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The Long-Term Effects of Universal Preschool in Boston

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ABSTRACT

We use admissions lotteries to estimate the effects of large-scale public preschool in Boston on college-going, college preparation, standardized test scores, and behavioral outcomes. Preschool enrollment boosts college attendance, as well as SAT test-taking and high school graduation. Preschool also decreases several disciplinary measures including juvenile incarceration, but has no detectable impact on state achievement test scores. An analysis of subgroups shows that effects on college enrollment, SAT-taking, and disciplinary outcomes are larger for boys than for girls. Our findings illustrate possibilities for large-scale modern, public preschool and highlight the importance of measuring long-term and non-test score outcomes in evaluating the effectiveness of education programs.

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1 Introduction

A substantial body of evidence establishes that early-life deficits have persistent negative impacts on lifetime well-being (see, e.g., Knudsen et al., 2006; Almond et al., 2018). High-quality early childhood interventions are seen as a promising tool to address such deficits, improve economic outcomes, and reduce socioeconomic disparities (Duncan and Magnuson, 2013; Heckman, 2013; Elango et al., 2016; Chaudry et al., 2017). Contemporary policy efforts in the United States focus on expanding public preschool programs, many funded by state and local governments. The share of US four-year-olds enrolled in state-funded preschool grew from 14% in 2002 to 34% in 2019.[1] By 2019, 44 states and 24 of the 40 largest US cities operated large-scale public preschool programs, and nearly half of four-year-olds attended some form of publicly-funded preschool (NIEER, 2003; 2019a; 2019b). Recent proposals at the federal, state, and local levels aim to continue this rapid expansion (Obama, 2013; Biden, 2021).[2]

Enthusiasm for public preschool derives in part from encouraging experimental evidence produced by small-scale demonstration programs in the 1960s and 1970s. The High/Scope Perry Preschool Project and Carolina Abecedarian Project randomly assigned small numbers of children to intensive preschool programs or to control groups without program access. Comparisons between the treatment and control groups show that the Perry and Abecedarian interventions improved short-term test scores and behavior as well as long-term outcomes such as educational attainment, crime, and earnings (Campbell and Ramey, 1994; Schweinhart et al., 2005; Campbell et al., 2012; Heckman et al., 2013; Garcia et al., 2020). Cost/benefit analyses suggest that these interventions are among the most cost-effective social programs on record (Barnett, 1985; Belfield et al., 2006; Heckman et al., 2010b; Hendren and Sprung-Keyser, 2020).

These promising results notwithstanding, it is unclear what lessons small-scale experiments from decades ago offer for more recent public preschool expansions. Evidence from larger-scale programs is mixed. Non-experimental studies of the federal Head Start program find initial test score gains that subsequently fade out, but positive effects often reappear for long-term outcomes (Currie and Thomas, 1995; Garces et al., 2002; Ludwig and Miller, 2007; Deming, 2009; Miller et al., 2019; Bailey et al., 2020; Pages et al., 2020). This pattern may be due to persistent impacts operating

[1] For comparison, 20% of four-year-olds attended a private preschool and 7% enrolled in the federal Head Start program in 2019 (NIEER 2019b).

[2] President Biden's proposed American Families Plan would provide free universal preschool for all three- and four-year-olds. 2020 ballot initiatives in Portland, St. Louis, San Antonio, and Colorado proposed to expand public preschool.

through non-cognitive channels (Heckman et al., 2013) or to biases in non-experimental research strategies (Hoxby, 2014). Two randomized trials evaluating Head Start and Tennessee's Voluntary Pre-Kindergarten Program find modest positive test score impacts that fade out by elementary school (Puma et al., 2010, 2012; Lipsey et al., 2018). Some analysts interpret these findings as reflecting ineffective programs, while others argue that medium-term test scores are a poor measure of program effectiveness (Mongeau, 2015; Bailey et al., 2017; Heckman, 2017; Whitehurst, 2017). These disagreements may stem from the fact that no study to date has used a randomized research design to study the long-term effects of a large-scale preschool program.[3]

We fill this gap by using a lottery-based research design to estimate the impacts of large-scale public preschool in Boston, Massachusetts on long-term post-secondary educational outcomes. Our approach compares students who were randomly lotteried in or out of public preschool as a result of tie-breaking embedded in Boston's school assignment mechanism. This analysis builds on earlier work based on tie-breaking in centralized assignment systems (Abdulkadiroğlu et al., 2011, 2017) as well as previous studies looking at short-term impacts of preschool in Boston (Weiland and Yoshikawa, 2013; Weiland et al., 2019).

We estimate causal effects of public preschool on college enrollment and persistence, grade progression and high school graduation, SAT and state achievement test scores, and behavioral outcomes related to truancy, suspension, and juvenile incarceration. Our study considers more than 4,000 randomized four-year-old applicants covering seven admissions cohorts from 1997-2003. We measure post-secondary outcomes from a special extract of the National Student Clearinghouse, covering roughly 99% of applicants. The lottery-based research design, together with high follow-up rates for long-term outcomes covering roughly 20 years following preschool enrollment, enable us to surmount many empirical challenges plaguing previous studies of early-childhood interventions.

Our analysis shows that preschool enrollment improves post-secondary outcomes. Attendance at a public preschool in Boston boosts on-time college enrollment by 8 percentage points, an 18% increase relative to the baseline college-going rate of 46%. Children who randomly win a seat at a Boston preschool are 5.5 percentage points more likely to attend a four-year college by the fall after projected high school graduation and 8.5 percentage points more likely to attend a Massachusetts college. Preschool enrollment leads to a 5.4 percentage point increase in the probability of ever enrolling in college and a 5.9 percentage point gain in the likelihood of ever attending a four-year

[3] In a review of the US early-childhood educational literature, Cascio (2021) concludes that there is no long-term evidence on the impacts of large-scale preschool programs from randomized social experiments.

college. Estimates for college graduation are also positive, though these results are less precise since some cohorts are too young to observe graduation outcomes.

To probe mechanisms for these results, we also study outcomes on the pathway to college. We find positive effects on several college preparatory outcomes. Preschool enrollment boosts the likelihood of graduating from high school by 6 percentage points. Preschool also causes a 9 percentage point increase in SAT test-taking and raises the probabilities of scoring above the bottom quartile and in the top quartile of the SAT distribution.

We measure effects on academic achievement using scores on Massachusetts Comprehensive Assessment System (MCAS) tests, and study impacts on student behavior by looking at suspensions, attendance, and juvenile incarceration. We find no evidence of impacts on student achievement in elementary, middle, or high school: estimated effects on MCAS scores in grades 3 through 10 are uniformly small and statistically insignificant. In contrast, we find significant effects on disciplinary outcomes. Preschool attendance reduces the frequency of suspensions and the probability that students are incarcerated while in high school. Aggregating several measures into a summary index, we find that preschool enrollment improves high school disciplinary outcomes by 0.17 standard deviations (σ) on average.

Studies of model early-childhood demonstration programs, Head Start, and state-funded preschool programs emphasize heterogeneity in impacts by sex, race, and income (Garces et al., 2002; Gormley et al., 2005; Anderson, 2008; Heckman et al., 2010a; Cascio, forthcoming). We therefore examine variation in the impacts of Boston's preschool program on these dimensions. The causal effects of preschool are generally larger for boys than for girls. Enrollment in a Boston preschool increases on-time college enrollment for both sexes, but effects on college graduation, high school graduation, SAT-taking, and the discipline index are positive and significant for boys and insignificant for girls. Differences in estimates by race and income are generally statistically insignificant.

Our analysis makes two main contributions to the literature. First, we present the first evidence from a randomized research design on the long-term effects of a large-scale preschool program. Previous randomized studies typically look at small-scale programs (Campbell and Ramey, 1994; Schweinhart et al., 2005) or are limited to short-term outcomes (Puma et al., 2010, 2012; Bitler et al., 2014; Walters, 2015; Kline and Walters, 2016; Feller et al., 2016; Lipsey et al., 2018; Weiland et al., 2019). Other studies look at large-scale programs using observational research designs (Garces et al., 2002; Gormley et al., 2005; Ludwig and Miller, 2007; Fitzpatrick, 2008; Wong et al., 2008; Deming, 2009; Carneiro and Ginja, 2014; Thompson, 2018; Johnson and Jackson, 2019; Bailey et al., 2020; De Haan and Leuven, 2020; Pages et al., 2020; Cascio, forthcoming).[4] Studying long-term impacts requires data following students over a long time horizon, which is rare among modern publicly-funded preschool programs. Boston operated an unusually large public preschool program by the late 1990s and allocated seats with a centralized mechanism, allowing us to study program impacts over multiple decades with a randomized design. The program is run by the Boston Public Schools district, so our results are relevant for evaluating expansions of preschool provided by state and local governments (Muralidharan and Niehaus, 2017). Our positive estimates for educational attainment are similar to those from model demonstration programs and non-experimental studies of Head Start, illustrating the potential for modern public preschool programs to improve long-term outcomes.

Second, our findings inform the debate regarding the link between short-term and long-term effects of education programs. Evaluations of new programs require assumptions to forecast long-term impacts using short-term data (Kline and Walters, 2016; Athey et al., 2019).[5] Previous evidence suggests that immediate test score gains may be a more reliable indicator of long-term effects than later test scores, and that non-cognitive outcomes are an important mediator of long-term impacts (Chetty et al., 2011; Heckman et al., 2013; Chetty et al., 2014). Our results corroborate these ideas by showing positive long-term impacts for an intervention that improves adolescent behavioral outcomes, but not test scores. Analyses of recent cohorts in the same Boston program studied here find initial test score gains during preschool that are no longer detectable in elementary school, a result that is consistent with our MCAS estimates for older cohorts (Weiland and Yoshikawa, 2013; Weiland et al., 2019). Our findings suggest that this pattern masks persistent effects on skill formation that ultimately result in higher educational attainment. More generally, our results highlight the importance of considering non-test score and long-term outcomes when assessing the effectiveness of education programs.

The rest of this paper is organized as follows. The next section provides background on Boston's preschool program and describes the data used to evaluate it. Section 3 outlines our empirical approach and conducts validity checks on our research design. Section 4 reports lottery-based estimates of preschool effects on post-secondary outcomes. Section 5 details results for grade progression, high school graduation, and SAT scores. Section 6 reports effects on MCAS test scores

[4] Related studies outside the US include Baker et al. (2008, 2019), Havnes and Mogstad (2011, 2015), Gertler et al. (2014), Cornelissen et al. (2018), and Felfe and Lalive (2018).

[5] Deming (2009) writes: "... without some sense of the connection between short-run and long-run, researchers must wait at least 15-20 years to evaluate the effect of early childhood programs."

and disciplinary outcomes. Section 7 investigates heterogeneity across subgroups and compares our estimates to results from related studies in the literature. The last section concludes.

2 Background and Data

2.1 Public Preschool in Boston

Boston Public Schools (BPS) operates separate kindergarten programs across grade levels K0 (three-year-olds), K1 (four-year-olds), and K2 (five-year-olds). Grade K0 programs enroll a small number of special needs students, and grade K2 corresponds to traditional kindergarten. We focus on K1 programs because they enroll four-year-olds, a common entry point for public preschool. Much of the growth in US public preschool enrollment in recent years has also come from expansions of programs for four-year-olds (NIEER, 2019b).

Public preschool in Boston is universal in the sense that the program is open to all children residing in Boston, regardless of income. As we show later, in practice the program enrolls a relatively disadvantaged student population with high shares of non-white and low-income students. Programs are housed in public school facilities, including elementary schools, early learning centers, and special school facilities covering early grades. BPS preschools are staffed by teachers who hold either bachelor's or master's degrees and must complete the same certification requirements as BPS teachers in higher grades. In a survey of 43 randomly-selected K1 classrooms during the 2005-2006 school year, Marshall et al. (2006) found that all K1 teachers held bachelor's degrees and 56% held master's degrees. On average, K1 teachers had 8 years of experience in BPS and 6 years at their current school. More than half of the teachers were non-Hispanic white, 11% were Hispanic or Latino, 10% were Black, and 6% described themselves as biracial.

