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WHY DOES EDUCATION INCREASE VOTING?
EVIDENCE FROM BOSTON'S CHARTER SCHOOLS

Sarah Cohodes
James J. Feigenbaum

Working Paper 29308
http://www.nber.org/papers/w29308

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
September 2021, Revised July 2025

We are grateful to Carrie Conaway, Matthew Deninger, Elana McDermott, Alison Bagg, Pierre Lucien, and the staffs of the Massachusetts Department of Elementary and Secondary Education and Boston area charter schools for facilitating the data access that made this project possible. We thank Alex Eble, Samantha Eyler-Driscoll, Don Green, Jeff Henig, Matt Kraft, Celia Paris, Celia Pastoriza, Corey Savage, Elizabeth Setren, Kirsten Slungaard Mumma, and conference and seminar participants at the Association for Education Finance and Policy Conference, the Association for Public Policy Analysis and Management Conference, the Centre for Economic Policy Research Education Economics Workshop, the Columbia Women's Microeconomics Lunch, the NYU IES-PIRT Seminar, the Society of Labor Economists Meeting (SOLE), the University of Michigan Ford School, and the University of California – Riverside for very helpful comments. Special thanks also go to Jacob Brown, Ryan Enos, Todd Rodgers, and the Harvard Multidisciplinary Program in Inequality and Social Policy. Cameron Arnzen, Selena Cardona, Grant Goehring, Elizabeth Huffaker, Erin Huffer, Katharine Parham Malhotra, and Vignesh Somjit provided excellent research assistance. This study was deemed exempt from human subjects review by the Teachers College and University of Michigan Institutional Review Boards. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

At least one co-author has disclosed additional relationships of potential relevance for this research. Further information is available online at http://www.nber.org/papers/w29308

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JEL No. D72, H75, I21

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 estimate the impacts of charter attendance on academic and voting outcomes. We first confirm that there are large academic gains from charter school attendance. Second, we find that charter attendance boosts voter participation, substantially increasing voter participation 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 voter registration. Rich data enable us to explore multiple potential channels of this voting impact, and our evidence suggests that charters increase voting by increasing noncognitive skills.

Sarah Cohodes
University of Michigan
Gerald R. Ford School of Public Policy
and NBER
scohodes@umich.edu

James J. Feigenbaum
Boston University
Department of Economics
and NBER
jamesf@bu.edu

A online appendix is available at http://www.nber.org/data-appendix/w29308

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 of 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 classes of 2006–2017 who were old enough to vote in the 2016 or prior elections. We generate causal

[1] We focus on evidence from the United States that comes from causal identification strategies beyond matching. As Sondheimer and Green (2010) write, "From the early work of Merriman and Gosnell (1924) to today, literally thousands of cross-sectional surveys have indicated that turnout rates climb with years of formal schooling."

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 2018.

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 participation 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 civic participation operates exclusively in presidential general elections. Voter turnout in nonpresidential general elections and primary elections is low and is not affected by charter attendance. We also present evidence that the effects that we identify operate via the turnout margin, not the registration margin. That is, 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.

In the final section of the paper, we 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, and voting. We proxy for noncognitive skills with attendance and proxy for cognitive skills with standardized test scores. 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.[2] 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

[2] Sondheimer and Green (2010) study the Perry Preschool Project (a randomized controlled trial [RCT] on access to a demonstration preschool), Project STAR (an RCT on class-size reduction in Tennessee), and I Have A Dream (a scholarship and support program evaluated through a comparison of the program cohort with neighboring cohorts). All three interventions increased voting in addition to generating educational benefits.

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 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. Fortson et al. (2015) show that regression and matching techniques can come close to replicating the impacts of charter schools on test scores identified by a lottery design, but they do not match up perfectly. They also highlight that the availability of pretreatment test scores is key in interpreting the validity of these comparisons. While McEachin et al. (2020) have pretreatment data on test scores, it is unclear what the appropriate pretreatment variable would be for voting or other nonacademic outcomes, an issue that may undermine matching techniques in this context.[3] 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. This literature includes the previously discussed Gill et al. (2020), as well as 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

[3]With respect to estimating charter impacts in Texas using observational data, Dobbie and Fryer (2020) point out that Chetty et al. (2014) show prior test scores do not fully account for determinants of earnings whereas family income and background do. If voting behavior also has unobserved antecedents other than test scores, matching techniques based on pretreatment test scores will be biased—a problem eliminated by lottery-based designs.

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 gains demonstrated in 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.[4] In our context, we test five major theoretical explanations for why education may 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. 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.

Finally, we contribute to the understanding of the gender gap in voting behavior, 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

[4]This is true of both the older literature exploiting compulsory schooling laws and the two recent papers linking charter school attendance and voting—Gill et al. (2020) and McEachin et al. (2020). Hillygus (2005), Nie and Hillygus (2008), Holbein et al. (2020) and Holbein and Hillygus (2020) are notable exceptions that explore multiple potential mechanisms. However, these studies rely mostly on regression adjustment and matching techniques and do not feature natural experiments. Holbein (2017) is able to test multiple mechanisms in the context of an RCT, but the intervention studied there—FastTrack—focuses on specifically on psychosocial skills, rather than on education more generally.

are that women's educational gains over the past half century (Goldin et al., 2006; Goldin and Katz, 2010), 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.

The paper proceeds as follows. We next describe the setting and related literature on charter schools, our data, and our sample in Section 2. Section 3 describes the methods. Sections 4 and 5 discuss the results for academic outcomes and voting, respectively. We describe the theoretical connections between education and voting and then explore these mechanisms in Section 6 and conclude in Section 7.

2 Context, data, and descriptive statistics

To estimate the effects of charter school enrollment on civic participation, we link students to voting behavior—our primary civic outcome—in a well-studied charter school context. In this section, we provide background information on the Boston charter school sector, document the Massachusetts education data and the voter files used in this analysis, and describe the sample population.

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 schools in Massachusetts are also subject to a cap on the percentage of student funding that they can receive, and the City of Boston reached its cap many years ago, which means that the stock of charter schools is established and stable.[5] 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., 2013a; 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., 2013a; Walters, 2018; Cohodes et al., 2021). This finding is persistent in many situations: test score gains exist for students with special needs (Setren, 2021), persist when disciplinary regimes are loosened (Felix, 2020), and are maintained even when charter schools expand to multiple campuses (Cohodes et al., 2021). Additionally, the increases in standardized test scores do not appear to be driven by differential test preparation (Cohodes, 2016). 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). Recent evidence shows that urban Massachusetts charters (including the Boston schools) boost 4-year degree attainment as well (Cohodes and Pineda, 2024).

[5]A recent change in the charter school law raised the funding cap in low-performing school districts (including Boston), which allowed new charters to open. It also restricted expansion to charter schools that could show they were "proven providers" with a track record of success. The students at these new campuses are too young to have reached voting age by the 2016 election and are thus excluded from this analysis. See Cohodes et al. (2021) for details on this expansion and test score impacts.

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 (see Cohodes and Roy (2025) for an overview). Where it is possible to measure longer-term outcomes, lottery-based evidence shows that urban charter attendance beyond Boston similarly increases college enrollment and decreases risky behavior (Dobbie and Fryer, 2015; Davis and Heller, 2019; Wong et al., 2014; Harris and Larsen, 2019; Reber et al., 2023). Outside of urban areas, charter school impacts on both test scores and other outcomes are more mixed (see Chabrier et al. (2016), Epple et al. (2016), Zimmer et al. (2019), and 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, ELA, and science 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).[6] Lottery records typically include student names and dates of birth, and

[6]The ACDPW sample includes Academy of the Pacific Rim, Boston Collegiate, Boston Preparatory, City on a Hill, Codman Academy, and MATCH High School. We add five campuses to these schools: Boston Green Academy, a second City on a Hill campus, Edward Brooke Roslindale, Excel Academy, MATCH Middle School, and the Mission Hill campus of Roxbury Preparatory Schools (formerly Roxbury Prep). Three closed schools with appropriately aged children do not participate (here, or in ACDPW): Frederick Douglass Charter School (closed 2005), Roxbury Charter High School (closed 2005), and Uphams Corner Charter School (closed 2009). Two charter schools declined to participate: Kennedy Academy for Health Careers (formerly Health Careers Academy)

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, with very few differences by lottery status (Appendix Table C.5).

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 as having a "waitlist offer." These two mutually exclusive variables serve as instruments in our instrumental variables setup, which we describe in detail in Section 3. Students are assigned to a "projected high school class" based on their grade of application. This is the spring 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.[7] 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.[8] These files include the presidential elections of 2008, 2012, 2016, and 2020 and the

and Helen Davis Leadership Academy (formerly Smith Leadership Academy). A number of additional charter school campuses have been opened in Boston beyond the long-standing elementary schools, but the students for whom we have lottery records at these campuses are too young to have reached voting age by the 2016 election. See Setren (2021) for the most comprehensive coverage of the lottery impacts of Boston charter schools.

[7]States vary in the accessibility of voter files. Massachusetts maintains a centralized record, but access is limited; see `https://www.mass.gov/info-details/mass-general-laws-c51-ss-47c`. Commercial vendors collect this information from authorized parties, add their own analytics, and make these files available for purchase for political purposes.

[8]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

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[9] 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.

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

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.

[9]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 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.

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 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. In the parent analyses, to increase the sample size of lotteries with parent information, we include some more recent charter school lotteries for students who were not yet old enough to vote in 2016. We measure parent voter participation after the charter school lottery and create several measures of civic participation, similar to those used for students. The parent "ever voted" outcomes count voting in any election after their child's charter school lottery, and the "first possible presidential" outcomes indicate voting in the first presidential election after the lottery. We also observe voting prior to the charter school lottery, which we use as a "placebo" outcome to demonstrate lack of selection in our matching techniques. This also means we can compare voting rates before and after the lottery to see if changes in voting behavior come from charter lottery winners or losers, since lottery losers may be motivated to vote by their negative experiences. Since we have no parent information for non-charter-applicant families, we cannot compare charter school applicant family voting patterns to other parents in Massachusetts. Appendix Section F.1.1 has details on how our data and estimation strategy differ for the parent voting analysis. Similarly, since we only have parent information from the charter lottery files, and not state data, we cannot compare charter applicant parent voting rates to non-charter-applicant voting rates.

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 (based on cohorts of BPS 9th grade students with projected high-school graduation classes that match the charter school sample), 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 attending a charter school at one point during their K-12 school experience.[10] Consistent with successful randomization in the lotteries, charter lottery winners and losers are broadly similar

[10]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.

on all dimensions other than charter school attendance (Appendix Table C.3).

3 Empirical framework

In this section, we detail the empirical specification that we use to estimate the causal effect of charter attendance on civic outcomes and present the first stage results validating that lottery winners are more likely to attend charter school at some point and to attain more years of education at charter schools.

3.1 Methods

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, as is typical in the charter school literature (Abdulkadiroğlu et al., 2011; Angrist et al., 2013a, 2016; Cohodes et al., 2021; Cohodes and Pineda, 2024). 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.[11] 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

[11] 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).

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 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, the demographic covariates described above, 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 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.

3.2 First stage

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.[12] 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. The second row of the table reports charter attendance in years of attendance and shows that, on average, nonoffered students attend a charter for approximately one-half of a year, and students with an

[12]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, the vast majority of outcomes, including all of the voting outcomes, use any observed charter attendance after the lottery as an endogenous variable in the first stage.

initial or waitlist offer attend an additional 1.6 and 1.0 years, respectively. Girls and boys are equally responsive to charter school offers and are just as likely to enroll if they receive an offer.

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 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.[13] Over the course of high school, Boston charters induce an additional

[13]To show test score impacts combining schools with different grade levels, we estimate impacts on the MCAS exams two years after charter school entrance: the 10th grade exam for schools that begin in 9th grade, the 6th

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.[14] 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.[15] The boost in 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. This is driven by a nonsignificant 3 percentage point increase in bachelors degree (BA) attainment and a 2 percentage point increase in associates degree (AA) attainment.[16]

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. Students in the comparison group have approximately 12.5 years of education, and the charter attendance effect is less than 0.1 and not statistically significant. This lack of difference occurs because at this election, there is only a small window over which educational trajectories could have diverged. 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 similar to the 10th grade test score estimates in Abdulkadiroğlu et al. (2011) and ACDPW and, as expected because we present two-year test score impacts here, approximately twice

grade exam for schools that begin in 5th grade, and the 7th grade exam for schools that begin in 6th grade.

[14]Note that graduation rates shown here will be lower than published graduation rates since we count students who have transferred out of state as nongraduates.

[15]We define college enrollment as enrollment for at least one semester of college within 18 months of expected high-school graduation. This definition allows time for late high-school graduates to enroll, though findings are similar when we restrict the college enrollment window to 6 months after high-school graduation, as shown in Appendix Table A.6, which also shows differences by in-state and out-of-state college enrollment.

[16]The any degree coefficient does not equal the sum of the BA and AA coefficients because students may obtain both degrees.

the per-year test score estimates in Abdulkadiroğlu et al. (2011), Cohodes et al. (2021), and Setren (2021). 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

Students who attend Boston charter schools benefit academically, but do these educational gains extend beyond the classroom to civic participation? In this section, we show that they do, with our findings summarized in Figure 1. Though charter lottery winners and losers appear on the Massachusetts voter rolls at similar rates, the lottery winners are more likely to vote in presidential elections.

5.1 Voter registration and voter participation

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 (Columns 1-3 of Appendix Table A.7). Eighty-eight percent of control compliers are registered to vote, a share similar to that for charter attendees.[17] 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. We see a that a little under half of voter registration occurs by a student's 19th birthday, with approximately 45 percent of students registering before they are 19; again, there is no difference by offer status. Approximately half of our sample of charter applicants are registered as Democrats, which is not surprising given their age, race, and economic background and that we are studying Massachusetts. The remaining students are primarily registered as independent

[17] Registering to vote in Massachusetts can be done online, through mail, or in person. Citizens are also automatically registered to vote when renewing a driver's license or state ID at the RMV or when applying for health insurance through the state health exchange unless the individual opts out of registering. In Massachusetts, individuals may also preregister to vote starting at 16. Once they turn 18, their status is converted from preregistered to registered, and they are notified of this change to their voter record. The Massachusetts Secretary of State maintains the processes and procedures of registering to vote and voting, which can be found at `https://www.sec.state.ma.us/ele/eleifv/howreg.htm`.

voters. We see no effects on party choice in registration or in presidential primary voting.[18]

Charter attendance does affect voter participation, 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 (Columns 7-10 of 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 number by 3 percentage points (Column 8). Observing voting in the first possible general election yields a charter increase in voting by 5.2 percentage points (Column 9). This is due to the fact that the majority of the sample's first general election is a presidential one.

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 and Columns 4-6 of Appendix Table A.7) but does not increase voting in nonpresidential general elections or primaries (Columns 10-12 of 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, while voting rates were 40 percentage points higher for those ages 60 and above (McDonald, 2019, based on the CPS). We thus focus on presidential elections as our main outcome of interest given the high level of turnout for these.[19]s

We thus turn to our key measures of voting: voting in the first possible presidential election

[18]Massachusetts has a semi-closed primary system, where voters can only vote in the primary of their party but registered voters can switch party registration on election day. Our findings on party registration stand in contrast to evidence from desegregation (Kaplan et al., 2019) and resegregation (Billings et al., 2021), both of which show that exposure to Black peers in school increases white voters' propensity to register as a Democrat/reduces the propensity to register as a Republican but does not affect turnout. However, the differences in peer group demographics induced by charter schools in Boston are small, which influences the lack of change we see here.

[19]Local elections could be interesting because they might be closer to the education policy process; however, we do not investigate municipal elections for several reasons. First, turnout in these elections is very low, making it difficult to detect differences across groups. In Boston, the relevant elections are for Mayor and City Council, which happen in nonpresidential, non-general-election years; the school board is appointed by the mayor. Additionally, many students move out of Boston, and the relevant local elections occur at different times in different communities, making it difficult to understand which election is relevant to each student and thus to voting estimate impacts.

and share of presidential elections voted in (Panel B of Figure 1). Boston charter attendance boosts voting in the first possible presidential election (the first presidential election after a student turns 18) 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 and thus to preserve our sample. Additionally, the first possible presidential 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 Figure 3 Panel A, which splits the sample between cohorts who experience their first presidential election close to the time they turn 18 (and are still potentially in high school) 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 significant. Notably, at this point in time, there is *no* difference in years of education obtained (Figure 3, 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 Figure 2, which documents a persistent charter effect into the second possible presidential election but convergence by the third. Panel A of this figure includes all cohorts of data; thus it is possible this pattern is a result of the composition of the sample changing across outcome windows. However, Panel B displays the impacts over time only for the cohorts for whom we observe all three presidential elections and shows the same, if not steeper, gradient. It is not possible to fully determine if this finding reflects a shorter-term effect or if charter students are more likely to move out of state by the time of the third possible presidential election. To bring some evidence to bear on this issue, Panels C and D exclude students who attend college out-of-state. This selected sample limits the causal interpretation of the impact estimates but illustrates the same pattern as the samples above, implying that convergence is the more likely explanation than out-of-sample moves.[20]

We also examine whether the Boston charter attendance effect on voting varies by student background characteristics in Figure 4 (details in Appendix Table A.8). To focus on a few key voting measures in our subsequent analyses, we primarily use the outcomes for ever registered to vote, share voted of presidential elections, and voted in first possible presidential election. 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).[21] 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 analysis by Cascio and Shenhav (2020) using cross-cohort and cross-state variation, and our findings add causalss evidence of the connection

[20]An alternative approach would be to link our Massachusetts sample to a national voter file. However, we are limited by the fact that both our education data only includes names and dates of birth. Further, with few exceptions, voter files do not include state of birth. Given the limited information available to us, concerns about false positive matching are strong enough to stop us from undertaking a national match. Linking to the Massachusetts voter file and to the voter files of neighboring states, in our view, is much less problematic.

