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Making summer matter: The impact of youth employment on academic performance

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This paper examines New York City's Summer Youth Employment Program (SYEP). SYEP provides jobs to youth ages 14–24, and due to high demand for summer jobs, allocates slots through a random lottery system. We match student-level data from the SYEP program with educational records from the NYC Department of Education and use the random lottery to estimate the effects of SYEP participation on a number of academic outcomes, including test taking and performance. We find that SYEP participation has positive impacts on student academic outcomes, and these effects are particularly large for students who participate in SYEP multiple times.

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1. Introduction

Unemployment rates for youth jumped to historical highs after the recession of 2008 and have been slow to recover. An important component of this jobs crisis is the lack

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of available summer jobs for high school students—especially low-income youth.[1] This dearth of employment opportunities for youth may hamper their development, with lasting negative consequences. Prior research suggests that adolescent employment improves net worth and financial well-being as an adult (Painter (2010), Ruhm (1997)). An emerging body of research indicates that summer employment programs also lead to decreases in violence and crime (Sum, Trubskyy, and McHugh (2013), Gelber, Isen, and Kessler (2014), Heller, Pollack, and Davis (2017)).[2] Work experience may also benefit youth, and high school students specifically, by fostering various noncognitive skills, such as positive work habits, time management, perseverance, and self-confidence (Lillydahl (1990), Mortimer (2005), Duckworth, Peterson, Matthews, and Kelly (2007)).[3]

Building on previous work (Leos-Urbel (2014)), this paper studies the impact of summer youth employment on students' academic achievement. We utilize a large data set including nearly 200,000 applicants to New York City's Summer Youth Employment Program (SYEP) from 2005–2008. We match the SYEP program data for each student to academic records from the New York City Department of Education (NYCDOE).

Importantly, since the number of applicants substantially exceeds the number that can be served, positions are allocated through a random lottery, offering an unusual opportunity to derive robust estimates of the impact of the program. We use data on New York State's "Regents" exams designed to assess performance in a variety of high school subjects including Mathematics, Sciences, English, and History. Further, we examine the way in which the impact of SYEP varies with repeated program participation over multiple summers and explore heterogeneity across key student subgroups.

Our estimates indicate that SYEP improves academic outcomes for the New York City (NYC) public school students who participate: SYEP increases the number of exams students attempt, the number of exams students pass, and the average score students achieve. The Two Stage Least Squares (2SLS) estimates using the lottery as an instrument for attendance indicate that participating in SYEP increases the number of Regents exams passed (with a score of at least 65) by a statistically significant (at the 1% level) 0.023 exams. To give some context to this effect size, we find that this estimated effect of SYEP is equivalent to the estimated effect on test passing rates of a 0.14 standard deviation increase in the 8th grade reading score and 20% of the difference in the pass rates for free lunch and nonfree lunch eligible students (where free lunch eligible is a common measure of poverty).

Further, we find that the improvements in test taking and passing increase with the number of years a student participates in SYEP—impacts are larger for second time participants and largest for those participating for the third time. While we cannot claim

[1]Summer jobs for low-income youth represented a major component of the American Recovery and Reinvestment Act (ARRA), which provided $1.2 billion for youth employment opportunities and funded 345,000 jobs during the summer of 2009 (Bellotti, Rosenberg, Sattar, Esposito, and Ziegler (2010)). However, these funds are no longer available, and many other publicly funded jobs have also experienced reductions in the number of youth they are able to employ.

[2]This is consistent with evidence that unstructured time with peers is associated with greater delinquent behavior (Anderson and Hughes (2009)).

[3]Heckman (2000) and Cunha, Heckman, and Schennach (2010) argued that noncognitive skills and motivation are critical for future skill development, and that these skills can be improved at later ages.

that these participation effects reflect entirely a causal dosage effect, because the decision to apply is an endogenous one, these effects are suggestive that allowing the program to enroll students voluntarily for multiple summers can have even larger effects on their academic performance than a single exposure to the program. Our findings suggest substantial heterogeneity in program effects, and thus an important avenue for policymakers to target the program to those who might benefit from it the most.

2. Relevant prior research

Much of the previous research examining the impact of high school student employment on academic outcomes has been limited to work during the school year, focusing on the potential tradeoffs between the developmental and financial benefits of working and the possible crowding out of time devoted to academics (Rothstein (2007), Sabia (2009), Kalenkoski and Pabilonia (2009)). This research largely suggests that working a moderate number of hours (i.e., fewer than 20 hours per week) during the school year has either a small positive effect or no effect on outcomes such as school attendance, time spent on homework, and GPA, and that working more than a moderate number of hours (i.e., more than 20 hours per week) has negative effects on these outcomes (Lillydahl (1990), Monahan, Lee, and Steinberg (2011), Rothstein (2007), Stern and Briggs (2001)). Most previous research, however, has explicitly excluded work experiences during the summer when there is considerably less risk of detracting attention from school responsibilities (Painter (2010), McNeal (1997)).

Walker and Vilella-Velez's (Walker and Vilella-Velez (1992)) evaluation of the Summer Training and Education Program (STEP) is one study that directly examines summer employment. They find that STEP improved reading and mathematics test scores for academically-behind 14- and 15-year-olds from poor urban families who participated in the program. STEP consisted of half-day summer jobs combined with half-days of academic course work (specially designed remedial reading and mathematics curricula). In addition to higher test scores, participating students had better grade point averages, showed more knowledge about responsible sexual and social behavior, and had higher attendance rates than students from a control group. SYEP is similar to STEP, with employment combined with some classroom instruction, although SYEP's classroom instruction is considerably less (about 10% of program hours, as described more fully below).

In the first research to study SYEP using the randomized admission lottery, Leos-Urbel (2014) estimated the impact of SYEP on student attendance for the 2007 cohort of students. He finds a significant increase in school attendance in the school year following SYEP participation, with larger effects among students likely to be at greater risk of low attendance—students 16 years and older with low attendance rates in the previous year. We expand on these findings by considering a broader range of academic outcomes including test taking and performance on a wide range of exam subjects. Further, and key to this analysis, we use data on four SYEP cohorts, constituting nearly 200,000 SYEP applicants, allowing us to study effects of repeated program exposure on individuals who participate multiple years. In more recent work on NYC's SYEP program using tax records and analyzing different outcomes, Gelber, Isen, and Kessler (2014) found that SYEP participation causes a decrease in incarceration rates and mortality but a modest decrease in average earnings for the 3 years after participation and no effect on college participation. Modestino (2019) studied a Boston SYEP and finds that program participation reduces criminal activity and improves conflict resolution skills. These findings suggest that our estimates of positive effects on academic outcomes in high school may not affect the college participation margin but may affect other later life outcomes.

Following the initial circulation of our study, a report issued by the MDRC group found small or no impacts of SYEP on certain educational outcomes (Valentine, Anderson, Hossain, and Unterman (2017)). Like the studies discussed above, the MDRC study focused primarily on later outcomes (high school and college graduation), and their findings are consistent with a lack of long-term impact. But, in addition, we note that there are several important differences in research design between our study and the MDRC report. First, the MDRC analysis included only first-time applicants, whereas we estimate much larger effects for second- and third-time applicants.[4] Second, the MDRC report examined later outcomes (attendance in up to 5 high school years following lottery, and high school and college graduation), comparing winners and losers of the first lottery entered. In this design, some of the lottery losers (the MDRC "control group") could have entered later lotteries and participated in SYEP, implying that the MDRC estimates may be underestimating the impact of SYEP participation because of their chosen estimand.[5] In contrast, we analyzed next-year outcomes after each year of potential participation, and thus our control group is not contaminated by noncompliance among lottery losers.

The plan for the remainder of the paper is as follows. The next section describes the institutional background and some key details of the administration of NYC's SYEP program. The following section describes the matched SYEP and NYC Department of Education data. Next, we discuss the econometric framework and the estimation results. We conclude by discussing the size of the effects relative to the cost of the program and important policy lessons suggested by the empirical analysis.

3. Institutional background

New York City's Summer Youth Employment Program (SYEP) is designed to introduce and prepare youth for future careers, foster skills important for success in the labor market, and provide supplemental income to families. SYEP participants work in a variety of entry-level jobs at community-based organizations (CBOs), government agencies and private sector businesses; most common worksites include summer camps and day care, followed by social or community service agencies and retail. Participants are paid for up to 25 hours per week for up to 6 (or, in some years, 7) weeks at minimum wage,

[4]We also find small or no treatment effects for first-time applicants.