During the time period of our study (1997-2003), BPS operated under an "autonomous district model" giving school principals freedom to hire teachers and choose curricula. Many programs used the Harcourt Trophies curriculum and later switched to Opening World of Learning (OWL) and Building Blocks (Schickendanz and Dickinson, 2005; Clements and Sarama, 2007).[6] Class sizes ranged from 10 to 25 students, with an average of 19 (Marshall et al., 2006). BPS Children's First estimates that the program costs roughly $13,000 (2020 dollars) for full-day preschool and about half as much for half-day programs (DESE, 1995). BPS preschool scores highly on observed metrics

[6] Marshall et al. (2006) report that in 2005, 60% of K1 classrooms used OWL, 40% used Building Blocks, 20% used Harcourt Trophies/Reading First, 20% used a self-developed curriculum based on best practices in the field, 12% used TERK Investigations, and 8% used Readers and Writers Workshop.

of program quality, receiving 8 out of 10 benchmarks and ranking 6th out of 40 city-wide programs in a recent NIEER report (NIEER, 2019a). Weiland and Yoshikawa (2013) describe more details on the curriculum and program implementation in Boston, focusing on the period just after our last application cohort.

Boston's preschool offerings expanded after a period of initial uncertainty. As of 1997, most K1 programs were half-day, with students attending preschool in either the morning or the afternoon for two and half hours. In 1997, the Boston school committee decided to partially phase out half-day K1 programs in favor of offering full-day, six-hour kindergarten for all five-year-olds (BPS, 1997; 1998). As a result, the number of K1 seats declined from roughly 2,500 to 900 and the number of programs dropped from about 60 to 40 between 1997 and 1998 (Figure 1).

In 1998, Boston opened three new Early Education Centers offering full-day programs. The district also opened five additional full-day programs over the next few years, resulting in a mix of full-day and half-day K1 programs. In 2005, Boston mayor Thomas Menino proposed expanding the supply of K1 seats citywide and created a dedicated Department of Early Childhood (Sachs and Weiland, 2010). Figure 1 shows that Boston preschool subsequently grew to about 2,500 K1 students per year. Recent administrations have attempted to provide enough capacity for all of Boston's four-year-olds, but as of 2019 there was only enough capacity to serve roughly half of Boston's four-year old students (Martin, 2021). The rationing of BPS preschool seats is a key element of our research design.

2.2 Data and Sample

The Boston Public Schools district provided data covering all preschool applicants from fall 1997 to fall 2003. The application files contain demographics, address information, and the inputs used to implement the school assignment algorithm (described further below), including students' rank-ordered choices over schools, admission priorities, and random tie-breaking numbers. BPS also provided a second post-application file recording school assignments, BPS preschool enrollment, and applicant names and dates of birth, which we link to the application file using a unique BPS identifier.

We measure outcomes for BPS preschool applicants by matching the applicant records to several additional data sources. The primary outcomes for our study are measures of college attendance, college type, and college graduation derived from a special National Student Clearinghouse (NSC) data extract. We submitted names and dates of birth for BPS applicants for matching to the

NSC in Spring 2020. Dynarski et al. (2015) reports that the NSC covered more than 90% of US undergraduate institutions as of 2011, the earliest year of college enrollment for our cohorts, and 95% of Massachusetts undergraduate institutions.

We use the NSC records to construct two sets of post-secondary outcomes distinguished by the timing of measurement. The "on-time" concept refers to whether a student achieves an outcome within a data window that assumes normal academic progress from his or her initial application. For example, a student who applied to preschool in fall 1997 would finish 12th grade on-time in spring 2011, enroll in college on-time by fall 2011, and graduate from a four-year college on-time by summer 2015. The "ever" concept records the same outcomes with no restrictions on the follow-up window, which allows us to capture late enrollment but implies a shorter data window for more recent applicant cohorts. Table A1 summarizes the data windows available to measure outcomes for each cohort. Since students applying for preschool in 2002 and 2003 would not finish a four-year college on time until after our NSC search, we do not observe college graduation outcomes for these cohorts.

Outcomes prior to college enrollment are measured by linking preschool applicants to administrative data from the Massachusetts Department of Elementary and Secondary Education (DESE). This database contains school enrollment records, demographics, and Massachusetts Comprehensive Assessment System (MCAS) test scores in grades 3-8 and 10 for students enrolled at Massachusetts public schools. The DESE data also record disciplinary outcomes including suspensions, truancy, and codes for students in juvenile incarceration, as well as SAT scores and high school graduation for Massachusetts public high schools. Appendix A provides further information on the procedures used to clean and link data sets and construct outcomes.

Table 1 summarizes the characteristics of our sample of BPS preschool applicants. Column (1) displays statistics for the sample of 8,786 first-time BPS preschool applicants who applied for a K1 slot between 1997 and 2003. As shown in Panel A, nearly three-quarters of preschool applicants are Black or Hispanic, 10% are classified as bilingual Spanish, and the typical applicant is 4.6 years old at the time of potential preschool enrollment. On average, students rank three schools on their application forms (Panel B, column (1)). Panel C displays neighborhood characteristics measured by matching an applicant's geographic information to block groups in the 2010 US Census. The average applicant lives in a neighborhood with a median family income of $54,000 and a poverty rate of 23%.

As discussed in the next section, our analysis focuses on applicants subject to random assignment, defined as those whose preschool offers are determined by a random tie-breaker. Column (2) of Table 1 shows descriptive statistics for the randomized subsample. In total, 4,215 applicants are subject to random assignment. Characteristics of randomized applicants are generally similar to those of the full applicant population.

3 Empirical Framework

3.1 Research Design

Our research design relies on random tie-breaking within Boston's centralized school assignment mechanism. Households applying to BPS preschools submit rank-ordered lists of preferences for preschool programs to the district. Applicants receive priorities at each program based on sibling status and geographic proximity (those within a program's "walk-zone" receive higher priority). Within priority groups, tie-breaking is based on a random number assigned by the district. The mechanism combines preferences, priorities, and random tie-breakers to output a single assignment for each applicant, which is either a specific BPS preschool program or no program. During our study period, the city used the immediate acceptance (or "Boston") mechanism to determine assignments (Abdulkadiroğlu and Sönmez, 2003).

Differences in assignments between students with the same preferences and priorities arise solely because of the random tie-breaking number. Few students share all the same preferences and priorities, but in practice the probability of an offer depends on a coarser set of school-level cutoffs. This motivates a strategy of controlling for the *assignment propensity score*, defined as the conditional probability of a preschool offer given an applicant's preferences and priorities (Abdulkadiroğlu et al., 2017). The propensity score theorem of Rosenbaum and Rubin (1983) implies that if preschool offers are random (independent of potential outcomes) conditional on preferences and priorities, then offers are also random conditional on the assignment propensity score. A special feature of the centralized school assignment setting is that the propensity score can be calculated with knowledge of preferences, priorities, and the structure of the assignment algorithm.[7] We compute the assignment propensity score using an analytic large-market approximation derived by Abdulkadiroğlu et al. (2017).[8] Our data include the random tie-breaker, so we code preschool offers based on whether a

[7]We compute the probability of assignment to any preschool by summing the propensity score associated with an offer at each ranked preschool program. This method allows us to isolate all randomly generated offers from the assignment mechanism and extract a greater number of applicants subject to random assignment than approaches that only consider first choices (c.f., Abdulkadiroğlu et al., 2011; Weiland et al., 2019).

[8]Abdulkadiroğlu et al. (2017) derive the propensity score for the deferred acceptance (DA) algorithm. Appendix

student's random number fell below the relevant cutoff.[9]

We use preschool assignment as an instrument for preschool enrollment, controlling for the assignment propensity score. The primary estimating equations for our analysis are:

$$Y_i = \beta D_i + \sum_p \alpha_p 1\{P_i = p\} + X_i'\gamma + \epsilon_i, \tag{1}$$

$$D_i = \pi Z_i + \sum_p \delta_p 1\{P_i = p\} + X_i'\lambda + \eta_i, \tag{2}$$

where Y_i is an outcome for student i, D_i indicates BPS preschool attendance, and Z_i indicates an offer to any BPS preschool program. Both equations include a saturated set of indicators for values of the propensity score P_i, which measures the probability of an offer to any Boston preschool (computed by summing the propensity scores for each individual program). We refer to the propensity score indicators as "risk" controls. Baseline covariates X_i include race, sex, and bilingual Spanish indicators. The parameter of interest is β, which represents the causal effect of public preschool attendance.

We estimate equations (1) and (2) by two-stage least squares (2SLS) in the sample of randomized applicants (those with values of P_i strictly between zero and one). The first stage fits equation (2) by ordinary least squares (OLS) and constructs predicted values $\hat{D}_i$. The second stage fits equation (1) by OLS after substituting $\hat{D}_i$ for D_i. The resulting estimate of β is interpretable as a weighted average of local average treatment effects (LATEs) for "compliers" induced to attend BPS preschools by random offers (Imbens and Angrist, 1994; Angrist et al., 1996).

Kline and Walters (2016) show that the LATE associated with public preschool attendance captures a treatment effect relative to a mix of counterfactual alternatives for compliers, which may include other preschools. While we do not observe data on alternative programs, Weiland et al. (2019) report that a large share of students lotteried out of Boston preschools in more recent years attend other center-based preschool programs (most commonly private preschools), so the counterfactual for our estimates likely includes other preschools as well. As Kline and Walters

A.10 of their paper shows that it is possible to construct the propensity score for the immediate acceptance algorithm by redefining priorities so that priority groups at a given school consist of applicants who share original priority status at the school and rank it the same way, then applying the formula for the DA propensity score.

[9] Appendix Table A2 shows that this coding replicates 94% of observed assignments. In 1997-1999, BPS used racial re-balancing to modify a small number of assignments after running the assignment algorithm, a practice that aimed to reduce segregation in Boston (Willie and Alves, 1996). These post-assignment moves drive the lower replication rates in 1997-1999, but do not contaminate our research design since our coding disregards rebalanced offers. Any differences between our coding of offers and final student assignments can be interpreted as non-compliance with the assignment algorithm.

(2016) note, the LATE represents the policy-relevant parameter for evaluating expansions of BPS preschool regardless of the mix of counterfactuals when alternative programs are not rationed. Our estimates therefore speak directly to policy debates regarding the expansion of public preschool in Boston and elsewhere.

3.2 Balance and Attrition

Before presenting our main estimates, we turn to two tests of the validity of our research design. Table 1 checks whether predetermined characteristics are balanced between offered and non-offered students, as would be expected under random assignment. Column (3) reports coefficients from OLS regressions of student characteristics on an offer indicator, controlling for cohort indicators but not adjusting for assignment risk. These contrasts show significant imbalances by Hispanic status, bilingual Spanish, and application and neighborhood characteristics, likely because students from different demographic groups and neighborhoods apply to different programs. Column (4) restricts the sample to randomized applicants and adds a saturated set of propensity score controls. Risk controls eliminate all of the statistically significant imbalances from column (3), illustrating the balancing properties of the assignment propensity score. By chance we find that females are more likely to receive offers conditional on risk. We account for this imbalance by controlling for a female indicator when estimating preschool effects.

Next, we investigate followup rates for our key outcomes. Even with random assignment of preschool slots, non-random attrition may compromise the comparability of lottery winners and losers, possibly generating selection bias. This has been a major concern in studies of preschool programs, where long time intervals between interventions and outcomes create the potential for substantial attrition (see, e.g., Armor, 2014; Elango et al., 2016). This possibility motivated us to conduct a custom search of NSC records for all preschool applicants based on the names and dates of birth provided by BPS.

Column (1) of Table 2 shows that information for roughly 99% of control group (non-offered) applicants was submitted to the NSC. This establishes that overall attrition for post-secondary outcomes in our study is very low. As shown in column (2) of Table 2, applicants who were offered a preschool seat were 0.8% more likely to be submitted to the NSC. This reflects a slight imbalance in the availability of names and dates of birth in the (post-treatment) data file we received from BPS, likely because missing information was updated for a few applicants who enrolled in preschool. With 4,215 randomized applicants and an offer rate of about one-third, the 0.8% gap corresponds to 11 extra offered students. Our impact estimates for post-secondary outcomes are unlikely to be affected by this small difference in follow-up.

