidential election in which the students could vote—prior to potential changes in quantity of education—implies that educational quality, not just educational quantity, matters with respect to the influence of education on civic participation.

We find that the effect of charters on voting operates exclusively in presidential general elections. Voter turnout in nonpresidential general elections and primary elections is low and is not affected by charter attendance. The effects that we identify operate via the turnout margin, not the registration margin: Charters do not change who appears on the voter rolls—and thus in the voter files—but only whether registered voters actually vote. Charter attendance does not change party affiliation.

We also make use of rich administrative data to explore potential mechanisms through which charters boost voting. Motivated by the literature on civic returns to education, we consider and test five possible channels of the voting effect: cognitive skills, civic skills, noncognitive skills, social networks, and policy feedback. We find evidence consistent with the idea that gains in noncognitive skills from charter attendance drive the voting effect. We do so by estimating the charter effect at the subgroup level on indices of cognitive skills, noncognitive skills (proxied for

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by attendance), and voting. We then correlate the skill indices with the voting measure and observe which skills—if any—rise in tandem with subgroup-level voting effects. We see that the subgroups with the largest voting boosts also experience the largest effects on the noncognitive index, consistent with the hypothesis that noncognitive skills explain the voting gains.

We cannot fully rule out alternative explanations, but the evidence for cognitive skills and for the policy feedback channel—explored via an analysis of parental voting—is weak. To test the civic skills hypothesis, we split the sample between charters with mission statements that emphasize civics and those with mission statements focusing on other values, and to test the social network channel, we explore how peers differ in the K-12 setting and voting contexts in the college setting. These exercises yield only small and suggestive evidence that civic skills or social networks play a role. We are also able to rule out income as a driving force behind the voting gains, as the voting boost comes early in life prior to young people establishing their careers. Our exploration of mechanisms thus adds to other evidence (Holbein, 2017; Holbein and Hillygus, 2020) that the noncognitive channel is key to boosting voting in the US.

This paper contributes to the literature in four main ways. First, to the literature linking education and civic participation, it adds rigorous, lottery-based evidence on the impacts of students attending schools that boost college enrollment. Much of this literature is based on compulsory schooling laws and interventions that are now decades old (Dee, 2004; Milligan et al., 2004; Oreopoulos and Salvanes, 2011). Moreover, past literature focuses on the effect of educational quantity rather than quality on voting. Our work is closest in spirit to that of Sondheimer and Green (2010), who show that exposure to three classic and well-studied educational interventions that increased high-school graduation also increased voting. However, the total sample size across the three sites that they study is only 1,636, less than 20 percent of our sample size. We demonstrate here that charter attendance boosts voting *prior* to any changes in years of education, showing that educational *quality*, in addition to quantity, is an important factor in civic participation.

Second, we add to the limited evidence on charter schools and voting. Gill et al. (2020) is the only other study that uses lotteries to estimate impacts on voting, and it finds that charter attendance

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increases voter participation by 12 percentage points. Since it studies a single, civics-focused school—Democracy Prep—that also boosts test scores (Corcoran and Cordes, 2015), it is difficult to know whether the civics-focused curriculum is a necessary component of the charter attendance treatment for its impact on voting to emerge. McEachin et al. (2020) also study charters, using matching and regression techniques to show that North Carolina charters boost voter registration and turnout in addition to generating gains in attendance and reductions in suspensions and criminal activity. While the estimation techniques in McEachin et al. (2020) reduce omitted variable bias, the authors' identification strategy does not feature a natural experiment. To show the impacts of charter schools on voting, our setting goes beyond a single school, offers clear identification through lotteries, and features rich data and institutional details that allow us to explore mechanisms.

Third, our evidence on voting contributes to the scant literature on the impacts of charter schools on a broader set of nonacademic outcomes, including Hastings et al. (2012), Wong et al. (2014), Dobbie and Fryer (2015), and Reber et al. (2023). These are the only other lottery-based studies that look beyond academic outcomes to find beneficial impacts of charter attendance on absenteeism, teen pregnancy, criminal activity, and risky behaviors. Imberman (2011), Spees (2019), and McEachin et al. (2020) also show, using observational evidence with regression and matching methods, that charters improve attendance and disciplinary outcomes. Together, this growing literature on voting, absenteeism, and risky behaviors shows that the benefits demonstrated by a subset of charter schools are not restricted to test scores. Outside of charter schools, Jackson et al. (2020) show that schools have separable impacts on test scores and socioemotional development and that longer-term outcomes such as college enrollment are better explained by both sets of effects, implying that successful schools impact young people in areas beyond academics.

We also present evidence on *how* education increases civic participation. While there are many potential theories about why education increases voting, prior works that show a causal connection between education and voting typically document the link, rather than explaining the mechanisms behind it.[2] In our context, we test five major theoretical explanations for why education may

[2]A notable exception is Holbein (2017), which tests multiple mechanisms in the context of

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increase voting: through its effects on cognitive skills, civic skills, noncognitive skills, or social networks or via policy feedback. We then show that in the cases where education does increase civic participation, its effect is likely due to gains in noncognitive skills. An understanding of these mechanisms helps clarify the settings under which we could expect to see impacts of education on voting and the cases where we would not. To the best of our knowledge, ours is the first paper that is able to test all five potential channels in a single setting and one of the few papers that is able to explore mechanisms in the context of well-identified impact estimates.

Finally, we contribute to the understanding of the voting gender gap, which we observe in our context in the first presidential election that students are eligible to vote in. In recent decades, women have outvoted men (Burns et al., 2018; Cascio and Shenhav, 2020). The gap is about 4 to 5 percentage points in favor of women in presidential elections. The typical explanations are that women's educational gains over the past half century, alongside changes in gender roles, women's economic power, and political role models, have contributed to women overtaking men when it comes to voting (Burns et al., 2018). Cascio and Shenhav (2020) show that gains in high school graduation are closely tied to gains in voter participation for women, but not for men. Using the same Massachusetts data, Arnzen and Cohodes (2025) show that differences in educational attainment explain a substantial portion of the gender voting gap. Here, we show that women's educational gains certainly contribute to the gender voting gap, but that the crucial resources imbued by education may well be noncognitive skills and not greater knowledge or income, as previously theorized.

2 Context, data, and descriptive statistics

2.1 Charter schools in Boston

Charter schools are public schools funded with public money but managed by private organizations. In Massachusetts, all charter schools are authorized solely by the state, and chartering entities are typically nonprofit boards (for-profit charter schools are not permitted in Massachusetts). Charter

an RCT, but the intervention studied there—FastTrack—focuses on specifically on psychosocial skills, rather than on education more generally.

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schools in Massachusetts are also subject to a cap on the percentage of student funding that they can receive, which means that the stock of charter schools in the period studied in this paper is established and stable. Similar to other states, most charter schools in Massachusetts do not participate in collective bargaining and have more flexibility around staffing, scheduling, and curriculum, in exchange for increased accountability through a 5-year "charter" under which a charter school can be closed if it does not meet its agreed-upon performance and management standards. The authorizing process in Massachusetts is rigorous, and the state has closed charters schools for both poor performance and poor management. Boston has the highest concentration of charter schools in the state. At the time the students in this study were enrolled, most of the charter schools in Boston used policies associated with the "No Excuses" charter school movement: longer school days and year, a focus on academic achievement and behavior management, in-school tutoring, frequent teacher feedback, and data-driven instruction (Angrist et al., 2013; Dobbie and Fryer, 2013).

Previous evidence uses charter school lotteries to show that a year of attendance at a Boston charter school boosts math standardized test scores by approximately a third of a standard deviation (σ) and English/language arts (ELA) scores by approximately 0.2σ (Abdulkadiroğlu et al., 2011; Angrist et al., 2013; Walters, 2018; Cohodes et al., 2021; Setren, 2021). The most recent of these studies (Cohodes et al. 2021, Setren, 2021) include coverage of almost all Boston charter schools, leaving little room for sample selection to be driving the findings. Boston charter attendance also boosts college preparation via AP test-taking and SAT scores, and it increases four-year college enrollment (while decreasing two-year college enrollment), though some students take an additional year to graduate high school (Angrist et al., 2016; Setren, 2021). Recent evidence shows that urban Massachusetts charters (including the Boston schools) boost 4-year degree attainment as well (Cohodes and Pineda, 2024).

The Boston findings are generally in line with studies of similar charter schools in Chicago, Denver, Los Angeles, New York City, Newark, and New Orleans and of KIPP schools, which typically use lotteries to show that attending an urban charter school boosts test scores, and where evidence is available, increases college enrollment and decreases risky behavior (see Cohodes

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and Roy, 2025). Outside of urban areas, charter school impacts on both test scores and other outcomes are more mixed (see Cohodes and Parham, 2021).

2.2 Data

The data sources for this project are school records from the Massachusetts Department of Elementary and Secondary Education (DESE), charter school lottery records, and Massachusetts voting records. DESE provided information on students' names, demographic characteristics, special needs status, and participation in the free/reduced price lunch program from the Student Information Management System (SIMS), test scores in math and ELA from the Massachusetts Comprehensive Assessment System (MCAS), standardized to mean 0 and standard deviation 1 within subject, grade, and year. Records of Advanced Placement taking and scores and SAT taking and scores are provided to the DESE from the College Board, and college enrollment and degree information come from the National Student Clearinghouse (NSC). We follow Angrist et al. (2016), henceforth ACDPW, to create outcomes from these data sources; see that paper for details.