[5]An extreme case is that if all of the first-time lottery losers eventually participated in SYEP, we might see no difference in actual SYEP participation between the winners and losers, and thus would estimate a zero effect, even if there is a substantial treatment effect. This compliance issue is separate from the noncompliance of the lottery winner group—some of the winners chose not to participate in SYEP.

\$8.75 per hour. In addition to work experience, 10% of participant hours are dedicated to education and training on topics related to time management, financial literacy, workplace readiness and etiquette, and career planning and finding employment.

The NYC Department of Youth and Community Development (DYCD) administers the program and contracts with a variety of CBOs to conduct intake and enrollment, as well as provide training and supervise job placement. All NYC residents ages 14–24 are eligible to apply to SYEP.[6] To apply to the program, youth submit an application directly online or through a paper application and select a CBO service provider. Both types of applications are entered into the central SYEP data system. The system cross-checks across all service provider applications for duplication by matching the social security number and name of the applicant to ensure that each youth submits only one application for the program. Each complete application is randomly assigned an identification number. After the application deadline, DYCD assigns each service provider the number of SYEP slots that they are contracted to serve. DYCD then runs a lottery using the data system for each provider. The computerized system, using a random selection algorithm, selects applicants using the identification numbers for each provider according to the number of slots they have been allocated. The system sees each application as an ID number belonging to a provider and does not use any applicants who have self-identified as having a disability. We exclude these students from the analysis.

SYEP is funded through a combination of federal (including Workforce Investment Act, Community Services Block Grant and American Recovery and Reinvestment Act funds), state (state TANF and general funds), city (through a city tax levy) and private funds, and changes in the availability of program funding have dictated fluctuations in the number of participants served over time. Specifically, the increase in city and state funding after 2005 allowed DYCD to increase the number of participants from 38,467 in 2005 to 42,956 participants by 2008. Expansion has not met demand, however, as the number of applications has almost doubled. SYEP received 53,005 applications in 2005; this number grew to 80,129 in 2008.

4. Data and sample

Student-level data for this study come from two primary sources: SYEP files from the DYCD and New York City Department of Education (NYCDOE) administrative data files. We matched students from each of these files for the 2005–2008 program years, encompassing 196,620 SYEP applications. Data from DYCD include an indicator of SYEP lottery result, the CBO provider the student applied to, and, for those students who participated, the type of SYEP work placement, the specific worksite, and number of hours worked. Variables from NYCDOE files include student demographics, school attendance, and information about standardized test-taking and performance.

[6]SYEP also includes a few separate programs targeted at special populations, including one that serves only youth with disabilities through a separate lottery competition, a special program targeting vulnerable youth in foster care, court-involved, or who are runaway/homeless youth that was added in 2009, and a school-year program funded through the Workforce Investment Act that does not use a lottery and guarantees admission. The results presented here focus on the larger general SYEP program and lottery only.

4.1 *Data matching*

Importantly, because the SYEP program is open to all NYC youth, including nonstudents and students not enrolled in NYC public schools (students enrolled in private religious and private nonreligious schools), a 100% match rate between the DYCD and NYCDOE files is impossible.[7] Since DYCD and NYCDOE files do not contain a common identifying number (e.g., social security number), data were matched on participant first and last names and date of birth. Matching was conducted by an independent NYCDOE-approved consultant in order to maintain student anonymity. The match rate was between 77 and 81% depending on the year. Unmatched participants then include students enrolled in private or parochial schools or enrolled in schools outside of NYC, as well as nonstudents. The match rate for NYCDOE students, if we were able to identify them directly from the SYEP data, is likely considerably higher, though we cannot directly test this. Therefore, determining the success rate for the match is complicated by this fact, and we instead conduct a number of tests of the relationship between probability of being matched and random lottery results. We find that student files matched to NYCDOE data have a similar proportion of lottery winners as the unmatched files (for which we only have DYCD data), indicating that winning the lottery is not related to matching of files. We conduct additional tests on the match rate as described below.

4.2 *NYCDOE data*

The NYCDOE data include student-level demographic information, as well as an academic record for each year in the NYC public schools. Student demographics include gender, race/ethnicity, English proficiency, participation in special education and ESL services, free and reduced price lunch eligibility, grade level, and age.

Each student record includes information on test-taking and performance on New York State standardized tests in a variety of subjects, including English, various mathematics exams (Math A, Math B, and Integrated Algebra and Geometry, which replaced Math A and B in later years), Global History, Earth Science, Biology, Physics and Chemistry. These tests, known as the “Regents Examinations,” are a series of tests aligned with New York State’s Learning Standards, and designed and administered by the New York State (NYS) Department of Education, under the authority of the Board of Regents of the University of The State of New York and prepared by teacher examination committees and testing specialists. Examination scores range from 0–100%. Although the specific requirements change over time and students have some flexibility in choosing which exam to take, starting with students who entered 9th grade in 2001, earning a NYS high school diploma (“Regents Diploma”) requires passing a set of these exams including mathematics, English, Global History and Geography, US History and Government, and at least one science (e.g., Biology, Chemistry, Physics, Earth Science). More specifically, in order to graduate with a high school diploma, students must score 65 or higher on any one

[7] Prior to matching the SYEP file to NYCDOE student ID numbers, we removed observations for youth who had indicated on their SYEP application that they had left high school before finishing, graduated from high school or completed a GED, or attended college, all of whom would not be expected to match with NYCDOE student records.

math exam—usually Math A,[8] English, Global History and Geography and US History and Government, and one science exam. To earn an Advanced Regents Diploma, students must pass an additional mathematics exam, Math B,[9] and one additional science (at least one life science and one physical science). Additionally, students entering 9th grade in 2007 and prior had the option of graduating with a "Local Diploma," which required passing any one of five Regents exams with a score of at least 55. This option was gradually phased out,[10] and the Local Diploma was not available for students entering 9th grade in 2008 and later. Regents exams in all subjects are offered in June each year, and a limited number of Regents are offered in January and August. There are no mandated grades in which students are eligible or required to take a specific exam, but they typically take the exam at the end of the related course. Because the graduation requirements reward passing but do not penalize failing, it is in a student's best interest to take these exams as early as possible. The majority of students elect to take the exams in June at the end of the school year.

Our analyses focus on the impact of SYEP participation on academic outcomes, including test-taking and test-performance. To assess student performance, we examine three test-related outcomes in turn: test taking, passing at various levels, and the level of the actual test score. We construct an indicator variable for whether the student took the Regents exam in a particular subject and variables measuring performance as z-scores for each exam.[11] We also include indicator variables for whether the student passed the exam at three cut-off points: 55 (the score required for a Local Diploma available to a subset of students in our sample); 65 (required for a Regents diploma), and 75 (required on English and Math A for admission to CUNY 4-year colleges). From these exam-specific indicators, we create seven measures to capture general performance on Regents exams: whether attempted any Regents exams in the school year following SYEP application and the total number of Regents exams attempted, the total number of exams passed with a score of 55 or above, the total number of exams passed with a score of 65 or above, an indicator for passing any exam with a score of 65 or above, the total number of exams passed with a score of 75 or above, and the average (mean) score on all exams taken that year.

4.3 *Sample: SYEP applicants*

Our sample includes all SYEP applicants who were matched to the NYC public school records and were enrolled public school students, representing 134,366 applicants to the

[8] Math A was last administered in January, 2009 and replaced by Integrated Algebra beginning in June 2008 and Geometry beginning in June 2009.

[9] Math B was last administered in June 2010, replaced by Algebra 2 and Trigonometry in June 2009.

[10] Students entering grade 9 in 2005 were required to score 65 or above on two of the five required Regents exams and score 55 or above on the remaining three; 2006 9th graders were required to score 65 or above on three of the five required exams, and 2007 9th graders were required to score 65 or above on four of the five required exams.

[11] Z-scores are standardized to have a mean of zero and a standard deviation of one across all students taking that Regents exam in that particular year.

Table 1. Sample by lottery outcome, 2005–2008.

Year	Winners	Losers	Total
2005	15,544	9,126	24,670
2006	17,165	11,609	28,774
2007	19,296	19,353	38,649
2008	19,963	22,310	42,273
Total	71,968	62,398	134,366

Note: Sample includes all applications for students expected to be in high school following SYEP. Applications are omitted if the student submits multiple applications or in ungraded special education following SYEP. Applications to vulnerable youth programs, programs based out of the city, or programs with a greater than 99% or less than 0% selection rate are omitted.

program from 2005–2008.[12] Table 1 includes the number of SYEP applicants in each year as well as the number selected ("Winners"), and not selected ("Losers"), by the lottery. Note that the number of applicants increased in each year, and that the percentage of applicants selected to participate decreased. Importantly, as discussed below, some students applied to SYEP more than one time during this time frame, and these 134,366 applications consist of 96,214 unique individuals.

