



REPORT

FINAL REPORT

The Impact of Democracy Prep Public Schools on Civic Participation

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CONTENTS

ACKNOWLEDGEMENTS	III
EXECUTIVE SUMMARY	VII
I. INTRODUCTION.....	1
Previous literature	2
II. RESEARCH DESIGN AND DATA	4
Using the admission lottery to assess impact.....	4
Using information from prior studies to estimate Democracy Prep’s true effects	4
Data and sample	5
III. RESULTS.....	7
Baseline equivalence of treatment and control groups.....	7
Enrollment rates, by treatment status	8
Impact of Democracy Prep on registration and voting.....	9
Estimates of Democracy Prep’s true effects informed by prior research.....	10
IV. CONCLUSION	14
REFERENCES.....	15
APPENDIX A: METHODOLOGICAL DETAILS	A-1
APPENDIX B: SUPPLEMENTAL TABLES.....	B-1

TABLES

Table III.1. Student baseline equivalence 7

Table III.2. Parent baseline equivalence 8

Table III.3. Enrollment rates by treatment status 8

Table B.1. Student sample.....B-2

Table B.2. Student entry grades for lottery winners.....B-3

Table B.3. Additional student baseline equivalenceB-4

Table B.4. Parent sample.....B-5

Table B.5. Student ITT model resultsB-6

Table B.6. Student TOT model results.....B-7

Table B.7. Student enrollment impacts (Bayesian estimates)B-8

Table B.8. Parent ITT model resultsB-9

Table B.9. Parent TOT model resultsB-10

FIGURES

Figure ES.1. Impact of enrolling in Democracy Prep on registration and voting (Bayesian estimates informed by prior research) viii

Figure III.1. Student ITT results: the impact of receiving an offer of admission..... 9

Figure III.2. Prior estimates 11

Figure III.3. Probability of a positive effect of enrolling in Democracy Prep 12

Figure III.4. Impact of enrolling in Democracy Prep on registration and voting (Bayesian estimates)..... 13

EXECUTIVE SUMMARY

The foremost purpose of public education is to prepare students for effective citizenship. Americans pay taxes to support public schools because an educated citizenry is essential to the health of a democracy. This public purpose of education motivated the establishment of common schools in the early decades of the nation and retains wide popular support among Americans today. Indeed, the current fractures in our political environment suggest that education for citizenship might be even more important now than in the past.

Even so, the effectiveness of public schools in developing engaged citizens has rarely been examined empirically. Early promoters theorized that public schools would promote citizenship based on their governance: common schools, operated by democratically elected officials in each community, would naturally inculcate the knowledge, values, and skills needed for effective citizenship. This theory survived largely untested for a century and a half until the 1990s, when charter schools were created, representing a new type of public school. Charter schools implicitly challenge the old theory, suggesting that schools that are publicly authorized, publicly funded, publicly regulated, and open to the public can promote citizenship while being operated autonomously, outside the direct control of elected officials.

Some charter schools and charter school networks have made the implicit challenge explicit, by making preparation for citizenship a primary goal (Lake and Miller 2012; Schmitt and Miller 2015). One of these is Democracy Prep, launched in 2006 as a charter middle school in New York City and now educating more than 5,000 students across multiple campuses and grades kindergarten through 12. Democracy Prep's mission as a charter school network is "to educate responsible citizen scholars for success in the college of their choice and a life of active citizenship."

The clearest indicators of Democracy Prep's success in promoting civic engagement are the registration and voting rates of its students after they become adults. In this report, we measure the impact of Democracy Prep on voter registration and participation in elections. We use Democracy Prep's randomized admissions lotteries to conduct a gold standard experimental analysis that distinguishes Democracy Prep's effect from the effects of families, students, and other outside factors. Because Democracy Prep used lotteries to determine entry to middle and high school grades for the past decade, many students who were offered admission, particularly in the early years, were old enough to register and vote in time for the 2016 election. This is the first study to rigorously measure the causal impact of charter schools on civic participation.

We find that receiving an offer of admission to Democracy Prep produced a statistically significant increase in students' probability of voting in the 2016 election of about 6 percentage points. The estimated effect on voter registration is similar in size but not statistically significant in conventional terms.

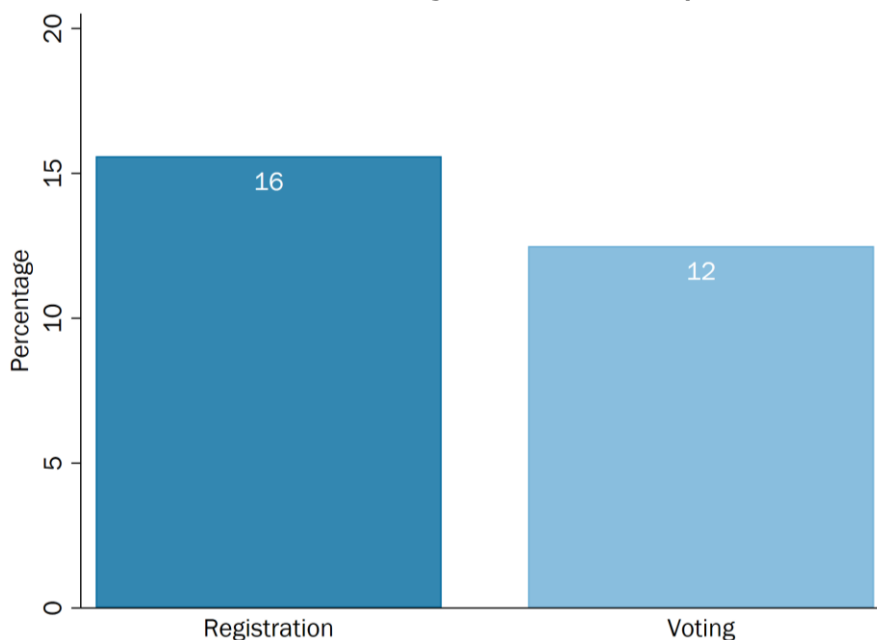
The impact of *enrolling* in Democracy Prep is larger than the impact of receiving an offer. Many students who received offers did not actually enroll in Democracy Prep, which suggests that the impact of receiving an offer must be driven by a larger impact on the subset of students who actually enrolled. A standard conversion method suggests that enrolling in Democracy Prep might increase both outcomes by about 24 percentage points (statistically significant for voting but not for registration). But these impacts are measured imprecisely, with a wide range of possible variation.

These estimated impacts of enrolling in Democracy Prep are also dramatically larger than the impacts found in previous literature on the effects of education on voting and registration, suggesting that random variation could have led to an overestimate of the size of the impacts. Because the estimates are both imprecise and surprisingly large, we conducted a complementary (Bayesian) impact analysis that grounded our original estimates of Democracy Prep’s impacts in the findings of previous research on the effects of education on registration and voting.

Previous published studies of the effects of education (none of which involved charter schools) found average impacts of about 8 percentage points on registration and 6 percentage points on voting. Incorporating this literature (and dividing the averages from the literature in half to account for the fact that positive impacts are more likely to be published), *we find a 98 percent probability that enrolling in Democracy Prep produced a positive impact on registration, and a 98 percent probability that enrolling produced a positive impact on voting in the 2016 election.*

The same literature-informed (Bayesian) analysis suggests that *Democracy Prep increases the voter registration rates of its students by about 16 percentage points and increases the voting rates of its students by about 12 percentage points* (Figure ES.1). Given the low registration and voting rates of young adults nationally, these are substantial impacts. In sum, even a conservative analysis (which accounts for possible overestimation of impacts in our original approach) suggests that enrolling in Democracy Prep has large positive effects on students’ democratic participation in adulthood.