The study sample includes all 12 Boston charter schools with lottery records that enrolled students who were at least 18 by the 2016 general election (see Appendix Table C.2 for details on which schools are included), adding 5 schools to the ACDPW sample. Lottery records typically include student names and dates of birth, and sometimes include addresses, and lottery information such as the lottery number, waitlist number, offer status, and priority status (sibling, out-of-area, etc.). We match lottery records to student information in the SIMS, primarily matching by name and birth date. Ties are broken using other information in the lottery files (gender, town of residence) and by only matching to students in the appropriate grade range. We use fuzzy matching techniques to connect almost all charter lottery applicants to a SIMS record.

Siblings, duplicate records, out-of-area applicants and other nonrandomized applicants, as well as repeat applicants, are excluded from the lottery-based analysis (see Appendix Table C.1 for details on sample exclusions). The remaining lottery applicants are the group subject to random lottery, and we count those who are offered a seat in the charter school on the date of the lottery as receiving an "initial offer." Students who receive an offer of spot off the waitlist are identified

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as having a "waitlist offer." These two mutually exclusive variables serve as instruments in our instrumental variables setup. Students are assigned to a "projected high school class:" the year they would graduate, assuming on-time grade progression after the lottery.

The Massachusetts voter file lists all voters that are registered in Massachusetts by name, date of birth, address, registration date, party registration, and participation in primary and general elections. We obtain the voter files from 2012, 2015, 2018, and 2020 from commercial vendors who collect this information for political campaign purposes. To account for out-of-state moves and nearby college attendance, we supplement the Massachusetts voter file with 2018 and 2020 voter files from neighboring states: Connecticut, Maine, New Hampshire, New York, Rhode Island, and Vermont.[3] These files include the presidential elections of 2008, 2012, 2016, and 2020 and the general elections between these dates. Given the age of our sample, the first possible presidential elections in which they could vote are 2008, 2012, and 2016, while the 2020 election contributes to the "ever voted" and "share of elections" variables. We match students from the SIMS data to the voter file based on name[4] and date of birth, and we use fuzzy matching techniques to account for minor differences in records. Details on our matching procedures are in Appendix B.

[3]We confirm that presence in the out-of-state voter files is highly correlated with attending college in that state. However, we do not use college attendance location in our links between the student data and the voter file data to avoid biasing our links with a potentially endogenous feature. See Appendix Table B.1, which shows a strong alignment between attending college in a nearby state and showing up in the voter file there. Regardless, many of these students are initially registered in Massachusetts, and most students who attend college out of state do not end up registering out of state, at least in the time period we observe.

[4]Women changing their name at marriage could affect our ability to match girls to the voter file. However, we note that the median age of marriage for women in Massachusetts is 30.1. The young women in our sample would typically be ages 18 to 22 at the date of the first possible presidential election in which they could vote, well younger than the typical age of marriage in Massachusetts. If we do undermatch or are unable to include the full voter records of women because of name changes, our estimates will be biased downward, and we will underestimate

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Students are counted as "ever registered" in Massachusetts or a neighboring state if they match to the voter records; we also can determine if students registered by a particular relative date, such as by their 19th birthday. We measure voter participation in multiple ways. "Ever voted" outcomes count participation in any election, which we further subgroup into particular election types: general elections (any November election), presidential elections, off-cycle elections (general elections in nonpresidential years), presidential primaries, and other primaries. "Share" outcomes measure the share of elections in which a student participated out of all the elections they were eligible to participate in, grouped by election type.

Our key voting outcome is participation in the first presidential election after a student turns 18. Since it is possible that charter school attendance influences outmigration from Massachusetts and neighboring states at different rates, a measure of participation in the election closest to students' 18th birthday will be less subject to that risk and also measures voter participation at the point closest to charter school attendance. Thus, the "first possible presidential" outcome is the primary outcome we track throughout this paper. We focus on the first presidential election because voting rates are very low in other elections, leaving little room to detect differences. Following Holbein et al. (2020), we also report impacts on the share of presidential elections voted in during our time horizon as a summary measure of voting over time. Since this is a proportion of elections in a time period, the actual elections contributing to this measure will vary between students of different ages, but the data are on a common scale. All voting outcomes are unconditional, such that students not present in the voter file and those who did not vote in an election are both counted as nonvoters. This is the standard approach in the literature when matching to voter files.

For a subset of students, the lottery files also contain parent names. We link parents by name to the Massachusetts voter file to assess if charter school attendance (or nonattendance due to losing the lottery) politicizes families. Since we have no birth date for parents, to reduce the number of

the magnitude of any gender gaps we observe that favor women. Indeed, we find the largest voting gender gap for the first possible presidential election and smaller gaps for the share of presidential elections, which may be due to name changes as women age.

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potential matches in the voter file, we restrict the voter file to Boston residents. We make several adjustments to our matching procedure to account for the lack of confirmatory information on parents; Appendix F details how our data and estimation strategy differ for the parent voting analysis.

Returning to measures of education, we use information on high-school enrollment, high-school graduation, college attendance, and college graduation to create variables that represent years of education at two important points in time paralleling our first possible presidential and share presidential voting outcomes. "Years of education at first presidential" refers to the years of education obtained at the time an individual experiences their first presidential election. "Years of education ever" refers to years of education measured in our college degree window, which includes potential college attendance and college graduation for up to six years after the projected high-school graduation. These measures enable us to assess changes in education quantity.

2.3 Descriptive statistics

Students in Boston Public Schools (BPS) and those who apply to charter school lotteries are a broadly similar group, as shown in Table 1, which reports mean demographic characteristics and outcomes for BPS students, students in the Boston charter lottery sample, and lottery winners and losers. Approximately 74 percent of students receive free or reduced price lunch, which is consistent across groups, and both BPS and charter applicants are primarily students of color, though charter applicants are more likely to be Black and less likely to be Latinx or Asian. BPS students and charter applicants both have baseline test scores well below the state mean, though charter students are slightly positively selected.

Charter applicants and BPS students are similarly likely to receive special education services, but charter applicants are much less likely to be English learners (ELs), though this gap has decreased over time (Setren, 2021). As expected, in the BPS sample, which is based on students who attended a BPS school in 9th grade, only a small percentage, 6.5 percent, attended a Boston charter school for at least a year in grades 5-12. This compares to 42 percent of Boston charter school lottery applicants. Just over 50 percent of applicants offered a seat in the lottery ended up attending a Boston charter school, while 24 percent of not-offered students ended up

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Table 1: Sample Characteristics and Outcomes

	Boston Public Schools (1)	Lottery Applicants (2)	Charter Attendees (3)
(A) Baseline characteristics			
Female	0.485	0.521	0.520
Asian	0.095	0.028	0.020
Black	0.417	0.585	0.621
Latinx	0.326	0.266	0.207
Other race	0.022	0.024	0.024
White	0.140	0.098	0.128
Special education	0.201	0.198	0.180
English learner	0.225	0.083	0.054
Subsidized lunch	0.751	0.738	0.688
Baseline MCAS ELA	-0.547	-0.426	-0.265
Baseline MCAS Math	-0.440	-0.383	-0.225
(B) Charter school enrollment			
Attend any charter in grades 5-12	0.062	0.408	1.000
(C) Academic outcomes			
MCAS Math	-0.315	-0.220	-0.143
MCAS ELA	-0.508	-0.300	-0.186
Took any AP	0.290	0.288	0.341
Score 3+ on any AP	0.143	0.090	0.107
Took SAT	0.494	0.531	0.600
SAT score (1600) (for takers)	915.576	864.465	882.550
Graduate high school (4 years)	0.531	0.537	0.560
Graduate high school (5 years)	0.599	0.617	0.680
Enroll in any college	0.451	0.556	0.577
Enroll in 4-year college	0.332	0.421	0.442
Enroll in 2-year college	0.139	0.165	0.170
(D) Voting outcomes			
Ever registered to vote	0.766	0.851	0.866
Share presidential	0.340	0.428	0.436
Voted in first possible presidential	0.308	0.401	0.415
N	46,957	9,560	8,624

Notes: This table shows demographic characteristics and outcome means, for various samples. The sample in Column 1 is restricted to students who attended Boston Public Schools in 9th grade in the projected high school classes of 2006 to 2017, who are are least 18 by the 2016 general election. The sample in Column 2 is restricted to charter school applicants enrolled Boston Public Schools or Boston charter schools at the time of application in the projected high school classes of 2006 to 2017 who are at least 18 by the 2016 general election. The sample in Column 3 is restricted to students who attended a Massachusetts school in 9th grade and who ever attended a Boston charter school in grades 5 through 12 in the projected high school classes of 2006 to 2017, who are are least 18 by the 2016 general election.

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attending a charter school at one point during their K-12 school experience.[5] Consistent with successful randomization in the lotteries, charter lottery winners and losers are broadly similar on all dimensions other than charter school attendance (Appendix Table C.3).

3 Empirical framework

To estimate the effect of Boston charter school attendance on voting outcomes and, as a benchmark, academic outcomes, we use a two-stage least squares (2SLS) strategy that exploits the natural experiment created by charter school lotteries. Charter attendance may be influenced by many factors we cannot account for, such as family background and motivation, that in turn also influence the choice to vote. We therefore use the randomly-assigned offer of a charter school seat as an instrument for charter attendance. The second stage equation in our 2SLS setup is:

$$y_i = \sum_j \delta_j d_{ij} + X_i'\Gamma + \rho C_i + \epsilon_i, \tag{1}$$

where y_i is an outcome, such as voting in any Massachusetts election, for student i. Charter attendance is captured by C_i, which indicates attendance at one of the lotteried Boston charters, if the student applied to that charter school, at any point before the outcome is measured.[6] The average causal effect of charter school attendance is ρ. We include a vector of baseline student characteristics, X_i, which are race, special education status, English learner status, subsidized lunch status, and a set of birth year fixed effects. We also control for lottery fixed effects, in the form of "risk sets." The risk sets, d_{ij}, are a set of dummy variables for every combination of charter school applications observed in the data. Including these risk sets takes into account that students

[5]These estimates do not exactly match the first stage estimates in Appendix Table C.7, since they include attendance at any charter school, not just one with a lottery.