Tables 2a–2b provides descriptive statistics on the population of SYEP applicants from NYC public schools. The modal grade during which a student first applied to SYEP was 9th grade (about 41% of the applicants), with 22% applying in 8th grade, and 24% applying in 10th grade for the first time. Compared to nonapplicants, SYEP applicants are more likely to be female. Reflecting the substantially more disadvantaged background of the applicants, SYEP applicants are more likely to be receiving free or reduced price lunch. In addition, the applicants are much more likely to be black.

Table 3 provides descriptive statistics for the outcomes of interest related to student Regents exam attempts and performance. 72% of the sample attempted at least one Regents exam, with an average of 1.76 exams attempted each year. Roughly half of the sample passed at least one Regents exam, with students passing an average of 1.13 exams (at score of 65 or higher) per year. The average z-score of −0.14 indicates that this sample performed 0.14 standard deviations below the city average. Note that these numbers do vary across cohort years.

Finally, Table 4 provides the "take-up" rate of SYEP placement offers. Depending on the year, between 73–84% of participants offered an SYEP placement (i.e., they won the SYEP lottery for the CBO they applied to) actually participated in the program and worked at their summer job.[13]

[12] We exclude duplicate observations for students who submit multiple SYEP applications within a year, and a subgroup who applied to vulnerable youth programs, WIA programs or programs that guaranteed summer jobs and did not use a lottery. We also exclude students who were are currently in grade 7 or lower and those who are in grade 12. We exclude students currently in grade 7, students currently in grade 12, and students in ungraded special education.

[13] Around 2007, DYCD made a push to advertise SYEP and increase the number of applicants. The number of applicants in our sample increased by about 40%, from 29,718 in 2006 to 40,233 in 2007. One possible explanation for the decline in take-up is that the large increase in applicants pulled from a wider pool of applicants, some of whom were less inclined to accept the SYEP offer.

Table 2a. Comparison of applicants and nonapplicants.

	First-Time Applicants	Nonapplicants	Difference	95% CI
Female	0.556	0.478	0.078	(0.074, 0.082)
White	0.052	0.148	−0.095	(−0.097, −0.093)
Black	0.502	0.295	0.207	(0.203, 0.211)
Hispanic	0.323	0.397	−0.073	(−0.077, −0.07)
Asian	0.116	0.151	−0.035	(−0.037, −0.032)
Free Lunch	0.722	0.643	0.079	(0.076, 0.083)
Red Lunch	0.105	0.090	0.015	(0.012, 0.017)
LEP	0.044	0.105	−0.061	(−0.062, −0.059)
ESL Not LEP	0.011	0.046	−0.035	(−0.036, −0.034)
Spec Ed	0.091	0.079	0.012	(0.01, 0.015)
Z Reading 8th Grade	−0.034	−0.010	−0.025	(−0.033, −0.017)
Z Math 8th Grade	−0.019	−0.015	−0.004	(−0.012, 0.004)

Note: First time applicants are defined as the first application made by a student after 2005 for students who did not apply in 2005. 2005 is excluded since we cannot see applications made before 2005, and thus we cannot distinguish first-time applicants from repeat applicants in 2005. Nonapplicants are defined as students in grades 8–11 and alternative special education who never apply between 2005 and 2008. Limited English Proficiency (LEP) is determined by score on the Language Assessment Battery exam. Z Reading and Z Math scores are 8th grade state test scores, standardized by grade and year of administration.

Table 2b. Grade of first application.

Grade	Fraction	Count
8	0.219	15,068
9	0.407	28,050
10	0.243	16,763
11	0.124	8517
Alt Specialized Program	0.007	505
Total Apply		68,903
Total Never Apply		451,464

Note: First time applicants are defined as the first application made by a student after 2005 for students who did not apply in 2005. 2005 is excluded since we cannot see applications made before 2005, and thus we cannot distinguish first-time applicants from repeat applicants in 2005. Nonapplicants are defined as students in grades 8–11 and alternative special education who never apply between 2005 and 2008.

Table 3. Regents exam outcomes in school year following SYEP (applications 2005–2008).

	N	Mean	Std. Dev.	Min	Max
Attempt any exam	134,366	0.72	0.45	0	1
Number attempted	134,366	1.76	1.52	0	8
Pass any exam (65+)	134,366	0.55	0.50	0	1
Number of exams passed (55+)	134,366	1.43	1.39	0	8
Number of exams passed (65+)	134,366	1.13	1.28	0	8
Number of exams passed (75+)	134,366	0.58	0.98	0	6
Avg. Z-score	96,200	−0.14	0.82	−7.19	2.32

Note: Sample includes all applications for students expected to be in high school following SYEP. Applications are omitted if the student submits multiple applications or in ungraded special education following SYEP. Applications to vulnerable youth programs, programs based out of the city, or programs with a greater than 99% or less than 0% selection rate are omitted.

Table 4. SYEP take-up rates (2005–2008).

	Fraction of Lottery Winners That Worked	Number of Winners
2005	82.1	15,544
2006	83.5	17,165
2007	73.4	19,296
2008	74.4	19,963
Total	77.9	71,968

Note: Sample includes all applications for students expected to be in high school following SYEP. Applications are omitted if the student submits multiple applications or in ungraded special education following SYEP. Applications to vulnerable youth programs, programs based out of the city, or programs with a greater than 99% or less than 0% selection rate are omitted.

4.4 *Testing lottery randomization*

In order to evaluate the possibility that admission to the program is not random, we estimated the effect of winning the lottery on each preexisting student characteristic (8th grade test scores, gender, race, free lunch status). If winning the lottery is random, it should be uncorrelated with any characteristic of the student at the time of application. Recall that each year, each CBO conducted its own separate lottery and, therefore, we need to test the *joint* hypothesis that all CBO lottery outcomes are unrelated to student characteristics. Conducting a single test where we treat all separate CBO lotteries as a single lottery likely biases the test. We test the randomization for each program year separately and conduct cross-equation tests.[14] Specifically, for each program year, and for each observed characteristic, we regress each characteristic on a full set of indicators for CBOs and indicators for winning the lottery interacted with CBO. Table 5 provides the results from a joint cross-equation, cross-model F-test that all treatment-by-CBO interaction coefficients are equal to zero. The results indicate that we cannot reject the hypothesis that the lottery was random at conventional significance levels.[15]

We also conducted a test of lottery randomization by testing whether winning the lottery predicts *pre*-SYEP academic outcomes (Table 13). Because this falsification test

Table 5. Lottery randomization results.

	2005	2006	2007	2008
F	0.99	1.01	0.99	1.04
Prob >F	0.56	0.44	0.57	0.19

Note: We test lottery randomization by regressing each student characteristic on a full set of lottery fixed effects and lottery fixed effects interacted with lottery outcome. We test the restriction that all lottery-by-outcome coefficients are zero. Recall that each year, each CBO conducted its own separate lottery and, therefore, we need to test the joint hypothesis that all CBO lottery outcomes are unrelated to student characteristics. Conducting a single test in which we treat all separate lotteries as a single lottery likely biases the test.

[14] Conducting the tests in this way is for convenience. We could also estimate a single regression in which we include CBO × year and CBO × year × lottery win variables.

[15] We also performed a related test in which we separately regress each student covariate on a full set of lottery fixed effects and an indicator for winning any lottery. Results are presented in the Online Supplementary Material (Schwartz, Leos-Urbel, McMurry, and Wiswall (2021)), Appendix Table B.1, and we cannot reject the hypothesis that student covariates are unrelated to lottery outcomes within lotteries.

uses the same outcomes as in our main analysis, we discuss the results of this test below, after the presentation of the main results. In short, on the basis of this falsification test, we cannot reject the hypothesis that the lottery was in fact random.

5. Empirical strategy

This paper investigates the impact of SYEP on student academic success in the school year following SYEP participation, exploiting the random assignment of program participants. By comparing academic outcomes of students offered SYEP placements (the treatment group) to outcomes of students not offered placements (control group), we derive intent-to-treat (ITT) estimates of the impact of SYEP. Since we also have data on whether the student actually participated in an SYEP program and the extent of this involvement, we can also estimate treatment effects of program participation among those who apply. Our key outcomes are student-level measures of attempting, passing, and performance (test scores) on the New York State standardized high school exams, including exams in Mathematics, English, History, and Science. Importantly, because SYEP participation is allocated via lottery, we are able to obtain causal estimates. If each SYEP lottery is random and there is no differential attrition, within any individual lottery a simple comparison of sample means on the outcome of interest between those offered an opportunity to participate in SYEP (treatment group) and those not (control group) provides unbiased estimates of the intent-to-treat effect, where the treatment is participating in SYEP. In our analyses, the comparison group is the set of students who applied to SYEP in a particular summer, but who were not offered a placement. These students should be otherwise similar to the students in the treated group across all dimensions and, most importantly, similar in the distribution of unobserved characteristics, such as motivation and other noncognitive attributes. As discussed above, we conduct several tests of the randomization of the lottery, including a standard test based on comparing observed characteristics of the lottery winners and losers, and a second test, a falsification test, using whether a lottery win predicts *prior* year outcomes. We cannot reject the hypothesis that the lottery is random.