To measure earlier outcomes in Massachusetts public schools, we link preschool applicants based on name and date of birth to records from the state's administrative database, known as the Student Information Management System (SIMS). About 91% of non-offered applicants are observed in the SIMS file, and applicants who receive offers are 2.8% more likely to be observed. This difference may reflect a causal impact of BPS preschool on the likelihood of attending a Massachusetts public school, perhaps because public preschool enrollment increases attachment to the public education system. Similarly, we are 3.8 percentage points more likely to observe a follow-up test score for students assigned to preschool, and we see an average of 0.5 more scores for the treatment group (out of a total of up to 14 math and reading scores in grades 3-8 and 10). As a result of these modest but significant differences in attrition, results for test score and behavioral outcomes derived from the Massachusetts administrative data should be interpreted with more caution than results for our primary post-secondary outcomes.

4 Effects on Post-Secondary Outcomes

Boston preschool attendance increases on-time college enrollment. We arrive at this result in Table 3, which reports 2SLS estimates of equations (1) and (2) for post-secondary outcomes. Column (2) reports estimates of the first-stage coefficient π, which show that a preschool offer increases the probability of preschool attendance by 65 percentage points. Column (3) displays the reduced form effect of an offer on the outcome, estimated by replacing D_i with Y_i on the left-hand side of equation (2). A preschool offer increases on-time college enrollment by 5.4 percentage points. Since the 2SLS model is just-identified, the 2SLS estimate in column (4) equals the ratio of the reduced form to the first stage, which reveals that enrollment at a Boston preschool increases on-time college enrollment by 8.3 percentage points. This estimate, which is statistically significant at the 1% level, implies an 18% increase in on-time college enrollment relative to the 46% rate for non-offered students (column (1)).

The bulk of this increase in college-going is driven by attendance at four-year and Massachusetts colleges. The second and third rows of column (4) in Table 3 suggest that preschool enrollment increases on-time enrollment in both two-year and four-year colleges, but the four-year estimate is larger (5.5 vs. 2.8 percentage points) and the two-year estimate is statistically insignificant.

Preschool enrollment increases the likelihood of on-time enrollment at a Massachusetts college by 8.5 percentage points ($p < 0.01$).[10] We find positive estimates for both public and private colleges, though only the private estimate is statistically significant for on-time enrollment.

The positive on-time enrollment effects of Boston preschools translate into positive effects on ever attending college. Columns (5)-(8) of Table 3 display results for post-secondary outcomes measured at any time (the "ever" outcome concept). The non-offered college attendance rate increases from 46% to 65% when we drop the on-time restriction, implying that many students enroll late. The 2SLS estimates show a slight convergence between treatment and control groups when we include late enrollment: the estimated effect on ever enrolling at any college equals 5.4 percentage points, which is marginally statistically significant ($p < 0.1$). We continue to see large estimated impacts on four-year enrollment (5.9 percentage points, $p < 0.1$) and enrollment at Massachusetts institutions (7.1 percentage points, $p < 0.05$). Estimates for both public and private institutions are positive, but the effect for public institutions is larger when we include late enrollment (5.1 percentage points, $p < 0.1$). Adding up the number of semesters enrolled across all institution types, we find that BPS preschool attendance increases postsecondary enrollment by 0.6 semesters, an 11% gain relative to the control mean of 5.6 semesters ($p < 0.1$).

The bottom rows of Table 3 display estimates for college graduation. Since we do not observe graduation outcomes for two out of seven applicant cohorts, the sample size for these outcomes falls by roughly 20%, and we have less statistical precision to detect effects. The estimated impact on ever graduating from any college in column (8) suggests that Boston preschool enrollment increases graduation by 5.2 percentage points. This is a quantitatively large estimate equal to 15% of the control graduation rate of 33%. Due to a lack of precision, however, we cannot rule out that this finding is due to chance.

Our results for postsecondary outcomes are robust to several reasonable changes in estimation strategy. The chance imbalance by sex shown in Table 1 motivates a sensitivity analysis that drops the control for female from equations (1) and (2). Appendix Table A3 shows that dropping this control leads to slightly larger estimates than those in Table 3 but does not change the key results. Similarly, Appendix Table A4 shows that our results are robust to dropping the 1997 applicant cohort, which attended BPS preschool prior to the restructuring discussed in Section 2.1. Finally, Appendix Table A5 reports an analysis that replaces the offer indicator Z_i with

[10] The most common colleges attended by BPS preschool students in our sample are Bunker Hill Community College, the University of Massachusetts campuses at Amherst, Boston, and Dartmouth, Newbury College, Framingham State, Cambridge College, Ben Franklin Institute of Technology, and the Urban College of Boston.

the randomly assigned tie-breaking number as the instrument in equation (2). Using the random number itself as the instrument reduces statistical power since this simpler strategy does not fully exploit the structure of the assignment mechanism, but the pattern of estimates and statistical significance is similar to our baseline results in Table 3. Estimates for college graduation are marginally statistically significant in specifications without the female control and using the random number as instrument.

Taken together, our results reveal a clear pattern of positive impacts of preschool attendance on post-secondary educational outcomes for students in Boston. These findings are noteworthy in light of large gaps in graduation rates and time to degree by race and income (Bowen and Bok, 2000). Our results show that BPS preschool boosts post-secondary education for a population with high shares of minority and low-income children: 72% are Black or Hispanic, and more than two-thirds are eligible for a free or reduced price lunch (a proxy for low family income).[11] We next turn to an analysis of outcomes prior to college to investigate the channels driving these results.

5 Effects on College Preparation

5.1 Grade Progression, Special Education, and High School Graduation

Studies of preschool often consider outcomes related to grade retention and special education status (Gramlich, 1986; Currie and Thomas, 1995; Currie, 2001; Magnuson et al., 2004; Deming, 2009; Miller and Bassok, 2017). Preschool may ease the transition to elementary school and reduce the need for remediation and special education services (Bailey et al., 2017). Currie (2001) emphasizes that prevention of special education and avoidance of grade retention is a potential cost savings created by preschool programs. Furthermore, special education classification and grade progression outcomes may contain information on skills and behaviors that are not captured by test scores.

We find no detectable impacts of BPS preschool on grade repetition and special education outcomes. The first row of Table 4 displays a 2SLS estimate of the effect of BPS preschool on starting 1st grade on time. The sample for this outcome is limited to those who are observed in 1st grade in a Massachusetts public school at some point. Eighty-seven percent of non-offered students start 1st grade on time, and the estimated effect of BPS preschool is small and statistically insignificant. Similarly, we find no effect on the probability of appearing in a BPS school in 6th or

[11] The fraction of students eligible for a subsidized lunch is calculated based on free or reduced price lunch status in the first year a student appears in the SIMS database using students who appear in the SIMS data in at least one year.

9th grade, and a small negative but statistically insignificant effect on repeating a grade (defined as appearing in the same grade in more than one year). The samples for these outcomes are restricted to students who appear in a Massachusetts public school in at least one year. The bottom rows of Panel A in Table 4 show small and insignificant estimates of effects on special education classification in 1st and 3rd grades for students observed in these grades.

In contrast, Panel B of Table 4 reveals that preschool attendance boosts high school graduation. Enrollment in a BPS preschool increases the probability that students graduate from a Massachusetts public high school on time by 5.4 percentage points ($p < 0.1$), and this effect grows to 6.0 percentage points when we include students who graduate at any time ($p < 0.05$). The estimated effect on ever graduating high school is a 9% increase relative to the non-offered graduation rate of 64%. Combined with the insignificant effects on grade repetition in Panel A, the high school graduation results suggest that BPS preschool increases the likelihood that students successfully complete high school rather than causing them to enroll in high school earlier. It's worth emphasizing, however, that these estimates are based on students who appear in the Massachusetts public school database at some point, so the results should be interpreted with some caution given the differential attrition documented in Table 2.

5.2 SAT Test-taking and Scores

The SAT is an important assessment for college-bound high school students since it is widely used for college admissions. Students usually take the SAT in 11th or 12th grade after taking standardized tests in Massachusetts required for high school graduation. The SAT outcome is also of particular interest for the low-income population studied here, since the SAT is seen as a significant hurdle for students who may not have access to test preparation (see, e.g., Bowen and Bok, 2000).

Enrollment in a BPS preschool raises the likelihood that students take the SAT. Column (1) of Table 5 shows that among non-offered BPS preschool applicants who attend a Massachusetts public high school, roughly two-thirds take the SAT. Preschool attendance causes a statistically significant 8.5 percentage point increase in the rate of SAT test-taking. The size of this impact is similar to the estimated effect of preschool attendance on on-time college enrollment, suggesting that taking the SAT may play a role in accounting for our results on increased college attendance.

Since preschool attendance affects SAT test-taking, we examine how preschool influences SAT performance defined by unconditional score thresholds. A student scores above a given quartile in the state distribution if she both takes the SAT and scores above the threshold defined by the state quartile. Students who do not take the test and those who take the test but fail to reach the relevant threshold are coded as zeros for these outcomes. Column (3) of Table 5 shows that by this definition, less than one-quarter of preschool applicants score above the state median on the SAT Reasoning test (defined as the sum of Math and Verbal scores). Likewise, column (5) shows that less than a quarter score above the state median on the SAT Composite test (defined as the sum of Math, Verbal, and Writing).

Preschool attendance affects the bottom and top quartiles of performance on the SAT. Column (4) of Table 5 shows that preschool attendance marginally increases the likelihood that a student scores above the bottom quartile on SAT Reasoning. Since the outcome is coded as zero for non-takers, this effect combines the extensive-margin impact on SAT-taking and any intensive-margin impacts on scores. The effect on scores in the bottom quartile is driven by a 5-7 percentage point improvement in the likelihood of clearing the bottom quartile in each component subject test, with the estimates for Math and Verbal significant at the 10% level.

Only about 10% of non-offered preschool applicants score in the top quartile of SAT scores in Massachusetts. The top quartile of the state distribution corresponds to an SAT Reasoning score of roughly 1,200 or higher, a range which is often seen as relevant for the top 100 most selective colleges.[12] BPS preschool generates a large positive effect on this part of the score distribution. Specifically, preschool attendance boosts the likelihood of an SAT Reasoning score in the top quartile by 3.6 percentage points. This effect is driven by a substantial increase in top-quartile Math scores. As shown in the column (2) of the bottom panel in Table 5, preschool enrollment causes a 5.7 percentage point gain in the probability of scoring in the top quartile of the state SAT Math distribution ($p < 0.01$), an effect which corresponds to a 60% increase from the non-offered mean of 9.7%.

The bottom row of Table 5 also reports 2SLS estimates of impacts on average SAT scores in the sample of test-takers. These conditional results are difficult to interpret since preschool offers have a direct impact on the likelihood of taking the test. Estimated effects on average SAT scores are imprecise and statistically indistinguishable from zero, but these estimates may be contaminated by composition effects due to the large impact of preschool attendance on SAT test-taking.

[12]SAT percentiles for test-takers in the US appear in College Board (2020).

6 Effects on Test Scores and Disciplinary Outcomes

6.1 MCAS Scores

Previous studies of Boston preschools show that for recent applicant cohorts, the program increased test scores measured during the preschool year, but test score impacts were not detectable by third grade (Weiland and Yoshikawa, 2013; Weiland et al., 2019). We do not observe test scores in the preschool year for our applicant sample, but we can study effects on medium-term test scores on the Massachusetts Comprehensive Assessment System (MCAS). Massachusetts started administering MCAS exams in 1998 with tests in grade 4 and 8. The state subsequently expanded tests to other grades, and tests are now administered in grades 3-8 and 10. MCAS performance is consequential for schools, since it factors into the state's accountability framework. A student must also pass MCAS Math and English Language Arts (ELA) tests to earn a high school diploma.