[6]Thus, C_i will vary across outcomes, with two primary measures. The first is attendance at a lotteried charter in the two years after the charter lottery, for the outcome of MCAS test scores two years after potential entrance to a charter. The second is attending a lotteried charter at any point between the charter lottery and high-school graduation (or exiting the data).

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who apply to more than one charter lottery are more likely to attend a charter school, limiting comparison to within students who apply to the same set of charter schools. ϵ_i is the error term.

We instrument for charter attendance using randomly assigned charter school offers: the "initial offer" instrument, Z_{i1}, indicates the offer of a charter school seat on the day of the charter school lottery; the "waitlist offer" instrument, Z_{i2}, indicates the offer of a charter school seat off a randomly ordered waitlist. These are coded as mutually exclusive dummy variables. In the randomized lottery sample, 30.8 percent of students were offered a seat on the day of the charter school lottery and an additional 29.6 percent were offered a seat off of the waitlist. A little under 40 percent of the students in the sample were not offered a seat in the lottery. Appendix Table C.2 lists the schools, application years, and lottery availability within each school by year. The analysis sample excludes repeat applicants, siblings, out-of-area applicants, late applicants, and any other applicants that were not subject to randomization through the lottery.

The first stage of the 2SLS setup follows:

$$C_i = \sum_j \mu_j d_{ij} + X_i'\beta + \pi_1 Z_{i1} + \pi_2 Z_{i2} + \eta_i, \tag{2}$$

where C_i is a function of the risk sets, demographic covariates, and the instruments. The effect of a charter offer on attendance is captured by π_1 and π_2, which measure the change in charter attendance induced by the initial and waitlist offer, respectively. Using two instruments increases precision, and the causal effect of charter attendance, ρ, is a weighted average of the attendance effects we would have estimated by using each instrument separately. We use robust standard errors.

Our lottery-based estimation strategy takes advantage of the random assignment process inherent to charter school lotteries, generating charter impact estimates that are independent of both observed and unobserved student characteristics like ability and interest in school choice. Appendix Table C.3 demonstrates that observed student characteristics are similar for offered and nonoffered students within risk sets, providing a check on the lottery randomization process. We provide further evidence of reliable randomization by predicting voter participation based

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on the voting rates of BPS students with similar demographics and test scores and using those predictions as an outcome in Equation 1. If students with higher likelihoods of voting are more likely to be offered a seat at a charter, using this prediction as an outcome would show positive impacts on voting rates. However, as shown in Appendix Table C.4, there are no differences in predicted voting. We also show that matching rates to the SIMS data are very similar for offered and nonoffered students (Appendix Table C.5) and that follow-up rates for various outcomes do not differ by offer status (Appendix Table C.6). Together, these pieces of evidence show that differential matching or attrition does not undermine random assignment.

Before we turn to charter impacts on academic and voting outcomes, we document that the charter school lottery offers do increase charter school attendance in Appendix Table C.7, which reports coefficients from Equation 2. An initial offer increases the likelihood that a student attends a charter during their time in the Massachusetts public schools by 46 percentage points, with a waitlist offer increasing charter attendance by 30 percentage points. Seven percent of nonoffered students do eventually attend a lotteried charter school at some point in their schooling career.[7] This occurs when a student gets a seat off the waitlist in the years following the lottery year, later receives sibling preference, or reapplies for a later lottery. The nonoffered mean is nontrivial, but some students have as many as seven years to gain entrance to a charter, and students that apply in 5th grade have additional opportunities to apply in 6th and 9th grades.

Since noncompliance is not incidental in this context, as shown in Column 1 of Appendix Table C.7, our preferred way to describe the counterfactual comparison for lottery impacts is as the control complier mean (CCM). The CCM is the average value of the outcome for nonoffered compliers: students who do not attend a charter when they do not receive an initial or waitlist offer in the first charter school lottery they apply to in our sample. We cannot directly observe

[7]The first stage used in outcome estimation will vary slightly from the first row of Appendix Table C.7 due to different sample sizes and because charter attendance was only counted in the years prior to an observed outcome. However, all of the voting outcomes use any observed charter attendance after the lottery as an endogenous variable in the first stage.

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the CCM, since charter lottery compliers and never-takers (those who would not attend a charter even if offered) will be commingled in the nonoffered average. Thus, as in Katz et al. (2001), we estimate the CCM as follows, using the methods of Abadie (2002):

$$y_i*(1-C_i)=\sum_j \lambda_j d_{ij}+X_i'\alpha+\tau(1-C_i)+\nu_i, \tag{3}$$

where τ is the estimate of the CCM, $(1-C_i)$ is instrumented by Z_{i1} and Z_{i2}, and risk sets and demographics are accounted for as in Equation 2.

4 Effects of charter schools on academic outcomes

Before turning to voting, we first benchmark the impact of charter school attendance on academic outcomes against similar estimates of Boston charter school attendance (Abdulkadiroğlu et al. 2011, ACDPW, Cohodes et al. 2021; Setren, 2021). Overall, the charter impacts we show in this sample are very similar to those previously found. Boston charters continue to show large gains in scores on state exams, AP test-taking, AP scores, SAT scores, and college enrollment, although charter students continue to take longer to graduate high school.

Appendix Tables A.1 through A.5 report Boston charter impacts on exams, high-school attendance and suspensions, and college attainment. Boston charter school attendance boosts MCAS math scores by approximately half of a standard deviation (henceforth σ) and ELA scores by 0.32σ two years after application. Over the course of high school, Boston charters induce an additional 12 days of attendance. Suspensions also increase, with charters increasing the likelihood of ever being suspended by 11 percentage points and the number of days suspended by half a day. Boston charter attendance also increases AP test-taking and test scores and SAT test-taking and test scores.

We confirm that Boston charter attendance increases time to high-school graduation, with 4-year graduation rates reduced by 9 percentage points. There are no statistically significant differences in 5- or 6-year high-school graduation rates. Boston charters boost enrollment in 4-year colleges by 8.5 percentage points, with approximately 55 percent of the effect due to an increase in enrollment in any college and approximately 45 percent due to a shift from community college. The boost in

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college enrollment persists through degree completion: Charter attendees are 4.1 percentage points more likely to graduate with a college degree within 6 years of projected high-school enrollment than their counterparts, driven by both bachelors degree (BA) and associates degree (AA) attainment.

We summarize the impacts on educational attainment via the years of education outcomes. At the time students are first eligible to vote in a presidential election, there is *no* difference in years of education. Over a longer time horizon, charter attendance raises final years of education by 0.26 years, reflecting the higher likelihood of charter attendees completing a degree.

Our findings are broadly in line with previous work on Boston charter schools. Test score estimates are very similar to those found in prior work. Charters continue to boost college preparation and enrollment, as in ACDPW, but there are some differences in magnitudes, which we discuss in Appendix D. We note that as a whole, our modeling choices will, if anything, depress the magnitude of our findings, meaning that we are taking a more conservative approach than prior work but still find substantial academic and college gains.