5.1 *Intent-to-treat (OLS)*

We begin with an analysis using an indicator for winning the lottery as the variable of interest to estimate an intent-to-treat effect. To construct the estimating equations it is important to recall that there is not just one SYEP lottery each year, but that each Community Based Organization (CBO) has a separate lottery. As described above, each CBO is associated with a potentially different set of jobs and programs.

Let Y_{itgbc} be the outcome of interest for student i, year t, grade level g, who applied to CBO b, and from an initial application cohort c. The initial application cohort c is defined as the grade and year of initial application.[16] Note that given some students apply to SYEP more than once and repeat grades, cohort is not collinear with grade and year.

[16]There are 24 unique first time application cohorts, for example, first-time applicants who were in 9th grade in year 2005 is one cohort, 10th grade in year 2005 is another, and so on. By including these, we control for any cohort specific factors that may shape the first time applicant pool and/or their outcomes.

Each of our outcomes is specified as

$$Y_{itgbc} = \beta \text{win}_{ibt} + X'_{igt}\alpha + \delta_{bt} + \gamma_c + \mu_g + \nu_{itgbc}, \tag{1}$$

where win_{ibt} takes a value of 1 if student i won CBO b's lottery in period t and was made an offer to participate in SYEP and 0 if he/she was not. Note the timing: the lottery in calendar year t associated with the win_{ibt} variable is for the summer *before* the academic year over which the outcome Y_{itgbc} occurs.[17] X_{igt} is a vector of student characteristics which may influence student performance, such as gender, race/ethnicity, free and reduced price lunch eligibility, limited English proficiency, special education status, and ESL status. X_{igt} is potentially grade-varying as students change their free lunch eligibility, ESL and other statuses as they progress through the school system.[18] δ_{bt} are fixed effects for each CBO interacted by calendar year. These fixed effects index each individual lottery and program offered by each CBO, allowing us to control for potential differences in the selection rates and applicant pools across CBOs and years. γ_c are cohort fixed effects, based on a student's first year of applying to SYEP and grade in the school year prior to first applying to SYEP. These fixed effects absorb any mean differences in cohort "quality" across the various application cohorts. μ_g are grade specific fixed effects which absorb any grade level differences in academic outcomes as students progress through school. ν_{itgbc} is the remaining residual error.[19]

In this model, β is the primary parameter of interest and captures the effect of being randomly offered (via lottery) a placement in SYEP. We estimate β using OLS. Below we consider various forms of heterogeneity in the impacts of SYEP, where the effects of SYEP vary by characteristics of the student and by the number of times applied to and participated in SYEP.

5.2 *Treatment-on-the-treated (2SLS)*

Because our data include not only lottery results (whether the student wins the lottery and is offered an SYEP placement), but also whether the lottery winners in fact participated in SYEP, we can estimate a second set of models using SYEP participation as the treatment variable and the lottery win variable as an instrument:

$$Y_{itgbc} = \breve{\beta}\text{SYEP}_{itgbc} + X'_{igt}\breve{\alpha} + \breve{\delta}_{bt} + \breve{\gamma}_c + \breve{\mu}_g + \breve{\nu}_{itgbc}, \tag{2}$$

$$\text{SYEP}_{itgbc} = \psi \text{win}_{ibt} + X'_{igt}\omega + \zeta_{bt} + \theta_c + \lambda_g + \epsilon_{itgbc}, \tag{3}$$

[17] For the test score outcomes, which are mainly recorded in June at the end of the academic year, the spacing between SYEP participation, in the summer before, and these outcomes is 9–11 months.

[18] The student characteristics vector is indexed with calendar time t and grade g because some characteristics change over time when students repeat a grade. For example, if a student repeats a grade they could come back the next year English proficient, when they were not the year previous. Out of the 134,366 students in the analysis sample, 18,959 students repeat a grade.

[19] Note that although covariates are not necessary to derive unbiased impact estimates when treatment is randomly assigned, including additional covariates can improve the small sample properties if the reduction in residual error variance outweighs the increase in imprecision due to the estimation of additional parameters. Given our very large sample sizes, it would seem clear that the reduction in residual error variance is the far more important factor. See Bloom (2006) for some discussion of this issue.

where SYEP_{itgbc} is an indicator variable equal to 1 if student i, in year t, grade g, cohort c participated in SYEP through CBO b, and 0 otherwise. win_{ibt}, as defined above, is the indicator of winning the lottery and being offered admission into SYEP. Equations (2) and (3) form a Two Stage Least Squares (2SLS) system, with equation (3) the first stage for the second stage given in equation (2). If the lottery is random, then winning the lottery serves as a valid instrument for participating in SYEP.

Given that about 73–84% of participants actually participated in the SYEP program if they won the lottery, the 2SLS estimate of SYEP participation $\breve{\beta}$ should be about a third larger than the intent-to-treat effect estimate β in equation (1). Because some individuals may not participate in SYEP even if they are offered admission (win the lottery), $\breve{\beta}$ identifies the average effect of the SYEP program on the treated (the treatment-on-the-treated, TOT), rather than the average effect in the population of applicants. β from the intent-to-treat analysis, on the other hand, identifies the average effect of being offered an SYEP placement. Both treatment parameters are average effects over the same complier population but differ in their relative magnitudes. We return to the issue of interpreting the magnitude of the estimates below.

6. Results

In this section, we present our baseline results. We examine the effects of SYEP using OLS (ITT) estimates with the lottery randomization variable directly and using the lottery as an instrument in an instrumental variables 2SLS (TOT) analysis. The next section examines heterogeneity in the effects of SYEP participation.

6.1 *OLS (intent-to-treat) results*

Table 6 presents results for models in which we estimate the impact of winning the SYEP lottery on Regents exam outcomes in the following school year. Because the variable of interest we use is the randomized lottery result, the OLS estimator is unbiased and consistent for the intent-to-treat effect. All models also include demographic controls including free and reduced-price lunch eligibility, race/ethnicity, gender, special education status, and Limited English Proficiency, as well as CBO, grade, and cohort fixed effects, as described above.[20]

We use seven key measures of academic success related to test-taking and test-performance (passing and z-scores). Our models examine performance across all Regents exams in the school year following SYEP application. These outcomes all capture important measures of educational progress, effort, and ultimately success. In addition to being a necessary precondition for graduation, attempting the Regents exams may also be a signal of academic interest, engagement, and effort. If participation in SYEP encourages students to increase their school effort, they may elect to take more Regents exams than the minimum required for graduation, potentially improving their chances at

[20]Results are similar if one drops the student level covariates from the regressions, as would be expected given the lottery is random. The initial application cohort c is defined as the grade $\times$ year of initial application. Given some students apply to SYEP more than once and repeat grades, cohort is not collinear with grade and year.

graduating from high school and improving their preparation for post-secondary study. Further, to the degree that participation in SYEP encourages academic effort, there may be an improvement in student performance on these exams—both in terms of passing and the actual score—if students are more attentive in class or spend more time studying and preparing for exams.[21]

Column 1 of Table 6 indicates that winning the SYEP lottery has a small positive and significantly different from zero (at the 10% level) effect on whether students attempt at least one Regents exam. Winning the SYEP lottery increases the probability of attempting any Regents exam in the following year by 0.4 percentage points. To get a sense of the magnitude of this effect, Table 3 indicates that in years following SYEP application, the average probability a student attempted any Regents exam was 72%. Column 2 indicates a small statistically significant positive effect of winning the SYEP lottery on the number of exams attempted—an increase of 0.021 exams from a baseline of 1.76 exams attempted on average (see Table 3).