[21]There are more girls than boys in the charter school applicant pool in this study—a fact consistent with broader findings that girls are more likely to enroll in charter schools than boys (Corcoran and Jennings, 2018). However, the greater number of girls does not account for the differences that we see between genders since we estimate impacts separately by gender. It is true that the disproportionate presence of girls in charter schools could contribute to a peer effect that contributes to the charter impacts, as classrooms with greater shares of girl students demonstrate such a peer effect (Hoxby, 2000; Lavy and Schlosser, 2011; Hu, 2015). However, the difference in the girl student share between BPS (48 percent girls) and the charter applicant pool (52 percent girls) is small, as are the gender peer effects estimated elsewhere. The estimates for Israel from Lavy and Schlosser (2011) on the effects of the girl student share on high-school test scores imply that a 10 percent increase in share of girls in a classroom (approximately twice the difference we observe) would increase test scores by 0.02 to 0.03σ. This is quite a small share of the test score impacts that we observe. A peer effect may operate differently for nontest outcomes, but Lavy and Schlosser (2011) also shows small gender peer effect impacts on high-school graduation and none on behavior incidents.

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.

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.

5.2 Threats to validity

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.1. We detail these findings in Appendix B.1.2. 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

Having established that Boston charter schools boost voting rates in addition to instilling an academic edge, we turn to considering *how* Boston charters increase civic participation. We begin with a discussion of the theoretical channels through which education might increase voting, and continue to testing each potential channel in this empirical setting.

Americans with more education vote more (McDonald, 2019). 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 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.[22] We demonstrate in Figure 3 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) 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

[22] Rigorous evidence from Europe generally finds less of a link between education and voting. Milligan et al. (2004), in contrast to their findings in the U.S., do not see a robust link between education and voting in the U.K. Comparing twins, Dinesen et al. (2016) find effects of education in Denmark but not in Sweden. Focusing on compulsory school laws, Siedler (2010) and Pelkonen (2012) find, respectively, that German and Norwegian education reforms that increased years of schooling did not increase voting rates. Lindgren et al. (2019) also find that a Swedish education reform that increased years of education did not boost overall voting rates, but it did increase participation for students from families in the lowest quartile of socioeconomic status. A related literature explores the connection between education and civic participation in the developing world (Wantchekon et al., 2015; Friedman et al., 2016; Croke et al., 2016; Larreguy and Marshall, 2017) and, for the most part, finds that education increases voting and related political engagement. By contrast, Croke et al. (2016) show that in the context of an authoritarian regime, education actually decreases voting, with nonparticipation being a form of protest or "deliberate disengagement."

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).[23] 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 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

[23]Developing cognitive skills might lead a student to the rational choice model of voting, which posits that voting is irrational because the likelihood of changing the outcome of an election is minuscule (Downs, 1957).

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. In the survey component of the NAEP, 52 percent of traditional public school students reported a class with a main focus on civics or U.S. history in 8th grade, compared to 47 percent of charter school students.[24]

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 (i.e., registration, finding one's polling place, researching candidates, and showing up on time) 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 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

[24]For NAEP results by school type, see `https://www.nationsreportcard.gov/dashboards/schools_dashboard.aspx`

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 new social norms; in particular, attending a residential college may introduce students to new communities that may be more central to the political process. 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. Campbell (2013) argues that a focus on individual factors has left the contribution of social networks to political participation understudied. 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) 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. For example, Michener (2018) shows that the unequal benefits and administrative burdens associated with Medicaid make beneficiaries less likely to vote. In the realm of education, Hastings et al. (2007) find 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. All of these cases are consistent with the idea that negative interactions with policies are particularly salient for motivating political participation. Charter schools might boost the participation of families that attend; however, the salience of negative policy interactions indicates that the experience of losing a charter school lottery might be a more powerful force.

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, charter schools represent increased privatization; exposure to them may induce students and families to be less likely to vote. Cook et al. (2020) find evidence that Ohio school districts with higher shares of charter school enrollment have slightly lower voter turnout at elections which include local school boards, giving credence to the possibility of negative effects. A lower rate of voting could also be due to dissatisfied, charter lottery-losing families heading to the ballot box at a higher rate, similar to district school families in Hastings et al. (2007).

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 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 setting lacks variation to test different mechanisms behind the boost in voting.[25] 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, as suggested by Wolfinger and Rosenstone (1980); Verba et al. (1995); Nie et al. (1996), 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

[25] Gill et al. (2020) do test whether parents' voting patterns differ after the charter school lottery and find no evidence of such a change.

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 4 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 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 6, with each group weighted by sample size. The specifics of these estimates are 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).

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, collected from their websites. We categorized a school as civics-oriented if their mission statement mentioned "civic(s)," "citizenship," or "community." Non-civics-oriented mission statements tended to be focused 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." College preparation could certainly involve a civics orientation, but it is not an explicit part of the stated mission. Schools with a citizenship or community focus may be more analogous to Democracy Prep (Gill et al., 2020), whose mission statement is: "The mission of Democracy Prep Public Schools is to educate responsible citizen-scholars for success in the college of their choice and a life of active citizenship."

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. This multiple endogenous variable approach has also been used in Angrist et al. (2013b) and Cohodes et al. (2021) to estimate charter effects by school type. We show the results from this setup in Figure 5, 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 share of presidential elections (6.5 percentage points versus 4.7 percentage points) and voting in the first possible presidential election (7.5 percentage points versus 3.4 percentage points). However, these differences are within the variation we would expect given a small number of schools, with p-values for the differences across school types indicating no statistically significant differences (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, and is a contrast to Holbein (2017), which shows that Fast Track, an intervention specifically designed to increase psychosocial skills, increases voting and has more direct measures of these skills. 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 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.[26]

As for the MCAS outcomes we use to consider the cognitive channel, for the noncognitive channel, we present the correlation between subgroup impacts on the attendance (our proxy for noncognitive skills) and impacts on voting in Panel B of Figure 6 (point estimates and standard errors are in Appendix Table A.10). 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 there are limitations behind this test (it does not establish causality, as a different underlying factor could move both noncognitive skill and voting), we conclude that this exercise provides strong suggestive evidence for the noncognitive channel.

The convergence of the charter voting effect over time (Figure 2) is 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 (McCrae et al., 2005; Costa et al., 2019; 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 via charter schools themselves as well as via college enrollment, charter school attendance has the potential to expose students to peers they would not otherwise interact with. We describe the new peers charter students are exposed to in both the K-12 and college communities and consider whether these new exposures

[26] We also construct an alternative measure of noncognitive skills, followingJackson (2018) and use the first principal component of a number of administrative measures of student outcomes as our noncognitive index. 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 attempt to follow Jackson (2018) but make some deviations because of data differences. 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.1 Panel B shows the same pattern of results we next describe using attendance alone.

can account for the observed voting gains.

6.5.1 School social networks

While the charter school lotteries are balanced on observable characteristics (Appendix Table C.3), it can still be the case that charter schools enroll a different peer group than counterfactual schools, since, as seen in Table 1, 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 causality of 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 7, following a similar exercise from ACDPW. Peer characteristics are measured at the school by 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 equations 1 and 3.

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 completing middle school in three years and now have traditional public school peers in their fourth post-lottery year.)

Focusing on predicted voting, the gap here has the least closure. It is worth discussing the source of the difference. It 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 this 5.9 percentage point result, the peer effect would have to be almost 100 percent (and larger than 100 percent in subsequent years) to explain the full result. 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 specifically. 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.[27]

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 secondary school social networks can account for a little more than 40 percent of the observed 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.[28] In sum, there is scope for peer effects to account for a small to meaningful fraction of the impact on voting but they cannot explain it all.

If we believe that attendance is a good proxy for noncognitive skills, 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. But again, peer effects would have to be transmitted at a very high rate to account for the charter induced attendance difference, as the gap in attendance in the first post-lottery year is

[27]Brown et al. (2023) also estimate effects of additional years of exposure to higher voting places in ages 0 to 12, but these are substantially lower, on the order of 0.015 points.

[28]The Brown et al. (2023) estimates account for all of the effects of living in a higher or lower turnout location, not just school environment. A 15 percent transmission rate would be the equivalent of living nearly 3 extra years in a higher voting location.

3.3 days compared to a treatment effect of 4.7 days in 9th grade (Appendix Table A.2). 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 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 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.[29] 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.[30] College location data from IPEDS identifies the county

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

[30]See https://uselectionatlas.org/.

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 schools 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 2. 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. 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. Using the estimates of college impacts on voting rates from Bell et al. (2024), 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 1.2 percentage points, or 20 percent of the voting gain we observe here ($\frac{0.012}{0.059}$). 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 2). 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 schools has become more politically polarizing (Reckhow et al., 2015).[31] 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

[31]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.

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.[32] 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 Table 3. 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 in that election, but we still find no effect on parental voting.

6.7 Summarizing the evidence on mechanisms

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 alignment between noncognitive skill gains and voting gains at the subgroup level is consistent with noncognitive skills being a mechanism for the voting effect. 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 very large. When it comes to the cognitive channel, our evidence points to it *not* being an explanation for any voting gains that we see. Finally, evidence from parent voting is not suggestive of the policy feedback mechanism. The evidence on noncognitive skills has the most support of the five mechanisms we test, which is consistent with Holbein (2017).

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

[32] Alternatively, voter participation could decrease for parents of charter lottery losers as losing the lottery may increase distrust in government systems, as in school choice lottery losers in Hastings et al. (2007).

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. 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. We have no evidence of traditional cognitive gains or policy feedback driving voting. There is some suggestive evidence for the civic education and social network channels. The early-in-life impacts also rule out the income channel.

Of course, charter school attendance is not the only educational intervention that increases noncognitive skills. Examples of such interventions are documented in Martins (2017), Battaglia et al. (2020), Holbein (2017), and Kautz and Zanoni (2024). 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; Deming, 2017). Inasmuch as girls are better able to develop their noncognitive skills at a 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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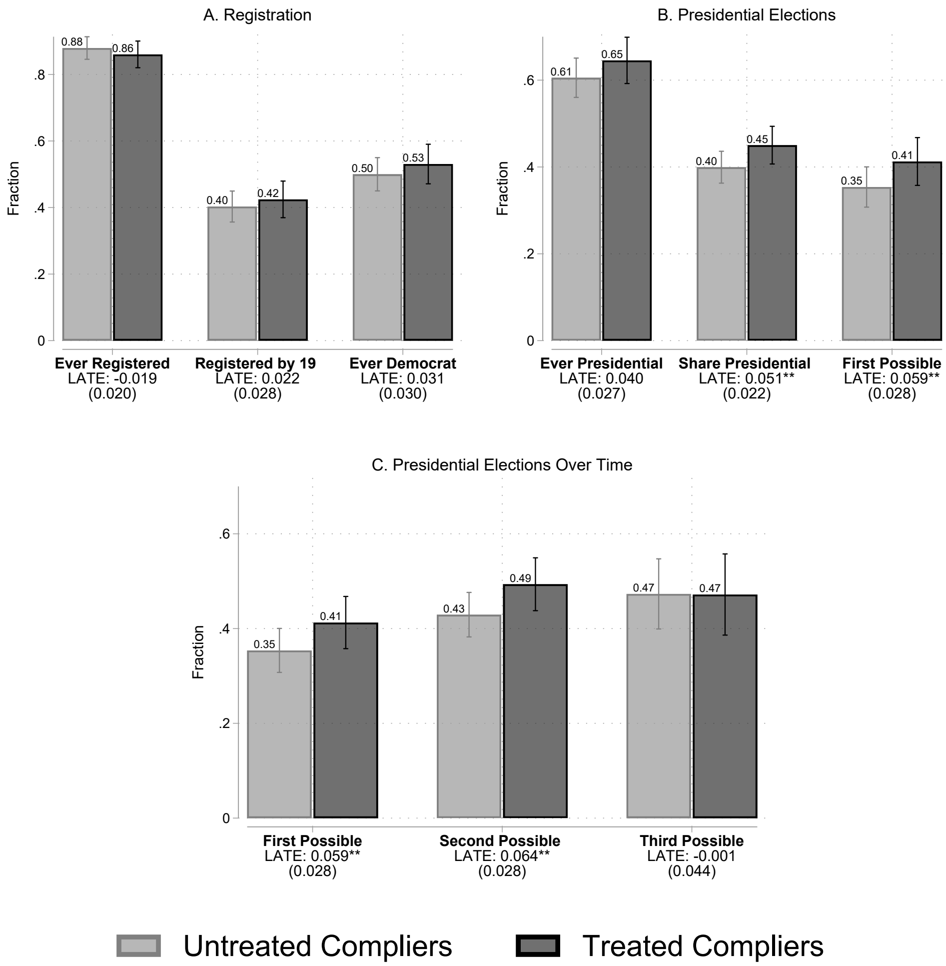
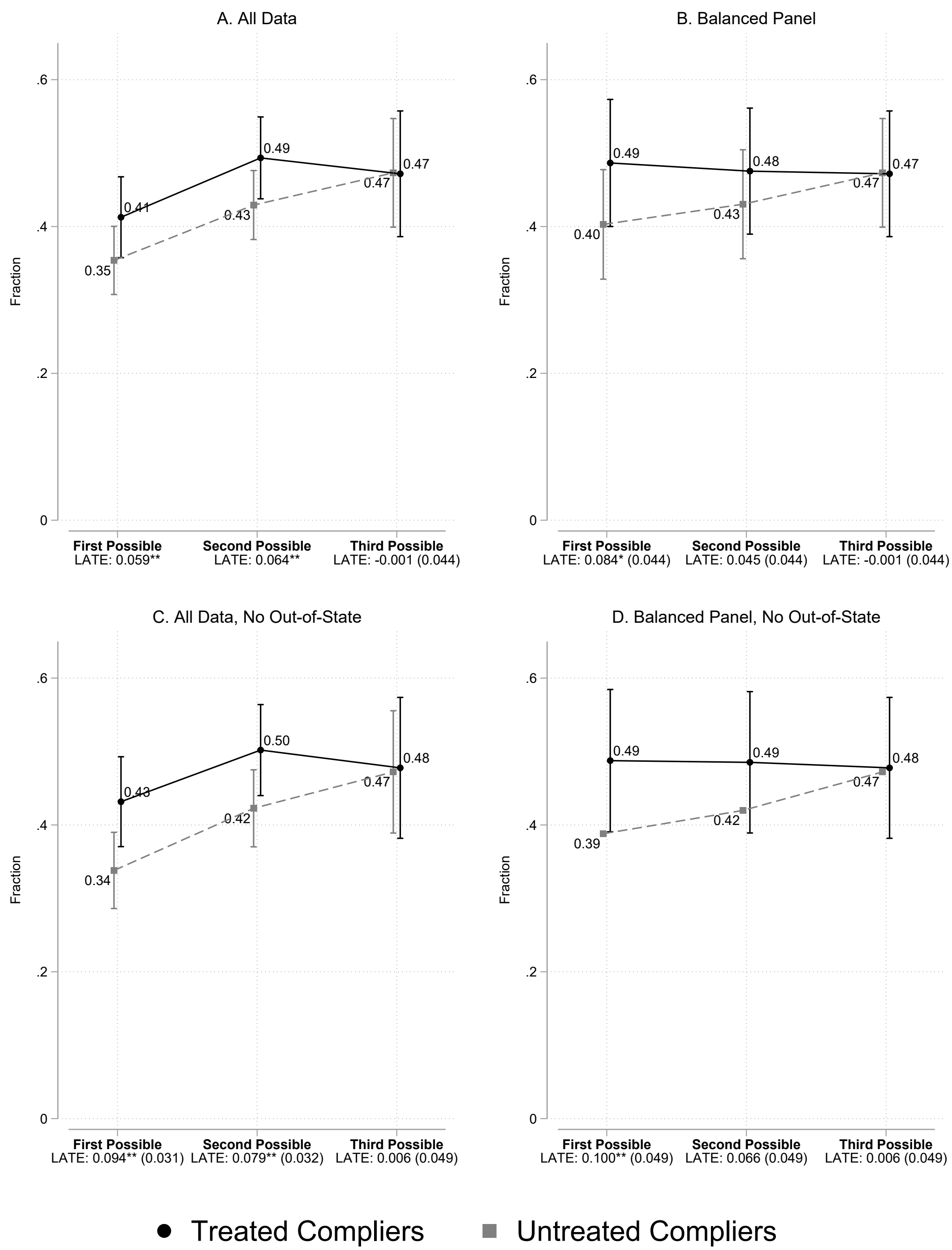
Figure 1: The Impact of Charter School Attendance on Voting