Figure ES.1. Impact of enrolling in Democracy Prep on registration and voting (Bayesian estimates informed by prior research)



Democracy Prep provides a test case of whether charter schools can successfully serve the foundational purpose of public education—preparation for citizenship—even while operating outside the direct control of elected officials. With respect to the critical civic participation measures of registration and voting, the answer is yes.

Given its explicit mission, Democracy Prep is probably not typical of all charter schools. Nonetheless, its success in raising the registration and voting rates of the low-income, minority students it serves provides a proof point for charter schools and conventional public schools alike: An education focused on preparing students for citizenship can in fact increase students' civic participation when they reach adulthood. Renewed attention to the foundational purpose of public schools might broadly increase civic participation across the country.

I. INTRODUCTION

The foremost purpose of public education is to prepare students for effective citizenship. Americans pay taxes to support public schools because an educated citizenry is essential to the health of a democracy. This public purpose of education motivated the establishment of common schools in the early decades of the nation and retains wide popular support among Americans today. Indeed, the current fractures in our political environment suggest that education for citizenship might be even more important now than in the past.

Even so, the effectiveness of public schools in developing engaged citizens has rarely been examined empirically. Early advocates of public education, such as Horace Mann, theorized that public schools would promote citizenship based on their governance: common schools, operated by democratically elected officials in each community, would naturally inculcate the knowledge, values, and skills needed for effective citizenship. This theory survived largely untested for a century and a half until the 1990s, when charter schools were created, representing a new type of public school. Charter schools implicitly challenge the old theory, suggesting that schools that are publicly authorized, publicly funded, publicly regulated, and open to the public can promote citizenship while operating autonomously, outside the direct control of elected officials.

Some charter schools and charter school networks have made the implicit challenge explicit, by making preparation for citizenship a primary goal (Lake and Miller 2012; Schmitt and Miller 2015). One of these is Democracy Prep, launched in 2006 as a charter middle school in New York City and now educating more than 5,000 students across multiple campuses and grades kindergarten through 12. Democracy Prep's mission as a charter school network is "to educate responsible citizen scholars for success in the college of their choice and a life of active citizenship."

Democracy Prep encourages civic behavior in students through a variety of curricular and experiential means, including visiting legislators, attending public meetings, testifying before legislative bodies, and discussing influential essays on civics and government. Each election day students participate in a "Get Out the Vote" campaign. Students receive tee-shirts and pamphlets with the slogan "I Can't Vote, but You Can!" and canvass highly frequented street corners to distribute the message (Lake and Miller 2012). As seniors, students enroll in a capstone course in which they develop a "Change the World" project to investigate a real-world social problem, design a method for addressing the issue, and implement their plan (Tripodo and Pondiscio 2017).

The clearest indicators of Democracy Prep's success in promoting civic engagement are the extent to which its students register to vote and participate in elections after they reach age 18. In this report, we measure the impact of Democracy Prep on the key civic outcomes of voter registration and participation in elections. We use Democracy Prep's randomized admissions lotteries to conduct a gold standard experimental analysis that distinguishes Democracy Prep's effect from the effects of families, students, and other outside factors. This is the first study to rigorously measure the causal impact of charter schools on civic participation.

Democracy Prep seeks not only to promote election participation among its graduates, but also among students' parents. The study therefore also measures the impact of Democracy Prep on the voter registration and election participation of parents.

Democracy Prep serves a disadvantaged student population. In the 2008–2009 through 2013–2014 academic years, 76 percent of Democracy Prep middle school students qualified for free or reduced-price meals, 69 percent were black, 30 percent were Hispanic, 23 percent spoke no English at home, and 17 percent received special education services (Corcoran and Cordes 2015). Many of these characteristics are associated (in adults) with lower rates of voter registration and election participation (File 2015). Among families applying to Democracy Prep for admission, the prior voter registration rate of parents was 60.1 percent, about 10 percentage points below the national average (U.S. Census Bureau 2017).

Previous literature

This is the first study to measure the impact of charter schools on registration and voting, but a few prior studies have attempted to assess the impact of educational attainment (Dee 2004; Milligan et al. 2004; Sondheimer and Green 2010); of private schools (Dee 2005; Carlson et al. 2016); or of civics education (Bachner 2010) on those outcomes.¹ Here, we briefly describe key findings from this literature; later, we use the findings from the literature to inform a Bayesian analysis of the probability that Democracy Prep produces positive effects on registration and voting.

Additional time in the classroom might lead to a more engaged citizenry through a variety of mechanisms. Educational attainment can provide a firmer understanding of political institutions and processes, meanwhile equipping students with the cognitive skill set necessary to actively participate in civic society. Additional years of secondary schooling and college entrance have been found to increase civic engagement—not only voter registration and voting, but also other measures of civic involvement, such as volunteering and newspaper readership (Dee 2004). High school graduation similarly has been found to increase voting and other civic behaviors, particularly in the United States (Milligan et al. 2004). Sondheimer and Green (2010) examined three distinct educational interventions related to high school graduation and found that educational attainment was strongly associated with increased voter turnout. All of these studies were observational or quasi-experimental; none used a randomized experimental design.

A small number of studies have attempted to measure the effect of private schools on registration and voting. In principle, private and religious schools might have either positive or negative effects on civic outcomes (Gill et al. 2007). Dee (2005) presented evidence that students who attended 10th grade at Catholic high schools were more likely to vote as adults, but unmeasured background characteristics of the students rather than the schools themselves might have driven the results. Carlson et al. (2016) used the randomized lotteries of a privately funded voucher program in New York City to conduct an experimental analysis, and they found little evidence for increased voter registration or voting.

The potential link between civics education courses and civic engagement is clearest: Education about government and electoral processes specifically aims to increase civic engagement. In a nonexperimental examination of high school civics coursework, the probability

¹ More than a decade ago, one study conducted a nonexperimental analysis of telephone survey data from parents in Washington, D.C., to compare civic activities and political tolerance of students in district and charter schools in that city (Buckley and Schneider 2004). That study found evidence of higher levels of civic activity and training in civic skills among charter students, and comparable levels of political tolerance, relative to students in district schools.

of voting increased with the completion of civics coursework. This increase in voting behavior was magnified when coursework was undertaken for longer periods (one versus two semesters) (Bachner 2010).

In sum, the existing literature suggests that education in general and civics courses in particular might positively affect registration and voting, but none of the favorable evidence comes from randomized experimental studies that would permit strong causal inferences. Further, no study examined the effects of charter schools, which constitute a new type of public school that departs from the historical model of public education in the United States. This study provides the first evidence on whether charter schools that specifically focus on civic preparation can improve the civic participation of their graduates.

II. RESEARCH DESIGN AND DATA

Using the admission lottery to assess impact

To assess the impact of Democracy Prep on voter registration and election participation, we use Democracy Prep’s admissions lotteries to identify treatment and control groups. Since its inception, Democracy Prep has conducted randomized lotteries to determine applicants to admit to its incoming classes. These admitted applicants constitute the treatment group. Because offers of admission were determined by chance, families who were not offered admission (lotteried out) did not differ from those offered admission—not only in terms of prior achievement and demographic characteristics, but also in terms of unmeasured characteristics such as student and parent motivation—and therefore constitute the control group. The admissions lottery created a randomized natural experiment that we use to develop the strongest possible inferences about the causal impact of Democracy Prep on voter registration and election participation of students and parents.