In addition to a positive effect of increasing the Regents exam attempts, we also find that SYEP improved test performance. Columns 3 and 5 indicate that SYEP lottery winners experienced a small significant increase in passing any Regents exam (at the 65 score or higher), as well as in the number of exams passed. Column 4 finds a small significant increase in the number of exams with a score of 55 or higher, and Column 6 indicates a small but not significant effect on the number of exams with a score of 75 or higher, an outcome which represents a high level of achievement. Finally, Column 7 indicates a small increase in the mean standardized scores on these exams by about 0.008 standard deviations, which is significantly different from 0 at the 10% level (p-value 0.08). Note that the sample size for this outcome is among those students who took tests. Taken together, these results suggest that SYEP has a small positive effect on taking and passing Regents exams.[22]

6.2 2*SLS (treatment-on-the-treated) results*

The results in Table 6 are OLS estimates for the intent-to-treat effect. Given that about 73–84% of students who are offered an SYEP placement (won the lottery) take-up the program and actually participate, the effects of program participation are higher than the OLS results above indicate. We next turn to instrumental variable estimates using winning the lottery as an instrument for SYEP participation, as described above. Table 7

[21] In interpreting these results, note that the effects of SYEP on test taking performance comes through two channels. First, SYEP induces more students to take tests. Second, SYEP can improve performance on tests for two groups of students: infra-marginal students who would have taken the test anyway, even in the absence of SYEP; and for marginal students who are induced to attempt the test by SYEP. If this marginal group of test takers is of sufficiently low ability relative to the inframarginal students who would always take the tests, then the SYEP effect of inducing lower ability students to attempt more tests could result in a 0 or negative average effect of SYEP on test performance.

[22] We also perform an F-test against the null hypothesis that all treatment effects for outcomes 1–6 are jointly zero. Appendix Table B.12 (Column 1) shows that we can reject the null hypothesis of no effect. We consider only outcomes 1–6 as the seventh outcome, average z-score, is undefined for those who took no exams, and thus the sample for this last group differs from that for outcomes 1–6.

Table 6. Intent-to-treat estimates.

	Dependent Variable:						
	Any Attempt (1)	N. Attempts (2)	Any Pass 65 (3)	N. Pass 55 (4)	N. Pass 65 (5)	N. Pass 75 (6)	ZScore (7)
Select	0.004	0.021	0.007	0.026	0.018	0.007	0.008
	(0.002)	(0.008)	(0.003)	(0.007)	(0.006)	(0.005)	(0.004)
Female	0.019	0.091	0.017	0.078	0.045	0.023	0.001
	(0.002)	(0.007)	(0.003)	(0.007)	(0.006)	(0.005)	(0.005)
Black	0.011	−0.007	−0.019	−0.067	−0.123	−0.204	−0.149
	(0.007)	(0.020)	(0.007)	(0.019)	(0.018)	(0.016)	(0.013)
Asian	0.022	0.150	0.017	0.154	0.171	0.192	0.059
	(0.008)	(0.023)	(0.008)	(0.023)	(0.022)	(0.020)	(0.015)
Hispanic	−0.004	−0.015	−0.011	−0.035	−0.080	−0.147	−0.091
	(0.007)	(0.021)	(0.007)	(0.020)	(0.018)	(0.016)	(0.013)
Free lunch	−0.036	−0.079	−0.053	−0.105	−0.115	−0.078	−0.074
	(0.004)	(0.012)	(0.004)	(0.012)	(0.011)	(0.009)	(0.007)
Red lunch	0.010	0.063	−0.002	0.036	0.018	0.005	−0.024
	(0.005)	(0.016)	(0.006)	(0.015)	(0.014)	(0.012)	(0.009)
LEP	0.186	0.681	0.128	0.378	0.223	0.081	−0.058
	(0.008)	(0.029)	(0.010)	(0.026)	(0.024)	(0.017)	(0.018)
ESL not LEP	0.019	0.085	0.010	0.074	0.064	0.076	−0.003
	(0.011)	(0.034)	(0.011)	(0.031)	(0.027)	(0.019)	(0.022)
Spec ed	−0.010	−0.094	−0.133	−0.307	−0.283	−0.112	−0.349
	(0.005)	(0.013)	(0.005)	(0.011)	(0.010)	(0.006)	(0.010)
Age	−0.117	−0.360	−0.121	−0.325	−0.286	−0.132	−0.102
	(0.002)	(0.006)	(0.002)	(0.005)	(0.004)	(0.003)	(0.004)
Zread	0.001	−0.002	0.051	0.092	0.171	0.221	0.245
	(0.002)	(0.006)	(0.002)	(0.005)	(0.005)	(0.004)	(0.004)
Zmath	0.045	0.156	0.101	0.258	0.298	0.266	0.294
	(0.002)	(0.006)	(0.002)	(0.005)	(0.005)	(0.004)	(0.004)
CBO × year FE?	Y	Y	Y	Y	Y	Y	Y
Cohort FE?	Y	Y	Y	Y	Y	Y	Y
Grade FE?	Y	Y	Y	Y	Y	Y	Y
Observations	134,366	134,366	134,366	134,366	134,366	134,366	96,200
R^2	0.169	0.281	0.235	0.290	0.321	0.340	0.395

Note: Heteroskedastic robust standard errors clustered at the student-level. Students in 12th grade, below 8th grade, and in ungraded special education are excluded. Cohort is an indicator for the year of first application to SYEP interacted with the grade of the student when first applied. There are 24 unique cohorts in the sample. Limited English Proficiency (LEP) is determined by score on the Language Assessment Battery exam. Zread and Zmath are 8th grade state test scores, standardized by grade and year of administration. Grade is current grade level in school which includes 8–11th grade and an additional category for alternative specialized programs (e.g., GED programs).

displays the 2SLS estimates of the TOT impact on test taking and performance. These results indicate that the average effects of participating in SYEP are small and positive, and these effects are approximately 1.2–1.4 times greater than the OLS (ITT) estimates reported in Table 6.

Table 7. Treatment-on-the-treated estimates.

	Dependent Variable:						
	Any Attempt (1)	N. Attempts (2)	Any Pass 65 (3)	N. Pass 55 (4)	N. Pass 65 (5)	N. Pass 75 (6)	ZScore (7)
Worked	0.005	0.026	0.009	0.033	0.023	0.010	0.010
	(0.003)	(0.010)	(0.003)	(0.009)	(0.008)	(0.006)	(0.006)
Female	0.019	0.091	0.017	0.078	0.045	0.023	0.001
	(0.002)	(0.007)	(0.003)	(0.007)	(0.006)	(0.005)	(0.005)
Black	0.011	−0.008	−0.019	−0.069	−0.124	−0.205	−0.150
	(0.007)	(0.020)	(0.007)	(0.019)	(0.018)	(0.016)	(0.013)
Asian	0.022	0.150	0.017	0.155	0.171	0.192	0.059
	(0.008)	(0.023)	(0.008)	(0.023)	(0.022)	(0.020)	(0.015)
Hispanic	−0.004	−0.016	−0.011	−0.035	−0.081	−0.147	−0.091
	(0.007)	(0.021)	(0.007)	(0.020)	(0.018)	(0.016)	(0.013)
Free lunch	−0.036	−0.079	−0.053	−0.105	−0.115	−0.078	−0.074
	(0.004)	(0.012)	(0.004)	(0.012)	(0.011)	(0.009)	(0.007)
Red lunch	0.010	0.063	−0.002	0.036	0.017	0.005	−0.024
	(0.005)	(0.016)	(0.006)	(0.015)	(0.014)	(0.012)	(0.009)
LEP	0.186	0.682	0.128	0.379	0.223	0.081	−0.058
	(0.008)	(0.029)	(0.010)	(0.026)	(0.024)	(0.017)	(0.018)
ESL not LEP	0.019	0.086	0.010	0.075	0.064	0.077	−0.003
	(0.011)	(0.034)	(0.011)	(0.031)	(0.027)	(0.019)	(0.022)
Spec ed	−0.010	−0.094	−0.133	−0.308	−0.283	−0.112	−0.349
	(0.005)	(0.013)	(0.005)	(0.011)	(0.010)	(0.006)	(0.010)
Age	−0.117	−0.360	−0.121	−0.324	−0.285	−0.131	−0.102
	(0.002)	(0.006)	(0.002)	(0.005)	(0.004)	(0.003)	(0.004)
Zread	0.001	−0.002	0.051	0.092	0.171	0.221	0.245
	(0.002)	(0.006)	(0.002)	(0.005)	(0.005)	(0.004)	(0.004)
Zmath	0.045	0.156	0.101	0.258	0.298	0.266	0.294
	(0.002)	(0.006)	(0.002)	(0.005)	(0.005)	(0.004)	(0.004)
CBO $\times$ year FE?	Y	Y	Y	Y	Y	Y	Y
Cohort FE?	Y	Y	Y	Y	Y	Y	Y
Grade FE?	Y	Y	Y	Y	Y	Y	Y
Observations	134,366	134,366	134,366	134,366	134,366	134,366	96,200
R^2	0.06	0.064	0.133	0.117	0.169	0.213	0.325

Note: Heteroskedastic robust standard errors clustered at the student-level. Students in 12th grade, below 8th grade, and in ungraded special education are excluded. Cohort is an indicator for the year of first application to SYEP interacted with the grade of the student when first applied. There are 24 unique cohorts in the sample. Limited English Proficiency (LEP) is determined by score on the Language Assessment Battery exam. Zread and Zmath are 8th grade state test scores, standardized by grade and year of administration. Grade is current grade level in school which includes 8–11th grade and an additional category for alternative specialized programs (e.g., GED programs).