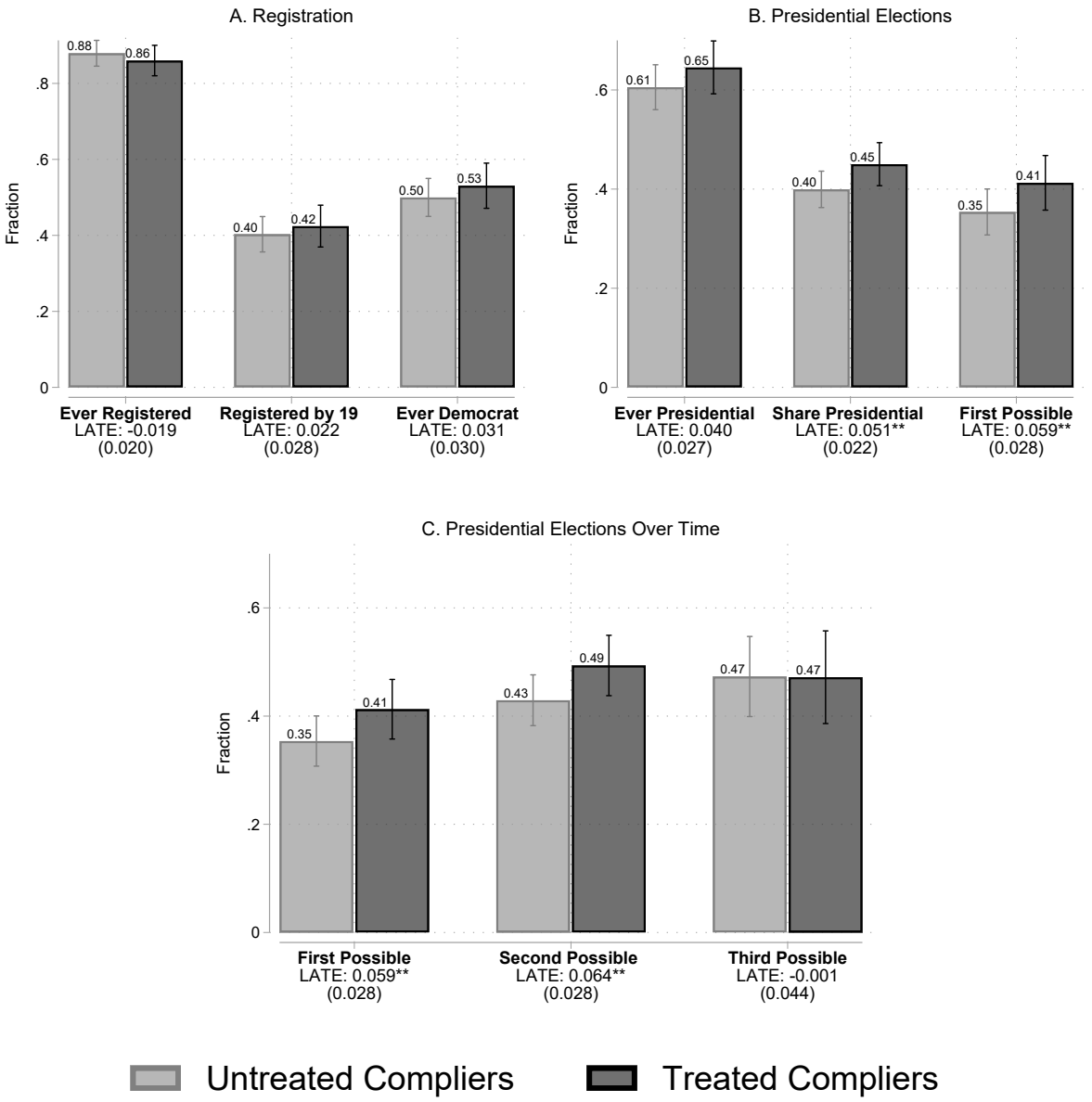
5 Effects of charter schools on voting

Boston charter attendance makes little difference in the likelihood of registering to vote in Massachusetts, as can be seen in Panel A of Figure 1 (details in Appendix Table A.7). Eighty-eight percent of control compliers are registered to vote, a share similar to that for charter attendees. As the voter files include only registered voters, these findings imply that we matched almost 90 percent of applicants to the voter file without differential matching by lottery winner or loser status. Roughly 45 percent of students register before their 19th birthdays; again, there is no difference by offer status. Approximately half of our sample of charter applicants are registered as Democrats and the remaining are primarily registered as independent voters, which is not surprising given their age, race, economic background and location. We see no effects on party choice in registration or in presidential primary voting.

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Figure 1: The Impact of Charter School Attendance on Voting



Notes: This figure shows impact estimates of Boston charter school attendance on voting outcomes. For details, see Appendix Table A.7. Panels A and B: N = 9,560, Panel C: N (first and second possible presidential elections) = 9,560, N (third possible presidential election) = 4,671. * p<0.10 ** p<0.05 *** p<0.01.

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Charter attendance does affect turnout, even if it does not change registration rates. Boston charters boost the rate at which students ever vote in a general election from approximately 63 percent to approximately 67 percent, a 3.4 percentage point increase, though the difference is not statistically significant (Appendix Table A.7). General elections include presidential elections and off-cycle general elections (for House members, Massachusetts governor, and Senate two-thirds of the time). The charter effect becomes marginally statistically significant when we look at the *share* of general elections in which students participated, with charters boosting this by 3 percentage points.

However, when separated by election type, greater differences emerge, driven by presidential elections: Boston charter attendance increases voting in presidential elections (Panel B of Figure 1) but does not increase voting in nonpresidential general elections or primaries (Appendix Table A.7). However, turnout in all of the various nonpresidential elections is generally very low, and only 14 percent of the charter applicant population votes in any off-cycle general election. For reference, fewer than 20 percent of 18- to 29-year-olds voted in the 2014 election (McDonald, 2020). We thus focus on presidential elections as our main outcome of interest given the high level of turnout for these.

We now turn to our key measures of voting: voting in the first possible presidential election and share of presidential elections voted in (Panel B of Figure 1). Boston charter attendance boosts voting in the first possible presidential election by approximately 6 percentage points. The counterfactual voting rate is 35 percent. We focus on the first possible presidential election to minimize the window in which students could leave Massachusetts or the region. Additionally, this election is the election closest to the charter school treatment, and therefore the one we think is most likely to show the influence of charter school attendance.

We consider the importance of measuring voting impacts closer versus farther away from high-school attendance in Appendix Figure A.1, which splits the sample between cohorts who experience their first presidential election close to the time they turn 18 and those who experience their first presidential election farther from the time they turn 18. Those who turn 18 close to their first possible presidential election do have a larger impact on voting (7.5 percentage points) than those who turn 18 farther from the relevant election (5.2 percentage points), but the difference is not statistically

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significant (Panel A). Notably, at this point in time, there is *no* difference in years of education obtained (Panel B), especially for those who turn 18 close to their first presidential election. This implies that the voting bump we observe for the first possible presidential election *cannot* be due to more years of education—the primary driver of civic participation identified in prior literature—and instead must be due to some other part of the educational experience. That we later see total years of education diverge (Appendix Table A.5) suggests that an underlying factor in charter school education is driving *both* voting and the likelihood of obtaining more years of education.

Turning to share of presidential elections voted in, control compliers vote in approximately 40 percent of these elections, whereas charter attendees vote in 45 percent (Figure 1, Panel B). Holbein (2020) argues that the share of elections is a good measure for condensing voting outcomes for cohorts with different time horizons of voting. We show voting over time in Panel C of 1, which documents a persistent charter effect into the second possible presidential election but convergence by the third. (We consider the robustness of gap closure in Appendix E, concluding that it most likely represent real convergence and not an artifact of the data.)

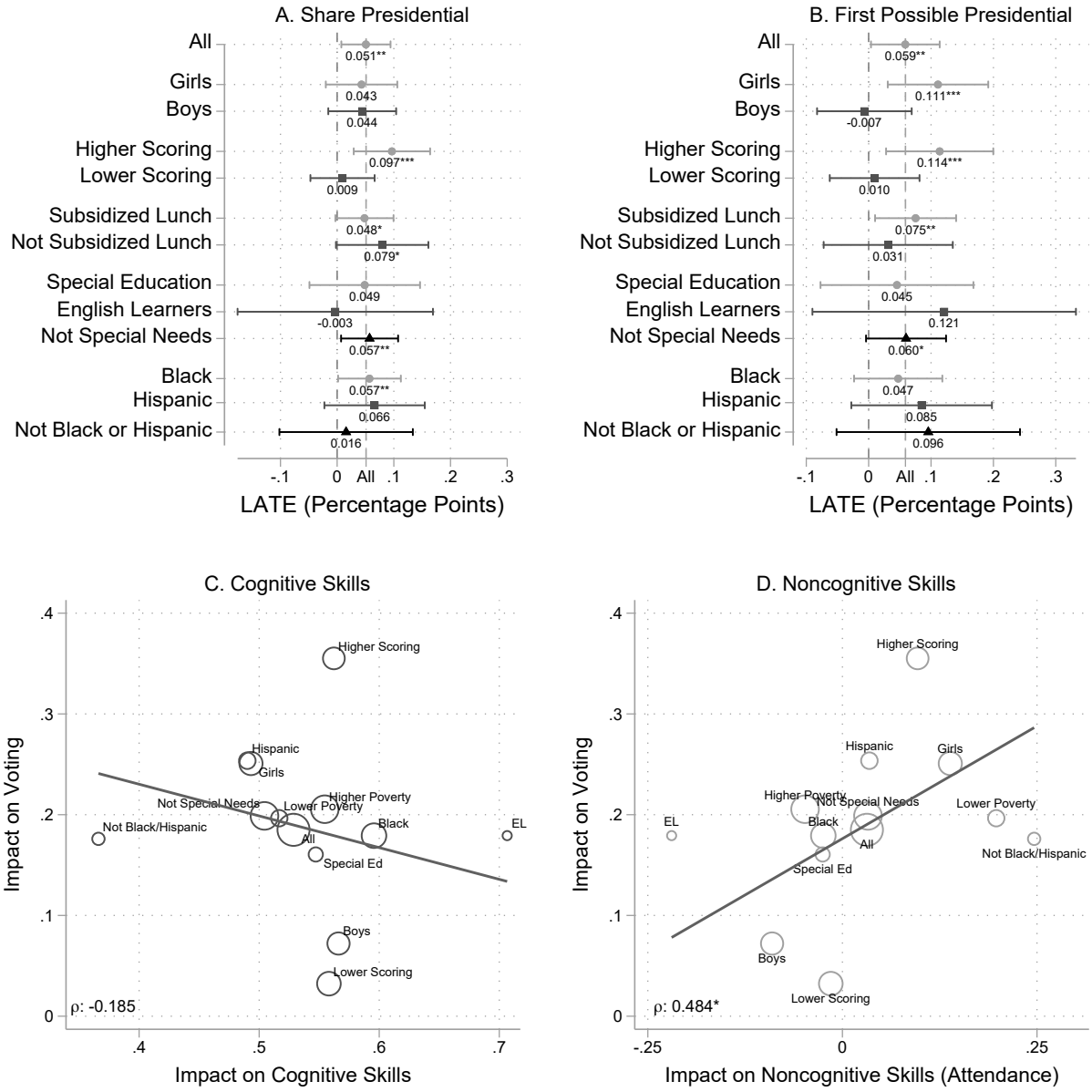
We also examine whether the Boston charter attendance effect on share of presidential elections and first possible presidential varies by student background characteristics in Panel I of Figure 2 (details in Appendix Table A.8). There is a striking difference by gender: young women who attended charter schools are 11 points more likely to vote in their first possible presidential election; there is no effect on young men. The difference is statistically significant (p-value = 0.038).[8] In recent decades, a gender gap in political participation has emerged, with women voting at higher rates than men (Cascio and Shenhav, 2020; Burns et al., 2018). This gap may be driven by education, as suggested in the analyses by Cascio and Shenhav (2020) and Arnzen and Cohodes (2025), and our findings add causal evidence of the connection between education and the voting gender gap. However, when we look at the share of presidential elections, young men and women vote at similar rates, meaning that boys catch up to girls over time.