To measure impacts on voter registration and election participation, we begin by conducting an intent-to-treat (ITT) analysis that compares outcomes of lottery winners and students who are lotteried out, controlling for any random differences between the groups in terms of age and gender. This enables us to rigorously estimate the impact of an *offer* of admission to Democracy Prep on the voter registration and election participation of students and parents.

The impact of an offer of admission is not the same as the impact of enrolling in Democracy Prep, because more than half of those receiving offers chose to enroll elsewhere (and a few lottery losers eventually found a way to enroll, perhaps through the wait list).² We therefore also use the lottery as an instrument in a two-stage analysis that estimates the impact of *enrolling* in a Democracy Prep school, also known as the effect of treatment-on-the-treated (TOT). The estimated impact of enrolling is necessarily larger than the estimated impact of receiving an offer, but the uncertainty about the impact of enrolling increases alongside the estimated size of the impact. Appendix A provides more details on the analytic approach.

Democracy Prep has tried to promote the civic participation of parents as well as students by, for example, including voter registration information in enrollment materials. We therefore use the admissions lotteries to conduct a secondary analysis of registration and voting among Democracy Prep parents.

Using information from prior studies to estimate Democracy Prep’s true effects

We also use information from outside of our study to assess the probability that the true effect of enrolling in Democracy Prep on voter registration and election participation is positive and the likely size of Democracy Prep’s true effects.

² The enrollment rate for students who received an offer was 43.5 percent, whereas the enrollment rate for students who did not receive an offer was 17.5 percent.

Estimating the probability that an intervention has a truly positive effect requires an externally informed understanding of the difficulty of the task. If similar interventions have rarely made large impacts on similar outcomes, then we would infer that it is hard to move the needle on registration and voting—this would in turn make a very large impact of Democracy Prep seem less plausible. By contrast, the more common large effects have been in the past, the more probable it is that a sizeable impact estimate in this study was the result of a true effect rather than random chance.³

For this evaluation, information is available from prior studies that estimated impacts of other educational interventions on civic engagement, as described in Chapter I. We use a Bayesian approach to incorporate this prior information into our analysis and ultimately assess the probability that Democracy Prep truly has positive effects and the likely size of those effects. We follow Gelman (2015) in defining the Bayesian prior in terms of information rather than beliefs, and we examine the sensitivity of findings to the selection of prior information. Technical details of our approach can be found in Appendix A.

Data and sample

Democracy Prep provided the admissions lottery data. Files include information on applicants, including names, dates of birth, gender, lottery priorities,⁴ lottery results, names of parents, and contact information.

The student sample for this study includes students who entered the lottery to attend any Democracy Prep school in New York City from 2007–2008 through 2015–2016. To be included in the analytic sample, students must be a first-time applicant in the family and be at least 18 years old by the 2016 election. The analytic sample includes 1,060 students, 35 percent of whom were offered admission through the lottery. Although Democracy Prep now includes schools at all grade levels, students who entered its elementary schools (which opened later) were not old enough to vote by 2016. Our student sample includes students who applied to enroll in Democracy Prep in grades 6 through 11. Almost three-fourths (72 percent) of students receiving offers through the lotteries in our sample were applicants to 6th grade. (See Appendix A for technical details on the construction of the analytic sample.)

The parent sample includes parents of the eligible students described above, less the requirement that the student be eligible for the 2016 election. Thus, parents can have students in any application grade. All parents are deemed eligible to vote in elections following their

³ A common mistake is to believe that the p -value—which depends only on data from the study at hand—can be used to assess the probability that the true effect of an intervention is positive. In 2016, the American Statistical Association issued a statement regarding the widespread misinterpretation of p -values (Wasserstein and Lazar 2016; Greenland et al. 2016). In the context of impact evaluations, the implication of the association’s statement is that p -values do not directly support probability statements regarding the true impact of an intervention. For example, a positive result with a p -value of 0.05 does not mean that there is a 95 percent probability that the true impact is larger than zero. Rather, a p -value of 0.05 signifies that a null hypothesis of no effect can be rejected with 95 percent confidence.

⁴ Applying students with an enrolled sibling or a simultaneously applying sibling who receives an offer, receive lottery priority. Also, applying students in a subdistrict with a Democracy Prep school receive lottery priority.

children's application year. For the parent analysis, the analytic sample includes 5,792 parents, 52 percent of whom had children offered admission through the lottery.

We matched lottery and enrollment records to outcome data provided by Catalist, which maintains a national database with comprehensive information on voting-age individuals. Appendix A provides details about the matching process. We examine impacts on 2016 registration and voting for students. This analysis disproportionately relies on earlier cohorts of students because students must have turned 18 by the 2016 election to be included in the analysis. This same fact prevents an analysis of earlier elections for students due to sample size limitations. See Appendix B, Table B.1 for student sample sizes by year and grade.

Our secondary analysis of impacts on parents involves examining 2016 registrations at any point after the start of the student's application year. We also examine impacts on voting in the 2014 and 2016 elections. This offers a comparison of both a presidential and nonpresidential election, which generally have different turnout rates (File 2015). See Appendix B, Table B.4 for parent sample sizes by year.

III. RESULTS

In this chapter, we begin by reporting the baseline (pre-lottery) characteristics of the treatment and control groups, then describe the differences between the actual enrollment rates of lottery winners and losers (because not all lottery winners chose to enroll, and a few lottery losers ultimately enrolled). We then present results of the impact analyses, beginning with standard ITT results that measure the effect of being offered admission to Democracy Prep through the lottery, followed by the effect of actually enrolling in Democracy Prep. Finally, we describe the probability that the impact of Democracy Prep is positive, as indicated by the Bayesian analysis that incorporates additional information from the literature on the effects of educational interventions on registration and voting.

Baseline equivalence of treatment and control groups

Due to the randomized nature of the lotteries, any baseline differences (measured at the time of the lottery) between students receiving offers (the treatment group) and those not receiving offers (the control group) are attributable to chance.

The student sample provides baseline data that enable us to compare pre-lottery characteristics in terms of age, gender, and the pre-application voter registration status of the student's parent. As indicated in Table III.1, we find no statistically significant baseline differences between students with and without offers in terms of age and parent's prior registration status. In contrast, there is a statistically significant chance difference in the gender composition of the treatment and control groups: Girls are represented among offered students at a rate about 11 percentage points higher than among non-offered students. A deep dive into the data and consultations with Democracy Prep and its lottery auditor uncovered no reason other than chance that would explain this difference. An analysis of baseline equivalence in a subset of students for whom additional data are available finds no statistically significant differences between treatment and control groups in prior test scores, English-language learner status, special education status, or eligibility for free or reduced-price lunch (details are available in Appendix B, Table B.3).

To ensure that random baseline differences between the experimental groups do not bias results, we include gender, age, and parent baseline voter registration as controls in our impact analyses.

Table III.1. Student baseline equivalence

Characteristic (percentage unless otherwise indicated)	Offer	No offer	Difference
Age at 2016 election	20.0	20.0	0.0
Female	54.9	43.7	11.1**
Male	42.7	51.3	-8.7*
Gender missing	2.5	4.9	-2.4
Parent registered to vote before September 1 of lottery application year	58.5	61.6	-3.0
Number	372	688	1,060

* $p < 0.05$, ** $p < 0.01$.

In the parent analysis, we focus our baseline comparison on gender, pre-application voter registration status, and pre-application voting behavior (age is not consistently available for parents). Pre-application voting behavior is defined as 2012 voting behavior for parents whose students apply for the 2013–2014 school year or later. Because this measure is defined for only a subset of parents, we do not include it as a covariate in our estimation models. As Table III.2 indicates, we find no statistically significant differences in baseline characteristics in the parent sample.