7. Effect heterogeneity

The models estimated above assume a constant effect of SYEP on academic outcomes. We next explore heterogeneity in the effects of SYEP participation in two dimensions:

(i) by student observable characteristics such as gender, race, free lunch status, and prior academic achievement, and (ii) by the number of times previously participated in SYEP.

7.1 *Heterogeneity by student characteristics*

We estimate heterogeneity by student characteristics by generalizing the 2SLS estimator in equations (2) and (3), allowing an interaction between SYEP participation and student characteristics in X (gender, race, English ability, free lunch, age, and 8th grade test scores), and using as instruments lottery results interacted with those same variables. The main equation we estimate is then

$$Y_{itgbc} = \breve{\beta}\text{SYEP}_{itgbc} + \text{SYEP}_{itgbc} \times X'_{igt}\breve{\Gamma} + X'_{igt}\breve{\alpha} + \breve{\delta}_{bt} + \breve{\gamma}_c + \breve{\mu}_g + \breve{\nu}_{itgbc}, \tag{4}$$

where $\breve{\Gamma}$ is a vector of treatment-by-covariate interaction coefficients. For each outcome Y, we then predict the SYEP *expected benefit* (EB) for each student based on their covariates. For a student with covariates $X = x$, their SYEP expected benefit is

$$\text{EB} = E(Y \mid X = x, \text{SYEP} = 1) - E(Y \mid X = x, \text{SYEP} = 0) = \widehat{\breve{\beta}} + x'\widehat{\breve{\Gamma}}.$$

Tables 8a–8b presents the estimated distribution of SYEP effects. In Table 8a, we present the results for students who applied to SYEP. The top row reports the LATE, the 2SLS estimate from Table 7. The next row reports the average expected benefit, averaging

Table 8a. LATE and expected benefit (appliers only).

	Any Attempt	N. Attempts	Any Pass 65	N. Pass 55	N. Pass 65	N. Pass 75	ZScore
LATE	0.005	0.026	0.009	0.033	0.023	0.01	0.01
	(0.002)	(0.007)	(0.002)	(0.007)	(0.006)	(0.004)	(0.004)
Avg. EB	0.004	0.018	0.011	0.025	0.018	0.007	0.014
	(0.003)	(0.009)	(0.003)	(0.008)	(0.007)	(0.006)	(0.006)
P01 EB	−0.029	−0.107	−0.021	−0.066	−0.069	−0.055	−0.073
	(0.009)	(0.021)	(0.012)	(0.031)	(0.032)	(0.019)	(0.028)
P10 EB	−0.013	−0.051	−0.007	−0.029	−0.029	−0.025	−0.02
	(0.004)	(0.012)	(0.004)	(0.012)	(0.011)	(0.008)	(0.008)
P50 EB	0.003	0.016	0.009	0.023	0.017	0.007	0.013
	(0.003)	(0.009)	(0.003)	(0.008)	(0.007)	(0.006)	(0.006)
P90 EB	0.023	0.088	0.031	0.079	0.065	0.039	0.044
	(0.004)	(0.011)	(0.004)	(0.011)	(0.01)	(0.008)	(0.01)
P99 EB	0.046	0.175	0.05	0.136	0.108	0.07	0.092
	(0.007)	(0.02)	(0.008)	(0.03)	(0.021)	(0.017)	(0.024)
P50 EB − P10 EB	0.016	0.066	0.016	0.051	0.046	0.032	0.033
	(0.003)	(0.008)	(0.003)	(0.008)	(0.008)	(0.006)	(0.008)
P90 EB − P50 EB	0.02	0.072	0.022	0.056	0.048	0.032	0.031
	(0.003)	(0.009)	(0.004)	(0.009)	(0.008)	(0.006)	(0.008)

Note: For each outcome, expected benefit (EB) is the predicted treatment effect of SYEP given student covariates and the 2SLS estimates of heterogeneous treatment effects by student covariates. Bootstrap standard errors in parantheses are calculated with 1000 bootstrap iterations, block clustered at the student level. Standard errors of LATE estimates differ from those in Table 7 as they are bootstrapped instead of asymptotic estimates.

Table 8b. Expected benefit (nonappliers only).

	Any Attempt	N. Attempts	Any Pass 65	N. Pass 55	N. Pass 65	N. Pass 75	ZScore
Avg. EB	0.006	0.027	0.012	0.026	0.015	0.007	0.009
	(0.004)	(0.012)	(0.004)	(0.011)	(0.01)	(0.008)	(0.007)
P01 EB	−0.033	−0.119	−0.026	−0.09	−0.1	−0.066	−0.109
	(0.012)	(0.031)	(0.015)	(0.04)	(0.039)	(0.026)	(0.034)
P10 EB	−0.012	−0.045	−0.009	−0.034	−0.039	−0.03	−0.035
	(0.005)	(0.015)	(0.006)	(0.016)	(0.017)	(0.015)	(0.012)
P50 EB	0.005	0.026	0.01	0.024	0.017	0.009	0.013
	(0.003)	(0.011)	(0.004)	(0.01)	(0.009)	(0.007)	(0.006)
P90 EB	0.025	0.098	0.035	0.09	0.069	0.043	0.045
	(0.005)	(0.017)	(0.006)	(0.014)	(0.012)	(0.009)	(0.011)
P99 EB	0.051	0.196	0.057	0.155	0.113	0.075	0.103
	(0.008)	(0.028)	(0.01)	(0.043)	(0.029)	(0.018)	(0.032)
P50 EB − P10 EB	0.017	0.071	0.019	0.058	0.056	0.039	0.048
	(0.004)	(0.011)	(0.005)	(0.012)	(0.013)	(0.011)	(0.012)
P90 EB − P50 EB	0.02	0.073	0.025	0.066	0.052	0.035	0.033
	(0.004)	(0.012)	(0.005)	(0.011)	(0.009)	(0.007)	(0.008)

Note: For each outcome, expected benefit (EB) is the predicted treatment effect of SYEP given student covariates and the 2SLS estimates of heterogeneous treatment effects by student covariates. Bootstrap standard errors in parantheses are calculated with 1000 bootstrap iterations, block clustered at the student level.

over the joint distribution of X variables in the sample of SYEP appliers. The difference between the LATE and average EB estimates reflects the implicit weighting of the 2SLS estimator over the distribution of compliers; a group whose distribution of characteristics may not be the same as that of the entire sample.

The next several rows of Table 8a report the percentiles of the distribution of SYEP expected benefits, and the large differences in EB provide evidence of relevant effect heterogeneity. We estimate that for 23–41% of appliers, SYEP participation would *negatively* affect their academic outcomes (depending on the outcome), reflecting a potential tradeoff of SYEP employment with academic participation and performance. We also estimate that the EB of SYEP participation for some students could be several times higher than the 2SLS/LATE estimates.

Table 8b reports the EB distribution for the sample of nonappliers. Although this sample did not participate in SYEP, we can predict effects for this group using their observed covariates. The distribution of benefits for the nonappliers is similar to that for appliers, suggesting that nonappliers are not selectively lower EB individuals, at least based on observable characteristics.

7.2 *Heterogeneity by past participation*

An important feature of the SYEP program is that students are allowed to participate in multiple years, and access to the program through the lottery process does not depend on past participation: each lottery outcome is unrelated to lotteries in the previous and subsequent years. Thus, there are a group of students who participate in t and apply again in $t+1$, and among this group of previous participants, a randomly-assigned group will be offered a placement in $t+1$. We can exploit this feature of the lottery random-

Table 9. Number of applications and selections.

N Apps	Percent	N Students	N Wins	Percent	N Students
			0	39.27	37,786
1	68.46	65,868	1	48.45	46,616
2	24.19	23,277	2	10.59	10,185
3	6.58	6332	3	1.59	1526
4	0.77	737	4	0.10	101
Total	100	96,214	Total	100	96,214

Note: Sample includes all applications for students expected to be in high school following SYEP. Applications are omitted if the student submits multiple applications or in ungraded special education following SYEP. Applications to vulnerable youth programs, programs based out of the city, or programs with a greater than 99% or less than 0% selection rate are omitted.

ization to estimate the effect of an additional year of SYEP participation, conditional on previous participation, for specific subgroups of repeat appliers.