[8]We do not believe gender differences in voting are driven by a gender peer effect; see Appendix E for details.

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Figure 2: Impacts and Correlations by Subgroups



Notes: Panels A and B of this figure show impact estimates of Boston charter school attendance on voting outcomes for different subgroups of students. N varies by subgroup. For details, see Appendix Table A.8. Panels C and D of this figure plot the impact estimates for voting against an index of cognitive and noncognitive skills as measured by 9th grade attendance (standardized) by subgroup. Marker sizes are proportional to the number of students in each subgroup. Lines show weighted linear fits by school level with weights proportional to the size of the subgroup. * p<0.10 ** p<0.05 *** p<0.01.

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There are other meaningful differences by subgroup. The most consistent differences are among higher- and lower-scoring students. Higher-scoring students (on baseline tests) have a larger voting boost than those who enter charters with lower test scores for both share of presidential elections and first possible presidential elections. There are some differences for other groups, but they are either small or not consistent across outcomes.

Before considering the mechanisms behind the increase in voting, we first demonstrate that our findings are robust to various alternative specifications and considerations in Appendix Table E. Presidential elections occur only every four years, and each is unique. Removing each election from the sample in turn shows that no single election is driving the findings. We also consider other modifications to our specification such as removing control variables and using an alternative endogenous variable, as well as how (lack of) residence in Massachusetts affects our findings. In short, Boston charters appear to boost voting across all presidential elections, and the particular specification does not affect the conclusions. Students residing outside of Massachusetts likely downward bias the impacts on voting.

6 Mechanisms connecting education and civic participation

Americans with more education vote more (McDonald, 2020). We show here, and others show in different contexts (Dee, 2004; Milligan et al., 2004; Sondheimer and Green, 2010; Oreopoulos and Salvanes, 2011), that at least some of the US education-voting premium is due to the causal effect of educational experiences rather than to education and civic participation correlating with a different underlying factor. We demonstrate in Figure A.1 that simply obtaining *more* education—the primary explanatory factor in much of the prior literature—cannot explain the voting differences we observe, since there is no difference in years of education at the time students vote in their first presidential election. Thus, understanding the mechanisms through which education plausibly affects voting outside of quantity and determining if any such mechanisms exist within the Boston charter experience can clarify our understanding of why education increases civic participation.

Why might education increase voting? Theoretical explanations are typically grounded in a "resource model" of voting, which posits that voters need resources (e.g., time, money, skills)

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in order to overcome barriers to voting and that education can endow those resources (Brady et al., 1995). When it comes to the specific resources endowed by education, we consider and test four of the most commonly suggested possibilities (Persson, 2015): cognitive skills, civic skills, noncognitive skills, and social networks. The observed relationship between education quantity and voting under the resource model assumes that those who spend more time in school have a greater opportunity to develop the relevant skills. Here, we consider whether education quality alone can improve the necessary skills. We also consider a fifth possibility in parallel to those inspired by the resource model: the "policy feedback" channel, where engagement with specific policies in turn engages participants in the political process. These are the theoretical channels we believe have the greatest likelihood of being changed by charter school attendance.

6.1 Five reasons why charter schooling might increase voting

The most prominent theoretical channel behind the relationship between education and voting is that education develops *cognitive skills* and that those with greater cognitive skills are more likely to vote because they have the language skills necessary to understand and form opinions about political topics and to navigate the political participation process (Wolfinger and Rosenstone, 1980; Verba et al., 1995; Nie et al., 1996). In terms of the aforementioned "resource model":voting is costly and increasing cognitive skill means that people have greater resources to overcome those barriers (Verba et al., 1995). Nie et al. (1996) identify verbal skills as particularly important, and Hillygus (2005) and Nie and Hillygus (2008) show that among college graduates, verbal SAT scores are more predictive of voter turnout than math. As the charter schools we study here have demonstrated gains in achievement tests, including measures of verbal ability, there is potential for the cognitive channel to drive the voting effects.

A related resource-based explanation is that education builds civic participation-specific human capital in students, typically called *civic skills*, through exposure to civics education and that such knowledge of the political system increases participation. One rationale for the public provision of education is to ensure an informed citizenry; most states make this explicit by requiring some form of civics education (Hansen et al., 2018). There is little rigorous evidence that exposure

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to a civics curriculum leads to increased voting. Green et al. (2011) randomize an additional civics curriculum across 59 high-school classes and find that students exposed to the curriculum do have greater knowledge of the content, but the authors do not directly test voting behavior. Nonexperimental evidence is mixed. Buckley and Schneider (2009) explore the impacts of D.C. charter schools on "building citizenship" using survey data and matching techniques, finding that charter students tend to have greater civic skills than their public school peers. Weinschenk and Dawes (2021) use high-school transcript data linked to voting records and family fixed effects to show that those exposed to more civics education do *not* have greater adult turnout. Hillygus (2005) and Nie and Hillygus (2008) do find that college graduates with more social studies credits vote more. Charter schools in our sample may expose students to more civics curricula in the form of Advanced Placement courses that focus on civics-related topics: AP United States History or AP Government. Furthermore, some of the charters in our sample have explicit civic or communitarian missions. However, when comparing charter schools to traditional public schools across the nation, there does not seem to be large differences. Consider the National Assessment of Educational Progress (NAEP), which includes a civics component. In 2018, 8th grade students in public schools scored 152 on the civics exam (out of 300), and charter school students scored 156.

Education can also increase so called *noncognitive skills* (Kautz et al., 2014; Jackson et al., 2020). These socioemotional or "soft" skills include self-regulation, persistence, and grit. Interventions that increase noncognitive skills increase voting (Holbein, 2017; Holbein and Hillygus, 2020), and "grit" is correlated with voting, even controlling for test scores and other characteristics (Holbein et al., 2020). Holbein and Hillygus (2020) argue that this noncognitive channel and the development of "follow-through" are a more critical resource to helping citizens surmount barriers to voting than academic skills or knowledge of government systems. They note that voting is a multi-step process and that, beyond some basic level of reading and knowledge of the voting process, follow-through is a much more important skill than knowledge when it comes to voting. This channel might be particularly relevant in the United States, where the franchise is not universal: noncognitive skills are necessary to navigate the voting process when the voting

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process is intentionally made difficult via long waits at polling places and ID requirements for voter registration and voting. Gallego (2010) and Chevalier and Doyle (2012) find that institutional barriers in the voting process explain the education-voting gradient in the United States and its absence in Europe, which does not have the same level of voter suppression. Indeed, simple interventions, like "preregistering" young people, can increase voting rates (Holbein and Hillygus, 2016), indicating that the barriers built into the American voting process are meaningful, and skills that help individuals surmount such barriers may be a pathway to franchise.

Urban charter schools, many of which employ models that emphasize behavior and an academic program that requires follow-through, may very well develop these noncognitive skills to a greater extent than their traditional public school counterparts. Interestingly, in some of the same Boston charter schools studied here, West et al. (2016) find that charter school enrollment *decreases* self-reported measures of conscientiousness, self-control, and grit. However, West et al. (2016) attribute these effects to "reference bias," that is, charter schools resetting the norms of what concepts like conscientiousness and grit mean to their students.

Education may introduce students to new *social networks* and social norms; in particular, attending a residential college may introduce students to new communities that may be more politically active. Nie et al. (1996) argue that education provides a "positional pathway" by moving people to more "politically important" social networks. Abrams et al. (2011) show a strong association between the voting rates of informal social networks (friends, family, and colleagues) and individuals' voting. Recently, Chyn and Haggag (2023) show that young people who moved neighborhoods as children due to public housing demolitions are more likely to vote as adults. They believe part of this is due to increased high-school graduation rates, but speculate that there is additional scope for social norms in the new neighborhood to shape voting behavior (though they do not have the data to address this). Angrist et al. (2016) show that Boston charters boost college enrollment, especially at four-year campuses, which draws students into new communities. There is evidence that colleges can induce students to vote at higher rates: Bell et al. (2024) show that attending a college with a 10 percentage point higher voting rate (as measured by earlier cohorts)

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increases voting by 4 percentage points. If these new peer networks and communities are more likely to have a norm of civic participation and to be more central to politically important social networks, the college boost from charter schools may increase voting by fostering these connections.

Education, of course, is government policy, and charter schools represent a deviation from the typical way government has provided education. As such, education, charter schools in particular, is subject to a concept long held in political science, wherein "new policies create new politics" (Schattschneider et al., 1935). Formalized into *policy feedback theory* (Pierson, 1993; Mettler and SoRelle, 2014), this idea holds that when people experience a government policy, it can in turn shape their political views and participation. An example of this in the context of education policy is Hastings et al. (2007), which finds that white and high-income parents of Charlotte-Mecklenburg students who lose a school choice lottery are more likely to vote. In Chicago, Nuamah and Ogorzalek (2021) find that experiencing school closures politicizes Black Americans in affected communities.