Table 6 reports estimated effects of preschool attendance on MCAS test scores. We standardize these scores to have mean zero and standard deviation one in the sample of all Massachusetts test-takers in each grade and year. Among non-offered BPS preschool applicants, mean scores on Math and ELA tests in elementary school are around -0.3σ to -0.4σ, implying achievement substantially below the state average. As shown in columns (2) and (4), we find that preschool attendance has no statistically detectable impact on these achievement levels. This result is consistent with Weiland et al. (2019) who report that attendance at a first-choice BPS preschool did not affect third-grade MCAS performance for cohorts applying between 2007 and 2011. More broadly, our findings echo those in other recent randomized studies of preschool programs, which often find limited achievement impacts in elementary school (Puma et al., 2010; Lipsey et al., 2018).

Our data offer the opportunity to study achievement impacts in middle and high school as well. As in elementary school, we find no evidence of impacts on MCAS scores in later grades. Table 6 shows a mix of positive and negative estimates for grades 6, 7, 8, and 10. None of the estimates are significantly different from zero. The bottom row shows 2SLS estimates from a model that stacks all observed MCAS scores in grades 3-8 and 10, with standard errors clustered by student. Estimates from this model are also statistically insignificant, and the precision of the estimates allows us to rule out positive impacts larger than 0.14σ in Math and 0.12σ in ELA. Although we observe at least one MCAS score for roughly 85% of the sample and an average of nine test scores per non-offered applicant, an important caution is that the MCAS follow-up differential shown in Table 2 may influence our estimates of test score effects.

6.2 Disciplinary Outcomes

While the results of the previous section suggest that BPS preschool has limited effects on test scores, previous studies suggest that preschool programs can generate persistent impacts through non-cognitive channels. For example, Heckman et al. (2010b) emphasizes the role of criminal justice outcomes in the high social rate of return to the Perry Preschool program. Heckman et al. (2013) demonstrate that the Perry intervention substantially improved externalizing behaviors (aggressive, antisocial, and rule-breaking behaviors), and that these effects account for the bulk of its long-term impacts. A related literature shows impacts of teachers and schools on non-test outcomes such as suspensions, truancy, absenteeism, course grades, and crime (Deming, 2011; Jackson, 2018; Petek and Pope, 2021). Bacher-Hicks et al. (2019) and Rose et al. (2019) argue that short-term effects of teachers and schools on non-cognitive outcomes predict longer-term effects on criminal behavior.

Motivated by these findings, Table 7 displays 2SLS estimates of the effects of preschool attendance on several disciplinary outcomes measured in high school. Preschool attendance reduces the total number of suspensions students receive in high school by 0.24 ($p < 0.1$). Estimates for the number of times a student is truant and the number of days a student is absent are also negative, though estimates for these outcomes are less precise. We measure juvenile incarceration based on whether a student is ever observed attending a Massachusetts Department of Youth Services (DYS) school. DYS operates the state's juvenile justice service and DYS facilities provide rehabilitation for students who have committed crimes. Only 0.7% of non-offered preschool applicants are ever incarcerated according to this definition.[13] Incarceration is a conservative measure of interaction with the criminal justice system, since a student must be arrested before they are incarcerated. Preschool enrollment is estimated to reduce juvenile incarceration by 0.8 percentage points ($p < 0.1$).[14]

Most of the estimates in Table 7 suggest that BPS preschool improves disciplinary outcomes, but several are statistically insignificant due to a lack of precision. To aggregate information across outcomes, we estimate impacts on a summary index of discipline. Following Jackson (2018), the disciplinary index equals the first principal component of all outcomes in the table, standardized to have mean zero and standard deviation one among non-offered students. When constructing the index, outcomes are coded so that a positive estimate reflects a decrease in discipline, which can be interpreted as an improvement in student behavior. We find that preschool attendance boosts

[13] This measure seems to be a reliable measure of incarceration, as we never observe a student simultaneously enrolled in a traditional public school and a DYS facility.

[14] The estimated incarceration effect is larger than the non-offered mean but smaller than the the complier control mean, which equals 0.8%.

this index by 0.17σ, a large and statistically significant impact ($p < 0.01$).

7 Further Results and Discussion

7.1 Effects on Subgroups

The literature on early childhood programs often finds important differences in treatment effects across student subgroups. In a reanalysis of the Abecedarian, Perry, and Early Training Projects, Anderson (2008) finds significant short- and long-term benefits for girls but no significant long-term effects for boys after adjusting for multiple testing. Heckman et al. (2010a) account for compromised randomization in the Perry experiment and find that Perry generated significant long-term benefits for both sexes. Research on Head Start has emphasized differences in effects between Black and white students (Currie and Thomas, 1995; Garces et al., 2002; Deming, 2009). Gormley and Gayer (2005) show that Tulsa's public preschool program produces larger gains for middle-income students than for low-income students. Cascio (forthcoming) finds larger test score impacts for universal state-funded preschool programs than for means-tested programs, including larger effects on low-income students.

Table 8 reports 2SLS estimates of the effects of BPS preschool attendance on key outcomes for student subgroups. We probe for effect heterogeneity by sex, race, and income. Since large differences in point estimates are likely to arise by chance with small subgroup samples, we also report p-values from tests of the hypothesis that effects are equal for each sample split.

The effects of BPS preschool attendance are generally larger for boys than for girls. As shown in columns (1) and (2) of Table 8, preschool enrollment is estimated to increase on-time college enrollment for both sexes, but effects on four-year college enrollment are driven by boys. In addition, we find a positive effect of 13 percentage points for graduation at any time for boys, while the corresponding estimate for girls is negative and statistically insignificant. Gender differences in estimates for ever enrolling in a four-year college and ever graduating from college are significant at conventional levels ($p \approx 0.05$). Similarly, for boys we find significant increases in SAT-taking (16 percentage points), high school graduation (16 percentage points), and the discipline index (0.33σ), with small and insignificant corresponding estimates for girls, and statistically significant differences by sex for each of these outcomes ($p < 0.07$).

Differences in estimates by race and income are generally statistically insignificant. Columns (3), (4), and (5) of Table 8 present estimates for Black, Hispanic, and white students. The pattern of point estimates suggests somewhat larger impacts for whites, but the white subgroup is small and none of the racial differences in estimates are statistically significant at conventional levels ($p > 0.11$). To assess heterogeneity by income, columns (6) and (7) show estimates for students eligible and ineligible for a free or reduced price lunch, a proxy for low family income. Students are included in this analysis if they appear in the SIMS database, and are classified as eligible for a subsidized lunch if they are recorded as receiving a free or reduced price lunch in their first year in the SIMS. Estimates for low- and higher-income students are generally not statistically distinguishable. We do see marginally significant differences for four-year college enrollment and MCAS scores ($p = 0.08$), but these may be chance findings given the many splits examined.

7.2 Comparison to Estimates in the Literature

Previous estimates of the effects of preschool programs on educational attainment come from small-scale experiments and non-experimental studies of the Head Start program for earlier cohorts. To understand whether the effects of large-scale preschool in Boston differ from effects for these earlier programs, we compare our results to estimates from prominent studies in the literature. Table 9 lists study characteristics and estimated educational attainment effects for our evaluation of the Boston preschool program along with several previous studies evaluating other programs including the Perry Preschool Project, Abecedarian Project, and Head Start.[15]

Three key patterns are evident in this comparison. First, as shown in columns (1)-(3), ours is the only study that combines a randomized design, long-term outcomes, and a large-scale program. The other studies listed in Table 9 each lack one of these characteristics. It is important to note, however, that several of the other studies are able to look at other long-term outcomes such as earnings and criminal activity, while we can only look at educational attainment. Second, the standard errors in columns (4) and (5) show that the precision of our design compares favorably to most previous studies. The precision of our post-secondary impact estimates is comparable to estimates from Bailey et al. (2020)'s study of the initial rollout of Head Start using the Social Security Administration Numident file (though our estimates for high school graduation are less precise). Third, our impact estimates for educational attainment are consistent with estimates from previous studies. Though the studies in Table 9 estimate a variety of parameters for multiple

[15]Some subsequent analyses building on the studies in Table 9 arrive at different estimates for the same programs due to changes in sample or methodology (see, e.g., Heckman et al., 2010a; Miller et al., 2019; Pages et al., 2020). We include studies in Table 9 to provide one set of benchmark estimates against which our estimates for Boston preschool can be compared.

programs using a mix of randomized and non-randomized research designs, the estimated effects on high school graduation and college enrollment are surprisingly similar.

The bottom rows of Table 9 formally investigate the similarity of effect sizes across studies. Specifically, we use classical minimum distance (CMD) to fit a model that assumes the effect in each study is the same, treating each study as an independent unbiased estimate of this single effect with variance equal to its squared standard error. Under the null hypothesis of no heterogeneity in effects across studies, the minimized CMD criterion function follows a χ^2 distribution with degrees of freedom equal to the number of studies minus one. This CMD procedure generates precise average effect estimates of 3.8 percentage points for high school graduation (s.e. $= 0.7$) and 6.5 percentage points for college attendance (s.e. $= 1.2$). The χ^2 goodness of fit test fails to reject for either outcome ($p > 0.23$), indicating that the differences in estimates across studies can be rationalized by sampling error. This exercise reveals a consistent picture of positive preschool effects on educational attainment across a diverse set of studies. Our results show that this pattern continues to hold in a randomized evaluation of a large-scale public preschool program.

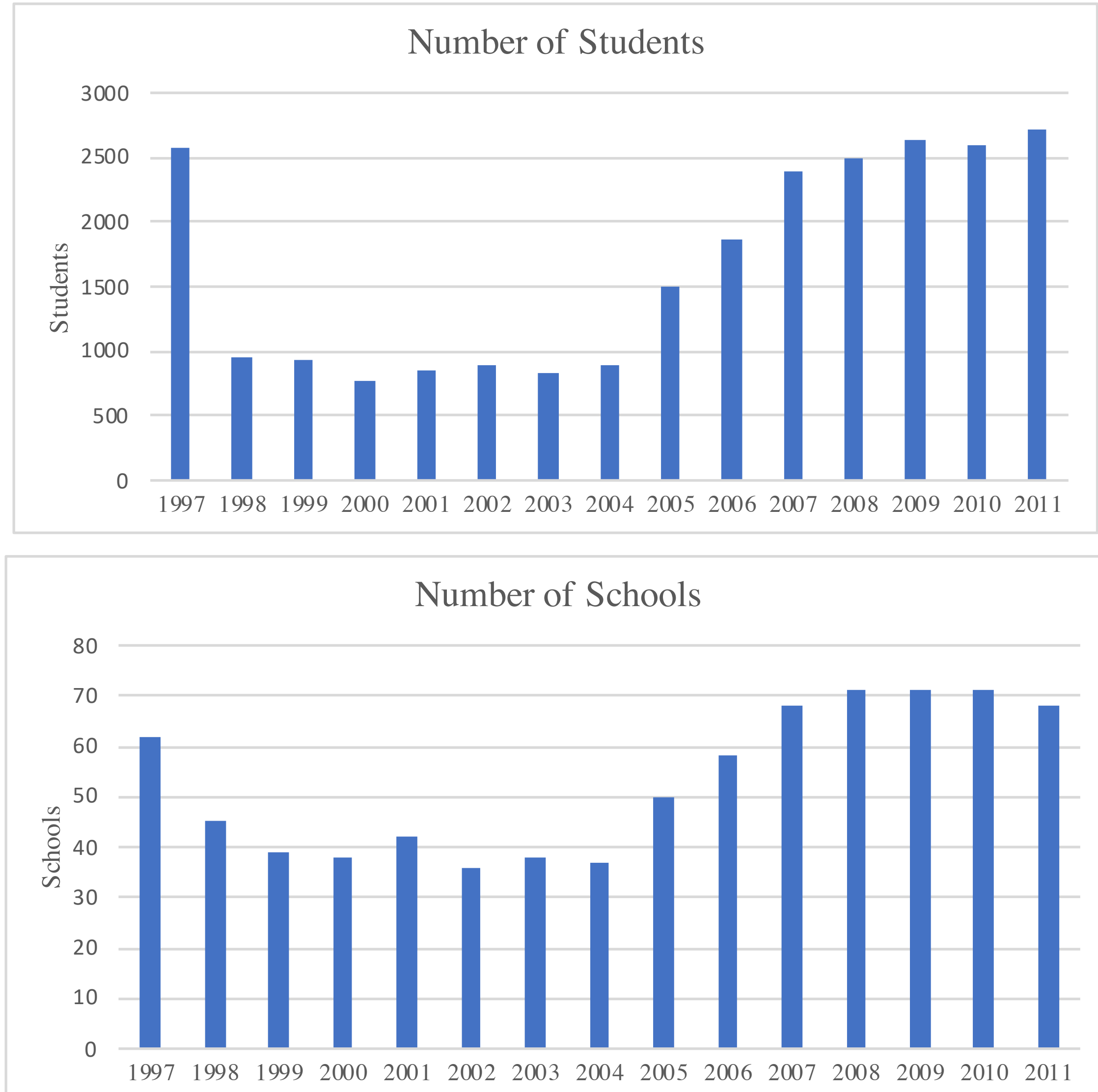
8 Conclusion

High-quality preschool programs have the potential to produce lasting impacts on skills and improve long-term outcomes for disadvantaged students (Elango et al., 2016). While public preschool has expanded rapidly in recent decades, little evidence exists on the long-term impacts of modern large-scale preschool programs. Such evidence is important both for understanding the efficacy of programs operating at scale and for interpreting links between short- and long-outcomes. This paper uses random variation from Boston's centralized school assignment mechanism to provide the first evidence on the long-term effects of a modern, large-scale public preschool program from a lottery-based research design.