Table 9 provides information regarding patterns in applications and selection by the SYEP lottery over the 4-year study period. While 68% of the sample applied in only 1 year, 32% applied more than once, with 24% applying twice, 6% applying three times, and less than 1% applying four times. Among these applicants, 39% never won the SYEP lottery, 48% won once, 11% twice, and about 1.6% three times.

In general, the estimated impact of SYEP may vary for those who had applied (and participated) in previous years for two main reasons. First, for those who apply, win the lottery, and participate in multiple years, there may be a dosage effect, in which participating in SYEP for more than one summer has a different effect than participating once. Second, although the SYEP lottery does not take into account whether a student had applied or participated before, the decision to apply for multiple years itself is not random, and it may be that the types of students who choose to apply for multiple years have different benefits from the program, even in the first year of participation. Given this selection by application behavior, for which we have no lottery randomization or other suitable instrument, our estimates reflect the *local* treatment effects for the subgroups who choose to apply for SYEP multiple times. Of course, our estimates of SYEP effects, even for the first lottery, are necessarily local to the population who applies at all to SYEP, and they may not extrapolate to the nonapplicant group. This is a common feature of many social programs: while lottery-based exogenous variation provides credible identification of particular causal effects, these effects are always local to the endogenous applicant group.

To estimate the effect of the second and third year of SYEP on those groups who previously participated once and twice, respectively, we reestimate our baseline intent-to-treat model (equation (1)) but limit the sample based on application and participation history. We divide the sample into three groups by application status and SYEP participation: *Group* 1 (first-time applicants), *Group* 2 (one-time past participators and second-time applicants), and *Group* 3 (two-time past participators and third-time applicants). To be clear, Groups 2 and 3 are students that had previously applied for SYEP, won the lottery, and participated in SYEP (once in the case of Group 2 and twice in the case of Group 3). To simplify the notation, we ignore the other control variables and drop the lottery/CBO, grade, and cohort effects in the equation specifications below, but we include all of these variables in the models we estimate. For each group of applicants k, the outcome for student i in period t is

$$Y_{ikt} = \beta_k \text{win}_{ikt} + \nu_{ikt}, \tag{5}$$

where win_{ikt} is the dummy variable for winning the lottery in summer t, and Y_{ikt} is the outcome in the academic year following that summer (e.g., if win_{ikt} is for Summer 2006, then Y_{ikt} is for the following academic year, Fall 2006–Spring 2007). The coefficient on the win_{ikt} variable, β_k, provides the effect of being offered additional SYEP placements for the kth group of students.

Table 10 presents estimates for the 3 groups. Panel A indicates no significant effect of winning the lottery for all first-time applicants, with one exception—a small significant increase in the number of exams passed with a score of 55 and 65 or higher. In contrast,

Table 10. Hetereogenous effects by past participation.

	Dependent Variable:						
	Any Attempt (1)	N. Attempts (2)	Any Pass 65 (3)	N. Pass 55 (4)	N. Pass 65 (5)	N. Pass 75 (6)	ZScore (7)
Panel A: First-time applicants (Group 1)							
Select	−0.0004	0.016	0.004	0.021	0.016	0.005	0.006
	(0.003)	(0.010)	(0.004)	(0.010)	(0.009)	(0.007)	(0.006)
CBO × Year FE?	Y	Y	Y	Y	Y	Y	Y
Cohort FE?	Y	Y	Y	Y	Y	Y	Y
Grade FE?	Y	Y	Y	Y	Y	Y	Y
Observations	66,973	66,973	66,973	66,973	66,973	66,973	49,143
R^2	0.169	0.299	0.244	0.308	0.333	0.355	0.416
Panel B: Second-time applicants, one-time past participators (Group 2)							
Select	0.016	0.034	0.015	0.033	0.009	0.016	0.004
	(0.006)	(0.021)	(0.007)	(0.019)	(0.017)	(0.013)	(0.012)
CBO × year FE?	Y	Y	Y	Y	Y	Y	Y
Cohort FE?	Y	Y	Y	Y	Y	Y	Y
Grade FE?	Y	Y	Y	Y	Y	Y	Y
Observations	17,836	17,836	17,836	17,836	17,836	17,836	13,195
R^2	0.193	0.286	0.250	0.298	0.336	0.352	0.387
Panel C: Third-time applicants, two-time past participators (Group 3)							
Select	0.003	0.063	0.012	0.088	0.067	0.015	0.019
	(0.017)	(0.051)	(0.017)	(0.044)	(0.038)	(0.026)	(0.030)
CBO × year FE?	Y	Y	Y	Y	Y	Y	Y
Cohort FE?	Y	Y	Y	Y	Y	Y	Y
Grade FE?	Y	Y	Y	Y	Y	Y	Y
Observations	3129	3129	3129	3129	3129	3129	2061
R^2	0.207	0.333	0.230	0.309	0.324	0.324	0.354

Note: Sample limited to outcomes in 2007–2009. Sample includes all applications for students expected to be in high school following SYEP. Applications are omitted if the student submits multiple applications or in ungraded special education following SYEP. Applications to vulnerable youth programs, programs based out of the city, or programs with a greater than 99% or less than 0% selection rate are omitted.

Panel B indicates substantial effects of winning the SYEP lottery for the group who has already participated once. That is, while by and large there is no significant effect of winning the lottery when averaging over all first-time appliers, the effect of winning for the second time is statistically and economically significant for those who participate and apply again. Panel C shows even larger effects of winning for the third time for the final group of third-time applicants who have participated twice. However, as this group is far smaller than groups 1 and 2, these estimates are noisier.[23] Appendix Table B.12 presents results from a test of the joint restriction that treatment effects for all outcomes are zero.[24] For only the full sample and Group 2 can we reject the null hypothesis of no effect of SYEP.

Interpreting these estimates for each subgroup requires some care. Although estimating equation (5) does indeed recover the causal effect of winning the kth lottery, this effect is, as discussed above, local to Group k which has endogenously formed via winning and participating in each previous lottery 1 through $k-1$.[25] β_k may represent a true dosage effect, where the effect of winning additional lotteries has a larger or smaller magnitude than previous lotteries. However, it also might be that Group k has selected into applying for the kth time based on the causal effects they enjoy. Said another way, the SYEP data do not include a randomized multiple treatment arm experiment, in which some groups are randomly offered 0, 1, 2, 3 years of participation in SYEP. Instead, students endogenously apply to multiple lotteries, and only for those students who apply to multiple lotteries do we observe multiple SYEP doses. Appendix A provides more detail and shows more formally that the dosage effect is not identified using this type of data.

However, using the rich set of covariates available to us in the NYCDOE data, we can characterize how Groups 1, 2, and 3 differ along observables. Table 11 shows results comparing Group 1 versus 2 and Group 2 versus 3.[26] Groups 1 and 2 differ substantially in their observable student composition. Relative to first-time applicants (Group 1), second-time applicants who have participated once (Group 2) are more likely to be black, less likely to have limited English proficiency or English as a second language, are older, and have lower 8th grade reading scores. Relative to Group 2, Group 3 students are more likely to be black, are older, and have lower 8th grade reading scores. As equation (5) specifies a homogenous treatment effect within each group, the different composition of students across groups might explain some of the differences seen in Table 10.

To more directly identify how selection-on-observables between Groups 1, 2, and 3 determines the group-specific treatment effect β_k, we use our estimates of heterogenous treatment effects by observable characteristics to calculate, for each outcome, the

[23]Note that the models we are estimating are not panel models per se: each model includes only one student observation. Therefore, the error structure is not clustered at the student level.

[24]As students who do not attempt any exams have no defined average score, the sample in outcomes 1–6 differs (is a strict superset) of the sample for outcome 7, the average score. Therefore, we restrict the F-test in Appendix Table B.12 to outcomes 1–6.

[25]For $k > 1$. Group 1 has endogenously formed via the decision to first apply.

[26]An alternative approach is to test the restriction that mean covariates are the same among all three groups. However, due to the sequential way in which each group is formed, it is more natural to directly test Group 2 against Group 1 and Group 3 against Group 2.

Table 11. Balance test—hetereogenous effects groups.