The actual explanation for the relationship between education and voting is likely a combination of the theories described above. If charter schools produce academic gains via improving student human capital, charter schools may induce increased civic participation via any of the skill as resource theories described. Charter schools—which break the link between neighborhoods and school attendance—may also introduce students and their families to the resource of new social networks through new school communities or by increasing college enrollment in new communities.

The relationship between charter schools and the policy feedback hypothesis is more ambiguous. If charter students and their families perceive charter schools to be a beneficial government intervention, they may increase their civic participation. Alternatively, exposure to the quasi-private aspect of charter schools may induce students and families to be less likely to vote. Or, losing the charter lottery could be a negative interaction with the government that induces lower voting rates.

Education may also affect voting by improving employment outcomes and income. Wealthier people are more likely to vote and increasingly dominate the political process in the United States (Schlozman et al., 2018). Increases in family income through cash transfers can increase children's voting (Akee et al., 2020). In our context, we do not expect income to be a major channel for

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voting behavior, as we focus on voting in the first election for which students are eligible and other elections during young adulthood, a time before we expect earnings differences (if any) to emerge. Contemporaneous family income may affect political participation, but the charter school random lottery setup balances this across students. Thus, we do not focus on income-related explanations.

Prior lottery-based work on charter school attendance and voting shows that one charter network focused specifically on civic participation, Democracy Prep, does increase voting rates (Gill et al., 2020). This provides suggestive evidence on the civics-specific skill channel, but the single-site setting limits the potential to test different mechanisms behind the boost in voting. North Carolina has an array of charter schools, and McEachin et al. (2020) use regression-adjusted inverse probability weighting to find that charter attendance increases civic participation, alongside decreasing absenteeism and criminal activity. The coincident decrease in absenteeism and criminal activity is suggestive of the noncognitive channel we explore; however, McEachin et al. (2020) do not explore potential mechanisms. Unlike prior work, our rich dataset provides enough context to test many of the theoretical possibilities.

We consider each potential mechanism below. In contrast to our impact estimates, which utilize the charter school lotteries to make causal claims, our exploration of mechanisms primarily relies on the correlation between the effect of charter attendance on voting and its effects on other outcomes that could be plausible channels of the voting effects. Thus, we consider these exercises suggestive.

6.2 Cognitive skills

If greater cognitive skill increases voting by enhancing the ability of potential voters to engage with complex political topics, then we would expect that gains in academic skill should follow similar patterns to voting outcomes in our sample. That is, we expect charter schools to increase voting for the same students for whom they are improving academic skills.

To test this, we estimate impacts on voting and academic skills for each subgroup from Figure 2 and plot these subgroup impact estimates against each other. If the impact estimates by subgroups rise together, that would give credence to the idea that cognitive skills drive academic gains. If they do not, that would demonstrate no evidence of the cognitive pathway in our data. Angrist

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et al. (2022), Angrist et al. (2023), and Cohodes et al. (2023) use similar strategies to visually demonstrate the connections between multiple outcomes and to establish plausible mechanisms.

To conduct this exercise, we create two standardized indices via principal component analysis (PCA) in the noncharter BPS sample to summarize voting and the cognitive channel. The first index is the voting index, which is an equally weighted index of the first possible presidential and share presidential outcomes. The second index is the cognitive index, which is an equally weighted index of the MCAS math and ELA scores two years after the lottery. We estimate impacts on each of the outcomes for every subgroup using the same model we used for the full sample, limited to the relevant group. We then plot each of the subgroup estimates against each other in Panel A of Figure 2, with each group weighted by sample size (detailed estimates in Appendix Table A.10). The impacts by subgroup on the cognitive channel and voting do *not* rise in tandem. If anything, it appears that subgroups with larger cognitive gains have lower voting gains. The correlation, weighted by subgroup size, between the voting and cognitive impact estimates is -0.185. We thus find no evidence to support the cognitive skill mechanism in our data.

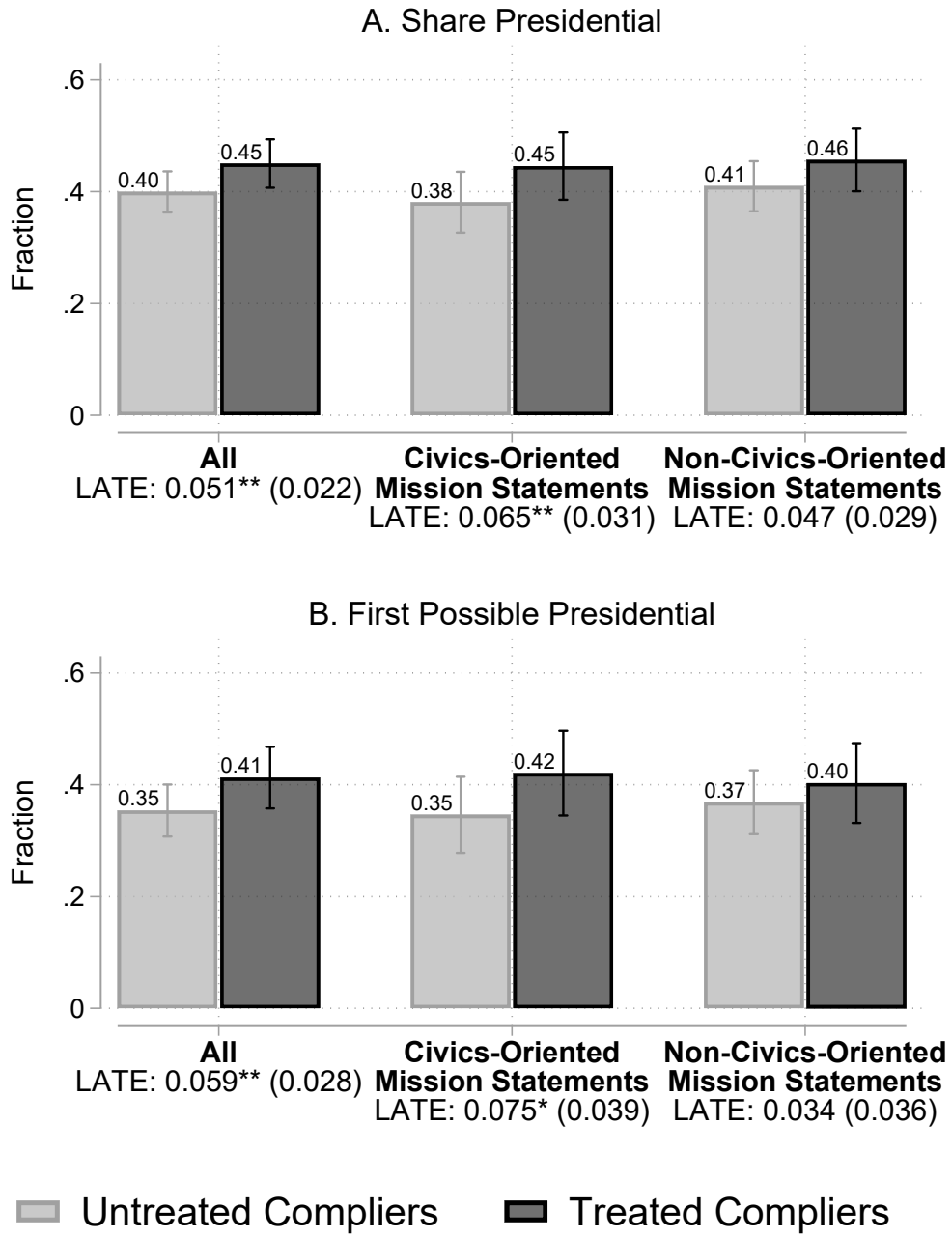
6.3 Civic skills

To test the civic skill pathway, we take a different approach. We compare charter schools with explicit civic orientations in their mission statements to charter schools that do not have such a focus (their mission statements instead tend to focus on academics). In many educational settings, civics curricula are one-off courses, required by state law, and unsurprisingly have little impact on civics outcomes. An approach that embeds a civic orientation more deeply into school culture, rather than simply offering an add-on course, may be more successful at transmitting civic skills. This is the idea behind Democracy Prep, which did show an impact on voting (Gill et al., 2020).

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Figure 3: The Impact of Charter School Attendance on Voting by School Mission Statement



Notes: This figure shows impact estimates of Boston charter school attendance on voting, split by whether or not a charter school's mission statement is "civics-oriented." For details on the estimates, see Appendix Table A.11; for details on the missions statements see Appendix Table A.12. N = 9,560, * p<0.10 ** p<0.05 *** p<0.01.

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As such, we compare voting impacts between civics-oriented and non-civics-oriented schools in the Boston context. Appendix Table A.12 displays the charter school mission statements. We categorized a school as civics-oriented if their mission statement mentioned "civic(s)," "citizenship," or "community." Non-civics-oriented mission statements tended to focus on academics, and while they may mention topics beyond academics and college, they do not have a civics or communitarian focus. An example of a civics-oriented mission statement comes from the City on a Hill: "City on a Hill graduates responsible, resourceful, and respectful democratic citizens prepared for college and to advance community, culture, and commerce, and to compete in the 21st century..." An example of a not-civics-oriented mission statement comes from Boston Collegiate: "The mission of Boston Collegiate Charter School is simple yet ambitious: to prepare each student for college."

To estimate impacts for each of the two school types, we modify Equations 1 and 2 by separately enumerating the offer and enrollment variables for civics- and non-civics-oriented campuses. We show the results from this setup in Figure 3, which reports the charter attendance effect for each school type from jointly estimated regressions. Civics-oriented charter schools have slightly larger impacts compared to non-civics-oriented charters on both voting outcomes. However, the differences are not statistically significant (Appendix Table A.11). We thus conclude that a broader school-level civics orientation may contribute to but does not fully explain the charter voting impacts.