The results of our analysis show that public preschool enrollment boosts post-secondary and college preparatory outcomes. Students randomly assigned to attend a Boston preschool experience fewer disciplinary incidents in high school, take the SAT and graduate high school at higher rates, and are more likely to enroll in college. These findings illustrate the potential for a universal preschool program to improve educational attainment for a disadvantaged student population.

Boston's public preschool program expanded rapidly after the time period of our study, and the program also scores highly on ratings of preschool quality (NIEER, 2019a). It's therefore possible that the effects reported here differ from effects for more recent cohorts in Boston or for lower-quality programs elsewhere. At the same time, Boston's program shares important features with other publicly-funded state and local preschool programs, so our estimates seem relevant for evaluating contemporary proposals for public preschool expansion (Biden, 2021). While we are able to document effects on educational attainment, other work has shown impacts of early-childhood programs on even longer-term outcomes such as employment, earnings, and criminal activity (Garcia et al., 2020). In future work, we hope to study impacts on these and other economic outcomes over the lifecycle.

Figure 1: Boston preschool students and schools by year (four-year-olds)



Notes: This figure plots the number of four-year-old students enrolled in Boston public preschools (top panel) and the number of schools offering preschool for four-year-olds (bottom panel) by year.

Table 1: Descriptive statistics and covariate balance

	Average characteristics		Offer differentials	
	All applicants (1)	Randomized applicants (2)	No controls (3)	Risk controls (4)
		A. Applicant demographics		
Black	0.432	0.407	-0.011	-0.015
			(0.011)	(0.017)
White	0.166	0.149	-0.012	-0.023*
			(0.008)	(0.012)
Hispanic	0.291	0.344	0.036***	0.020
			(0.011)	(0.015)
Female	0.495	0.488	0.011	0.060***
			(0.011)	(0.020)
Age at enrollment	4.569	4.580	-0.025	-0.031
			(0.017)	(0.031)
Bilingual Spanish	0.108	0.187	0.044***	0.004
			(0.008)	(0.005)
		B. Application characteristics		
Number of programs ranked	3.055	2.949	-0.098***	0.041
			(0.028)	(0.038)
First choice walkzone	0.215	0.176	0.154***	-0.005
			(0.010)	(0.005)
		C. Neighborhood characteristics		
Population	1255.2	1252.7	-56.747***	-8.681
			(12.067)	(21.408)
Median family income	53731.9	54039.2	1339.143*	1605.203
			(765.277)	(1230.354)
Poverty rate	0.234	0.232	0.004	-0.011
			(0.004)	(0.007)
Share Black	0.388	0.399	0.027***	-0.012
			(0.007)	(0.010)
Share white	0.366	0.357	-0.030***	0.014
			(0.007)	(0.009)
Share Hispanic	0.251	0.260	-0.014***	-0.002
			(0.004)	(0.006)
Sample size	8786	4215	8786	4215

Notes: This table displays average characteristics and differences in characteristics by offer status for applicants to BPS K1 programs from 1997-2003. Panel A shows results for applicant demographics, panel B reports on application characteristics, and panel C shows results for characteristics of an applicant's block group measued in the 2010 US Census. Column (1) shows characteristics for all applicants, and column (2) shows characteristics for applicants subject to random assignment (those with assignment propensity scores strictly between zero and one). Column (3) reports coefficients from regressions of each characteristic on an offer indicator, controlling for year indicators. Column (4) adds controls for assignment risk and restricts the sample to applicants subject to random assignment. Robust standard errors in parentheses. *significant at 10%; ** significant at 5%; ***significant at 1%.

Table 2: Attrition

	Non-offered followup rate (1)	Offer differential (2)
Name submitted to NSC	0.987	0.008**
		(0.003)
		4215
Ever observed in SIMS	0.910	0.028***
		(0.010)
		4215
Any MCAS score	0.845	0.038***
		(0.013)
		4215
Number of MCAS scores	9.052	0.520***
		(0.184)
		4215

Notes: This table reports followup rates and offered/non-offered differences for key outcomes. The sample includes all randomized BPS preschool applicants. Column (1) displays the fraction of non-offered applicants observed in each sample. Column (2) reports coefficients from regressions of followup on an offer indicator with controls for assignment risk.
*significant at 10%; ** significant at 5%; ***significant at 1%.

Table 3: Effects of preschool attendance on post-secondary outcomes

	Enrollment on-time				Enrollment at any time			
	Non-offered mean	First stage	Reduced form	2SLS estimate	Non-offered mean	First stage	Reduced form	2SLS estimate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any college	0.459	0.645***	0.054***	0.083***	0.650	0.645***	0.035*	0.054*
		(0.015)	(0.019)	(0.030)		(0.015)	(0.019)	(0.029)
	2669		4175		2669		4175	
Two-year college	0.096		0.018	0.028	0.291		0.019	0.030
			(0.012)	(0.019)			(0.018)	(0.028)
	2669		4175		2669		4175	
Four-year college	0.363		0.035*	0.055*	0.506		0.038*	0.059*
			(0.019)	(0.029)			(0.020)	(0.030)
	2669		4175		2669		4175	
Massachusetts college	0.329		0.055***	0.085***	0.504		0.045**	0.071**
			(0.019)	(0.029)			(0.020)	(0.030)
	2669		4175		2669		4175	
Public college	0.260		0.025	0.038	0.474		0.033*	0.051*
			(0.018)	(0.027)			(0.020)	(0.031)
	2669		4175		2669		4175	
Private college	0.200		0.029*	0.045*	0.316		0.015	0.024
			(0.016)	(0.025)			(0.018)	(0.028)
	2669		4175		2669		4175	
Number of semesters					5.567		0.396*	0.614*
							(0.220)	(0.340)
					2669		4175	
Graduation	0.208	0.621***	0.003	0.005	0.325	0.621***	0.033	0.052
		(0.017)	(0.018)	(0.029)		(0.017)	(0.021)	(0.034)
	2108		3281		2108		3281	
Graduation from four-year	0.207		0.005	0.008	0.297		0.022	0.035
			(0.018)	(0.029)			(0.020)	(0.033)
	2108		3281		2108		3281	

Notes: This table reports two-stage least squares (2SLS) estimates of the effects of Boston preschool attendance on post-secondary outcomes. Outcomes in columns (1)-(4) measure college enrollment within 6 months of a student's projected high school graduation date given his/her BPS preschool application year. Columns (5)-(8) display results based on college attendance at any time. On-time graduation equals one if a student graduates from a four-year college by the end of the fourth calendar year after projected high school graduation or a two-year college by the end of the second calendar year after projected high school graduation. The sample for graduation excludes students who applied to preschool in 2002 or 2003, who would not finish a four-year college on-time given normal academic progress. Columns (1) and (5) show mean outcomes for non-offered students. Columns (2) and (6) display coefficients from regressions of preschool attendance on the preschool offer. Columns (3) and (7) show coefficients from regressions of outcomes on the offer. Columns (4) and (8) report 2SLS coefficients instrumenting preschool attendance with the offer. All models control for a saturated set of indicators for the assignment propensity score along with sex, bilingual status, and race.

*significant at 10%; ** significant at 5%; ***significant at 1%.

Table 4: Effects of preschool attendance on grade progression, special education, and high school graduation

	Non-offered mean (1)	2SLS estimate (2)
Started 1st grade on time	0.874	0.016
		(0.023)
	1529	2375
Enrolled in BPS in 6th grade	0.810	0.032
		(0.024)
	2459	3883
Enrolled in BPS in 9th grade	0.806	0.031
		(0.025)
	2459	3883
Repeated a grade	0.325	-0.036
		(0.029)
	2459	3883
Special education in 1st grade	0.090	0.009
		(0.015)
	1529	2375
Special education in 3rd grade	0.144	0.002
		(0.015)
	2459	3883
	B. High school graduation	
Graduated high school on time	0.624	0.054*
		(0.030)
	2459	3883
Ever graduated high school	0.636	0.060**
		(0.030)
	2459	3883

Notes: This table reports two-stage least squares (2SLS) estimates of the effects of Boston preschool attendance on grade progression, special education classification, and high school graduation. The on-time first grade outcome equals one if a student appears in first grade by the expected year given his or her BPS preschool application year. The sample for this outcome is restricted to students observed in first grade in a Massachusetts public school. BPS enrollment outcomes equal one if a student is ever observed enrolled in a BPS school for the relevant grade. The grade repetition outcome equals one for students who are ever observed in the same grade in multiple years. Samples for BPS enrollment and grade repetition include students who are ever observed in a Massachusetts public school. Samples for 1st and 3rd-grade special education include students observed in the relevant grade. On-time high school graduation equals one if a student is recorded as graduating from a Massachusetts public high school by the end of his/her projected 12th grade year. Samples for the graduation outcomes include students who are ever observed in a Massachusetts public school. Column (1) displays the non-offered mean for each outcome. Column (2) reports 2SLS coefficients from models instrumenting preschool attendance with the preschool offer. All models control for the risk of an offer along with sex, bilingual status, and race.

*significant at 10%; ** significant at 5%; ***significant at 1%.

Table 5: Effects of preschool attendance on SAT test-taking and scores

	Taking		Reasoning (1600)		Composite (2400)	
	Non-offered mean	2SLS estimate	Non-offered mean	2SLS estimate	Non-offered mean	2SLS estimate
	(1)	(2)	(3)	(4)	(5)	(6)
Took SAT	0.685	0.085**				
		(0.034)				
Score above MA bottom quartile			0.384	0.062*	0.376	0.057
				(0.036)		(0.035)
Score above MA median			0.225	0.008	0.216	-0.001
				(0.030)		(0.029)
Score in MA top quartile			0.096	0.036*	0.0920	0.031
				(0.021)		(0.021)
N						2559
Average score (Ffor takers)			940.6	3.9	1392.0	7.2
				(16.6)		(24.2)
N						1863

	Math (800)		Verbal (800)		Writing (800)	
	Non-offered mean	2SLS estimate	Non-offered mean	2SLS estimate	Non-offered mean	2SLS estimate
	(1)	(2)	(3)	(4)	(5)	(6)
Score above MA bottom quartile	0.419	0.070*	0.366	0.063*	0.379	0.050
		(0.037)		(0.036)		(0.036)
Score above MA median	0.241	0.002	0.202	0.007	0.205	0.057*
		(0.030)		(0.029)		(0.030)
Score in MA top quartile	0.0968	0.057***	0.103	0.002	0.0865	0.004
		(0.021)		(0.022)		(0.020)
N						2559
Average score (for takers)	482.5	-0.6	458.1	4.5	451.4	3.3
		(8.8)		(9.1)		(8.7)
N						1863

Notes: This table reports two-stage least squares (2SLS) estimates of the effects of Boston preschool attendance on SAT test-taking and scores. The sample is restricted to students with a 10th-grade MCAS score. Outcomes for scoring above the bottom quartile, above the median, and in the top quartile are coded to zero for students who did not take the SAT. Column (1) displays mean outcomes for non-offered students, and column (2) displays 2SLS estimates instrumenting BPS preschool attendance with the preschool offer. All models control for the risk of an offer along with sex, bilingual status, and race.
*significant at 10%; ** significant at 5%; ***significant at 1%.