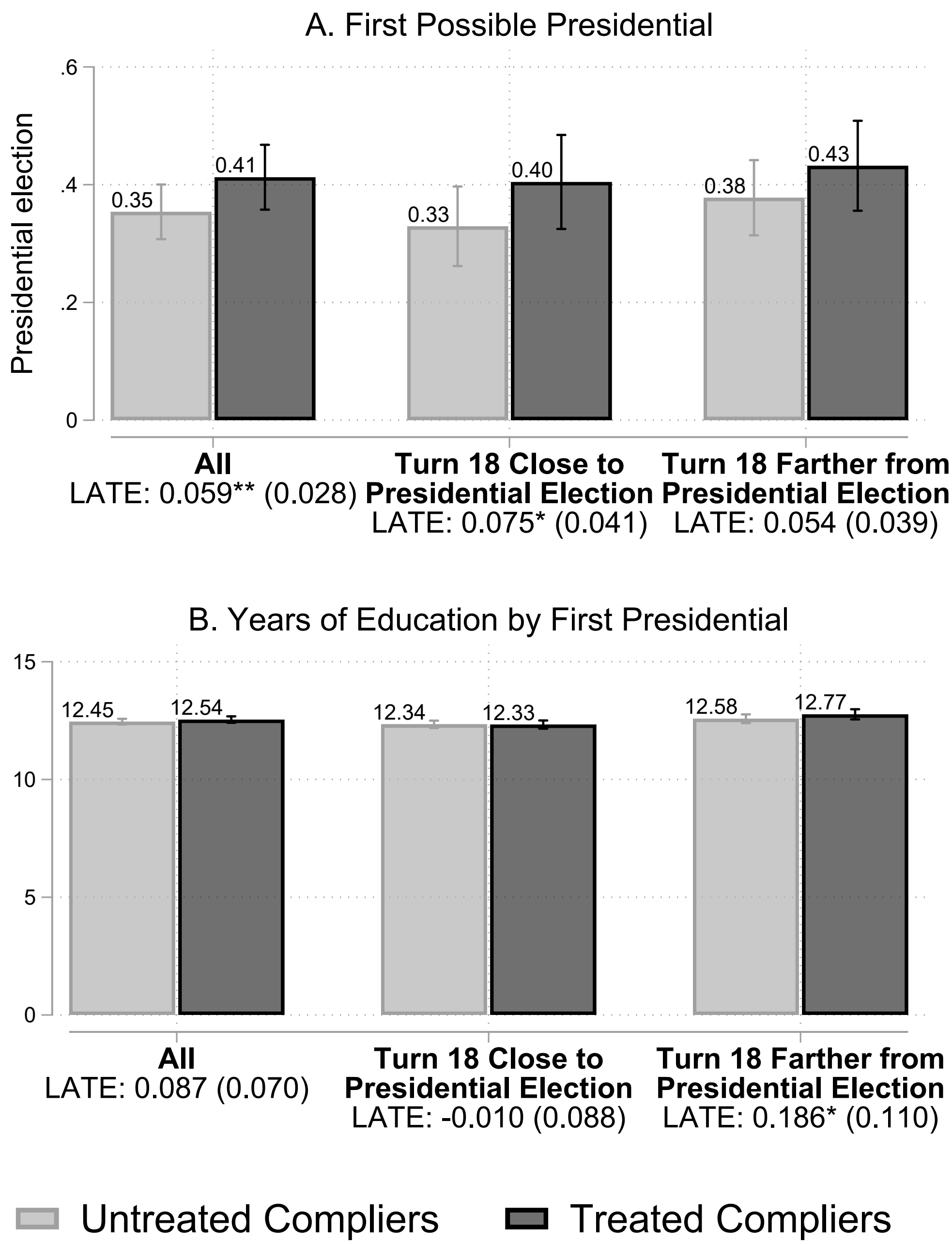
Notes: This figure shows impact estimates of Boston charter school attendance on voting outcomes. For details, see Appendix Table A.7. N = 9,560, * p<0.10 ** p<0.05 *** p<0.01.

Figure 2: The Impact of Charter School Attendance on Voting Over Time



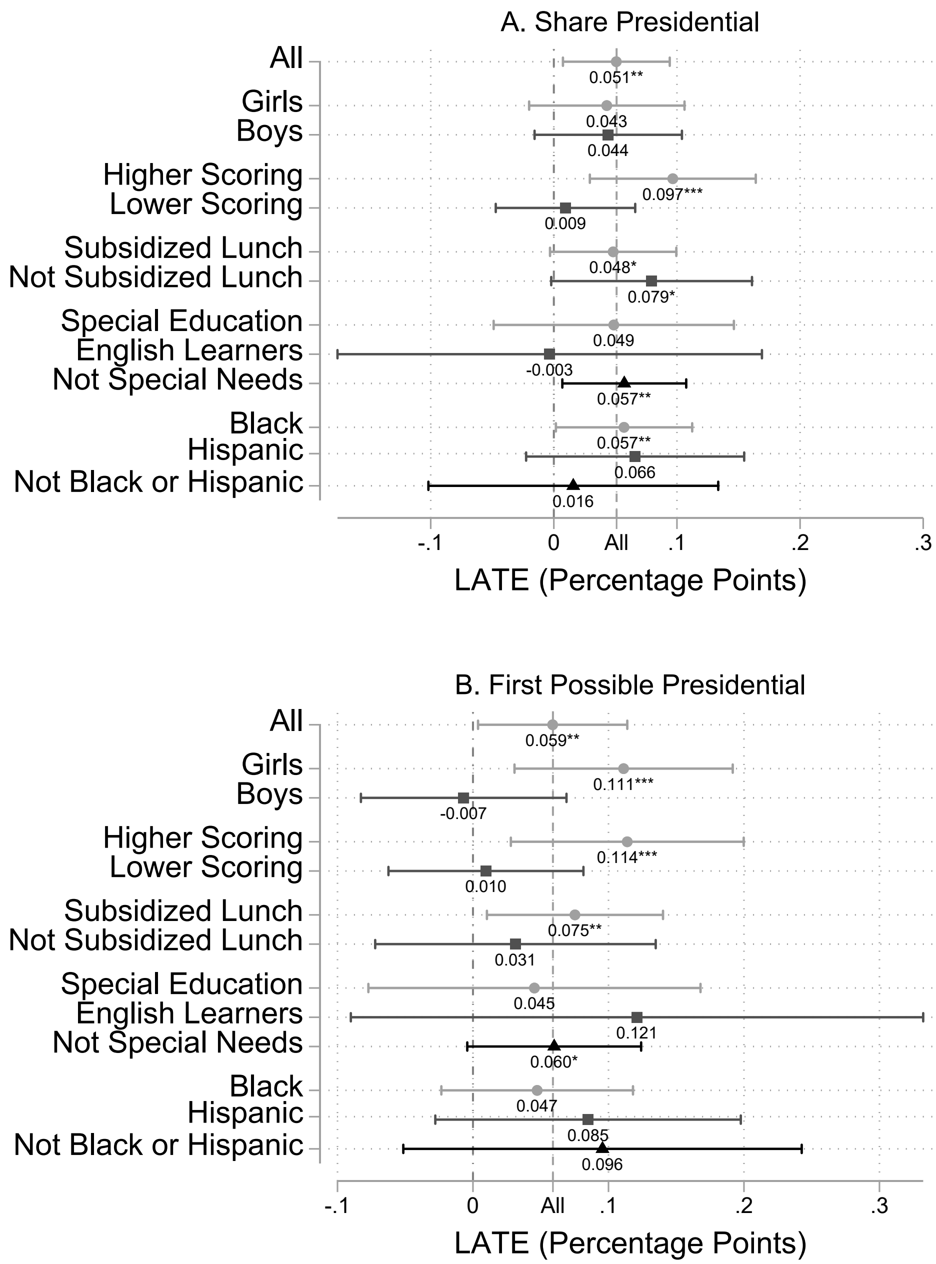
Notes: This figure shows impact estimates of Boston charter school attendance on voting in presidential elections over time. Panel A: N (first and second possible presidential elections) = 9,560, N (third possible presidential election) = 4,671, Panel B: N = 4,671, Panel C: N (first and second possible presidential elections) = 8,147, N (third possible presidential election) = 3,914, Panel D: N = 3,914,* p<0.10 ** p<0.05 *** p<0.01.

Figure 3: The Impact of Charter School Attendance on Voting and Years of Education by Age when First Eligible to Vote



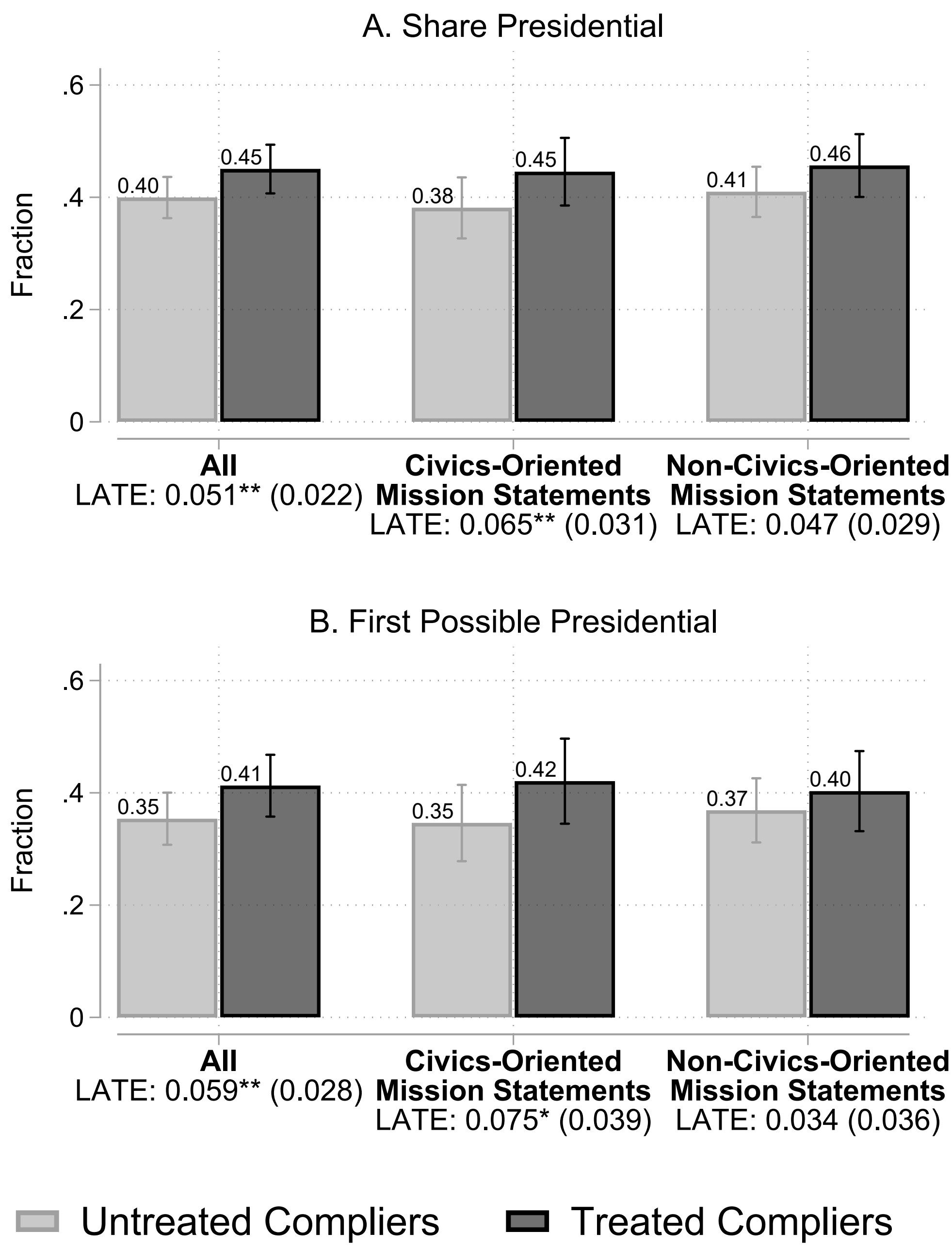
Notes: This figure shows impact estimates of Boston charter school attendance on voting in the first possible presidential election and years of education measured at that election date, split by students turned 18 in the two years prior to the election or later. For details on the estimates, see Appendix Table A.9. N = 9,560, * p<0.10 ** p<0.05 *** p<0.01.

Figure 4: The Impact of Charter School Attendance on Voting by Subgroups



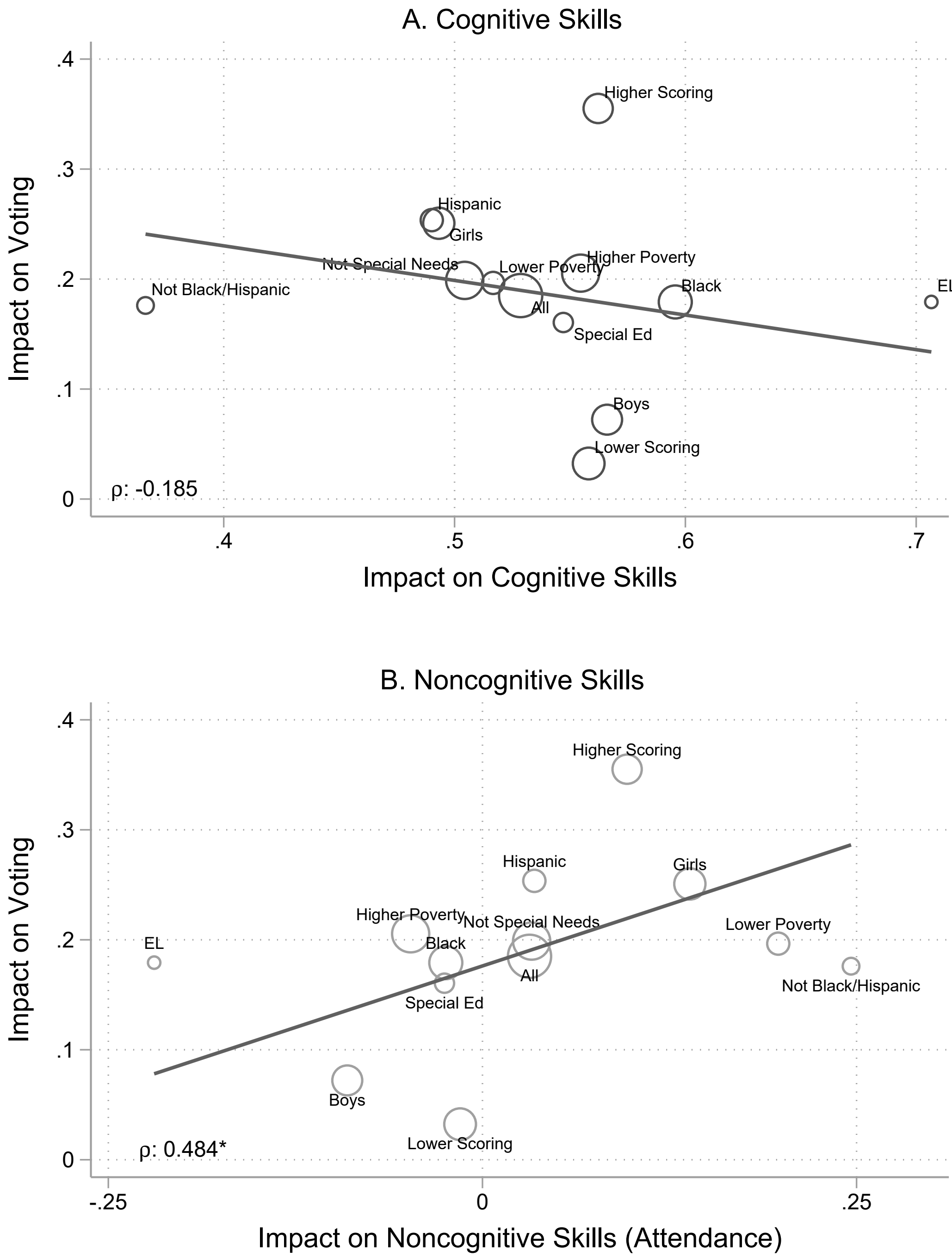
Notes: This figure shows impact estimates of Boston charter school attendance on voting outcomes for different subgroups of students. N varies by subgroup. For details, see Appendix Table A.8. * p<0.10 ** p<0.05 *** p<0.01.