Table III.2. Parent baseline equivalence

Characteristic (percentage unless otherwise indicated)	Offer	No offer	Difference
Female	67.3	65.7	1.7
Male	9.5	11.2	-1.7
Gender missing	23.1	23.1	0.0
Registered to vote before September 1 of application year	60.1	60.1	0.0
Voted in 2012 election prior to application year (2013-14 or later)	38.8	36.8	2.0
Number	3,032	2,760	5,792

Note: The sample size for pre-application voting behavior is 1,453 parents of offered students and 1,399 parents of students who did not receive an offer.

* $p < 0.05$, ** $p < 0.01$

Enrollment rates, by treatment status

Not all students who received an offer through the lottery chose to enroll in Democracy Prep, and some students who did not receive lottery offers nonetheless succeeded in enrolling (through the wait list, in another year, or through some other mechanism). Even so, receiving an offer to attend Democracy Prep through the lottery makes it more likely that a student will enroll, which is critical to making our analysis possible. Table III.3 shows enrollment rates of the lottery winners and losers in the student and parent analyses.

Table III.3. Enrollment rates by treatment status

Enrollment rate	Offer	No offer	Difference
Students			
Enrolled (application year)	43.5	17.5	25.9**
Ever enrolled	44.4	19.2	25.2**
Number	372	688	1,060
Parents			
Enrolled (application year)	29.7	7.4	22.3**
Ever enrolled	30.3	7.9	22.4**
Number	3,032	2,760	5,792

* $p < 0.05$, ** $p < 0.01$

There is a statistically significant enrollment advantage of about 26 percentage points for offered versus non-offered students. The enrollment rate differential is similar whether examining enrollment in the application year or enrollment at any point following the initial application to Democracy Prep.

The enrollment rate differential in the parent sample mirrors the student sample. There is an advantage of about 22 percentage points for parents of offered students versus parents whose student did not receive an offer.

Impact of Democracy Prep on registration and voting

We first estimate the effect of receiving an offer on students’ registration and voting in the 2016 election. This is the difference in outcomes for students who did and did not receive an offer, or an ITT estimate. *Receiving an offer of admission to Democracy Prep via the lottery leads to an increase in both registration and voting of about 6 percentage points* (Appendix B, Table B.5). In the case of voting, the estimated impact is statistically significant (at 0.05), although the impact on registration is not significant. Taken in concert, the two estimates suggest that the receiving an offer meaningfully boosts involvement in the electoral process. Figure III.1 graphically illustrates the impact of receiving an admission offer, comparing the actual registration and voting rates of the treatment group with the registration and voting rates that would have been expected if they had not been offered admission (in other words, the regression-adjusted registration and voting rates of the lottery losers who were not offered admission).

Figure III.1. Student ITT results: the impact of receiving an offer of admission



Note: This figure shows the unadjusted treatment group mean and regression-adjusted control mean.
 * $p < 0.05$, ** $p < 0.01$ for significance of difference between group offered admission and not offered admission.
 ITT = intent-to-treat.

We can use receiving an offer through the lottery as an instrument to estimate the impact of enrollment. In essence, we convert the ITT impact estimate to an estimate of the impact based on the assumption that any effect of offering admission could occur only by affecting the subset of lottery winners who actually enrolled. The 6 percentage point impact of the admissions offer is averaged across students who enrolled and students who did not enroll. Because students who

did not enroll could not have experienced an impact, all of the impact must have been experienced by the subset of students who enrolled.

The TOT analysis scales up the estimated ITT impact by a factor that depends on the difference in the actual enrollment rates of lottery winners and losers. Because that difference is only 26 percentage points (as shown in Table III.3), the TOT impact estimate is nearly four times the ITT impact estimate. The statistical uncertainty around the estimate, however, similarly scales up, creating a confidence interval that is four times as large as the confidence interval for the ITT impact estimate.

The TOT impact estimates (about 24 percentage points for both registration and voting: see Appendix B, Table B.6 for details) are surprisingly large, especially for voting, implying that Democracy Prep more than doubled the expected voting rates of its students. Because the uncertainty around the TOT impact estimates is quite large, we believe those results should not be taken at face value and likely overestimate the true impact of Democracy Prep. To assess this possibility systematically, we conduct a Bayesian analysis in the next section.

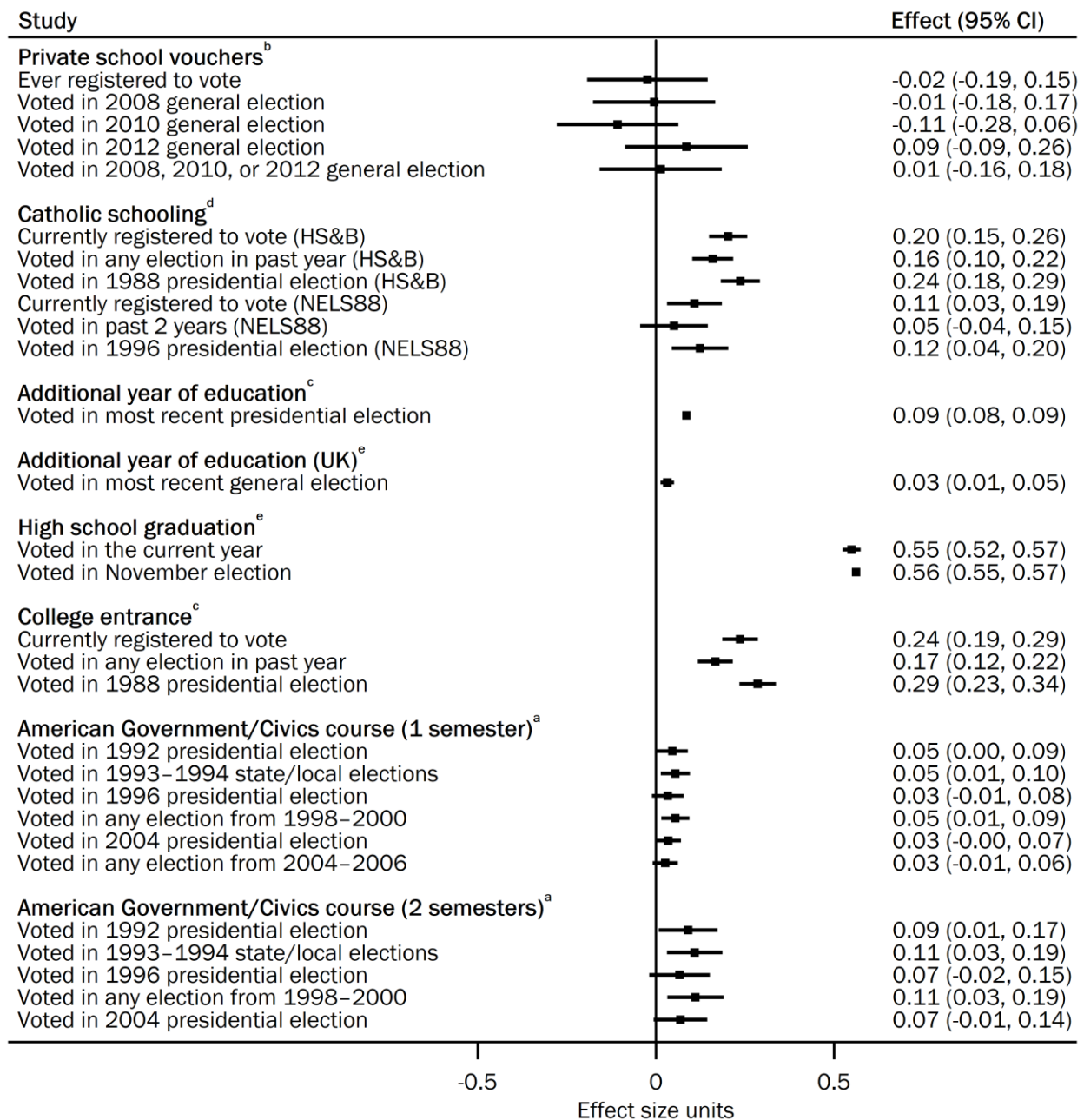
We do not find effects on the registration and voting rates of the parents of Democracy Prep students. In the parent analysis, the estimated impacts of an admissions offer on registration and voting (in 2014 and 2016) are much smaller than the estimated impacts on students (see Appendix B, Table B.8 for full details). None of these estimates are statistically significant. The TOT impacts on parents whose children enroll in Democracy Prep are similarly not significant, and are likewise much smaller than the estimated impacts on students (see Appendix B, Table B.9 for full results). In sum, we find no evidence that Democracy Prep increases the registration and voting rates of students' parents.