6.4 Noncognitive skills

Along similar lines to the cognitive channel, if noncognitive skills explain the voting impacts, we would expect a correlation between a particular subgroup's voting impact and its impact on noncognitive skills. As voting in the United States often involves navigating sign-up processes, planning ahead, and following through, having the skills to navigate such challenges is a channel that could plausibly affect voting.

Our data has no direct measure of noncognitive skills, which in other cases is typically a survey-based measure of self-control or grit. This is a limitation of our study, however, there is reason to believe that self-reports of noncognitive skills in the Boston charter sector may not accurately reflect noncognitive skills. West et al. (2016) find that Boston charter school students

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score *lower* on self-reported measures of noncognitive skills, despite attending schools that increase academic scores and attendance and that seem likely to provide scaffolding for noncognitive skills given their emphasis on conduct and learning. The authors explain that their findings are likely due to charter schools providing a different reference point for the context-dependent survey measures used to construct noncognitive skill indices—so that charter students have a different definition of what reflects grit, self-control, etc.—and that there likely *are* actual skill gains in these dimensions.

Thus, in lieu of a direct measure of noncognitive skills, we follow the "observed behavior" approach of Holbein and Hillygus (2020) use attendance in ninth grade (standardized) as our measure of noncognitive skills. Attendance and suspensions are often used as a proxy for noncognitive skills (Gershenson, 2016; Holbein and Ladd, 2017; Jackson, 2018; Jackson et al., 2020), and Holbein et al. (2020) show an association between grit and attendance. Holbein and Hillygus (2020) argue that this "observed behavior" approach, which relies on administrative data, identifies a factor separate from cognitive skills and family background. We do not rely on suspension data in our main measure of noncognitive skills because the charter treatment increases suspension, moving it in the opposite direction of an increase in noncognitive skill.[9]

For the noncognitive channel, we present the correlation between subgroup impacts on the

[9]We also construct an alternative measure of noncognitive skills, followingJackson (2018) with minor changes due to data differences, using the first principal component of a number of administrative measures of student outcomes. Specifically, we conduct PCA in the BPS sample and use days of 9th-grade attendance, on-time entry to 10th grade, and an indicator for the student having ever been suspended in 9th grade as our measures of noncognitive skill. We use days of attendance in lieu of days absent and omit course grades. This exercise yields a standardized index that positively weights attendance and on-time entry to 10th grade and negatively weights the suspension indicator. Notably, the overall charter effect on the noncognitive index is negative (Appendix Table A.10), which is due to the fact that charter schools *increase* suspensions under their strict behavior codes. Nevertheless, Appendix Figure A.2 Panel B shows the same pattern of results we next describe using attendance alone.

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attendance (our proxy for noncognitive skills) and impacts on voting in Panel B of Figure 2 (detailed estimates in Appendix Table A.10), parallel to the cognitive channel. Here, the story diverges from that of the cognitive channel, and we observe that the noncognitive indicator and voting index rise in tandem. The weighted correlation between the two types of impact estimates is 0.484. While this test does not establish causality, we conclude that this exercise provides strong suggestive evidence for the noncognitive channel.

The convergence of the charter voting effect over time (Figure 1 and Appendix Figure E.1) is also consistent with a noncognitive skill explanation. *All* young people experience an increase in conscientiousness (the measure in the "Big Five" personality scales that corresponds most closely to economists loser definition of noncognitive skills) during adolescence and young adulthood (Bleidorn et al., 2022). Hoeschler et al. (2018) finds that grit increases during adolescence as well. Thus our findings are consonant with the idea that charter school attendance *accelerates* obtaining noncognitive skills but that counterfactual students catch up over time due to their natural development.

6.5 Social networks

Education has the potential to change social networks both within K-12 education and via college enrollment. We describe the new peers charter students are exposed to in both these communities and consider whether these new exposures can account for the observed voting gains.

6.5.1 School social networks

While the charter school lotteries are balanced on observable characteristics, it can still be the case that charter schools enroll a different peer group than counterfactual schools, since charter lottery applicants are different from Boston Public School students in a few important ways. They are more likely to be female or Black and less likely to be an English learner. Charter applicants have test scores that are about 0.1σ higher than BPS students. These differences do not compromise the lottery. However, they imply that part of the charter school treatment is exposure to different peers.

We examine the differences in peer characteristics for charter lottery compliers in Figure 4, following a similar exercise from ACDPW. Peer characteristics are measured at the school by

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year by grade level in the four years after charter students enter the lottery. We report predicted voting, attendance, and test scores all measured *prior* to the charter lottery. For predicted voting, we use the same measure based on non-charter BPS student likelihood to vote as in our balance check in Appendix Table C.4 applied to the full sample of students, then aggregated (as with all peer characteristics) to the school by year by grade level and used as an outcome in equation 1.

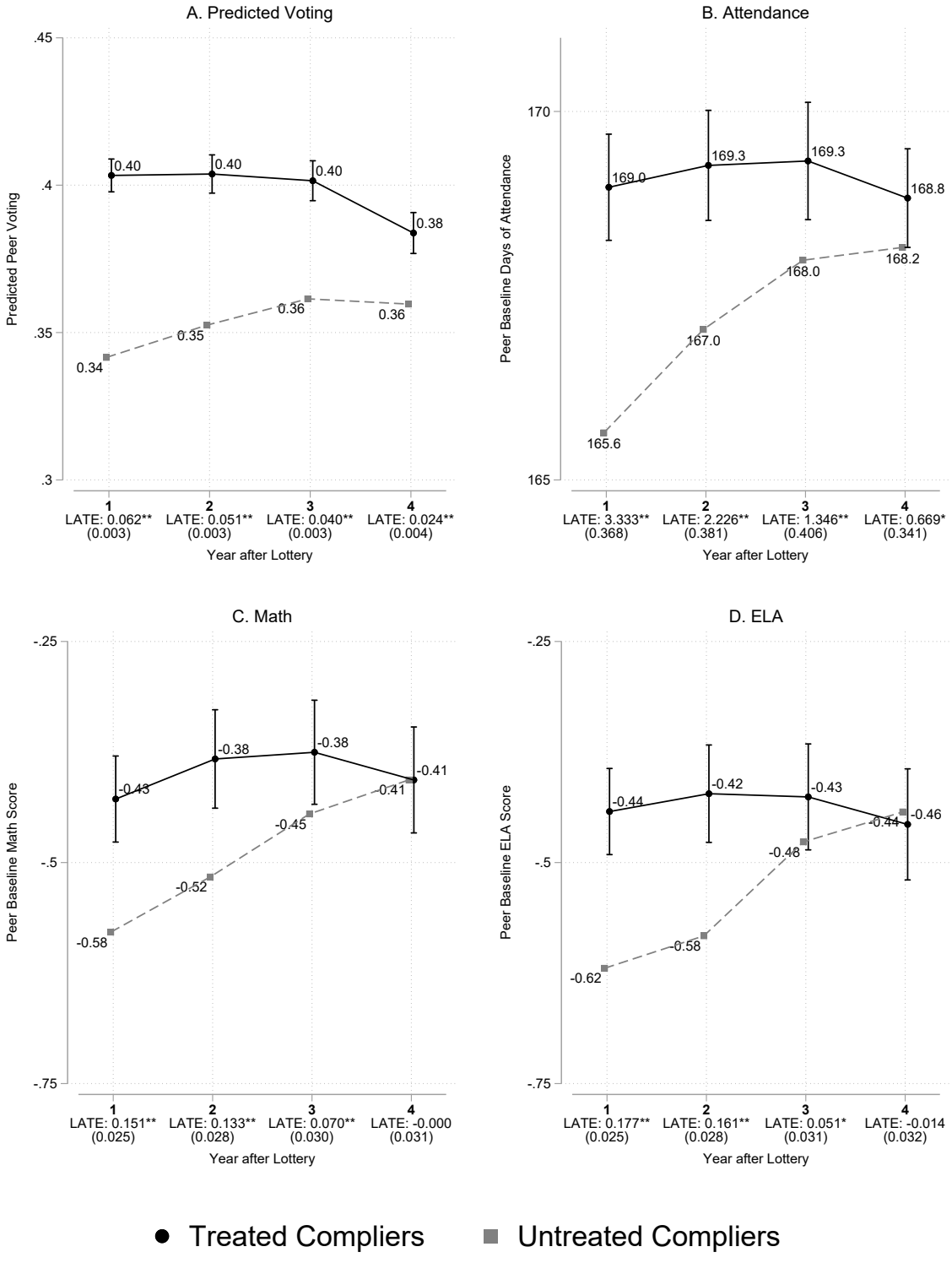
On all measures of peer characteristics, students enter the first post-lottery year with a positive charter school differential, reflecting the fact that charter school peers have higher predicted voting, baseline attendance, and baseline test scores. However, in all cases, untreated compliers catch up to some extent over the years. By the fourth year after the lottery, peer baseline test scores are the same for both treated and untreated compliers, and the gap is closed to some extent for predicted voting and attendance. This is due to the fact that negatively selected peers drop out of counterfactual schools. (The small dip in the fourth year for treated compliers reflects that some students leave charters after three years of middle school.)