Figure 5: The Impact of Charter School Attendance on Voting by School Mission Statement



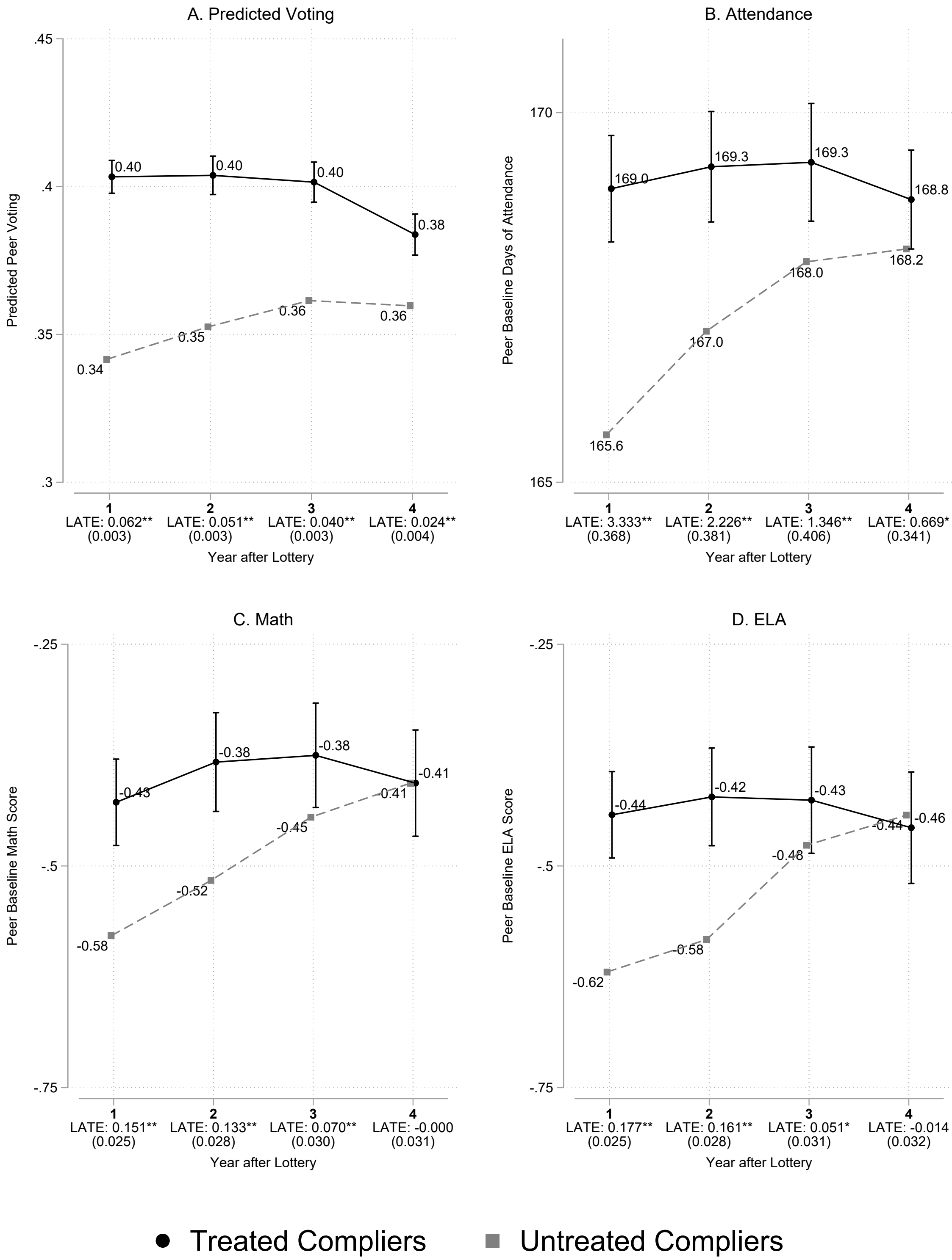
Notes: This figure shows impact estimates of Boston charter school attendance on voting, split by whether or not a charter school's mission statement is "civics-oriented." For details on the estimates, see Appendix Table A.11; for details on the missions statements see Appendix Table A.12. N = 9,560,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

Figure 6: Correlation between Charter School Impacts



Notes: This figure plots 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.

Figure 7: 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.

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.

Table 2: The Impact of Charter School Attendance on College-Community Civic Participation and Other College Outcomes

	College-Community Civic Participation					Other College Outcomes		
	College Registration Rate (1)	College Ever Voting Rate (2)	College First Presidential Rate (3)	County Registration Rate (4)	County Turnout Rate (5)	College Quality (6)	College Persistence (7)	Solo College Attendee (8)
2SLS	0.017^{**}	0.030^{***}	0.018^{**}	0.004	0.006^{*}	3841.463^{***}	0.091^{***}	0.060^{**}
	(0.007)	(0.011)	(0.008)	(0.002)	(0.003)	(1053.161)	(0.028)	(0.024)
CCM	0.795	0.580	0.349	0.623	0.674	37808.266	0.423	0.209
N	9,560	9,560	9,560	9,528	9,528	9,560	9,560	9,560

Notes: The outcomes here substitute college or community registration/voting rates for individual voter behavior, with voting rates based on the voting patterns of non-charter Massachusetts students or county level measures (based on college location). College quality is the estimated 2014 earnings of college attendees from the 1980-1982 birth cohorts from Chetty, et al. (2017). Solo college attendee means that the student is the only person from their gender and high school attending their college. All other notes are the same as in Online Appendix Table A.7 and A.5.

Table 3: The Impact of Charter School Attendance on Parent Voting

	Registration		Voting After Lottery			Placebo
	Ever Registered (1)	Registered After Lottery (2)	First Poss. Pres. (3)	Any Pres. (4)	Pres. 2016 (5)	Pres. Before Lotto (6)
(A) All Matches, Unweighted						
2SLS	-0.001	-0.008	0.007	-0.006	0.010	-0.025
	(0.030)	(0.024)	(0.030)	(0.029)	(0.031)	(0.031)
CCM	0.690	0.185	0.609	0.703	0.581	0.575
(B) All Matches, IPW						
2SLS	0.010	-0.037	0.022	0.011	0.016	-0.000
	(0.035)	(0.026)	(0.033)	(0.031)	(0.034)	(0.035)
CCM	0.663	0.178	0.619	0.702	0.610	0.585
(C) Matched to 1, Unweighted						
2SLS	-0.018	-0.016	0.010	-0.002	0.023	-0.010
	(0.034)	(0.029)	(0.034)	(0.035)	(0.035)	(0.034)
CCM	0.653	0.226	0.453	0.578	0.413	0.403
(D) Matched to 1, IPW						
2SLS	-0.021	-0.044	0.007	-0.002	0.011	-0.008
	(0.038)	(0.034)	(0.038)	(0.039)	(0.039)	(0.038)
CCM	0.618	0.223	0.421	0.544	0.404	0.372
(E) Collapsed, Unweighted						
2SLS	-0.010	-0.022	-0.000	-0.007	0.014	-0.030
	(0.030)	(0.025)	(0.032)	(0.030)	(0.032)	(0.032)
CCM	0.683	0.195	0.596	0.687	0.566	0.566
(F) Collapsed, IPW						
2SLS	-0.019	-0.059*	0.024	0.007	0.017	-0.006
	(0.036)	(0.029)	(0.036)	(0.033)	(0.036)	(0.037)
CCM	0.672	0.193	0.593	0.682	0.589	0.569

Notes: Each coefficient labeled 2SLS is the instrumental variables estimate of attending a Boston charter with a lottery and parent name information in the first two years after the lottery. Indicator variables for a lottery offer on the day of the lottery (initial offer) and lottery offer off of the waitlist (waitlist offer) are the instruments for charter attendance. The control complier mean is labeled CCM. All regressions control for lottery risk sets and a vector of demographic characteristics. The sample is restricted to students enrolled Boston Public Schools or Boston charter schools who applied to charter schools in 2008 to 2016 in lotteries with parent name information. Panels A and B (N = 10,861) include all matches to the voting data, including multiple matches for parents with common names. Panels C and D (N = 7,132) include only the parents matched to a single name in the voter file, or matched to no names, excluding those matched to multiple matches. Panels E and F (N = 7,615) are averaged at the student level, which takes the mean voting outcome both for students associated with multiple parents, and those associated with multiple matches. Panels B, D, and F inverse propensity weight based on likelihood of having a match in the voter data. Robust standard errors clustered by student are in parentheses (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001).

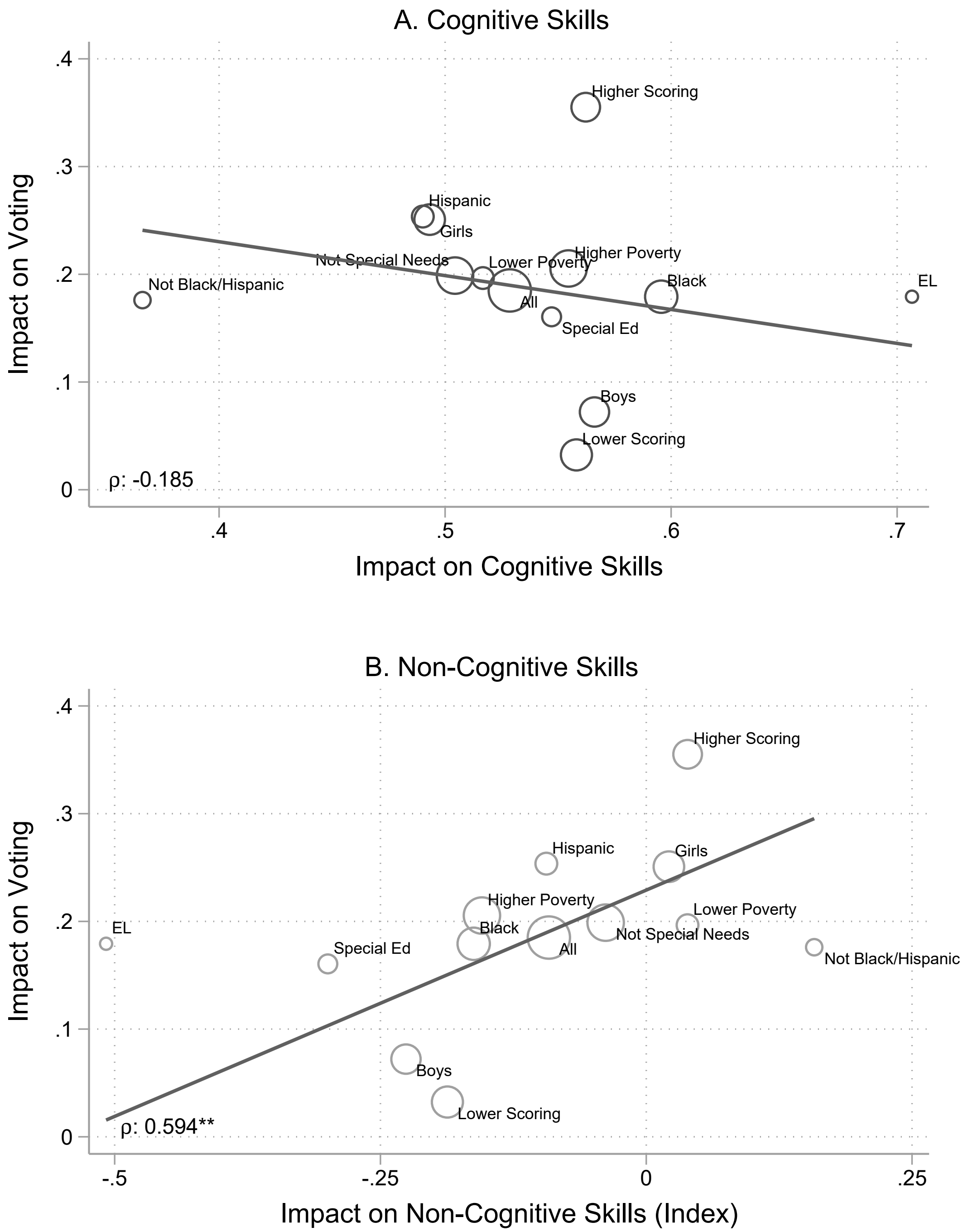
Online Appendix

Why Does Education Increase Voting?

Sarah R. Cohodes and James J. Feigenbaum

Appendix A: Detailed Impact Estimates and Additional Results

Figure A.1: Correlation between Charter School Impacts, Alternative Non-Cognitive Skill Measure



Notes: This figure plots the impact estimates for voting against indices of cognitive and noncognitive skills (attendance, suspension, and on-time grade progression) 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.

Table A.1: The Impact of Charter School Offers and Attendance on Academics

	MCAS		Advanced Placement (AP)					SAT	
	Math Score (1)	ELA Score (2)	Took Any AP (3)	Number of APs (4)	Score 3+ Any AP (5)	Took US Hist or Gov't (6)	Score 3+ US Hist or Gov't (7)	Took SAT (8)	Reasoning Score (1600) (9)
Reduced Form									
Initial Offer	0.247***	0.154***	0.050***	0.159***	0.016**	0.056***	0.010**	0.023*	17.994***
	(0.025)	(0.024)	(0.013)	(0.037)	(0.008)	(0.009)	(0.005)	(0.014)	(5.672)
Waitlist Offer	0.198***	0.142***	0.048***	0.133***	0.017**	0.044***	0.010**	0.018	18.926***
	(0.027)	(0.025)	(0.013)	(0.037)	(0.008)	(0.009)	(0.005)	(0.014)	(5.983)
Non-offered mean	-0.344	-0.381	0.269	0.621	0.087	0.080	0.028	0.529	853.255
Second Stage									
2SLS	0.498***	0.318***	0.113***	0.353***	0.037**	0.124***	0.022**	0.051*	38.465***
	(0.048)	(0.046)	(0.026)	(0.077)	(0.017)	(0.018)	(0.010)	(0.029)	(11.002)
CCM	-0.370	-0.397	0.279	0.661	0.096	0.081	0.028	0.544	863.656
N	7,555	7,659	8,937	8,937	8,937	8,937	8,937	8,937	4,947

Notes: Each coefficient labeled Initial Offer or Waitlist Offer is the reduced form estimate of an offer of seat at a Boston charter on the outcome listed in the row heading. Each coefficient labeled 2SLS is the instrumental variables estimate of attending a Boston charter with a lottery at any period of time before the outcome listed in the row heading occurred. Indicator variables for a lottery offer on the day of the lottery (initial offer) and lottery offer off of the waitlist (waitlist offer) are the instruments for charter attendance. The control complier mean is labeled CCM. All regressions control for lottery risk sets and a vector of demographic characteristics including indicators for race, birth year, and baseline special education, English learner, and free or reduced price lunch status, all interacted with gender. The sample is restricted to students 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. MCAS scores are for the first attempt at the exam two years after the charter school lottery (the 10th grade exam for schools that begin in 9th grade, the 6th grade exam for schools that begin in 5th grade, and the 7th grade exam for schools that begin in 6th grade). AP and SAT outcomes are available for the class of 2007 and later.

Table A.2: The Impact of Charter School Attendance on High School Days Attended

	9th Grade (1)	10th Grade (2)	11th Grade (3)	12th Grade (4)	All High School (5)
(A) Days Attended					
Reduced Form					
Initial Offer	2.101**	0.281	1.750*	1.870*	5.630**
	(0.975)	(0.918)	(1.033)	(1.023)	(2.389)
Waitlist Offer	2.395**	0.115	0.914	1.234	5.336**
	(1.026)	(0.973)	(1.100)	(1.061)	(2.576)
Non-offered mean	160.152	160.995	157.192	153.763	654.158
Second Stage					
2SLS	4.719**	0.552	3.505*	3.801*	11.974**
	(1.943)	(1.835)	(2.067)	(2.011)	(4.675)
CCM	163.350	164.165	156.641	153.340	657.644
N	8,258	7,701	7,144	7,136	6,354
(B) Present in Data					
Reduced Form					
Initial Offer	0.015*	0.002	-0.011	-0.011	-0.002
	(0.009)	(0.011)	(0.012)	(0.012)	(0.013)
Waitlist Offer	0.001	-0.001	-0.006	-0.012	-0.005
	(0.010)	(0.011)	(0.012)	(0.012)	(0.013)
Non-offered mean	0.867	0.811	0.757	0.754	0.672
Second Stage					
2SLS	0.030	0.004	-0.023	-0.026	-0.006
	(0.019)	(0.023)	(0.025)	(0.025)	(0.027)
CCM	0.891	0.842	0.805	0.813	0.718
N	9,560	9,560	9,560	9,560	9,560

Notes: Each coefficient labeled 2SLS is the instrumental variables estimate of attending a Boston charter with a lottery at any period of time before the outcome listed in the row heading occurred. Indicator variables for a lottery offer on the day of the lottery (initial offer) and lottery offer off of the waitlist (waitlist offer) are the instruments for charter attendance. The control complier mean is labeled CCM. All regressions control for lottery risk sets and a vector of demographic characteristics including indicators for race, birth year, and baseline special education, English learner, and free or reduced price lunch status, all interacted with gender. The sample is restricted to students 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. Robust standard errors are in parentheses (* p<0.10 ** p<0.05 *** p<0.01).