Estimates of Democracy Prep's true effects informed by prior research

In this section we present our Bayesian estimate of the probability that the true effect of Democracy Prep is positive, given (1) the TOT impacts we estimated in this study and (2) 29 published estimates of the impacts of eight other educational interventions on voting and registration. We then use the same framework to estimate the true effect of enrollment, grounding the original TOT impact estimates in the published impacts of the relevant literature. We conduct these exercises only for the student results, because we find no evidence of an impact on parents.

Almost all of the prior impact estimates in the literature are positive (Figure III.2). Because these are estimated impacts, not true effects, it could be unwise to take them entirely at face value. Random chance differences between treatment and control groups affect these estimates (just as they affect our estimates). Furthermore, systematic errors such as publication bias (that is, the tendency of journals to publish only findings that are statistically significant) could also affect these estimates. To prevent these biases from propagating through to the current analysis of Democracy Prep, our main Bayesian approach adjusts for such issues.

We also include two Bayesian sensitivity analyses. Our first sensitivity analysis applies a more stringent correction for issues such as publication bias than the main approach. Our second sensitivity analysis takes the prior studies at face value, making no correction for such issues. Appendix A provides detailed technical descriptions of these analyses.

Figure III.2. Prior estimates

Note: Estimates are in effect size units.

^a Bachner 2010.

^b Carlson et al. 2016.

^c Dee 2004.

^d Dee 2005.

^e Milligan et al. 2004.

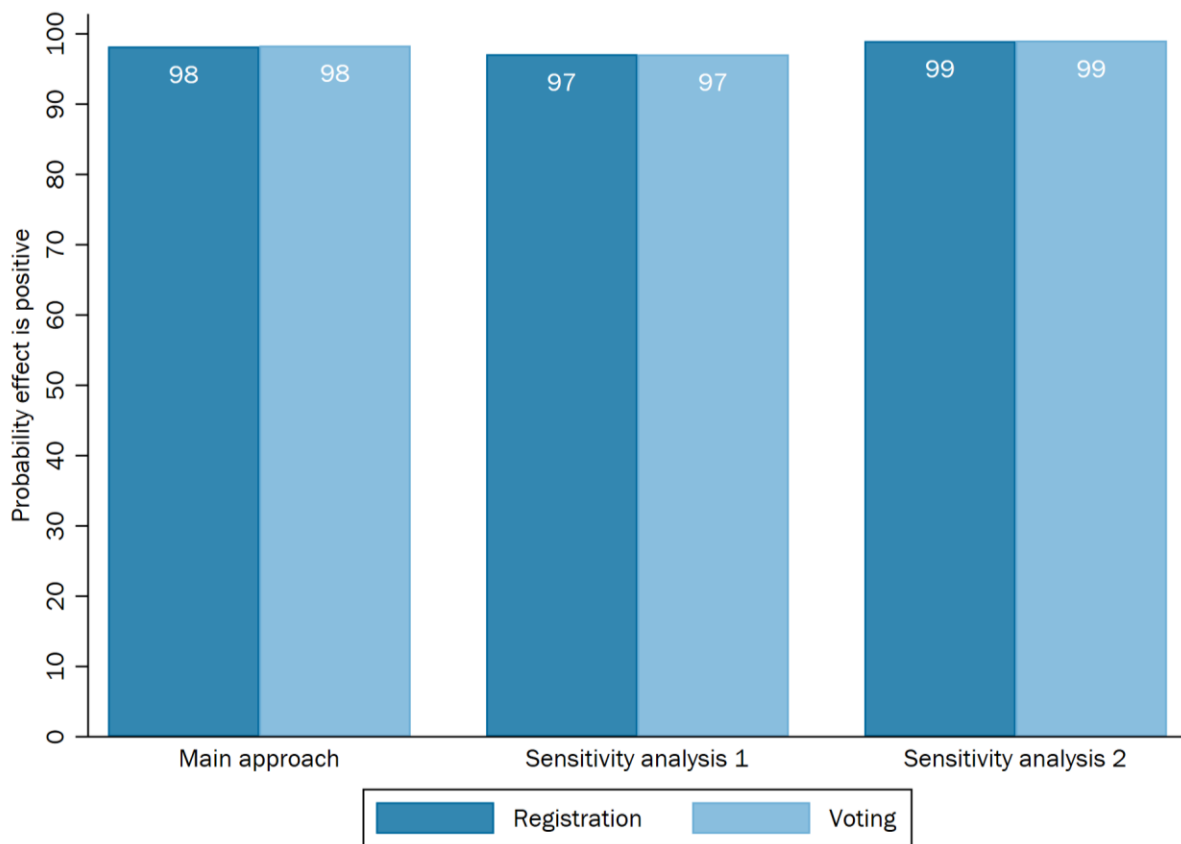
CI = confidence interval; HS&B = High School & Beyond; NELS88 = National Education Longitudinal Study of 1988; UK = United Kingdom.

Given the chances of having a positive effect that we see from past studies, combined with the TOT impact estimate of Democracy Prep that we calculated in this study, *we estimate a 98 percent probability that enrolling in Democracy Prep increased voter registration and the same probability that Democracy Prep increased voting in the 2016 election.*

Our results are robust to the selection of prior information (Figure III.3). The findings from the sensitivity analyses are as follows:

1. If we make a more stringent adjustment to the impact estimates from past studies to account for issues such as publication bias (specifically, we assume that the average true effect of interventions in this literature is actually zero rather than positive), we estimate probabilities of 97 percent that enrolling in Democracy Prep increased registration and voting.
2. If we take past results at face value (using a weighted average from the literature and not adjusting for issues such as publication bias), we estimate probabilities of 99 percent that enrolling in Democracy Prep increased each of the two outcomes.