Table 6: Effects of preschool attendance on MCAS test scores

	Math scores		ELA scores	
	Non-offered mean	2SLS	Non-offered mean	2SLS
	(1)	(2)	(3)	(4)
Grade 3	-0.424	-0.048	-0.400	0.024
		(0.068)		(0.094)
	2025	3241	677	1092
Grade 4	-0.340	-0.025	-0.302	-0.063
		(0.067)		(0.066)
	2020	3219	2022	3226
Grade 5	-0.366	0.071	-0.276	0.022
		(0.080)		(0.076)
	1316	2056	1319	2059
Grade 6	-0.311	0.027	-0.221	-0.023
		(0.072)		(0.067)
	1690	2625	1948	3113
Grade 7	-0.203	0.049	-0.180	-0.003
		(0.064)		(0.064)
	1948	3109	1950	3114
Grade 8	-0.194	-0.009	-0.157	0.024
		(0.065)		(0.063)
	1936	3087	1939	3093
Grade 10	-0.158	0.066	-0.096	-0.031
		(0.062)		(0.064)
	1801	2852	1785	2847
Average Score	-0.279	-0.012	-0.228	-0.034
		(0.055)		(0.056)
	2279	3615	2247	3569
All grades (stacked)	-0.283	0.029	-0.215	0.005
		(0.056)		(0.057)
	2279	3615	2249	3569

Notes: This table reports two-stage least squares (2SLS) estimates of the effects of Boston preschool attendance on Massachusetts Comprehensive Assessment System (MCAS) achievement test scores. MCAS scores are standardized to have mean zero and standard deviation one among all Massachusetts test-takers. The bottom row stacks all observed test scores in grades 3-8 and 10, and clusters standard errors by student. Columns (1) and (2) show results for math scores, while columns (3) and (4) show results for English Language Arts (ELA) scores. Columns (1) and (3) display mean outcomes for non-offered students. Columns (2) and (4) show 2SLS coefficients from models instrumenting preschool attendance with the preschool offer. All models control for the risk of an offer along with sex, bilingual status, and race.

*significant at 10%; ** significant at 5%; ***significant at 1%.

Table 7: Effects of preschool attendance on disciplinary outcomes

	Non-offered mean	2SLS
	(1)	(2)
Ever suspended	0.166	-0.021
		(0.023)
	2099	3335
Number of suspensions	0.166	-0.060*
		(0.035)
	2099	3335
Ever truant	0.654	0.027
		(0.029)
	2099	3335
Times truant	7.011	-1.102
		(0.889)
	2099	3335
Days absent	16.55	-1.617
		(1.262)
	2099	3335
Juvenile incarceration	0.007	-0.008*
		(0.005)
	2099	3335
Disciplinary index	0.000	0.167***
		(0.063)
	2099	3335

Notes: This table reports two-stage least squares (2SLS) estimates of the effects of Boston preschool attendance on disciplinary outcomes measured in high school (grades 9-12). The sample is restricted to students to students observed in a Massachusetts public school in 9th grade. Juvenile incarceration equals one if a student is ever recorded as incarcerated or attending a Department of Youth Services institution. The non-cognitive index is the first principle component of all outcomes in the table, standardized to have mean zero and standard deviation one among non-offered students. Column (1) displays the non-offered mean for each outcome and column (2) reports coefficients from 2SLS models instrumenting preschool attendance with the preschool offer. All models control for the risk of an offer along with sex, bilingual status, and race. *significant at 10%; ** significant at 5%; ***significant at 1%.

Table 8: Effects of preschool attendance for subgroups

	By sex		By race			By free/reduced price lunch	
	Boys	Girls	Black	Hispanic	White	FRPL	Not FRPL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Any college enrollment (on-time)	0.117**	0.076*	0.032	0.134***	0.214**	0.068*	0.105**
	(0.046)	(0.042)	(0.047)	(0.048)	(0.100)	(0.038)	(0.058)
	2099	2076	1692	1405	643	2554	1329
Joint *p* -value	0.510			0.136		0.426	
Four-year college enrollment (on-time)	0.087**	0.017	0.027	0.052	0.231**	0.029	0.119**
	(0.044)	(0.041)	(0.045)	(0.045)	(0.100)	(0.036)	(0.059)
	2099	2076	1692	1405	643	2554	1329
Joint *p* -value	0.245			0.158		0.174	
College graduation (on-time)	0.069	-0.049	0.014	-0.019	0.166	0.003	0.050
	(0.044)	(0.043)	(0.041)	(0.041)	(0.109)	(0.035)	(0.054)
	1657	1624	1340	1096	503	2029	995
Joint *p* -value	0.055			0.265		0.475	
Any college enrollment (ever)	0.074	0.053	0.040	0.069	0.159*	0.033	0.100*
	(0.045)	(0.039)	(0.046)	(0.048)	(0.089)	(0.037)	(0.051)
	2099	2076	1692	1405	643	2554	1329
Joint *p* -value	0.734			0.471		0.436	
Four-year college enrollment (ever)	0.126***	-0.006	0.059	0.041	0.211**	0.022	0.103*
	(0.046)	(0.042)	(0.047)	(0.049)	(0.096)	(0.038)	(0.058)
	2099	2076	1692	1405	643	2554	1329
Joint *p* -value	0.034			0.257		0.079	
College graduation (ever)	0.130**	-0.011	0.089*	0.012	0.254**	0.053	0.135**
	(0.052)	(0.048)	(0.049)	(0.053)	(0.111)	(0.042)	(0.057)
	1657	1624	1340	1096	503	2029	995
Joint *p* -value	0.048			0.119		0.372	
MCAS math scores (stacked)	0.070*	-0.064*	0.056	-0.131***	0.194**	-0.049	0.125**
	(0.041)	(0.036)	(0.038)	(0.040)	(0.088)	(0.031)	(0.051)
	1797	1760	1446	1194	539	2393	1164
Joint *p* -value	0.226			0.112		0.084	
Took SAT	0.158***	0.028	0.132**	0.068	0.028	0.098**	0.103*
	(0.055)	(0.045)	(0.052)	(0.062)	(0.094)	(0.044)	(0.056)
	1247	1260	1015	791	371	1680	828
Joint *p* -value	0.069			0.540		0.936	
Ever graduated high school	0.163***	-0.035	0.066	0.009	0.161*	0.070*	0.087*
	(0.046)	(0.040)	(0.047)	(0.049)	(0.090)	(0.037)	(0.053)
	1899	1861	1526	1268	552	2480	1278
Joint *p* -value	0.002			0.175		0.792	
Ever suspended	-0.039	-0.022	-0.052	0.030	-0.090	-0.008	-0.051
	(0.037)	(0.029)	(0.041)	(0.035)	(0.062)	(0.029)	(0.039)
	1616	1597	1296	1067	460	2174	1051
Joint *p* -value	0.712			0.137		0.369	
Ever incarcerated	-0.017	-0.000	-0.013	-0.008		-0.010	-0.009
	(0.011)	(0.000)	(0.011)	(0.005)		(0.007)	(0.006)
	1616	1597	1296	1067		2174	1051
Joint *p* -value	0.129			0.182		0.854	
Disciplinary index	0.331***	0.035	0.224**	0.118	0.090	0.234***	0.075
	(0.108)	(0.074)	(0.109)	(0.093)	(0.169)	(0.079)	(0.103)
	1616	1597	1296	1067	460	2174	1051
Joint *p* -value	0.023			0.697		0.220	

Notes: This table reports two-stage least squares (2SLS) estimates of the effects of Boston preschool attendance on key outcomes for subgroups. Columns (1) and (2) compare estimates for boys and girls, columns (3)-(5) display estimates by race, and columns (6) and (7) show estimates by free/reduced price lunch (FRPL) status. Students are included in the FRPL analysis if they appear in the SIMS database, and are classified as FRPL if they receive a subsidized lunch in their first year in the SIMS. Joint *p* -values come from tests of the null hypothesis that effects are equal across subgroups. Estimates are 2SLS coefficients instrumenting preschool attendance with the preschool offer. All models control for the risk of an offer along with sex and race. The estimated effect on juvenile incarceration for white students is omitted because very few white students in the data are incarcerated.

*significant at 10%; ** significant at 5%; ***significant at 1%.

Table 9: Comparison of study characteristics and educational attainment estimates from preschool evaluations

Study	Program	Randomized design (1)	Long-term outcomes (2)	Large-scale program (3)	Impact estimates (standard errors): High school grad. (4)	Impact estimates (standard errors): College attendance (5)
This paper	Boston preschool	Yes	Yes	Yes	0.060 (0.030)	0.054 (0.029)
Belfield et al. (2006)[a]	Perry Preschool Project	Yes	Yes	No	0.165 (0.084)	-
Campbell et al. (2012)[b]	Abecedarian Project	Yes	Yes	No	0.068 (0.072)	0.170 (0.068)
Garces et al. (2002)[c]	Head Start	No	Yes	Yes	0.037 (0.053)	0.092 (0.056)
Deming (2009)[d]	Head Start	No	Yes	Yes	0.086 (0.031)	0.057 (0.036)
Bailey et al. (2020)[e]	Head Start	No	Yes	Yes	0.024 (0.012)	0.054 (0.028)
Puma et al. (2010)	Head Start	Yes	No	Yes	-	-
Lipsey et al. (2018)	TN-VPK	Yes	No	Yes	-	-
				Average effect:	0.038 (0.007)	0.065 (0.012)
				P-value for test of no heterogeneity:	0.239	0.564

Notes: This table compares study characteristics and educational attainment estimates from prominent evaluations of preschool programs. The top row shows estimates for the Boston preschool program studied in this paper, and the remaining rows display literature studies of other programs. Column (1) labels studies that use research designs with random assignment, column (2) labels studies that look at long-term outcomes, and column (3) labels studies that evaluate large-scale programs. For evaluations with long-term outcomes, column (4) shows each program's estimated impact on ever graduating high school and the corresponding standard error, while column (5) reports estimates and standard errors for the effect of attending college. Average effects and p-values come from a classical minimum distance (CMD) procedure that treats each estimate in a column as an independent estimate of the same effect. The average effect is the CMD estimate and the p-value comes from a comparison of the minimized CMD criterion function to a chi-squared distribution with degrees of freedom equal to the number of estimates minus one.

[a]High school graduation estimate and standard error are derived from the age 40 counts and percentages in Table 1. College impact is not reported because few students attended college.

[b]Estimates and standard errors are derived from counts and percentages in Table 3. Estimate in column (5) is impact on graduation because the impact on any attendance is not reported.

[c]Mother fixed effects estimates from Table 2.

[d]Family fixed effects estimates from Table 5.

[e]ATET estimates from Table 1. Standard errors are calculated as the width of the 95% confidence interval divided by 3.92.