Table A.3: The Impact of Charter School Attendance on High School Suspensions

	9th Grade (1)	10th Grade (2)	11th Grade (3)	12th Grade (4)	All High School (5)
(A) Ever Suspended					
Reduced Form					
Initial Offer	0.046***	0.018*	0.025**	0.009	0.058***
	(0.010)	(0.010)	(0.010)	(0.009)	(0.015)
Waitlist Offer	0.007	-0.003	0.012	0.001	0.016
	(0.010)	(0.010)	(0.010)	(0.009)	(0.015)
Non-offered mean	0.124	0.124	0.096	0.083	0.234
Second Stage					
2SLS	0.085***	0.031	0.049**	0.016	0.109***
	(0.020)	(0.020)	(0.019)	(0.017)	(0.028)
CCM	0.146	0.141	0.092	0.080	0.231
N	8,258	7,701	7,144	7,136	6,354
(B) Days Suspended					
Reduced Form					
Initial Offer	0.122	0.032	0.019	-0.041	0.256*
	(0.089)	(0.087)	(0.049)	(0.063)	(0.148)
Waitlist Offer	-0.061	-0.028	0.051	0.047	0.055
	(0.118)	(0.100)	(0.082)	(0.084)	(0.199)
Non-offered mean	0.453	0.391	0.303	0.273	1.108
2SLS	0.194	0.047	0.054	-0.057	0.479*
	(0.193)	(0.168)	(0.110)	(0.120)	(0.288)
CCM	0.606	0.511	0.271	0.295	1.065
N	8,258	7,701	7,144	7,136	6,354

Notes: Each coefficient labeled 2SLS is the instrumental variables estimate of attending a Boston charter with a lottery at any period of time before the outcome listed in the row heading occurred. Indicator variables for a lottery offer on the day of the lottery (initial offer) and lottery offer off of the waitlist (waitlist offer) are the instruments for charter attendance. The control complier mean is labeled CCM. All regressions control for lottery risk sets and a vector of demographic characteristics including indicators for race, birth year, and baseline special education, English learner, and free or reduced price lunch status, all interacted with gender. The sample is restricted to students 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. Robust standard errors are in parentheses (* $p<0.10$ ** $p<0.05$ *** $p<0.01$).

Table A.4: The Impact of Charter School Offers and Attendance on High School Progress

	High School Progress				High School Graduation		
	On-time 10th (1)	On-time 11th (2)	On-time 12th (3)	Repeat Grade (4)	4 year (5)	5 year (6)	6 year (7)
Reduced Form							
Initial Offer	-0.012	-0.027**	-0.026**	0.033**	-0.041***	-0.015	-0.007
	(0.012)	(0.013)	(0.013)	(0.014)	(0.014)	(0.013)	(0.013)
Waitlist Offer	-0.011	-0.024*	-0.026*	0.015	-0.023*	-0.018	-0.010
	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)	(0.013)
Non-offered mean	0.741	0.690	0.679	0.345	0.561	0.631	0.671
Second Stage 2SLS	-0.027	-0.061**	-0.061**	0.066**	-0.087***	-0.036	-0.017
	(0.026)	(0.027)	(0.027)	(0.027)	(0.029)	(0.028)	(0.027)
CCM	0.760	0.726	0.719	0.327	0.586	0.663	0.712
N	9,560	9,560	9,560	8,258	9,560	9,560	9,560

Notes: Each coefficient labeled Initial Offer or Waitlist Offer is the reduced form estimate of an offer of seat at a Boston charter on the outcome listed in the row heading. Each coefficient labeled 2SLS is the instrumental variables estimate of attending a Boston charter with a lottery at any period of time before the outcome listed in the row heading occurred. Indicator variables for a lottery offer on the day of the lottery (initial offer) and lottery offer off of the waitlist (waitlist offer) are the instruments for charter attendance. The control complier mean is labeled CCM. All regressions control for lottery risk sets and a vector of demographic characteristics including indicators for race, birth year, and baseline special education, English learner, and free or reduced price lunch status, all interacted with gender. The sample is restricted to students 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 graduation rates shown here will be lower than published graduation rates since we count students who have transferred out of state as nongraduates.

Table A.5: The Impact of Charter School Offers and Attendance on College

	College Enrollment			Degree Completion			Years of Education	
	Any	4 year	2 year	Any	BA	AA	By First Pos. Pres.	Ever
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reduced Form								
Initial Offer	0.022	0.038***	-0.020*	0.017	0.013	0.009	0.035	0.114**
	(0.013)	(0.013)	(0.010)	(0.011)	(0.011)	(0.006)	(0.033)	(0.050)
Waitlist Offer	0.027*	0.029**	-0.004	0.018	0.013	0.006	0.048	0.097*
	(0.014)	(0.014)	(0.011)	(0.012)	(0.011)	(0.006)	(0.034)	(0.051)
Non-offered mean	0.547	0.403	0.175	0.224	0.194	0.044	12.314	13.212
Second Stage 2SLS	0.052*	0.085***	-0.038*	0.041*	0.031	0.020*	0.087	0.256**
	(0.028)	(0.028)	(0.022)	(0.024)	(0.023)	(0.012)	(0.070)	(0.105)
CCM	0.554	0.403	0.185	0.215	0.196	0.031	12.455	13.251
N	9,560	9,560	9,560	9,560	9,560	9,560	9,560	9,560

Notes: Each coefficient labeled Initial Offer or Waitlist Offer is the reduced form estimate of an offer of seat at a Boston charter on the outcome listed in the row heading. Each coefficient labeled 2SLS is the instrumental variables estimate of attending a Boston charter with a lottery at any period of time before the outcome listed in the row heading occurred. Indicator variables for a lottery offer on the day of the lottery (initial offer) and lottery offer off of the waitlist (waitlist offer) are the instruments for charter attendance. The control complier mean is labeled CCM. All regressions control for lottery risk sets and a vector of demographic characteristics including indicators for race, birth year, and baseline special education, English learner, and free or reduced price lunch status, all interacted with gender. The sample is restricted to students 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. College enrollment is defined enrollment for at least one semester of college within 18 months of expected high school graduation. Degree completion is within 6 years of projected high school graduation. It is possible to complete both an AA and a BA.

Table A.6: The Impact of Charter School Attendance on College Enrollment by College Location

	Within 6 months			Within 18 months		
	Any (1)	4 year (2)	2 year (3)	Any (4)	4 year (5)	2 year (6)
(A) All Institutions						
2SLS	0.003	0.041	-0.035**	0.052*	0.085***	-0.038*
	(0.028)	(0.027)	(0.016)	(0.028)	(0.028)	(0.022)
CCM	0.458	0.361	0.094	0.554	0.403	0.185
(B) In Massachusetts						
2SLS	-0.024	0.014	-0.036**	0.019	0.045	-0.043**
	(0.028)	(0.026)	(0.015)	(0.029)	(0.028)	(0.021)
CCM	0.376	0.285	0.090	0.472	0.334	0.181
(C) Out of state						
2SLS	0.026*	0.028*	0.001	0.039**	0.040**	0.004
	(0.016)	(0.015)	(0.004)	(0.017)	(0.017)	(0.005)
CCM	0.084	0.077	0.003	0.093	0.069	0.004

Notes: The notes for this table are the same as in Online Appendix Table A.5 but for an expanded set of college outcomes. N = 9,560.

Table A.7: The Impact of Charter School Offers and Attendance on Voting

	Registration			Presidential Elections		
	Ever (1)	By 19th Birthday (2)	Ever Democrat (3)	Ever (4)	Share (5)	First Possible (6)
Reduced Form						
Initial Offer	-0.008	0.008	0.010	0.020	0.024**	0.028**
	(0.010)	(0.013)	(0.015)	(0.013)	(0.011)	(0.013)
Waitlist Offer	-0.011	0.015	0.027*	0.005	0.011	0.013
	(0.010)	(0.014)	(0.015)	(0.013)	(0.011)	(0.014)
Non-offered mean	0.853	0.413	0.543	0.629	0.424	0.398
Second Stage						
2SLS	-0.019	0.022	0.031	0.040	0.051**	0.059**
	(0.020)	(0.028)	(0.030)	(0.027)	(0.022)	(0.028)
CCM	0.879	0.403	0.500	0.606	0.399	0.354
N	9,560	9,560	8,138	9,560	9,560	9,560

	General Elections				Other Elections	
	Ever (7)	Share (8)	First Possible (9)	First Pos. Off-Cycle (10)	Presidential Primary (11)	Other Primary (12)
Reduced Form						
Initial Offer	0.017	0.016*	0.025**	0.001	-0.000	0.002
	(0.013)	(0.009)	(0.012)	(0.009)	(0.011)	(0.011)
Waitlist Offer	0.004	0.005	0.011	-0.009	0.002	-0.006
	(0.013)	(0.009)	(0.012)	(0.010)	(0.012)	(0.011)
Non-offered mean	0.643	0.320	0.267	0.135	0.238	0.203
Second Stage						
2SLS	0.034	0.032*	0.052**	-0.002	0.001	0.000
	(0.027)	(0.018)	(0.024)	(0.020)	(0.024)	(0.023)
CCM	0.626	0.306	0.207	0.141	0.234	0.203
N	9,560	9,560	9,560	9,560	9,560	9,560

Notes: Each coefficient labeled Initial Offer or Waitlist Offer is the reduced form estimate of an offer of seat at a Boston charter on the outcome listed in the row heading. Each coefficient labeled 2SLS is the instrumental variables estimate of attending a Boston charter with a lottery at any period of time before the outcome listed in the row heading occurred. Indicator variables for a lottery offer on the day of the lottery (initial offer) and lottery offer off of the waitlist (waitlist offer) are the instruments for charter attendance. The control complier mean is labeled CCM. All regressions control for lottery risk sets and a vector of demographic characteristics. The sample is restricted to students 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. Voting outcomes come from the Massachusetts voter file, supplemented with voting records from nearby states. The elections are general elections in non-presidential years. Robust standard errors are in parentheses (* p<0.10 ** p<0.05 *** p<0.01).

Table A.8: The Impact of Charter School Attendance on Voting, by Subgroups

	High Scorers (1)	Low Scorers (2)	Special Education (3)	English Learner (4)	Not Special Needs (5)	Free/ Reduced Lunch (6)	Not F/R Lunch (7)	Black (8)	Latinx (9)	White, Asian, and Other Race (10)
Ever Registered	0.015	-0.048*	-0.001	0.002	-0.023	-0.022	0.001	-0.023	0.020	-0.059
	(0.030)	(0.028)	(0.044)	(0.100)	(0.023)	(0.025)	(0.036)	(0.025)	(0.044)	(0.055)
CCM	0.858	0.896	0.911	0.836	0.875	0.882	0.862	0.891	0.828	0.915
Share Presidential	0.097***	0.009	0.049	-0.003	0.057**	0.048*	0.079*	0.057**	0.066	0.016
	(0.034)	(0.029)	(0.050)	(0.088)	(0.026)	(0.026)	(0.042)	(0.028)	(0.045)	(0.060)
CCM	0.381	0.416	0.394	0.370	0.399	0.386	0.416	0.394	0.381	0.455
First Pos. Pres.	0.114***	0.010	0.045	0.121	0.060*	0.075**	0.031	0.047	0.085	0.096
	(0.044)	(0.037)	(0.063)	(0.108)	(0.033)	(0.033)	(0.053)	(0.036)	(0.058)	(0.075)
CCM	0.304	0.393	0.393	0.239	0.350	0.331	0.399	0.370	0.321	0.342
N	4,393	5,167	1,890	793	7,088	7,054	2,506	5,591	2,541	1,428

Notes: This table shows 2SLS estimates for subgroups of students from regressions limited to the sample listed in the header. Students categorized as are neither special education nor English learner students. White, Asian, and other race students are combined into a single category due to small sample sizes in these groups. All other notes are the same as in Online Appendix Table A.7. Robust standard errors are in parentheses (* p<0.10 ** p<0.05 *** p<0.01).

Table A.9: The Impact of Charter School Attendance on Voting, by Age at First Presidential

	Ever Registered (1)	Share Pres. (2)	Voted in 1st Possible Pres. (3)	Year of Ed by by 1st Pres. (4)	Years of Ed Ever (5)
Turn 18 Close to Election	-0.027	0.058*	0.075*	-0.010	0.135
	(0.030)	(0.032)	(0.041)	(0.088)	(0.153)
CCM	0.874	0.394	0.329	12.341	13.480
N	5,473	5,473	5,473	5,473	5,473
Turn 18 Farther from Election	0.004	0.058*	0.054	0.186*	0.400***
	(0.028)	(0.031)	(0.039)	(0.110)	(0.144)
CCM	0.875	0.399	0.378	12.580	12.998
N	4,087	4,087	4,087	4,087	4,087
p-value	0.446	0.988	0.709	0.164	0.208

Notes: Each coefficient labeled 2SLS is the instrumental variables estimate of attending a Boston charter with a lottery at any period of time before the outcome listed in the row heading occurred. Indicator variables for a lottery offer on the day of the lottery (initial offer) and lottery offer off of the waitlist (waitlist offer) are the instruments for charter attendance. The control complier mean is labeled CCM. All regressions control for lottery risk sets and a vector of demographic characteristics including indicators for race, birth year, and baseline special education, English learner, and free or reduced price lunch status, all interacted with gender. The sample is restricted to students 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. Close to presidential election is defined as students who turn 18 within two years prior to their first possible presidential vote. Farther from presidential election is all other students. The p-value from a test of equality of the close to and farther from coefficients is listed in the final row of the table. Robust standard errors are in parentheses (* p<0.10 ** p<0.05 *** p<0.01).

Table A.10: The Impact of Charter School Attendance on Voting, Cognitive, and Non-Cognitive Indices, by Subgroups

Index	All (1)	Higher Scores (2)	Lower Scores (3)	Special Ed (4)	EL (5)	Not Special Needs (6)	F/R Lunch (7)	Not F/R Lunch (8)	Black (9)	Latinx (10)	White, Asian, and Other Race (11)
Voting	0.185**	0.355***	0.032	0.161	0.179	0.199**	0.205**	0.197	0.179*	0.254	0.176
	(0.079)	(0.123)	(0.104)	(0.178)	(0.310)	(0.092)	(0.093)	(0.149)	(0.101)	(0.163)	(0.214)
CCM	0.183	0.073	0.273	0.233	-0.050	0.177	0.122	0.284	0.198	0.097	0.269
Cognitive	0.529***	0.562***	0.558***	0.547***	0.707***	0.504***	0.555***	0.517***	0.596***	0.490***	0.366**
	(0.055)	(0.065)	(0.072)	(0.139)	(0.218)	(0.061)	(0.066)	(0.100)	(0.071)	(0.111)	(0.144)
CCM	0.008	0.560	-0.510	-0.769	-0.822	0.250	-0.091	0.205	-0.196	0.123	0.585
Attendance	0.031	0.097	-0.015	-0.025	-0.219	0.033	-0.048	0.198*	-0.024	0.035	0.246
	(0.059)	(0.081)	(0.084)	(0.141)	(0.187)	(0.068)	(0.071)	(0.108)	(0.073)	(0.127)	(0.163)
CCM	0.083	0.196	-0.008	0.092	0.376	0.085	0.087	0.068	0.143	0.007	0.030
Noncog. Index	-0.092	0.039	-0.187**	-0.299**	-0.508**	-0.038	-0.155**	0.039	-0.162**	-0.094	0.158
	(0.063)	(0.084)	(0.089)	(0.152)	(0.224)	(0.071)	(0.076)	(0.112)	(0.079)	(0.133)	(0.169)
CCM	0.277	0.452	0.139	0.283	0.618	0.284	0.262	0.312	0.316	0.244	0.325
N	8,258	3,726	4,532	1,729	688	6,036	6,164	2,094	4,866	2,183	1,209

Notes: This table shows 2SLS estimates for subgroups of students from regressions limited to the sample listed in the header. Students categorized as are neither special education nor English learner students. White, Asian, and other race students are combined into a single category due to small sample sizes in these groups. All other notes are the same as in Online Appendix Table A.7. Robust standard errors are in parentheses (* p<0.10 ** p<0.05 *** p<0.01).