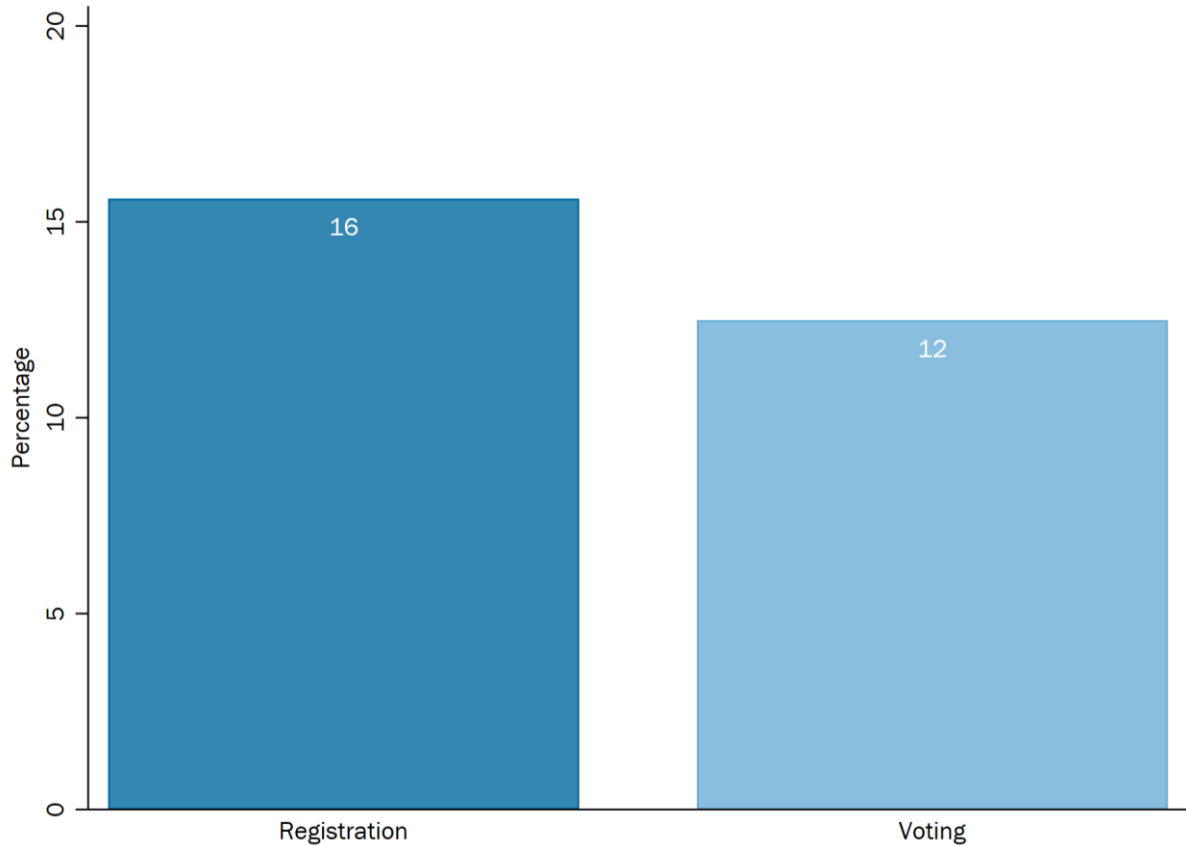
Figure III.3. Probability of a positive effect of enrolling in Democracy Prep



Using the same (Bayesian) framework of incorporating information from the broader literature on civic-minded interventions, we generate a complementary set of impact estimates. Our main approach uses the original estimated TOT impacts of Democracy Prep enrollment, correcting for potential biases in the magnitude of reported impacts in the literature. This analysis suggests that *Democracy Prep increases the voter registration rate of its students by*

about 16 percentage points and increases the voting rate of its students by about 12 percentage points (Figure III.4).

Figure III.4. Impact of enrolling in Democracy Prep on registration and voting (Bayesian estimates)



We explore the same sensitivity analyses detailed earlier to ensure findings are robust to different assumptions about the extent to which positive estimates are more likely to be published (see Appendix A for technical details). These findings support the conclusions of the main approach (see Appendix B, Table B.7 for full results):

1. If the average true effect of educational interventions is zero, we estimate that the impact of Democracy Prep enrollment is about 16 and 13 percentage points for registration and voting, respectively.
2. If the average in the literature is an unbiased indicator of the average true effect of educational interventions, we estimate increases in registration and voting of 17 and 14 percentage points for registration and voting, respectively.

IV. CONCLUSION

Democracy Prep provides a test case of whether charter schools can successfully serve the foundational purpose of public education—preparation for citizenship—even while operating outside the direct control of elected officials. With respect to the critical civic participation measures of registration and voting, the answer is yes. We estimate that enrolling in Democracy Prep substantially increases the probability that students will vote when they reach adulthood. The estimated effect on registration is likewise large, though not statistically significant in conventional terms. A conservative Bayesian analysis finds a 98 percent probability that Democracy Prep caused an increase in each of the two outcomes: increased voter registration and increased voting in the 2016 election.

Importantly, young adult voter turnout is consistently lower than voter turnout rates for older Americans. In most presidential elections in the past half century, the differential in voter turnout between young adults (18 to 24 years old) and older groups of voters has been 10 to 25 percentage points (File 2014). This makes large gains in voting among young adults particularly notable.

Given its explicit mission, Democracy Prep is probably not typical of all charter schools. Nonetheless, its success in raising the registration and voting rates of the low-income, minority students it serves provides a proof point for charter schools and conventional public schools alike: an education focused on preparation for citizenship can in fact increase students' civic participation when they reach adulthood. Renewed attention to the foundational purpose of public schools might broadly increase civic participation across the country.

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APPENDIX A:
METHODOLOGICAL DETAILS

Estimating impacts

This evaluation takes advantage of oversubscribed Democracy Prep admissions lotteries to estimate Democracy Prep’s impact on civic outcomes. Admissions lotteries approximate a randomized controlled trial. With this design, the average difference in outcomes for individuals who receive a Democracy Prep offer and those who do not provides an unbiased estimate of the impact of Democracy Prep. We estimate the following linear probability model:

$$(1) Y_i = \delta_L + \beta T_i + \gamma X_i + \varepsilon_i,$$

where Y is an indicator reflecting voter registration or election participation for student (or parent) i ; δ is a set of fixed effects (grade-by-year indicators) for lottery L ; T is the treatment indicator variable that takes a value of 1 if student i received a Democracy Prep offer (won the lottery); X is a vector of baseline characteristics included to improve precision (including, in the student analysis, the prior registration status of a parent), and ε is a random error term. β will reflect the impact in percentage points of receiving an offer to attend a Democracy Prep school on the likelihood that student i registers to vote or votes. This is an intent-to-treat (ITT) analysis, as it captures the impact of receiving an offer to attend a Democracy Prep school. To estimate the impact of attending a Democracy Prep school, we also estimate treatment-on-the-treated (TOT) models, using a standard instrumental variables approach. We estimate the parameter:

$$(2) \beta_{IV} = \frac{cov(T_i, Y_i)}{cov(T_i, E_i)},$$

where β_{IV} reflects the ratio of the covariance between treatment status (T) and an indicator for either registration or election participation (Y) to the covariance between treatment status and enrollment status (E).

Rules for inclusion or exclusion in analysis sample

Students must be first-time applicants to be eligible for the analysis sample. This means that no siblings have previously applied for admission and ensures that sibling preference and familiarity or experience with the application process do not confound offer rates. There are two types of lottery priority: sibling priority and district priority. Students with sibling priority status are automatically admitted and therefore excluded from the analytic sample. Students who live in the local district (the neighborhood) have priority over students who live elsewhere in New York City. Students with and without district priority are potentially eligible for the analytic sample, depending on whether there are any open seats after all in-district students receive offers. In most instances, those with in-district priority filled all available seats, and we use the in-district lottery in the analytic sample. The analytic sample includes out-of-district students when a lottery was conducted for them (that is, when in-district applicants did not fill the available seats). We required cohort offer rates to fall between 10 and 90 percent to ensure that a lottery was used for the cohort, as opposed to a data anomaly. The secondary parent analysis includes only parents of these eligible students.

