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) ,

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 ; *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[[1]](#footnote-1) 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) ,

where reflects the ratio of the covariance between treatment status () and an indicator for either registration or election participation () to the covariance between treatment status and enrollment status ().

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 () divided by the total number of lottery applicants (). That is, the probability of an offer for student is . Because we have multiple cohorts (strata), the probability of an offer for student in stratum is where is the treatment group size within the stratum and is the size of the stratum. The base weights are the inverse of the probability of being in the student’s treatment condition.

(3)

(4)

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 () 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)

(6)

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)

(8)

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 (). We then use the probability and the number of same-grade sibling pairs () to determine the estimated slots () occupied by same-grade sibling pairs.

(9)

(10)

(11)

(12)

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)

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 ( 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)

By construction, the probability of receiving an offer () 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 () 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 to index the estimates (with denoting published estimates and denoting estimates of the impact of Democracy Prep on registration and voting, respectively). We use to index the interventions (with denoting interventions studied in the published literature and denoting the Democracy Prep intervention).

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 , with equal to the mean impact across prior interventions. (We estimate as the posterior mean of under the model used for sensitivity analysis 2, which takes the prior studies at face value.) The main model is given by:

(15)

(16)

(17)

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 and variance . The last equation, often called a hyper-prior, is the distribution of the intervention-specific mean impacts 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 . The variance of the intervention-specific mean impacts is given by .

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)

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

(19)

Fitting the models

We assume flat (uniform) priors for and . 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 to (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.](http://www.stat.columbia.edu/~gelman/book/) (2013). We use the monitor () function from R’s rstan package (Stan Development Team 2016) to validate Gibbs sampler performance.

Appendix B:
Supplemental tables: effects on students

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. 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.3. 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.4. 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. Model includes weights, baseline covariates, and lottery fixed effects.

\* *p* < 0.05, \*\* *p* < 0.01.

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

Table B.5. Student TOT model results, without baseline covariates

|  |  |  |  |
| --- | --- | --- | --- |
|  | First stage(treatment–control difference in enrollment rate) | Reduced form(ITT impact) | 2SLS(TOT impact) |
| Registered before 2016 election | 0.259\*\*(0.032) | 0.072\*(0.035) | 0.277\*(0.140) |
| Voted in any 2016 election | 0.259\*\*(0.032) | 0.072\*(0.029) | 0.280\*(0.123) |
| **Number** | **1,060** | **1,060** | **1,060** |

Note: Standard errors in parentheses. Model includes weights and lottery fixed effects.

\* *p* < 0.05, \*\* *p* < 0.01.

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

Table B.6. 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.

Appendix C:
Effects on Parents

Description of parent analysis

Democracy Prep has tried to promote the civic participation of parents 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.

The parent sample includes parents of the eligible students described in the main text, 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 children’s application year. As shown in Table C.1, the analytic sample includes 5,792 parents, 52 percent of whom had children offered admission through the lottery.

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 C.2 indicates, we find no statistically significant differences in baseline characteristics in the parent sample. Parents in the treatment group (received an offer) are approximately 22 percentage points more likely to have a child who enrolls in Democracy Prep (Table C.3).

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 (Table C.4). 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 (Table C.5). In sum, we find no evidence that Democracy Prep increases the registration and voting rates of students’ parents.

Table C.1. 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 C.2. Baseline equivalence, parent sample

|  |  |
| --- | --- |
| Characteristic (percentage unless otherwise indicated) |  |
| **Offer** | **No offer** | **Difference** |
| Age at 2016 election |  |  |  |
| 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 lottery application yeara | 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

Table C.3. Enrollment rates by treatment status, parent sample

|  |  |  |  |
| --- | --- | --- | --- |
| Enrollment rate | Offer | No offer | Difference |
| 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

Table C.4. 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 C.5. 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 | 0.265\*\*(0.016) | 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. Model includes weights, baseline covariates, and lottery fixed effects.

\* *p* < 0.05, \*\* *p* < 0.01.

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

Table C.6. Parent TOT model results, without baseline covariates

|  |  |  |  |
| --- | --- | --- | --- |
|  | First stage(treatment–control difference in enrollment rate) | Reduced form(ITT impact) | 2SLS(TOT impact) |
| Registered before 2016 election | 0.225\*\*(0.011) | -0.012(0.015) | -0.054(0.068) |
| Voted in any 2014 election | 0.264\*\*(0.016) | 0.013(0.015) | 0.048(0.057) |
| Voted in any 2016 election | 0.225\*\*(0.011) | 0.004(0.016) | 0.017(0.071) |
| **Number** | **3,296-5,792** | **3,296-5,792** | **3,296-5,792** |

Note: Standard errors in parentheses. Model includes weights and lottery fixed effects.

\* *p* < 0.05, \*\* *p* < 0.01.

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

1. While we prefer the models with baseline covariates, we also provide results for models that exclude baseline covariates. As shown in Appendices B and C, results are similar across both specifications. [↑](#footnote-ref-1)