Table A.11: The Impact of Charter School Attendance on Voting, by Civics-Oriented Charter School Mission Statements

	Ever Registered (1)	Share Pres. (2)	Voted 1st Pos. Pres. (3)	Took AP US Hist/Gov (4)	3+ AP US Hist/Gov (5)
Mission Statement with Civics	-0.004	0.065**	0.075*	0.096***	0.019
	(0.028)	(0.031)	(0.039)	(0.023)	(0.012)
CCM	0.886	0.381	0.346	0.067	0.014
Mission Statement without Civics	-0.022	0.047	0.034	0.159***	0.033**
	(0.026)	(0.029)	(0.036)	(0.025)	(0.015)
CCM	0.868	0.410	0.369	0.088	0.036
N	9,560	9,560	9,560	8,937	8,937
p-value	0.599	0.635	0.391	0.040	0.416

Notes: This table shows a modified version of the main specification, in which the endogenous variable and offer variables are accounted for separately for charter schools with mission statements with civics orientations and for those without. All other notes are the same as in Online Appendix Tables A.1 and A.7. See Online Appendix Table A.12 for details on categorization of mission statements. The p-value from a test of equality of the civics-oriented and non-civics-oriented coefficients is listed in the final row of the table. Robust standard errors are in parentheses (* p<0.10 ** p<0.05 *** p<0.01).

Table A.12: Charter School Mission Statements, by Civics-Orientation

School (1)	Mission Statement (2)	Civics? (Reason) (3)
Academy of the Pacific Rim	For 24 years, APR has been committed to serving students of Boston such that they achieve their full intellectual and social potential, and we now have over 600 alumni. Our work is grounded in the vision that graduates of APR have a focused mind and a big heart. They are ready for post-secondary education because they have mastered college and career ready academic and social skills, including the knowledge and dispositions to joyfully pursue future opportunities based on their passions and participate in and transform civic life.	Yes (civic life)
Boston Collegiate	The mission of Boston Collegiate Charter School is simple yet ambitious: to prepare each student for college.	No
Boston Green Academy	Boston Green Academy welcomes diverse students of all abilities, educates and empowers them to succeed in college and career, and prepares them to lead in the sustainability of our community and world.	Yes (community)
Boston Preparatory	At Boston Prep, we are dedicated to attaining our mission of preparing students to succeed in college and embody lifelong ethical growth. We have carefully designed the Boston Prep program with intention and purpose to provide our students the greatest chance of future success.	No
Brooke Roslindale	Engage together, grow together, achieve together.	No
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. We do so by emphasizing academic achievement, citizenship, teacher leadership, and public accountability.	Yes (community and citizenship)
Codman Academy	Our mission is to provide an outstanding, transformative education to prepare students for success in college, further education and beyond.	No
Excel Academy	Excel Academy's mission is to prepare students to succeed in high school and college, apply their learning to solve relevant problems, and engage productively in their communities.	Yes (community)
MATCH	Success in college and beyond for every student.	No
Mission Hill	Roxbury Prep schools are aligned around the "3 C's" — Curriculum, Character, and Community — that have laid the foundation for Roxbury Prep since its opening.	Yes (community)

Notes: This table lists charter school mission statements and their categorization as civic-oriented, or not. Mission statements were collected from charter school websites in March 2021. For charter schools without explicit mission statements on their website, the main descriptive text about the school was used instead. The mission statements for City on a Hill and MATCH refer to both their campuses, respectively.

Appendix B: Matching Student Data to the Voter Files

This Appendix details our procedures for matching student and parent information to the voter files.

B.1.1 Students

We attempted to match all student records in the SIMS database to the voter files. To increase the likelihood of matching, we included all variations of name and birth dates associated with a student ID in the SIMS database. For example, a student might have one record in the SIMS data with a middle name and one without. The resulting student-level records were then matched with voter records for Massachusetts, using voter files from 2012, 2015, and 2018. We supplemented the Massachusetts voter files with voting records from 2018 for nearby states: Connecticut, Maine, New Hampshire, New York, Rhode Island, and Vermont.[33]

The 2018 voter files also contain a measurement of confidence in voter dates of birth from the vendor. These range from complete date to valid year and month or date to valid year to missing birthdate. These levels of confidence vary by state, as does the presence of date of birth, and thus our matching procedures vary by state. Each of the state voter files is detailed below. The Massachusetts voter file has 4.04 million verified birth dates out of 4.05 million voter records, allowing for the greatest accuracy in the state we are most likely to observe students. Almost all of the records in the Connecticut, New York, and Rhode Island voter files also have verified birth dates, with relatively few missing values for birth dates. In contrast, the New Hampshire voter file is missing roughly 20 percent of voters' birth dates and includes a large number of younger voters who are missing exact birth date. Many birth dates in Maine only include information on year of birth. Finally, the Vermont file has varying levels of birth date information, including some records with complete birth date information and some only containing correct year or correct year and month.

Appendix Table B.1 details the rate at which students in the lottery sample, and Massachusetts as a whole, appear in the voter files for any of these states, by the state of the college they attended (or in a line for no college). We count students for each state they are registered in, so a student may appear in more than one state. We see that students are most likely to be registered in Massachusetts, no matter the state of the college that they attend, and that many students who attend college out of state remain registered solely in Massachusetts. College state and state of registration align closely, which is a check that matches outside of Massachusetts are likely good ones.

To begin our matching procedures, we searched for exact matches between the SIMS and voter information on first name, last name, and date of birth in the Massachusetts voter records. Students matched in this way were declared as matches and set aside. We then employed fuzzy matching techniques to account for minor discrepancies in identifying information between the two data sources for the remaining students. We make use of two string distance metrics. The first is Jaro-Winkler Distance (JWD) which ranges from 0 to 1, with 0 meaning an exact match, measures edits to convert one string to another with more weight (penalty) for discrepancies early in the string. The second is Cosine String Distance which yields the distance between q-gram profiles of strings; for example, cosine distance with $q = 4$ depends on how many 4-letter sequences two strings share. Cosine distances with larger values of q are particularly good at matching students with hyphenated last names which are often transposed in different sources. We also use Soundex encoding. Based on careful review of the voter files and the student data, we developed several variants of fuzzy matching:

1. Require exact matches on first name and last name; require two of birth day, birth month, and birth year to match; require birth year to be off by no more than two years; require middle initial to match; if a middle name is reported in both sources (relatively rare), require middle name to be within 0.1 in JWD.

2. Require exact matches on first name and date of birth; require last names to be within 0.2 in JWD or 0.2 in cosine distance with $q = 1$; require last names to be within 0.5 in cosine distance with $q = 3$.

[33] The New England states have a tuition-compact where regional students do not have to pay full out-of-state tuition rates at public colleges and universities. For details, see: `https://nebhe.org/tuitionbreak/`.

3. Require exact matches on last name and date of birth; require first names to be within 0.2 in JWD or 0.2 in cosine distance with $q = 1$; require first names to be less than 1 in cosine distance with $q = 4$ or agree on soundex code or within 0.2 in JWD.

4. Require exact matches on birthdate; require first name to be within 0.2 in JWD; require last name to be within 0.2 in JWD; require last names to be less than 1 in cosine distance with $q = 4$ or the sum of JWD in first and last name to be less than 0.15; require gender to match.

5. Require exact matches in last name and date of birth; require first name to match middle name from SIMS to voter file or from voter file to SIMS; require first letter of first name to match first letter of middle name (in both directions). This captures students reversing first and middle names between SIMS and the voter file.

6. Require exact matches in first and last name; require year of birth to match; require day of birth to match month of birth (in both directions). This captures students reversing their day and month of birth.

We then supplemented the Massachusetts records with voter files from neighboring states. We attempt to match all students, including those matched above to the Massachusetts voter file, to recover voting history for students who move out of state whether or not they have previously registered to vote in Massachusetts. Due to the state-level variance in the date of birth confidence levels (especially out of Massachusetts) and to ensure that we are matching a student record to the correct voter record, we employ four rounds of matching with different stipulations. In the first round of matching, students are matched with voter records based on exact matches in first name, last name, and birth date. Again, these matches are set aside before we employ fuzzy matching (with more restrictions than in our matching within Massachusetts because we know, in general, that the students in our sample are mostly likely to be in MA). In the second round, we focus on records in the voter file with only a valid year and month or day of birth. We match exactly on first name, last name, and gender, require middle initial to match, and require birth year and birth month to match, and if a middle name is reported in both sources (relatively rare), require middle name to be within 0.1 in JWD. In the third round, we focus on records in the voter file with only a valid year. We match exactly on first name, last name, and gender, require middle initial to match, and require birth year to match, and if a middle name is reported in both sources (relatively rare), require middle name to be within 0.1 in JWD. In the fourth round, we focus on records in the voter file with a missing birth date and students in SIMS who are the only student with their exact first and last name in SIMS. We match exactly on first name, last name, and gender, require middle initial to match, and if a middle name is reported in both sources (relatively rare), require middle name to be within 0.1 in JWD.

B.1.2 Parents

Some charter schools that provided us with the charter lottery data also provided us with parent information for the students. We include charter school lotteries where over 90 percent of the student entries included parent information. This includes the following charter school lotteries: Academy of the Pacific Rim (2011, 2012, 2013); Boston Collegiate (2009); Boston Preparatory (2005, 2007, 2010, 2012, 2013); City on a Hill (2005, 2006, 2007, 2008, 2009, 2010, 2011, 2012); Codman Academy (2008, 2010, 2011); and Roxbury Prep (2002, 2003, 2004, 2005, 2009, 2011). This resulted in a sample of 8,302 parents, representing 6,388 students (since students can have two parent/guardians in the lottery records). Restricting this to students who met sample criteria (baseline covariates present and Boston residents at baseline) reduced the sample to 5,845 students with 7,635 parents.

To match the parent records with their respective voter records, we employed a similar technique as described for the student matching. In this case, parent information from the charter school lotteries is sparse. To address this, we only matched parents with Massachusetts voter records for individuals residing in a Boston zip code to reduce the likelihood of a false match; we further require parents to be between 14 and 60 years old when their charter lottery child was born to filter out implausible matches. We use Jaro-Winkler distance matching to create a measurement of similarity between parent names and voter names. To allow for normal variation in name formats (hyphenated names, multiple last names, misspellings, etc.), we consider records with both first and last names with JWD scores of 0.1 or lower as an accurate match. This produces 18,258 potential-parent-voter records for analysis, since many parents are matched to multiple voter records. Because we do not have an additional piece of information, like date of birth, for parents, we cannot distinguish which voter record is the correct one when a parent name matches to multiple voter records. We thus retain all potential methods and estimate several models that account for parent matches in different ways, as discussed in the main text.

Table B.1: Presence in State Voter Files

College State	N	MA	CT	ME	NH	NY	PA	RI	VT
(A) Charter applicants									
Massachusetts	5,958	0.912	0.001	0.001	0.003	0.006	0.003	0.005	0.001
Connecticut	50	0.800	0.160	0.000	0.000	0.020	0.000	0.000	0.000
Maine	25	0.880	0.000	0.280	0.000	0.000	0.000	0.000	0.000
New Hampshire	294	0.881	0.000	0.000	0.078	0.000	0.000	0.003	0.000
New York	189	0.889	0.000	0.005	0.000	0.148	0.005	0.005	0.000
Pennsylvania	71	0.915	0.000	0.000	0.000	0.014	0.324	0.014	0.000
Rhode Island	181	0.878	0.000	0.000	0.000	0.000	0.006	0.149	0.000
Vermont	48	0.875	0.000	0.021	0.021	0.042	0.000	0.000	0.042
Other States	503	0.682	0.002	0.002	0.000	0.004	0.002	0.014	0.000
No College	2,825	0.742	0.001	0.000	0.000	0.004	0.002	0.004	0.001
All	9,562	0.847	0.002	0.002	0.003	0.007	0.004	0.007	0.001
(B) Massachusetts									
Massachusetts	446,397	0.887	0.006	0.005	0.008	0.011	0.004	0.007	0.002
Connecticut	7,925	0.868	0.148	0.003	0.005	0.035	0.005	0.007	0.002
Maine	4,721	0.799	0.007	0.271	0.013	0.018	0.005	0.004	0.005
New Hampshire	31,589	0.856	0.005	0.008	0.108	0.005	0.002	0.005	0.003
New York	28,059	0.868	0.010	0.006	0.007	0.168	0.008	0.006	0.003
Pennsylvania	10,443	0.838	0.009	0.006	0.005	0.052	0.280	0.005	0.002
Rhode Island	29,186	0.883	0.008	0.005	0.006	0.014	0.005	0.079	0.002
Vermont	12,438	0.887	0.008	0.015	0.013	0.030	0.011	0.007	0.091
Other States	59,658	0.709	0.006	0.006	0.005	0.026	0.008	0.005	0.002
No College	223,094	0.638	0.004	0.003	0.004	0.006	0.002	0.007	0.001
All	789,592	0.799	0.007	0.006	0.010	0.016	0.007	0.009	0.003

Notes: This table shows the rates in which students appear in voter files for Massachusetts and nearby states, by state of college attended. The charter applicants are restricted to students who applied for a Boston charter school lottery. The Massachusetts panel shows all 9th grade students in Massachusetts.

Appendix C: Charter Lottery Details

Table C.1: Lottery Records: Sample Restrictions

	All Years	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Total number of records	23,200	545	435	898	980	1,206	1,599	1,830	2,085	2,438	3,017	3,420	4,747
Excluding disqualified	23,107	545	435	898	979	1,206	1,597	1,828	2,072	2,433	2,993	3,397	4,724
Excluding late applications	22,900	535	431	888	976	1,206	1,597	1,808	2,070	2,433	2,993	3,307	4,656
Excluding out-of-area	22,659	535	431	888	976	1,203	1,596	1,808	2,052	2,389	2,948	3,286	4,547
Excluding siblings	21,597	515	422	863	955	1,182	1,538	1,706	1,940	2,215	2,795	3,115	4,351
Excluding unmatched	21,464	510	419	857	948	1,172	1,531	1,694	1,924	2,203	2,778	3,097	4,331
Excluding repeat applications	21,457	510	419	857	948	1,172	1,531	1,694	1,924	2,203	2,777	3,095	4,327
Reshaping to student-level	12,814	438	419	626	669	851	1,077	1,091	1,197	1,258	1,573	1,685	1,892
Has baseline demographics	11,388	324	342	559	606	746	986	958	1,057	1,141	1,420	1,515	1,734
In Boston at baseline	10,527	295	328	534	565	687	934	878	986	1,033	1,319	1,400	1,568
At least 18 by 2016 election	9,562	295	328	534	565	686	933	878	986	1,032	1,318	1,383	624

Notes: This table shows sample restrictions for the lottery records. Disqualified records are duplicate applications to the same lottery and applications to the wrong grade.

Table C.2: Lottery Records: Participation by Schools and Cohorts

	Middle Schools			Combined Schools (5-12 or 6-12)			
	Brooke Roslindale (5) (1)	Excel East Boston (5) (2)	Mission Hill (6) (3)	Academy of the Pacific Rim (5/6) (4)	Boston Collegiate (5) (5)	Boston Prep (6) (6)	Match MS (6) (7)
2002			Yes*		Yes	Not open	
2003	No records		Yes*	No records	Yes		
2004		No records	Yes*		Yes	Incomplete records	Not open
2005			Yes	Yes	Yes	Yes**	
2006	Yes**		Yes	Yes	Yes	Yes	
2007	Yes		Yes	Yes	Yes	Yes	
2008	Yes	Yes	Yes	Yes	Yes	Yes**	Yes
2009	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2010	Not entry grade	Too young	Yes	Too young	Too young	Too young	Yes
N	569	261	1,816	1,511	1,889	2,153	1,423

	High Schools				
	Boston Green Academy (9) (8)	City on a Hill (9) (9)	City on a Hill II (9) (10)	Codman Academy (9) (11)	Match HS (9) (12)
2002		Yes**		No records	Yes
2003		No records		Incomplete records	Yes
2004		Yes		Yes**	Yes
2005		Yes		Incomplete Records	Yes
2006	Not open	Yes			Yes
2007		Yes	Not open	No records	Yes
2008		Yes		Yes	Yes
2009		Yes		Yes	Yes
2010		Yes		Yes	Yes
2011	Yes	Yes		Yes	
2012	Yes**	Yes		Yes	Not entry grade
2013	Yes	Yes	Yes**	Yes	
N	2,924	11,947	2,796	5,828	7,008

Notes: This table shows lottery information by school and application year. The entry grade is listed in parentheses after the school name. Some schools have since added grades or changed entry grades outside of the sample period. Brooke, APR, and Match changed their entry grade during the sample period. Mission Hill refers to the original campus of the Roxbury Prep Uncommon Schools network, which was called Roxbury Prep at the time. The ACDPW sample includes APR, Boston Collegiate, Boston Preparatory, City on a Hill, Codman Academy, and MATCH HS. We add five campuses to these schools: Boston Green Academy, a second City on a Hill campus, Edward Brooke Roslindale, Excel Academy, MATCH Middle School, and the Mission Hill campus of Roxbury Preparatory Schools (formerly Roxbury Prep). Three closed schools with appropriately aged children do not participate (here, or in ACDPW): Frederick Douglass Charter School (closed 2005), Roxbury Charter High School (closed 2005), and Uphams Corner Charter School (closed 2009). Two charter schools declined to participate: Kennedy Academy for Health Careers (formerly Health Careers Academy) and Helen Davis Leadership Academy (formerly Smith Leadership Academy).
* Indicates that there is no initial offer information. ** Indicates that the waitlist was exhausted.