Matching lottery data to registration and voting data

Outcomes provided by Catalist include voter registration and participation in the 2012, 2014, and 2016 elections. Fields we provided to Catalist to facilitate matching include first and

last name, gender, date of birth, address, and phone number. Before sending records to Catalist, we made substantial efforts to standardize fields (for example, we corrected misspellings of *Manhattan*). Furthermore, we required that records have at least a first and last name as well as one of date of birth, address, email address, or phone number. After we received the matched data set, we scrutinized matched records in which key information varied (such as date of birth), resulting in a small number of rejected matches. We treated records that Catalist could not match as non-registrants and non-voters—indicating that there was no corresponding registrant or voter record associated with the demographic information provided. Although any record linkage method can result in misidentification, there is no theoretical reason to expect misidentification to vary by offer status. Therefore, this does not pose a threat to the validity of the randomization framework.

Weights

We construct student weights based on the offer rate in the student's cohort (grade-by-year). In the simplest case, the probability of an offer is the number of applicants offered admission (N_T) divided by the total number of lottery applicants (N). That is, the probability of an offer for student i is $p_i = \frac{N_T}{N}$. Because we have multiple cohorts (strata), the probability of an offer for student i in stratum j is $p_{ij} = \frac{N_{Tj}}{N_j}$, where N_{Tj} is the treatment group size within the stratum and N_j is the size of the stratum. The base weights are the inverse of the probability of being in the student's treatment condition.

$$(3) W_{Tij} = \frac{1}{p_{ij}}$$

$$(4) W_{Cij} = \frac{1}{(1-p_{ij})}$$

Within stratum, we then normalized weights such that the weights of each experimental group sum to one-half the overall size of the stratum. This way, the size of the application stratum factors into the overall distribution of weights. These normalization factors (NF) are specific to each stratum and experimental group. We then multiplied each factor by the student's base weight to achieve a final weight.

$$(5) NF_{Tj} = \frac{0.5 * N_j}{\sum_{i=1}^{N_{Tj}} W_{Tij}}$$

$$(6) NF_{Cj} = \frac{0.5 * N_j}{\sum_{i=1}^{N_{Cj}} W_{Cij}}$$

All simultaneously applying students receive an offer as long as one family member wins the lottery. Thus, siblings have a higher probability of receiving an offer, for which the weights must account. In the case of a single pair of same-grade siblings (twins), the probability of receiving an offer is affected both for siblings and non-siblings. For the single set of same-grade siblings, the probability of an offer is simply the probability that either receives an offer. Then, we use the probability of the set of same-grade siblings receiving an offer to determine the probability of an offer for non-siblings:

$$(7) p_i^{sib} = 2 * \frac{N_{Tj}}{N_j} - \frac{N_{Tj}}{N_j} * \frac{N_{Tj-1}}{N_{j-1}}$$

$$(8) p_i^{non-sib} = \frac{N_{Tj}}{N_{j-2}} * (1 - p_i^{sib}) + \frac{N_{Tj-2}}{N_{j-2}} * p_i^{sib}$$

When there are multiple pairs of same-grade siblings (twins), the outcomes of all sets of same-grade siblings affect the probability of one set of same-grade siblings receiving admission. To account for this, we first calculate the probability of one sibling receiving an offer, and the other sibling not receiving an offer—a win-by-sibling (*WBS*). We then use the *WBS* probability and the number of same-grade sibling pairs (N_{SP}) to determine the estimated slots (*ES*) occupied by same-grade sibling pairs.

$$(9) p_{WBSj} = 2 * \frac{N_{Tj}}{N_j} * \frac{N_{Cj}}{N_{j-1}}$$

$$(10) ES_j^{sib} = (N_{SPj} - 1) * P_{WBSj}$$

$$(11) ES_j^{non-sib} = N_{SPj} * P_{WBSj}$$

$$(12) p_i = \frac{N_{Tj} - ES_j}{N_j - ES_j}$$

Another distinct scenario is different-grade siblings who simultaneously apply. The different-grade siblings will be in separate cohorts; therefore, we must appropriately adjust the probability of an offer for both cohorts. To do so, we have to know the number of students receiving an offer in each sibling's grade. In the case of different-grade siblings A and B, the resulting probability that at least one students receives an offer is:

$$(13) p_i^{sibA} = 1 - \left(\frac{N_{Cj}^{sibA}}{N_j^{sibA}} * \frac{N_{Cj}^{sibB}}{N_j^{sibB}} \right)$$

The final scenario is the union of the preceding scenarios: same- and different-grade siblings in the same cohort. To account for this, we first construct a set of intermediate probabilities (p_{INTi}), which are the probabilities of receiving an offer as if there are no different-grade siblings, but incorporating same-grade siblings. Then, we use the intermediate probabilities to construct the final probability of receiving an offer, at this stage accounting for different-grade siblings. Depending on whether the cohort contains a single set of same-grade siblings or multiple sets of same-grade siblings, we apply the formulas discussed earlier: Equations (7) and (8) and Equations (9–12), respectively. For all students who *do not* have a different-grade sibling, the final probability of receiving an offer is the same as their intermediate probability. For different-grade siblings, we modify Equation (13) to account for the intermediate probabilities:

$$(14) p_i^{sibA} = 1 - ((1 - p_{INTi}^{sibA}) * (1 - p_{INTi}^{sibB}))$$

By construction, the probability of receiving an offer (p_i) is the same for each family member. Regardless of the cohort composition (number of offers, number of same- and

different-grade siblings, and so on), the construction of base weights and application of normalization factors (NF) follows the same procedures described earlier (Equations [3–6]).

We constructed parent weights using the student’s probability of receiving an offer, which is constant within a family. We then created base weights using Equations (3) and (4). For parents, the normalization factors are designed such that the weights of each experimental group sum to one-half the number of parent applicants in the year. This contrasts with student normalization factors in that the factors are yearly, rather than grade-by-year.

Constructing probability estimates

The literature search used to assess the likelihood of a truly positive effect of Democracy Prep on the student population included studies estimating the impacts of education on students’ registration and/or voting. We combined point estimates and standard errors into a single data set, then transformed them to generate effect sizes and standard errors of the effect sizes, respectively. Due to variation in the statistics reported, we conducted this transformation by dividing point estimates and standard errors by the square root of the variance of the outcome.

Notation

As shown in Chapter III, Figure III.2, a given *intervention* (such as Democracy Prep) can have multiple impact *estimates* (such as an estimate of the effect on registration and an estimate of the effect on voting). We use j to index the estimates (with $j = 1, \dots, J = 29$ denoting published estimates and $J + 1 = 30$ and $J + 2 = 31$ denoting estimates of the impact of Democracy Prep on registration and voting, respectively). We use k to index the interventions (with $k = 1, \dots, K = 8$ denoting interventions studied in the published literature and $K + 1 = 9$ denoting the Democracy Prep intervention).

$$\begin{aligned} \theta_{J+1} &= \text{the impact of Democracy Prep on registration} \\ \theta_{J+2} &= \text{the impact of Democracy Prep on voting} \\ \theta_1, \dots, \theta_J &= \text{the true effects that published studies sought to estimate} \\ \hat{\theta}_1, \dots, \hat{\theta}_{J+2} &= \text{estimates} \\ s_1, \dots, s_{J+2} &= \text{their standard errors} \end{aligned}$$