Table A1: Application cohorts and outcome windows

Calendar year	1997		1998		1999		2000		2001		2002		2003	
	Grade	Age	Grade	Age	Grade	Age	Grade	Age	Grade	Age	Grade	Age	Grade	Age
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
1997	K1	4												
1998	K2	5	K1	4										
1999	1	6	K2	5	K1	4								
2000	2	7	1	6	K2	5	K1	4						
2001	3	8	2	7	1	6	K2	5	K1	4				
2002	4	9	3	8	2	7	1	6	K2	5	K1	4		
2003	5	10	4	9	3	8	2	7	1	6	K2	5	K1	4
2004	6	11	5	10	4	9	3	8	2	7	1	6	K2	5
2005	7	12	6	11	5	10	4	9	3	8	2	7	1	6
2006	8	13	7	12	6	11	5	10	4	9	3	8	2	7
2007	9	14	8	13	7	12	6	11	5	10	4	9	3	8
2008	10	15	9	14	8	13	7	12	6	11	5	10	4	9
2009	11	16	10	15	9	14	8	13	7	12	6	11	5	10
2010	12	17	11	16	10	15	9	14	8	13	7	12	6	11
2011	post-hs	18	12	17	11	16	10	15	9	14	8	13	7	12
2012	post-hs	19	post-hs	18	12	17	11	16	10	15	9	14	8	13
2013	post-hs	20	post-hs	19	post-hs	18	12	17	11	16	10	15	9	14
2014	post-hs	21	post-hs	20	post-hs	19	post-hs	18	12	17	11	16	10	15
2015	post-hs	22	post-hs	21	post-hs	20	post-hs	19	post-hs	18	12	17	11	16
2016	post-hs	23	post-hs	22	post-hs	21	post-hs	20	post-hs	19	post-hs	18	12	17
2017	post-hs	24	post-hs	23	post-hs	22	post-hs	21	post-hs	20	post-hs	19	post-hs	18
2018	post-hs	25	post-hs	24	post-hs	23	post-hs	22	post-hs	21	post-hs	20	post-hs	19
2019	post-hs	26	post-hs	25	post-hs	24	post-hs	23	post-hs	22	post-hs	21	post-hs	20
2020	post-hs	27	post-hs	26	post-hs	25	post-hs	24	post-hs	23	post-hs	22	post-hs	21
On-time 1st grade	n.a.		n.a.		By 2001		By 2002		By 2003		By 2004		By 2005	
On-time hs grad	By 2011		By 2012		By 2013		By 2014		By 2015		By 2016		By 2017	
On-time college enroll	By 2011		By 2012		By 2013		By 2014		By 2015		By 2016		By 2017	
On-time 4-year college grad	By 2015		By 2016		By 2017		By 2018		By 2019		n.a.		n.a.	

Notes: This table displays the timing of outcome measurements by preschool applicant cohort. Application years refer to the fall of the school year that a student applies, and on-time outcome years refer to the calendar year of measurement. The Student Information Management System (SIMS) data begins in 2001, so we do not observe on-time 1st grade enrollment for the 1997 or 1998 cohorts, who would reach first grade prior to 2001. Our search of the National Student Clearinghouse occurred in spring 2020, so we do not observe on-time four-year graduation for the 2002 and 2003 applicant cohorts, who would graduate on-time in summer 2020 or later. Grey areas indicate grades in which we observe Massachusetts Comprehensive Assessment System (MCAS) test scores.

Table A2: Mechanism replication rate

Application year	Total applicants (1)	Number incorrect (2)	Percent correct (3)
1997	2144	170	92%
1998	1463	152	90%
1999	1494	123	92%
2000	1450	54	96%
2001	1357	41	97%
2002	1383	52	96%
2003	1306	46	96%
All	10597	638	94%

Notes: This table reports the number and share of applicants whose reconstructed assignments based on preferences, priorities, and random tie-breakers match the observed assignments recorded by BPS.

Table A3: Effects of preschool attendance on postsecondary outcomes without female control

	Enrollment on-time				Enrollment at any time			
	Non-offered mean	First stage	Reduced form	2SLS estimate	Non-offered mean	First stage	Reduced form	2SLS estimate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any college	0.459	0.645***	0.060***	0.094***	0.650	0.645***	0.041**	0.063**
		(0.015)	(0.020)	(0.030)		(0.015)	(0.019)	(0.029)
	2669		4175		2669		4175	
Two-year college	0.096		0.019	0.030	0.291		0.022	0.034
			(0.012)	(0.019)			(0.018)	(0.028)
	2669		4175		2669		4175	
Four-year college	0.363		0.041**	0.064**	0.506		0.044**	0.068**
			(0.019)	(0.029)			(0.020)	(0.030)
	2669		4175		2669		4175	
Massachusetts college	0.329		0.060***	0.093***	0.504		0.051***	0.079***
			(0.019)	(0.029)			(0.020)	(0.030)
	2669		4175		2669		4175	
Public college	0.260		0.028	0.044	0.474		0.038*	0.058*
			(0.018)	(0.027)			(0.020)	(0.031)
	2669		4175		2669		4175	
Private college	0.200		0.032**	0.050**	0.316		0.021	0.032
			(0.016)	(0.025)			(0.018)	(0.028)
	2669		4175		2669		4175	
Number of semesters					5.567		0.483**	0.749**
							(0.221)	(0.342)
					2669		4175	
Graduation	0.208	0.621***	0.009	0.015	0.325	0.621***	0.040*	0.064*
		(0.017)	(0.018)	(0.029)		(0.017)	(0.021)	(0.034)
	2108		3281		2108		3281	
Graduation from four-year	0.207		0.011	0.017	0.297		0.028	0.045
			(0.018)	(0.029)			(0.020)	(0.033)
	2108		3281		2108		3281	

Notes: This table reports two-stage least squares (2SLS) estimates of the effects of Boston preschool attendance on post-secondary outcomes. Unlike the estimates in Table 3, the 2SLS models in this table do not control for a female indicator. Outcomes in columns (1)-(4) measure college enrollment within 6 months of a student's projected high school graduation date given his/her BPS preschool application year. Columns (5)-(8) display results based on college attendance at any time. On-time graduation equals one if a student graduates from a four-year college by the end of the fourth calendar year after projected high school graduation or a two-year college by the end of the second calendar year after projected high school graduation. The sample for graduation excludes students who applied to preschool in 2002 or 2003, who would not finish a four-year college on-time given normal academic progress. Columns (1) and (5) show mean outcomes for non-offered students. Columns (2) and (6) display coefficients from regressions of preschool attendance on the preschool offer. Columns (3) and (7) show coefficients from regressions of outcomes on the offer. Columns (4) and (8) report 2SLS coefficients instrumenting preschool attendance with the offer. All models control for a saturated set of indicators for the assignment propensity score along with bilingual status and race.

*significant at 10%; ** significant at 5%; ***significant at 1%.

Table A4: Effects of preschool attendance on postsecondary outcomes without 1997 cohort

	Enrollment on-time				Enrollment at any time			
	Non-offered mean	First stage	Reduced form	2SLS estimate	Non-offered mean	First stage	Reduced form	2SLS estimate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any college	0.450	0.684***	0.051**	0.075**	0.636	0.684***	0.035*	0.051*
		(0.016)	(0.021)	(0.031)		(0.016)	(0.021)	(0.030)
	2241		3378		2241		3378	
Two-year college	0.096		0.019	0.028	0.284		0.021	0.031
			(0.013)	(0.020)			(0.020)	(0.029)
	2241		3378		2241		3378	
Four-year college	0.355		0.032	0.047	0.490		0.036*	0.053*
			(0.021)	(0.030)			(0.022)	(0.031)
	2241		3378		2241		3378	
Massachusetts college	0.320		0.053**	0.077**	0.490		0.042*	0.061*
			(0.021)	(0.030)			(0.022)	(0.032)
	2241		3378		2241		3378	
Public college	0.258		0.032	0.046	0.462		0.047**	0.068**
			(0.019)	(0.028)			(0.022)	(0.032)
	2241		3378		2241		3378	
Private college	0.192		0.020	0.029	0.295		0.013	0.019
			(0.017)	(0.025)			(0.020)	(0.029)
	2241		3378		2241		3378	
Number of semesters					5.171		0.374*	0.547*
							(0.226)	(0.329)
					2241		3378	
Graduation	0.199	0.670***	-0.006	-0.008	0.304	0.670***	0.023	0.034
		(0.019)	(0.020)	(0.030)		(0.019)	(0.023)	(0.035)
	1680		2484		2241		2484	
Graduation from four-year	0.199		-0.006	-0.008	0.276		0.010	0.016
			(0.020)	(0.030)			(0.023)	(0.034)
	1680		2484		2241		2484	

Notes: This table reports two-stage least squares (2SLS) estimates of the effects of Boston preschool attendance on post-secondary outcomes. The sample excludes students who applied to BPS preschool in 1997. Outcomes in columns (1)-(4) measure college enrollment within 6 months of a student's projected high school graduation date given his/her BPS preschool application year. Columns (5)-(8) display results based on college attendance at any time. On-time graduation equals one if a student graduates from a four-year college by the end of the fourth calendar year after projected high school graduation or a two-year college by the end of the second calendar year after projected high school graduation. The sample for graduation excludes students who applied to preschool in 2002 or 2003, who would not finish a four-year college on-time given normal academic progress. Columns (1) and (5) show mean outcomes for non-offered students. Columns (2) and (6) display coefficients from regressions of preschool attendance on the preschool offer. Columns (3) and (7) show coefficients from regressions of outcomes on the offer. Columns (4) and (8) report 2SLS coefficients instrumenting preschool attendance with the offer. All models control for a saturated set of indicators for the assignment propensity score along with sex, bilingual status, and race.
*significant at 10%; ** significant at 5%; ***significant at 1%.

Table A5: Effects of preschool attendance on postsecondary outcomes using random number as instrument

	Enrollment on-time			Enrollment at any time		
	First stage	Reduced form	2SLS estimate	First stage	Reduced form	2SLS estimate
	(1)	(2)	(3)	(4)	(5)	(6)
Any college	-0.753***	-0.055**	0.073**	-0.753***	-0.046*	0.061*
	(0.020)	(0.028)	(0.036)	(0.020)	(0.026)	(0.034)
		4175			4175	
Two-year college		-0.036**	0.048**		-0.056**	0.075**
		(0.016)	(0.022)		(0.025)	(0.033)
		4175			4175	
Four-year college		-0.019	0.025		-0.034	0.045
		(0.027)	(0.035)		(0.027)	(0.036)
		4175			4175	
Massachusetts college		-0.038	0.050		-0.039	0.052
		(0.026)	(0.035)		(0.028)	(0.036)
		4175			4175	
Public college		-0.040	0.053		-0.057**	0.076**
		(0.025)	(0.033)		(0.028)	(0.037)
		4175			4175	
Private college		-0.015	0.020		-0.014	0.018
		(0.023)	(0.030)		(0.026)	(0.034)
		4175			4175	
Number of semesters					-0.628**	0.833**
					(0.316)	(0.417)
					4175	
Graduation	-0.705***	-0.014	0.020	-0.705***	-0.052*	0.074*
	(0.024)	(0.026)	(0.036)	(0.024)	(0.029)	(0.041)
		3281			3281	
Graduation from four-year		-0.016	0.022		-0.036	0.050
		(0.026)	(0.036)		(0.028)	(0.040)
		3281			3281	

Notes: This table reports two-stage least squares (2SLS) estimates of the effects of Boston preschool attendance on post-secondary outcomes. The instrument for preschool attendance is the randomly assigned tie-breaking number, which ranges from zero to one with lower numbers indicating higher priority. Outcomes in columns (1)-(3) measure college enrollment within 6 months of a student's projected high school graduation date given his/her BPS preschool application year. Columns (4)-(6) display results based on college attendance at any time. On-time graduation equals one if a student graduates from a four-year college by the end of the fourth calendar year after projected high school graduation or a two-year college by the end of the second calendar year after projected high school graduation. The sample for graduation excludes students who applied to preschool in 2002 or 2003, who would not finish a four-year college on-time given normal academic progress. Columns (1) and (4) display coefficients from regressions of preschool attendance on the random number. Columns (2) and (5) show coefficients from regressions of outcomes on the random number. Columns (3) and (6) report 2SLS coefficients instrumenting preschool attendance with the random number. All models control for a saturated set of indicators for the assignment propensity score along with sex, bilingual status, and race.
*significant at 10%; ** significant at 5%; ***significant at 1%.

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A Data Appendix

The data used for this study come from several sources. Lists of preschool applicants, applicant rank order lists, and assignments are constructed from annual records from the Boston Public Schools (BPS) school assignment system. Information on schools attended and student behavior come from the Student Information Management System (SIMS), a centralized database that covers all public school students in Massachusetts. Achievement test scores are from the Massachusetts Comprehensive Assessment System (MCAS). SIMS, MCAS, and SAT data were provided by the Massachusetts Department of Elementary and Secondary Education (DESE). College attendance information comes from the National Student Clearinghouse (NSC). This Appendix describes each data source and details the procedures used to clean and match them.