Table C.3: Covariate Balance

	Non-offered Mean (1)	Initial Offer Differential (2)	Waitlist Offer Differential (3)
Female	0.527	0.000	0.001
		(0.012)	(0.012)
Asian	0.032	-0.003	0.002
		(0.004)	(0.004)
Black	0.564	0.007	-0.003
		(0.012)	(0.012)
Latinx	0.273	-0.006	0.003
		(0.011)	(0.011)
Other race	0.024	0.003	-0.007+
		(0.004)	(0.004)
White	0.107	-0.000	0.005
		(0.007)	(0.007)
Special education	0.196	0.002	-0.012
		(0.010)	(0.010)
English learner	0.076	-0.009	0.012+
		(0.007)	(0.007)
Free/reduced price lunch	0.733	0.003	-0.003
		(0.010)	(0.011)
Baseline MCAS ELA	-0.404	-0.036	0.033
		(0.025)	(0.025)
Baseline MCAS Math	-0.371	-0.037	0.036
		(0.023)	(0.024)
	p-value	0.716	0.419

Notes: This table shows means and offer differentials for baseline characteristics. The sample is restricted to students 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. Column 1 shows the proportion of non-offered students with a given characteristic. Columns 2 and 3 report coefficients from regressions of the student characteristic on initial and waitlist offer dummies, including controls for risk sets, application grade, an (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ *** $p<0.001$). The p-values are from tests of the hypothesis that all coefficients on each offer are zero. $N = 9{,}560$.

Table C.4: The Impact of Charter School Attendance on Predicted Voting

	Ever Registered (1)	Share Presidential (2)	First Possible Presidential (3)
2SLS	-0.004	-0.005	-0.005
	(0.004)	(0.005)	(0.004)
CCM	0.840	0.394	0.372
N	9,560	9,560	9,560

Notes: Each coefficient labeled 2SLS is the instrumental variables estimate of attending a Boston charter with a lottery at any period of time before the outcome listed in the row heading occurred. Indicator variables for a lottery offer on the day of the lottery (initial offer) and lottery offer off of the waitlist (waitlist offer) are the instruments for charter attendance. The control complier mean is labeled CCM. All regressions control for lottery risk sets and a vector of demographic characteristics including indicators for race, birth year, and baseline special education, English learner, and free or reduced price lunch status, all interacted with gender. The sample is restricted to students 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. Predicted voting likelihoods are calculated in the non-charter BPS sample using demographics and baseline test scores, with predicted values applied to the charter lottery population. Robust standard errors are in parentheses (* p<0.10 ** p<0.05 *** p<0.01).

Table C.5: Match Rate to SIMS

Projected HS Class	Non-offered Mean (1)	Initial Offer Differential (2)	Waitlist Offer Differential (3)	Number of Applications (4)
2006	0.986	-0.008	0.008	515
		(0.012)	(0.009)	
2007	0.997	-0.011	-0.033	422
		(0.017)	(0.038)	
2008	0.996	-0.015	0.008	863
		(0.011)	(0.009)	
2009	0.994	-0.001	-0.002	955
		(0.008)	(0.008)	
2010	0.994	-0.001	-0.001	1,182
		(0.009)	(0.009)	
2011	0.996	0.000	-0.002	1,537
		(0.005)	(0.008)	
2012	0.992	-0.001	0.000	1,708
		(0.005)	(0.005)	
2013	0.993	-0.004	0.003	1,940
		(0.006)	(0.005)	
2014	0.994	0.001	0.000	2,215
		(0.005)	(0.005)	
2015	0.996	0.000	-0.001	2,795
		(0.004)	(0.003)	
2016	0.994	-0.003	0.002	3,114
		(0.004)	(0.004)	
2017	0.995	-0.003	0.002	4,351
		(0.003)	(0.003)	
All cohorts	0.995	-0.003+	-0.000	21,597
		(0.002)	(0.001)	

Notes: This table shows the match between lottery records and the SIMS data by projected high school class. The sample excludes disqualified, late, out-of-area, and sibling applications. It includes students who are under the age of 18 at the time of the 2016 election since birth date is only available for students who match to the SIMS data. Individuals can be in the sample multiple times if they apply to multiple schools. Columns 2 and 3 report coefficients from regressions of the student characteristic on initial and waitlist offer dummies, including controls for risk sets (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001).

Table C.6: Attrition

	Fraction of Non-Offered With Outcome (1)	Initial Offer Differential (2)	Waitlist Offer Differential (3)
Has ELA score (2 years after lottery)	0.803	0.006	0.016+
		(0.009)	(0.010)
Has math score (2 years after lottery)	0.784	0.008	0.013
		(0.009)	(0.010)
Present in 12th grade in data	0.754	-0.005	-0.007
		(0.010)	(0.011)
Sent to NSC	0.959	0.008+	-0.000
		(0.004)	(0.005)

Notes: This table shows follow-up rates for MCAS scores two years after charter application, presence in the data in 12th grade, and an indicator for being sent to the NSC to be matched to college outcome data for Boston charter school applicants. The sample is restricted to students 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. Column 1 shows the proportion of non-offered students with a given outcome. Columns 2 and 3 report coefficients from regressions of the student characteristic on initial and waitlist offer dummies, including controls for risk sets (* p<0.10 ** p<0.05 *** p<0.01). N = 9,560.

Table C.7: The Impact of Charter School Offers on Charter Attendance

	Non-offered Mean (1)	Initial Offer (2)	Waitlist Offer (3)
Ever attend charter	0.072	0.463***	0.302***
		(0.011)	(0.011)
Years attended charter	0.557	1.557***	1.015***
		(0.055)	(0.052)

Notes: This table shows the impact of a charter school offer on charter school attendance. The sample is restricted to students 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. Column 1 shows the proportion of non-offered students with a given dimension of charter school attendance. Columns 2 and 3 report coefficients from regressions of charter attendance on initial and waitlist offer dummies, including controls for demographic characteristics and risk sets. Robust standard errors are in parentheses (* $p<0.10$ ** $p<0.05$ *** $p<0.01$). N = 9,560.

Appendix D: Comparisons to prior work

Appendix Table D.1 goes through the exercise of progressively modifying the estimates presented here to make them more similar to the estimates in ACDPW, first omitting the additional schools and cohorts added since more time has passed, then adjusting the specification to reflect changes in modeling choices. There are two notable differences, the first for Advanced Placement (AP) and the second for college enrollment.

AP impacts are smaller here, with the largest contributing factor being the inclusion of more recent cohorts, as BPS has expanded its AP offerings over this time period, meaning that students attending counterfactual schools have more AP offerings and take up more AP options. In projected high-school classes of 2012 and prior (corresponding to ACDPW), the control complier rate of AP test-taking was 24 percent; in the more recent cohorts, the control complier mean was 31 percent. There are also differences in AP outcomes due to the inclusion of Boston charter middle schools in our sample. Some Boston charter schools that serve high-school grades require or strongly encourage APs, whereas students who attended a middle school charter but then went to the traditional public school system may not encounter a high-school curriculum similarly focused on APs.

The magnitude of the 4-year college enrollment impact is also smaller than the estimates reported in ACDPW—8.5versus 18.3 percentage points. Differences are largely due to the fact that ACDPW conditions on presence in 10th grade in Massachusetts. Once we apply the same sample and modeling choices, our comparable estimate is 16 percentage points versus ACDPW's 18.3 percentage points. This remaining difference is due to minor variations in data processing (i.e., matching more eligible students to the SIMS, processing the college data differently, and updates to the databases supplied by DESE, in particular the NSC data). There is no charter impact on presence in the data in 10th grade (Appendix Table A.2, so the estimates which condition on presence in 10th grade are still causal, but the students who contribute to the analysis are different. In short, there are some differences between our estimates and the prior literature, but as a whole, all of the estimates show substantial academic and attainment gains for charter students. Had we made the same modeling choices as ACDPW in this paper, the impact on voting in the first possible presidential election would have been about twice as large as the estimate we highlight here.

Table D.1: Alternative Specifications: Comparison to Angrist et al. (2016)

	10th Grade Math (1)	Took Any AP (2)	Above 3 Any AP (3)	Took SAT (4)	SAT Score (1600) (5)	Enroll 4-Year College (6)	Enroll 2-Year College (7)	Voted in 1st Possible Presidential (8)
Main specification	0.433***	0.113***	0.037**	0.051*	38.465***	0.085***	-0.038*	0.059**
	(0.052)	(0.026)	(0.017)	(0.029)	(11.002)	(0.028)	(0.022)	(0.028)
+ Omitting new schools	0.407***	0.140***	0.039	0.085**	37.104***	0.101***	-0.065**	0.053
	(0.068)	(0.040)	(0.026)	(0.041)	(13.973)	(0.035)	(0.028)	(0.035)
+ Omitting new cohorts	0.440***	0.227***	0.068*	0.067	58.962***	0.109*	-0.087*	0.090*
	(0.101)	(0.058)	(0.041)	(0.063)	(20.732)	(0.060)	(0.052)	(0.052)
+ Adding 10th grade year dummies	0.443***	0.236***	0.070	0.045	66.888***	0.156**	-0.097*	0.115**
	(0.101)	(0.064)	(0.046)	(0.061)	(21.202)	(0.067)	(0.058)	(0.057)
+ Changing endogenous variable	0.454***	0.241***	0.071	0.048	69.959***	0.159**	-0.098*	0.119**
	(0.102)	(0.066)	(0.047)	(0.063)	(21.859)	(0.068)	(0.060)	(0.058)
Angrist, et al. (2016)	0.489***	0.298***	0.122*	0.084	78.1**	0.183*	-0.108+	–
	(0.146)	(0.062)	(0.051)	(0.063)	(23.9)	(0.073)	(0.61)	

Notes: The first row of the table repeats the main specification reported in the other tables, see Tables A.1 and A.7 for details (N = 9560). Each subsequent row shows an alternative specification. The row labeled "+ Omitting new schools" excludes charter schools added to the sample in addition to the schools in Angrist et al. (2016) (N = 8185). The row labeled "+ Omitting new cohorts" excludes projected high school classes added to the sample in addition to the classes in Angrist et al. (2016) in addition to the above change (N = 3854). The row labeled "Adding 10th grade year dummies" adds a year fixed effect based on 10th grade year, which conditions on presence in the data in 10th grade, as in Angrist et al. (2016), in addition to the above changes (N = 3194). The row labeled "Changing endogenous variable" switches the endogenous variables to one that only counts charter attendance in 9th or 10th grade, as in Angrist et al. (2016), in addition to the above changes (N = 3194). The final row reprints the corresponding estimates from Angrist et al. (2016) (N = 3,205). Sample sizes are from the voting outcomes and differ for other outcomes. Robust standard errors are in parentheses (* p<0.10 ** p<0.05 *** p<0.01).

Appendix E: Robustness

The first election of President Barack Obama in 2008 had record voter turnout. Additionally, the election of the first Black president in the United States may have influenced voter participation for Boston charter applicants—a majority of whom are Black students. To determine if the charter vote effect is primarily driven by an interaction with the "Obama effect," we exclude students whose first possible presidential election was in 2008 in Row 2 of Appendix Table E.1. The charter effect on both share of presidential elections and first possible presidential election are a bit smaller (both are 4.3 percentage points, down from 5.0 and 5.8 in the baseline sample). The coefficient on share remains statistically significant, while the coefficient on first possible presidential is no longer statistically significant with the smaller sample. We thus conclude that the first election of President Obama led to an even larger than typical boost in voting in the first possible presidential election but that the overall impact on voting patterns remained the same even after excluding the 2008 Obama voters. We also exclude cohorts for whom 2012 was the first possible election, noting that the Obama effect could also exist for first time voters in 2012 (row 3) and exclude cohorts for whom 2016 was the first possible presidential voting opportunity, noting that the 2016 contest was also unusual.[34] Excluding the 2012 and 2016 cohorts increases the standard errors, but the voting impacts follow a pattern very similar to that of the overall results, though they sometimes lose statistical significance, likely because of the reduction in precision from the smaller sample sizes.

Other robustness checks in Appendix Table E.1 show few differences in charter impacts on voting outcomes. Excluding covariates does not affect the magnitudes or statistical significance of the results in a meaningful way. Adding baseline scores increases the magnitude of the effects, but slightly changes who is in the sample (since baseline scores are available for only a subset of students). Changing the endogenous variable to charter attendance in the first two years after the charter lottery, rather than attendance at any time before an outcome is observed, results in essentially the same findings. Using initial offer as the only instrument results in slightly larger magnitudes. Finally, clustering standard errors by 10th grade school by year does not change our results. Appendix Figure E.1 shows that excluding individual high-school graduation cohorts or charter schools introduces small fluctuations in the magnitudes of voting effects but that they generally are quite similar and not driven by a particular cohort or school. School "J" does appear to have an outsize effect, but this is a relatively large school, and effects omitting this school are still positive and overlap with the main estimates.

Since Boston charter attendance increases enrollment in out-of-state colleges by 4 percentage points (Appendix Table A.6), the charter effect on voting may be underestimated if students vote in their college state rather than their home state.[35] Within Massachusetts, attending a charter school shifts students from 2-year institutions to 4-year institutions, while the out-of-state increases are all due to increased enrollment, mostly in 4-year institutions. Evidence from Appendix Table E.2, which shows voting outcomes for various samples, confirms that the increase in out-of-state college enrollment reduces voting because we do not observe nonregional voting.[36]

A different way to show that the college attendance patterns of charter applicants likely leads to

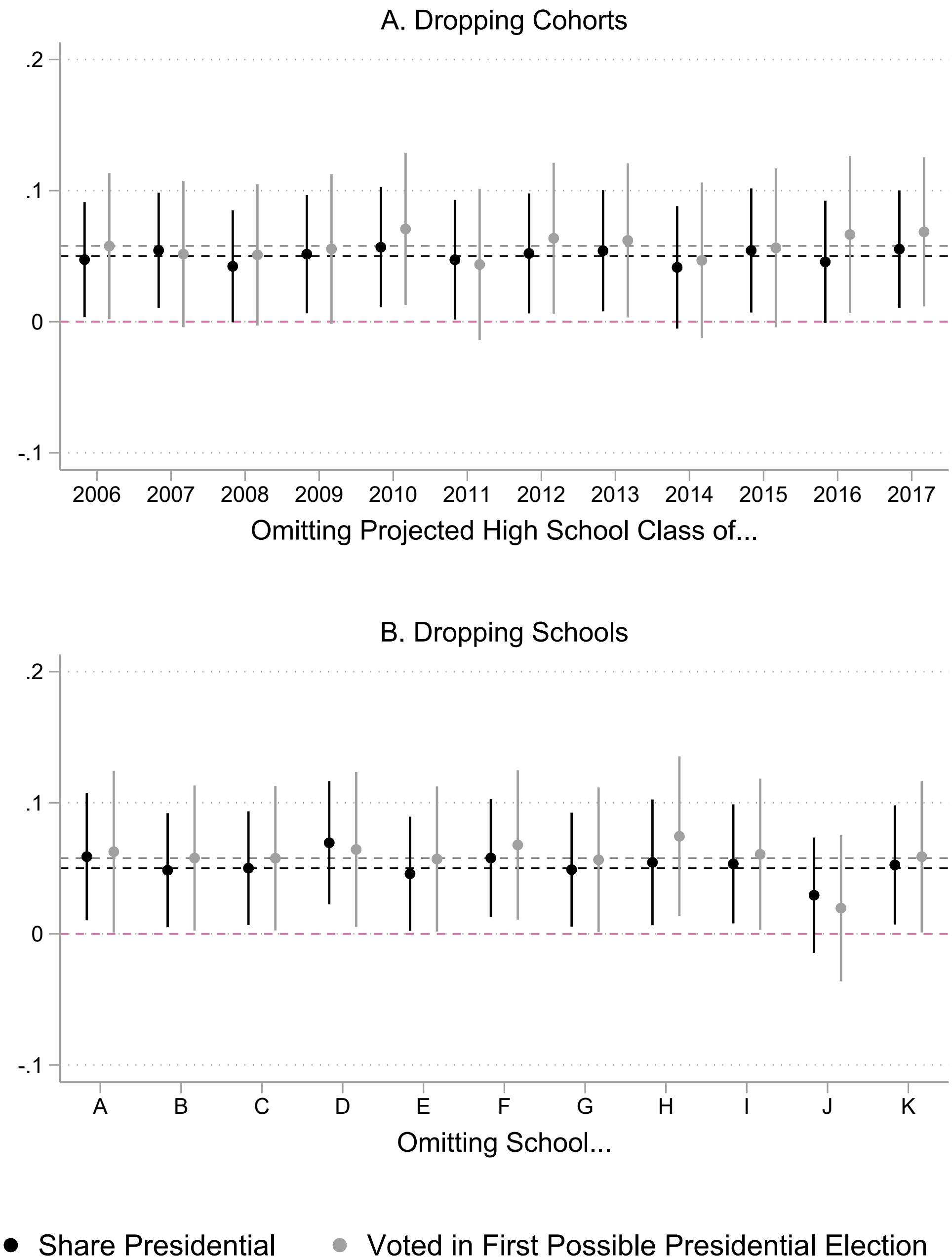
[34] The 2020 election, also a very unusual election, contributes to our "share" and "ever voted" variables but not to our first possible presidential variables, as our lottery sample was too old to include any first-time voters in 2020.