Main approach

Almost all of the published estimates are positive. This could be at least partly due to publication bias, so-called p -hacking, and/or the garden of forking paths, whereby researchers—consciously or not—tend to present the most favorable of a large number of possible results from any given analysis (Gelman and Loken 2013). To prevent these biases from propagating through to the current analysis of Democracy Prep, our main approach presumes that the prior estimates are exaggerated by a factor of two, on average (Gelman 2014). The prior mean is thus taken to be $\hat{\mu}/2$, with μ equal to the mean impact across prior interventions. (We estimate $\hat{\mu}$ as the posterior mean of μ under the model used for sensitivity analysis 2, which takes the prior studies at face value.) The main model is given by:

$$(15) \text{ Likelihood: } \hat{\theta}_j | \theta_j \sim N(\theta_j, s_j^2)$$

$$(16) \text{ Prior: } \theta_j \sim N(a_{k[j]}, \sigma^2)$$

$$(17) \text{ Hyper-prior: } a_k \sim N(\hat{\mu}/2, \tau^2)$$

The first equation is the likelihood, which states that each impact estimate has a normal sampling distribution with mean equal to the true, unknown effect and variance equal to the squared standard error. The second equation is the prior, which describes the distribution of impacts across the outcomes affected by an intervention. (In the case of Democracy Prep, for example, this would be the distribution of true effects across two outcomes—voting and registration.) This distribution is assumed to be normal with an intervention-specific mean a_k and variance σ^2 . The last equation, often called a hyper-prior, is the distribution of the intervention-specific mean impacts a across the population of evaluated interventions. We adjust for our assumption that the prior estimates are exaggerated by a factor of two, on average, by centering the hyper-prior on $\hat{\mu}/2$. The variance of the intervention-specific mean impacts is given by τ^2 .

Sensitivity analyses

Of course, we do not really know what the adjustment for issues such as publication bias should be. We therefore try two alternative approaches to determine whether our conclusions are robust. In each sensitivity analysis, we consider an alternative hyper-prior. We maintain the same likelihood and prior throughout.

1. Our first sensitivity analysis corrects for issues such as publication bias more stringently than our main approach by presuming that, on average, impacts in this set of interventions are zero.

$$(18) a_k \sim N(0, \tau^2)$$

2. Our second sensitivity analysis takes the prior studies at face value, presuming there is no upward bias in the past results.

$$(19) a_k \sim N(\mu, \tau^2)$$

Fitting the models

We assume flat (uniform) priors for μ , β , σ and τ . This implies that we are estimating these parameters based only on data from the current literature review, rather than bringing in external information, such as how program impacts vary with program lengths from previous reviews of the literature.

We cut feedback from $\{\hat{\theta}_{J+1}, s_{J+1}, \hat{\theta}_{J+2}, s_{J+w}\}$ to $\{\sigma, \tau, \mu, \beta\}$ (Rougier 2008), because Democracy Prep is not exchangeable with the prior studies. This implies that we based our estimates of those four parameters only on information from the prior studies and not on information from our analysis of Democracy Prep.

We fit the models using a Gibbs sampler coded in the statistical programming language R, as described in Gelman et al. (2013). We use the monitor function from R's rstan package (Stan Development Team 2016) to validate Gibbs sampler performance.

APPENDIX B:
SUPPLEMENTAL TABLES

Table B.1. Student sample

Year	Grade	Offered	Not offered	Total	Percentage offered
2007–2008	Grade 6	95	201	296	32.1
2007–2008	Grade 7	13	88	101	12.9
2008–2009	Grade 6	92	118	210	43.8
2008–2009	Grade 7	23	83	106	21.7
2008–2009	Grade 8	5	2	7	71.4
2009–2010	Grade 6	57	163	220	25.9
2010–2011	Grade 6	24	13	37	64.9
2012–2013	Grade 8	14	11	25	56.0
2012–2013	Grade 9	28	3	31	90.3
2012–2013	Grade 10	16	4	20	80.0
2015–2016	Grade 11	5	2	7	71.4
Total		372	688	1,060	35.1

Table B.2. Student entry grades for lottery winners

Grade	Number	Percentage
Grade 6	268	72.0
Grade 7	36	9.7
Grade 8	19	5.1
Grade 9	28	7.5
Grade 10	16	4.3
Grade 11	5	1.3
Total	372	100.0

Table B.3. Additional student baseline equivalence

Characteristic (percentage unless otherwise indicated)	Number	Offer	No offer	Difference
Math z-score	804	-0.363	-0.349	-0.014
Reading z-score	791	-0.189	-0.252	0.063
English-language learner	849	7.6	7.5	0.1
Special education status	849	15.3	18.7	-3.4
Free or reduced-priced lunch eligibility	876	83.8	79.7	4.1

Note: Additional demographic data are available for only a subset of students in the analysis sample.

* $p < 0.05$, ** $p < 0.01$.

Table B.4. Parent sample

Year	Offered	Not offered	Total	Percentage offered
2007–2008	103	281	384	26.8
2008–2009	113	196	309	36.6
2009–2010	66	184	250	26.4
2010–2011	141	83	224	62.9
2012–2013	1,156	617	1,773	65.2
2013–2014	299	57	356	84.0
2014–2015	349	150	499	69.9
2015–2016	805	1,192	1,997	40.3
Total	3,032	2,760	5,792	52.3

Table B.5. Student ITT model results

	Registered	Voted
Democracy Prep offer	0.063 (0.035)	0.062* (0.029)
Age at election	0.010 (0.026)	-0.006 (0.022)
Female	0.116** (0.036)	0.127** (0.030)
Gender missing	0.161 (0.096)	0.123 (0.079)
Parent registered to vote before September 1 of lottery application year	0.007 (0.036)	0.008 (0.030)
Number	1,060	1,060

Note: Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$.

ITT = intent-to-treat.

Table B.6. Student TOT model results

	First stage (treatment–control difference in enrollment rate)	Reduced form (ITT impact)	2SLS (TOT impact)
Registered before 2016 election	0.258** (0.033)	0.063 (0.035)	0.244 (0.140)
Voted in any 2016 election	0.258** (0.033)	0.062* (0.029)	0.239* (0.119)
Number	1,060	1,060	1,060

Note: Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$.

2SLS = two-stage least squares; ITT = intent-to-treat; TOT = treatment-on-the-treated.

Table B.7. Student enrollment impacts (Bayesian estimates)

	Registration	Voting
Main approach	0.156 (0.081)	0.125 (0.064)
Sensitivity analysis 1	0.160 (0.088)	0.128 (0.070)
Sensitivity analysis 2	0.173 (0.079)	0.138 (0.063)

Note: Standard errors in parentheses.

Table B.8. Parent ITT model results

	Registered	Voted	
	2016	2014	2016
Democracy Prep offer	-0.009 (0.008)	0.019 (0.014)	0.005 (0.014)
Female	-0.003 (0.015)	-0.030 (0.025)	0.028 (0.025)
Gender missing	-0.006 (0.017)	-0.023 (0.028)	0.042 (0.026)
Registered to vote before September 1 of application year	0.854** (0.010)	0.233** (0.013)	0.578** (0.013)
Number	5,792	3,296	5,792

Note: Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$.

ITT = intent-to-treat.

Table B.9. Parent TOT model results

	First stage (treatment–control difference in enrollment rate)	Reduced form (ITT impact)	2SLS (TOT impact)
Registered before 2016 election	0.225** (0.011)	-0.009 (0.008)	-0.040 (0.036)
Voted in any 2014 election	0.225** (0.011)	0.019 (0.014)	0.070 (0.055)
Voted in any 2016 election	0.225** (0.011)	0.005 (0.014)	0.024 (0.060)
Number	3,296-5,792	3,296-5,792	3,296-5,792

Note: Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$.

2SLS = two-stage least squares; ITT = intent-to-treat; TOT = treatment-on-the-treated.

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