BPS Assignment Data

We received assignment data from BPS for all applicants to prekindergarten between 1997-2003. These data cover applications for K0, K1, K2, 6th grade, and 9th grade. The data include random tie-breakers, preference lists of schools, final assignments, and walk-zone priority status for each applicant. In the assignment data, applicants rank programs. Individual schools usually contain multiple programs, including bilingual or special education programs. Assignment data also include the applicant's geocode, a list of 800 geographic areas that determine walk-zone priority.

Our analysis sample consists of applicants to the first round of assignment for K1 preschool programs. We restrict our analysis to valid, complete applications to K1 between 1997 and 2003. The number of valid applications to K1 in the fall of each year are shown in column (1) of Table B1. The sample is further restricted to exclude applicants who have empty preference lists (column (2) of Table B1). Finally, a small number of applicants who have duplicate random lottery numbers and who have the same parents listed are dropped, and applicants who had previously applied to a BPS prekindergarten program are also excluded (column (3) of Table B1). Applicants with duplicate random lottery numbers are likely cases of twins (or triplets, etc.), which are treated differently in the assignment system and therefore excluded.

Using a student's random tie-breaker, preference list, walk-zone priority, and sibling status, we recreate student priorities. Some information on priorities used to assign applicants are not contained in the assignment files. The data show if an applicant was assigned to a school as a result of their sibling priority, but do not record sibling status for other unassigned schools.

Since siblings are generally guaranteed admission, we use sibling priority at the assigned school to infer sibling priority in the assignment process. Prior to 2001, BPS engaged in post-assignment rebalancing of offers for some students to reduce racial segregation. We do not observe the criteria used to conduct this re-balancing, so we treat any discrepancies between final assignments and the assignments implied by the observed preferences, priorities, and tie-breakers as non-compliance with the assignment mechanism. Preferences, priorities, and capacities for each program (computed by summing the number of observed offers) are used to replicate the match. We achieve a replication accuracy of 94.6% across all years, shown in Table A2.

In addition to the application file, we also received a second post-enrollment extract from BPS. This data set includes enrollment records for all students who applied to a BPS preschool between 1997-2003, allowing us to determine which applicants attended a BPS preschool program. The post-enrollment extract also includes applicant names, dates of birth, race, first language, and free lunch status. The application and post-enrollment data files include a unique BPS applicant identifier that links records across these files.

SIMS Data

Our study uses SIMS data from the 2001-2002 through the 2017-2018 school year. Each year of data includes files corresponding to October and June. The SIMS files include information on student demographics, special education status, limited English proficiency, subsidized lunch status, and schools attended for students in Massachusetts public schools each year. SIMS also contains a student identifier known as the SASID, which is used to match students to the MCAS and SAT data sets.

We use the SIMS data to code school enrollment, grade repetition, special education, subsidized lunch status, and high school graduation. If a student attended multiple schools within the same school year, we assign the student to the school they attended longest. Students classified as special education or eligible for a free or reduced price lunch in any record within a school-year-grade retain that designation for the entire year-grade. The SIMS includes exit information indicating a student's status at the end of each time period, which we use to determine high school graduation.

We also use SIMS data to track disciplinary outcomes in high school. The SIMS tracks the number of days a student is registered at a school, the number of days a student attended a school, and the number of days a student is truant. If a student enrolled in multiple schools within the same school year, we add days registered, days absent, and days truant across all schools. We use this information to track number of absences, number of truancies, rates of absence, and rates of truancy. Juvenile incarceration is measured based on a unique enrollment code corresponding to a Department of Youth Services facility.

MCAS Data

The MCAS is the standardized state exam for Massachusetts. We use MCAS Math and English Language Arts (ELA) data from the 2001-2002 school year through the 2017-2018 school year. Each observation in the MCAS database corresponds to a single test score. These scores are associated with SASIDs linking MCAS data to SIMS data. We use MCAS ELA and Math test scores from the 3rd, 4th, 5th, 6th, 8th and 10th grade exams. Since the Math 3, Math 5, ELA 5, and ELA 6 tests were created between 1997 and 2003, some cohorts are missing these tests. Table B2 shows MCAS availability by subject. The raw test score variables are standardized to have mean zero and standard deviation one within a subject-grade-year among all Massachusetts test-takers. If a student has multiple scores for a subject-grade, we use the best score the student achieves.

SAT Data

We use SAT data files provided to the Massachusetts Department of Elementary and Secondary Education by College Board for Massachusetts public school students taking the SAT. The SAT files include scores for every section of the SAT and every cohort but application year 2003. If a student took the SAT more than once, the file records the most recent exam. The SAT file also includes a student's SASID, which is used to merge SAT outcomes with the SIMS database.

NSC Data

Data on college outcomes comes from the National Student Clearinghouse (NSC) database, which captures enrollment for 96% of US college students (Hindley and Eaton, 2018). We submitted all available names and dates of birth for BPS preschool applicants to the NSC in May 2020. The resulting data set includes each student/school enrollment record found in the NSC database. We use the date of enrollment, date of exit, type of school (2/4 year, in state, out of state, private, public) as well as exit status. Exit status includes graduation or transfer.

Data Linking Procedure

To match our assignment data to the SIMS and NSC data sets, we associate a unique name and SASID with each identifier ("student number") in the assignment file. We first use the BPS post-enrollment extract to associate a unique name and date of birth for every student number. Some student numbers have multiple names in the post-enrollment extract, usually due to variations in spellings or the use of middle names for students who applied multiple times. Where there are multiple names and dates of birth, we match BPS student numbers to the earliest observed name and date of birth. We refer to this name and date of birth combination as the Primary-ID. We refer to the name component of the Primary-ID as the Primary Name. We are able to identify a Primary-ID for every unique BPS student number in the BPS assignment file.

After constructing the Primary-ID, we link students to the SIMS by finding a unique SASID for each Primary-ID. We do this using the following algorithm.

- For every SIMS time period (October or end of year)
    - For every Primary-ID
        1. Find every SIMS name with the same first initial, last initial, and date of birth.
        2. Calculate the Levenshtein string distance between the Primary-Name and each associated SIMS name.
        3. Keep the SIMS name with the smallest Levenshtein string distance. This links a SASID to every Primary-ID.
        4. Keep the match if the string distance between the Primary-Name and the SIMS name is less than 0.2.
- Once we have done this for every time period, we append all Primary-ID/SASID matches together. For each Primary-ID, we keep the SASID associated with the earliest MCAS test score. If a Primary-ID still has multiple SASIDs, we keep the SASID with the closest name to the Primary-Name in terms of Levenshtein string distance.

Following this procedure, we have a crosswalk that links each student number to a unique SASID.

Census Block Group Data

Data on census block characteristics, including median household income, poverty rates, and racial group shares come from the 2010 United States Census. The block group map was obtained from the TIGER/Line shapefiles.

Census block group data are merged to applicant files by first linking the 2010 census block group map to a map of Boston's geocodes (centroids). The geocodes are then matched directly to geocode identifiers contained in the BPS assignment file.

Final Data Construction and Attrition

To construct the final data we match applications to the NSC and the SIMS using the Primary ID as described above. Table B1 shows the number of valid applications to K1 that can be linked to the NSC and SIMS. Because we sent every available name to the NSC, we were able to conduct the search for all students who could be linked to the Primary ID.

The non-offered attrition rate and differential between those students who received an offer and those who did not is shown in Table B3. Among non-offered applicants 98.7% had names and dates of birth that we searched for in the NSC data. Offered applicants were 0.8% more likely to be included in the NSC search. Among non-offered applicants, 91% were found in the SIMS data. Offered applicants were 2.9% more likely to be found in the SIMS and linked to a SASID.

Table B1: Sample construction and linkage to outcome files

						Randomized applicants		
Application year	All first round applicants (1)	Dropping invalid priority or preference (2)	Dropping duplicate random number or ID (3)	Has Primary ID/NSC (4)	Has SIMS (5)	Dropping duplicate random number or ID (6)	Has Primary ID/NSC (7)	Has SIMS (8)
1997	2,144	2,125	2,121	2,101	1,854	808	797	703
1998	1,463	1,460	1,343	1,340	1,219	880	877	805
1999	1,494	1,486	1,244	1,232	1,148	608	603	571
2000	1,450	1,437	1,081	1,072	1,004	563	558	523
2001	1,357	1,342	991	986	937	447	446	422
2002	1,383	1,366	1,047	1,042	988	482	479	461
2003	1,306	1,284	959	907	870	427	415	398
All	10,597	10,500	8,786	8,680	8,020	4,215	4,175	3,883

Notes: This table describes the restrictions we impose to construct our analysis sample from the BPS preschool applicant records. Column (1) shows the number of first round applications in the applicant data file. Column (2) removes applications with invalid priority or preference data. Column (3) drops applicants with duplicate random numbers or duplicate unique identifiers. Column (4) drops applications that cannot be linked to the BPS enrollment file from which the Primary ID (name and date of birth) is obtained. Column (5) drops applications that cannot be linked to the Student Information Management System (SIMS). Columns (6)-(8) show the effects of these restrictions in the sample with non-degenerate preschool assignment risk.

Table B2: MCAS followup rates

Application year	Grade						
	3	4	5	6	7	8	10
				A. Math			
1997	-	0.848	-	0.809	0.809	0.811	0.758
1998	-	0.831	-	0.835	0.831	0.824	0.740
1999	-	0.849	0.813	0.802	0.809	0.802	0.727
2000	-	0.822	0.807	0.780	0.784	0.786	0.744
2001	0.791	0.803	0.801	0.799	0.794	0.787	0.709
2002	0.824	0.844	0.826	0.785	0.787	0.777	0.727
2003	0.799	0.814	0.809	0.802	0.807	0.799	0.726
All	0.805	0.832	0.811	0.805	0.806	0.801	0.736
				B. ELA			
1997	0.831	0.845	-	-	0.822	0.812	0.765
1998	0.862	0.830	-	0.817	0.822	0.821	0.748
1999	0.872	0.846	0.814	0.806	0.806	0.795	0.723
2000	0.820	0.828	0.799	0.778	0.780	0.782	0.751
2001	0.803	0.803	0.799	0.801	0.791	0.789	0.709
2002	0.826	0.835	0.826	0.787	0.783	0.779	0.711
2003	0.799	0.812	0.807	0.802	0.802	0.794	0.721
All	0.835	0.831	0.809	0.799	0.804	0.799	0.737

Notes: This table displays the fraction of Boston preschool applicants with observed Massachusetts Comprehensive Assessment System (MCAS) test scores by application cohort and grade. Panel A shows followup rates for Math scores, and Panel B shows followup rates for English Language Arts (ELA) scores. The sample is restricted to students matched to the Student Information Managment System (SIMS) data.

Table B3: NSC and SIMS followup rates

Application year	Sample size (1)	NSC followup: Non-offered followup rate (2)	NSC followup: Offer differential (3)	SIMS followup: Non-offered followup rate (4)	SIMS followup: Offer differential (5)
1997	808	0.977	0.013*	0.856	0.030
			(0.007)		(0.033)
1998	880	0.995	0.003	0.904	0.021
			(0.002)		(0.022)
1999	608	0.990	0.003	0.934	0.004
			(0.002)		(0.021)
2000	563	0.987	0.032**	0.911	0.043*
			(0.015)		(0.026)
2001	447	0.997	0.002	0.914	0.069***
			(0.002)		(0.018)
2002	482	0.993	-0.001	0.957	0.010
			(0.010)		(0.021)
2003	427	0.966	0.013	0.918	0.034
			(0.019)		(0.026)
All	4215	0.987	0.008**	0.910	0.028***
			(0.003)		(0.010)

Notes: This table displays non-offered follow-up rates and offer differentials in followup rates for the National Student Clearinghouse (NSC) and Student Information Management System (SIMS) data sets. The sample is restricted to Boston preschool applicants subject to random assignment. Columns (1) and (3) show followup rates for non-offered applicants. Columns (2) and (4) report coefficients from regressions of an indicator for followup on an offer indicator, controlling for assignment risk. Robust standard errors in parentheses.

*significant at 10%; ** significant at 5%; ***significant at 1%.