Focusing on predicted voting, the gap here has the least closure. This gap is due to the fact that charter students are more likely to be Black or female (and Black and female), groups with high voting rates in the United States. The initial gap in predicted voting in the first post-lottery year is 6.2 percentage points, very similar to our main impact estimate of 5.9 percentage points. However, for the new social network to drive the impact estimate, the peer effect would have to be almost 100 percent. Peer effects for test scores are typically estimated to be in the range of 0 to 0.30σ (Sacerdote, 2014). We are not aware of any peer effects literature on the transmission of voting in secondary schools. The two closest sources are Bell et al. (2024), which documents a 40 percent transmission rate for college in voting and Brown et al. (2023), which uses a movers design to show that an additional year of exposure between ages 13-19 to a place with a 1 percentage point higher turnout rate increases voting rates in someone's first eligible presidential election by 0.057 points.

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Figure 4: The Impact of Charter School Attendance on Peer Characteristics



Notes: This figure plots mean peer characteristics in the years after the lottery for treated and untreated charter lottery compliers. Peer test scores and attendance are measured as the average baseline characteristic for other students in the same school, year, and grade.

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Thus post-lottery school social networks have some scope to contribute to the voting edge, but cannot account for all of it. If we assume a peer effect on the high end of 40 percent (that is, 40 percent of a difference in peers is transmitted) and use the largest gap in predicted voting rates, then school social networks can account for a little more than 40 percent of the impact on voting in the first possible presidential election. A more realistic estimate might be to average the gap in predicted voting across years (4.4 percentage points) and assume a 15 percent transmission rate, which would account for about 11 percent of the voting differential. In sum, there is scope for K-12 peer effects to account for a small to meaningful fraction of the impact on voting but they cannot explain it all.

It is possible that some of the noncognitive skills gained by charter students (which in turn contribute to voting) are due to the peer channel. Again using attendance as a proxy for noncognitive skills, we see the gap in peer prior attendance. However, the first post-lottery year peer gap is 3.3 days compared to a treatment effect of 4.7 days in 9th grade (Appendix Table A.2). Again, the transmission rate for the peer effect would need to very large to account the treatment effect on attendance. As argued above, we do not believe the cognitive channel is driving the voting effect, but even it were, the peer contribution to test score gains (Panels C and D) would have to be very large to be a major explanatory factor.

6.5.2 College social networks

Turning to post-K-12 social networks, students enter new communities when they matriculate to college. We see in Appendix Table A.5 that Boston charters induce enrollment in 4-year colleges, likely exposing students to different communities than they would have been in otherwise. If college communities have a more pervasive norm of civic participation than students' home communities, or if college-going better connects students to the political process through interaction with elites, college education may induce voting through these social connections.

We do not have a direct measure of individuals' post-K-12 social networks, but we can measure the civic participation rates of the communities that students enter via college in two different ways. Using all noncharter students in Massachusetts in the same time period, we measure the Massachusetts voting rates at all post-secondary institutions that Massachusetts students

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attend, based on first college attended in the NSC data. That is, we take the voting records of all Massachusetts' noncharter students attending, for example, the University of Massachusetts at Amherst, and calculate the voting rate of that population. We then assign voting rates by college community based on first institution attended in the lottery sample. Students who do not attend college are assigned the voting rate of Boston students who do not attend college.[10] This outcome then defines the change in community voting rate for a similar age population as induced by changing communities due to attending college.

Since communities' civic values are defined by more than just the voting rates of young people, we define a second measure of community voting engagement using the community turnout rate at the county level collected in Dave Leip's Atlas of U.S. Presidential Elections, which compiles election data from primary sources. College location data from IPEDS identifies the county of each student's first post-secondary institution. We match that location to two measures of county-level voter engagement: total votes cast divided by the number of registered voters in the county (community turnout rate) and registered voters divided by total population (community registration rate). Registration and voting information comes from the year of the most recent presidential election prior to students' high-school graduation, as a measure of the community civic participation that exists prior to students joining that community. We cannot determine a specific county for students who did not attend college and those who attended online colleges. Thus, we are assign such students to Boston's Suffolk County as a default, which assumes they stay in Boston after they enter the charter lottery. In the case of both the college voting rates and the community rates, we cannot account for community changes that are not induced by college.

Overall, charter schools—by inducing students to attend colleges, particularly colleges outside of their home community—push students into communities that have slightly higher voting rates, as seen in Table A.13. The registration and voting rates, defined by Massachusetts high schoolers, are approximately 1.7 to 2.7 percentage points higher than the rates in the counterfactual condition.

[10]Students who attend colleges with fewer than 10 Massachusetts students are assigned college community voting rates by college sector.

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County-level turnout is also approximately half a percentage point higher, again showing that charters move students to social contexts with higher civic participation rates. However, these differences are small, and if the charter voting effect is transmitted solely through these new communities, that would imply the community effect accounted for an impact even larger than the difference in community voting rates—an implausible position. We can assign some numbers to that proposition. If we use our largest estimate—a 3 percentage point increase in college voting rate—and apply the 40 percent transmission rate from Bell et al. (2024) that would imply that college voting norms account for 20 percent of our main voting impact estimate. We thus consider this suggestive evidence that social networks contribute to increased voting rates but cannot account for the full charter impact even under the most generous assumptions.

These community rates do not account for direct social interactions, so we attempt to proxy for being pushed into new social networks with our data. We measure the charter school impact on being the only person of one's gender from one's high-school graduating class to attend one's college (which we label as "Solo College Attendee.") This outcome is a proxy for the extent to which a student would need to enter new social networks in college because she lacks connections from high school to rely on. Attending a charter increases the likelihood of entering college without an extant social network by 6 percentage points (Table A.13). Our measure is only a proxy for new social networks, but alongside the differences in community voting rates, it does show some potential for social networks to explain the charter voting effect. Overall, without a direct measure of social networks, we can neither confirm such networks as the channel for voting impacts nor completely eliminate them. The small magnitudes of the differences we can observe, though, implies that that social networks can at most account for only some of the observed voting differences.

6.6 Policy feedback

To explore the policy feedback channel, we use the voting records of charter school parents. Contact with charter schools could politicize both parents and students, especially as the question of charter

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schools has become more politically polarizing (Reckhow et al., 2015).[11] In 2016, for example, voters in Massachusetts rejected a ballot initiative to expand charter schools. More than $42M was spent for and against the measure—more than twice what was spent, cumulatively, on three other ballot initiatives that election year in Massachusetts. Additionally, while there could be skill or network spillovers from children to their parents, parents' civic participation is much less likely to be directly affected by anything happening at school. If there are no voting effects for parents, despite the voting effects for students, that suggests one of the skill channels or a social network channel is likely causing the charter voting impacts. If there is a difference for parents, that gives credence to the policy feedback mechanism, though the mechanisms for parents and students may differ.

It is also possible that having a child attend a charter school could make parents *less* likely to vote, as charter schools are a form of government privatization that may lead parents to become less connected to government. Cook et al. (2020) show some evidence that this occurs, though they rely on the existence of charter schools rather than direct evidence from charter lotteries.[12] Regardless of the direction, we consider parent voting as the best channel to test the policy feedback mechanism.

We detail our construction of the parent voting sample and our analysis in Appendix F. Ultimately, a child's charter school attendance appears to have no impact on the likelihood that a parent votes, as can be seen in Appendix Table F.1. For all samples, with all weights, the charter impact on parents' voting after the lottery is small and indistinguishable from zero, whether measured by voting in the first possible presidential election, voting in any presidential election, or voting in the 2016 election. We focus on the 2016 election in particular, when charter school staffs may have directly encouraged parents to vote given the charter school cap ballot measure

[11]Winning or losing a charter lottery could also change the political advocates parents are in contact with, though we lack the correct empirical experiment to address this directly. With children attending charters, parents might be more exposed to education reformers; parents with children at public schools might be more connected to teachers unions and anti-charter advocates.

[12]Alternatively, parents of charter lottery losers may vote less if losing the lottery increases their distrust of government, as in school choice lottery losers in Hastings et al. (2007).

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in that election, but we still find no effect on parental voting.

7 Discussion

In this paper, we show that Boston charter schools not only generate impressive educational gains but also boost civic participation. During the time period that we observe, Boston charters boosted both voting in the first presidential election in which enrolled students were eligible to vote and the share of elections in which charter students voted. Notably, the voting bump in the first presidential election in which the students were eligible to vote came *prior* to any divergence in years of education. Thus, we show that educational quality, not just educational quantity, can drive civic participation.

Despite some evidence (Cook et al., 2020) that the presence of charter schools modestly diminishes voter turnout in odd-year elections, we show here that the effect of charter school attendance is positive, as in Gill et al. (2020) and McEachin et al. (2020). This implies that arguments against charter schools on the basis of their detraction from civic engagement do not have direct evidence for students who participate in the charter school treatment.

We test plausible mechanisms that might drive these charter voting gains. None of our exercises above can causally link the charter school voting gain to a particular mechanism. However, we can show evidence that is consistent with some channels and not with others. The noncognitive skills induced by charter attendance seem the most likely channel for the turnout boost, as suggested by the fact that the subgroup-level increases in noncognitive skills covary with the voting gains. In the case of both civic skills and social networks, we have some evidence suggestive of each having some role in the charter voting gains, but neither effect is large. We have no evidence of traditional cognitive gains or policy feedback driving voting. The early-in-life impacts also rule out the income channel. The evidence on noncognitive skills has the most support of the five mechanisms we test, which is consistent with Holbein (2017).

Noncognitive skills may be the mechanism that underlies some of the surprising patterns of the effects from early childhood education, which seem to fade out on academics before returning to affect employment, earnings, and criminal activity later in students' lives (Heckman et al., 2013; Bailey et al., 2017). Inasmuch as girls are better able to develop their noncognitive skills at a

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young age, such skills may be one of the underlying sources of the education and voting gender gaps. The evidence here suggests that building these noncognitive skills yields not only individual but also social returns to education.

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