[35] There is little evidence on where college students register to vote. We are aware of one survey of college students about voting in the 2004 election, and it shows that two-thirds of college students are registered in their hometown, even when their college is located elsewhere (Niemi and Hanmer, 2010). We similarly find that most students who go to college out of state do not appear on the voter rolls in their new state; see Appendix Table B.1.

[36] When the sample is restricted to those students who attended 12th grade in Massachusetts, the charter attendance voter participation effect is slightly larger, at 9 percentage points, for voting in the first possible presidential election. In Appendix Table C.6, we show that charter lottery winners are no more likely to appear in the data in 12th grade, so the voting effect does not reflect differential likelihood of appearing in the data. For students that do not appear in the 12th grade data, counterfactual means on voter participation are lower, and there is no impact of charter attendance. This likely reflects two situations. First, students who leave Massachusetts are much less likely to vote in Massachusetts. Second, individuals who drop out of high school are less likely to vote (McDonald, 2020). A similar pattern exists when excluding students who attend college out-of-state, however, and since charter attendance does affect the likelihood of attending an out-of-state institution, it is difficult to interpret the slightly larger impact on voting (9-10 percentage points).

underestimation of the voting effect is included in Appendix Table E.3. Here, we impute voter registration and voting for out-of-state college attendees. Our main specification (Panel A) already has a form of imputation: we assume that everyone who does not show up in the Massachusetts voter file or that of a nearby state is a nonvoter. However, many of these students, especially those who move out of the area, may be voters in another state. Since most out-of-state college students are already included in our data as nonvoters, when we impute zeroes for all out-of-state college students to form a lower bound, as in Panel B, the point estimates are only slightly different and remain statistically significant. In Panel C, we assume that out-of-Massachusetts college attendees register and vote at the same rates as their counterparts in BPS attending 2- and 4-year institutions. Specifically, we impute predicted voting outcomes based on the voting rates of *in-state* college-going students in BPS, calculated separately for 4- and 2-year college attendees and adjusted for demographics. In this case, the charter effects on share of presidential elections and first possible presidential election increase to 5.9 and 6.8 percentage points, respectively. This estimate likely also underestimates the impact of charter attendance on voting, since it reflects the voting rates of BPS without including any charter effect. Panel D runs through the same exercise, but imputes voting only for college students out of the area for whom we have no out-of-state voter files. Both imputations show that, if anything, our estimates of the charter voting impact underestimate the extent to which charter school attendance boosts civic participation.

Figure E.1: Voting Impacts Omitting Cohorts and Schools



Notes: This figure shows 2SLS impacts (dots) and a 95% confidence interval (lines) for estimates of charter school attendance on voting, omitting each projected high-school cohort (Panel A) and charter school of application (Panel B) in turn. A red dashed line indicates 0. Dashed black and grey lines indicate the impact estimates without omissions.

Table E.1: Alternative Specifications

	Ever Registered (1)	Share Presidential (2)	First Possible Presidential (3)
Main specification	-0.019	0.050**	0.058**
	(0.020)	(0.022)	(0.028)
Excluding 2008 cohorts	-0.020	0.043*	0.043
	(0.021)	(0.023)	(0.028)
Excluding 2012 cohorts	-0.026	0.064**	0.058
	(0.027)	(0.029)	(0.035)
Excluding 2016 cohorts	-0.006	0.041	0.083*
	(0.030)	(0.033)	(0.044)
No covariates	-0.017	0.052**	0.060**
	(0.021)	(0.023)	(0.028)
Add baseline tests	-0.015	0.067***	0.073**
	(0.022)	(0.024)	(0.030)
Alternative endogenous	-0.018	0.050**	0.057**
	(0.020)	(0.022)	(0.028)
Initial offer only	-0.006	0.058**	0.067*
	(0.027)	(0.029)	(0.036)
Cluster S.E.'s	-0.019	0.050**	0.058**
	(0.019)	(0.022)	(0.028)

Notes: The first row of the table repeats the main specification reported in the other tables, see Table A.7 for details (N = 9562). Each subsequent row shows an alternative specification. The row labeled "Excluding 2008 cohorts" excludes students whose first opportunity to vote in a presidential election was on November 4th, 2008 (N = 8186). The row labeled "Excluding 2012 cohorts" excludes students whose first opportunity to vote in a presidential election was on November 6th, 2012 (N = 6271). The row labeled "Excluding 2016 cohorts" excludes students whose first opportunity to vote in a presidential election was on November 8th, 2016 (N = 4667). The row labeled "No covariates" removes baseline demographic characteristics from the main specification (N = 9562). The row labeled "Add baseline tests" adds baseline test scores to the main specification, which also restricts the sample to those students with reported baseline test scores (N = 8179). The row labeled "Alternative endogenous" uses charter attendance in the first two years after the lottery as the endogenous variable rather than any attendance before the outcome (N = 9562). The row labeled "Initial offer only" uses the offer on the day of charter school lottery as the only instrument (N = 9562). The row labeled "Cluster S.E.'s" clusters the standard errors by school of attendance after the lottery and year (N = 9562). Robust standard errors are in parentheses (* $p<0.10$ ** $p<0.05$ *** $p<0.01$).

Table E.2: The Impact of Charter School Attendance on Voting, Sample Restrictions

	Expected to Vote in MA			Not Expected to Vote in MA		
	Ever Registered (1)	Share Presidential (2)	Voted in First Possible Presidential (3)	Ever Registered (4)	Share Presidential (5)	Voted in First Possible Presidential (6)
(A) 12th grade status	In MA 12th grade			Not in MA 12th grade		
2SLS	0.015	0.071***	0.077**	-0.146**	-0.021	-0.015
	(0.020)	(0.024)	(0.031)	(0.068)	(0.054)	(0.070)
CCM	0.882	0.422	0.368	0.875	0.305	0.308
N	7,127	7,127	7,127	2,435	2,435	2,435
(B) College location	Not out-of-state college			Out-of-state college		
2SLS	-0.022	0.063***	0.073**	-0.003	-0.031	-0.013
	(0.022)	(0.024)	(0.030)	(0.054)	(0.056)	(0.071)
CCM	0.886	0.395	0.355	0.837	0.438	0.328
N	8,447	8,447	8,447	1,115	1,115	1,115
(C) 12th grade and college	Not OOS College + MA 12th grade			OOS college or not MA 12th		
2SLS	0.015	0.083***	0.093***	-0.083*	-0.011	-0.006
	(0.021)	(0.026)	(0.033)	(0.047)	(0.041)	(0.052)
CCM	0.883	0.419	0.366	0.870	0.355	0.329
N	6,352	6,352	6,352	3,210	3,210	3,210
(D) Predicted likelihood of MA college	High predicted MA college			Low predicted MA college		
2SLS	-0.012	0.093**	0.138***	-0.015	0.048	0.035
	(0.034)	(0.039)	(0.049)	(0.028)	(0.029)	(0.037)
CCM	0.862	0.393	0.291	0.879	0.375	0.361
N	3,709	3,709	3,709	4,842	4,842	4,842

Notes: This table varies the sample of students in the regressions, all other notes are the same as in Online Appendix Table A.7. Predicted likelihood of attending an MA college is calculated in the non-charter BPS sample, with predicted values applied to the charter lottery population. Robust standard errors are in parentheses (* p<0.10 ** p<0.05 *** p<0.01).

Table E.3: The Impact of Charter School Attendance on Voting, with Imputations for Out-of-State College Attendees

	Share Presidential (1)	First Possible Presidential (2)
(A) Main specification, missing voting = 0		
2SLS	0.050**	0.058**
	(0.022)	(0.028)
CCM	0.400	0.355
(B) Out-of-state college attendees imputed with 0		
2SLS	0.043*	0.053*
	(0.023)	(0.028)
CCM	0.354	0.318
(C) Out-of-state college attendees imputed with predicted		
2SLS	0.059***	0.068**
	(0.021)	(0.027)
CCM	0.407	0.366
(D) Out-of-nearby-states college attendees imputed with predicted		
2SLS	0.059***	0.063**
	(0.022)	(0.027)
CCM	0.407	0.366

Notes: This table varies shows various imputations for out-of-state college attendees. In the main specification, individuals missing voting information are assumed to be non-registrants or non-voters. In the imputed specifications, out-of-state college attendees have various values imputed for their registration and voting statuses, as indicated in the headers. Predicted status is based on the regression adjusted registration/voting rates of Boston charter lottery applicant college attendees, who did not receive an offer and did not attend a charter school, separately for 2- and 4-year institutions. All other notes and sample sizes are the same as in Online Appendix Table A.7. N = 9,562. Robust standard errors are in parentheses (* p<0.10 ** p<0.05 *** p<0.01).

Appendix F: Parents

This appendix describes our procedures for estimating impacts of child charter school attendance on parents' voting, as well as what we find.

F.1.1 Estimating parent voting impacts

The analysis sample differs for parents. First, not all of the lottery records that schools provided us included parent information, and this differed by year within charter schools. We thus exclude all lotteries without parent information as well as those that have parent information, but only for a subset of parents, typically parents of students on the waitlist. We also include some more recent lotteries that had parent information but for whom the students involved were not yet 18 by the 2016 election. This resulted in a student sample size about three-quarters of the size of our main sample. We show in Appendix Table F.1 that there are no differences by offer status in terms of student characteristics, which, as in the main sample, is consistent with no manipulation of the lotteries themselves.

There are also some differences in the matching and estimation strategies. We use parent name information to match to the voter files, limited to voters residing in Boston. We augment our Massachusetts voter files with an earlier Boston voter file from 2009 to be able to include parents who were registered circa 2009 but may have moved away or dropped their registration prior to our later files.[37] We have no consistent, additional parent information like date of birth or address, so we cannot differentiate between cases when a parent has a common name and matches to multiple people in the voter file.[38] We thus do not conduct any fuzzy matching exercises, since we do not have any audit information to help make a confirmatory match.

We account for multiple matches, which occur when a parent has a common name, by presenting several models that include or exclude multiple matches in different ways. The three models are: 1) including all matches and clustering standard errors at the student level; 2) eliminating multiple matches and limiting the analysis sample to those who match to only one name or no name in the voter file, or 3) collapsing the data to the student level, which averages multiple matches. Since students can have two parents associated with them, even without the multiple matches, we would include all parent information and cluster our standard errors at the student level, or collapse the data with an average at the student level. Additionally, we present each sample with and without inverse propensity to match weights, based on demographics and name characteristics.

In Appendix Table F.2, we examine parent name characteristics by lottery offer status. Overall, the vast majority of lottery records in the parent analysis sample (98.5%) have parent information (Panel A), with no differences for students who receive an offer. For the students that do have parent name information, Panel B shows that the length and commonness of their name is the same by offer status. The last panel, Panel C, shows that there is no difference in terms of matching to only a single voting record or to multiple voting records, which is expected based on similar name characteristics.

F.1.2 Testing the policy feedback mechanism

A child's charter school attendance appears to have no impact on the likelihood that a parent votes, as can be seen in Table 3. 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 in that election, but still find no effect on parental voting.

Because parents have a voting history prior to interacting with a charter school, unlike the student sample, we can examine a placebo outcome: voting in elections *before* a child participates in the charter school lottery. Since there is no possible way a child's charter school attendance could affect civic behavior prior to it occurring, any "impacts" we found here would imply some sort of bias in our estimation

[37] This file does not contribute to our student analysis since students in our sample are generally yet old enough to vote in 2009 and it is restricted to Boston only.

[38] We have limited address data for about one-third of the parent sample, which often confirmed correct matches. However, we do not use this information to refine cases with multiple matches since it is only available for a small subset of parents and families may move.

strategy. This placebo test is shown in Columns 6 of Table 3, which reports impacts on presidential elections prior to the relevant charter school lottery in our sample. Point estimates are quite small, and none are statistically significant, indicating that our estimation strategy is sound. Additionally, comparing the control complier mean from the election before the lottery to the one after the lottery shows similar voting rates. This demonstrates that, unlike in Hastings et al. (2007), losing the lottery did not spur political participation for parents of students who missed the opportunity to attend. As a whole, the lack of parent voting differences due to charter school attendance implies that there is no evidence for the policy feedback channel, at least for adults.

Table F.1: Covariate Balance, Lotteries with Parent Information, Student Characteristics

	Non-offered Mean (1)	Initial Offer Differential (2)	Waitlist Offer Differential (3)
Female	0.507	0.001	-0.007
		(0.014)	(0.014)
Asian	0.026	0.007	-0.001
		(0.004)	(0.004)
Black	0.513	-0.002	0.007
		(0.013)	(0.014)
Latinx	0.345	-0.006	-0.005
		(0.013)	(0.013)
Other race	0.043	0.006	-0.003
		(0.006)	(0.005)
White	0.073	-0.004	0.001
		(0.006)	(0.006)
Special education	0.211	0.008	-0.032**
		(0.011)	(0.011)
English learner	0.194	0.005	-0.002
		(0.009)	(0.010)
Free/reduced price lunch	0.779	-0.003	0.003
		(0.012)	(0.012)
Baseline MCAS ELA	-0.529	-0.026	0.053+
		(0.028)	(0.029)
Baseline MCAS Math	-0.493	-0.018	0.034
		(0.027)	(0.028)
	p-value	0.809	0.398

Notes: This table shows means and offer differentials for student and parent characteristics in the parent lottery sample. The sample is restricted to students enrolled Boston Public Schools or Boston charter schools who applied to charter schools in 2008 to 2016 who applied to lotteries with parent name information. Student characteristics are from the SIMS data and the data is limited to one observation per student (n = 7,760). Column 1 shows the proportion of non-offered students with a given characteristic. Columns 2 and 3 report coefficients from regressions of the student characteristic on initial and waitlist offer dummies, including controls for risk sets (+ $p<0.10$ * $p<0.05$ ** $p<0.01$ *** $p<0.001$).

Table F.2: Covariate Balance, Lotteries with Parent Information, Parent Characteristics

	Non-offered Mean (1)	Initial Offer Differential (2)	Waitlist Offer Differential (3)
(A) Has parent name			
Parent name present in lottery records	0.990	0.006*	-0.001
		(0.003)	(0.003)
(B) Parent name characteristics			
Length of first name	6.066	0.027	-0.018
		(0.039)	(0.041)
Length of last name	6.445	0.078	-0.012
		(0.049)	(0.051)
Commonality of name	222.751	0.777	-4.555
		(14.038)	(14.317)
Not common name	0.842	0.000	0.002
		(0.010)	(0.010)
	p-value	0.427	0.971
(C) Linked to voting data			
Linked to one voting record	0.479	-0.002	0.008
		(0.013)	(0.014)
Linked to multiple voting records	0.255	-0.007	-0.001
		(0.011)	(0.011)
Linked to no voting records	0.256	0.015	-0.008
		(0.012)	(0.012)

Notes: This table shows means and offer differentials for student and parent characteristics. The sample is restricted to students enrolled Boston Public Schools or Boston charter schools who applied to charter schools in 2008 to 2016 who applied to lotteries with parent name information. Parent name characteristics (Panel B) are derived from parent names and thus are conditional on existence of a parent name. There are multiple observations per student if a student has two parent names associated with their information (Panel A: N = 11,007, Panels B and C: N = 10,865); in this case, standard errors are clustered by student. Column 1 shows the proportion of non-offered students with a given characteristic. Columns 2 and 3 report coefficients from regressions of the student characteristic on initial and waitlist offer dummies, including controls for risk sets (+ p<0.10 * p<0.05 ** p<0.01 ***p<0.001).