	Group 2 Less Group 1	95% CI	Group 3 Less Group 2	95% CI
Female	−0.003	(−0.011, 0.005)	−0.008	(−0.027, 0.011)
White	−0.010	(−0.013, −0.007)	0.001	(−0.006, 0.007)
Black	0.065	(0.057, 0.073)	0.030	(0.011, 0.048)
Asian	−0.025	(−0.03, −0.02)	−0.008	(−0.019, 0.002)
Hispanic	−0.030	(−0.038, −0.022)	−0.022	(−0.039, −0.005)
Free lunch	−0.011	(−0.019, −0.002)	−0.005	(−0.024, 0.013)
Reduced lunch	0.010	(0.004, 0.016)	−0.005	(−0.019, 0.009)
LEP	−0.016	(−0.018, −0.014)	−0.003	(−0.007, 0.001)
ESL not LEP	−0.008	(−0.009, −0.006)	−0.005	(−0.008, −0.001)
Spec Ed	0.016	(0.011, 0.021)	0.007	(−0.005, 0.018)
Age	0.652	(0.636, 0.667)	0.637	(0.611, 0.663)
Zread	−0.037	(−0.052, −0.021)	−0.041	(−0.076, −0.006)
Zmath	−0.005	(−0.021, 0.011)	−0.010	(−0.045, 0.026)

Note: Displayed point estimates are differences in average covariate value between Groups 2 and 1 and Groups 3 and 2, respectively. Group 1 consists of all first-time applicants in years 2006–2008. Group 2 is all students who applied for the second time in 2006–2008 and had applied, won, and participated in the prior year. Group 3 is all students who applied for the third time in 2006–2008 and had applied, won, and participated in each of the 2 years prior.

average expected benefit (EB) in each group. Table 12 shows that for most outcomes, Groups 2 and 3 are made up of students with higher predicted treatment effects than Group 1. This suggests that selection-on-observables in the decision to apply and participate drives some of the differences in causal effects across groups. Therefore, although we cannot identify exactly how much of the different effect estimates are dosage versus selection, we have suggestive evidence that there is clear selection based on observables.

8. Robustness checks

8.1 *Match rates*

As described above, the SYEP program is open to nonstudents and students not enrolled in NYC public schools (enrolled in private religious and nonreligious schools) and, therefore, the match rate between the SYEP program data and the NYCDOE data is about 77–81% depending on the year.

Table 12. Average expected benefit.

Group	Any Attempt	N. Attempts	Any Pass 65	N. Pass 55	N. Pass 65	N. Pass 75	ZScore
1	0.0056	0.0262	0.0099	0.0314	0.0221	0.0095	0.0111
2	0.0059	0.0304	0.0094	0.0374	0.0268	0.0112	0.0111
3	0.0059	0.0335	0.0081	0.0416	0.0289	0.0112	0.0091

Note: For each outcome, expected benefit (EB) is the predicted treatment effect of SYEP given student covariates and the 2SLS estimates of heterogeneous treatment effects by student covariates. Group 1 consists of all first-time applicants in years 2006–2008. Group 2 is all students who applied for the second time in 2006–2008 and had applied, won, and participated in the prior year. Group 3 is all students who applied for the third time in 2006–2008 and had applied, won, and participated in each of the 2 years prior.

We test for whether winning the lottery and being offered a SYEP placement directly affected the match rate by using the full sample of all NYC public school students (matched SYEP applicants and unmatched students). We consider only the sample of first-time applicants to SYEP because, as estimated above, winning the lottery is correlated with second and third applications. Our test consists of regressing a dummy variable for the student being matched on an indicator for winning the lottery and grade and lottery fixed effects. The estimated coefficient on the indicator for whether the student won a SYEP lottery is not statistically significant at conventional levels (p-value = 0.28) and the estimated coefficient is small in magnitude at 0.003 (see Appendix Table B.3). This result indicates that the match rate of SYEP and NYCDOE data is unrelated to the student winning the lottery.

8.2 *Attrition*

We also tested whether winning the SYEP lottery affected whether students remain in the NYC public schools and, therefore, continue to appear in our matched NYCDOE-SYEP data. We define "attrition" as a case in which a student who was in the NYCDOE records in the year prior to applying to SYEP does not appear in the NYCDOE data in the year following the SYEP lottery. Table B.5 reports results from a test of whether winning the SYEP lottery is related to student attrition in the NYCDOE records by replacing the outcome variable in our main estimating Equation (1) with an indicator for attrition. Our estimates indicate that winning the lottery is unrelated to attrition at conventional significance levels across all grade levels.

8.3 *Falsification test*

If the SYEP lottery is truly random, then winning the lottery should be uncorrelated with *past* student outcomes. Using our main specification equation (1), we replace the future outcomes for the academic year following the summer of SYEP lottery offer with past outcomes for the academic year prior to SYEP application. Table 13 reports results

Table 13. Falsification test (outcomes year prior to lottery).

	Dependent Variable:						
	Any Attempt	N. Attempts	Any Pass 65	N. Pass 55	N. Pass 65	N. Pass 75	ZScore
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Select	0.003	0.003	0.002	0.003	−0.001	−0.003	−0.008
	(0.002)	(0.004)	(0.002)	(0.004)	(0.004)	(0.004)	(0.008)
CBO × year FE?	Y	Y	Y	Y	Y	Y	Y
Cohort FE?	Y	Y	Y	Y	Y	Y	Y
Grade FE?	Y	Y	Y	Y	Y	Y	Y
Observations	138,162	138,162	138,162	138,162	138,162	138,162	50,906
R^2	0.477	0.546	0.391	0.513	0.440	0.279	0.157

Note: Sample differs from main analyses due to lagged outcome. Sample includes all applications for students expected to be in high school following SYEP. Applications are omitted if the student submits multiple applications or in ungraded special education following SYEP. Applications to vulnerable youth programs, programs based out of the city, or programs with a greater than 99% or less than 0% selection rate are omitted.

from this falsification test. Across the outcomes we examine, we find that winning the lottery does not have a statistically significant effect on past outcomes, and coefficient estimates are small in magnitude. These results provide additional evidence in favor of the validity of our research design.

9. Discussion

Our estimates suggest that participation in SYEP has, on average, a positive, albeit small, effect on taking and passing the standardized tests administered by New York State to measure progress in high school subjects. The results offer evidence that SYEP improves educational outcomes that have proven stubbornly resistant to interventions. As an example, New York City's Conditional Cash Transfer program offered high school students $600 incentive for each Regents exam passed—up to five—but yielded no significant effect (Riccio, Dechausay, Miller, Nuñez, Verma, and Yang (2013)).[27]

9.1 *Policy implications of effect heterogeneity*

Program evaluation can be thought of as answering three questions: is a particular program effective, do the benefits of the program justify its costs, and what are the mechanisms through which its benefits are realized? For a given program which is unique in its institutional details, it is reasonable to think that the first two questions are of primary importance, and the results above show that answering these questions by estimating homogenous effects alone understates the effectiveness of SYEP for some groups. That is, the estimated average causal effects across all students mask considerable heterogeneity across both student covariates and multiple years of participation. These findings of larger effects of SYEP for some groups warrant further discussion.

First, it bears repeating that when examining heterogeneous treatment effects by observable characteristics, we do not set out to "find" some group that enjoyed a larger benefit than the average effects suggest. Such an exercise is in principle valid, but care must be taken to avoid spurious conclusions generated by multiple hypothesis testing. Instead, we simply ask the question of whether we can detect a nondegenerate distribution of treatment effects, exploiting the rich data on student characteristics and relatively large sample size available to us to estimate causal effects for demographic cells of nontrivial dimension. Indeed, we do find considerable variation in treatment effects that suggests there are students for whom SYEP is very effective. Put another way, modest average effects imply neither a small homogenous benefit of the program nor even a small effect for the marginal student. Efforts to better target this and other similar programs may be fruitful.[28]

[27] Interestingly, larger effects were found for students who were deemed proficient in English Language Arts and Mathematics at the time they enrolled in high school, suggesting this is a subgroup worthy of future investigation in the SYEP analysis.

[28] A recent study (Davis and Heller (2020)) carries out a similar analysis evaluating a Chicago SYEP. These authors use a machine learning model to identify subgroups who benefit from participation, and they find evidence of subgroups for whom the treatment effect is much larger than the ITT. Their method allows

We find a considerable difference in the impact of participating in SYEP the first time and participating the second (or third) time for select groups of students. Disentangling these effects reveals, in fact, little effect of a single year of participation, but larger, positive effects for the second and third year of participation.

It may be that students experience a dosage effect by which they realize larger benefits with additional years of participation, for a variety of possible reasons. Alternatively, these larger effects for those who have participated in the past may be due to self-selection, which we might think of as a particularly inexpensive form of program targeting. Although the SYEP lottery is random in any given year, the decision to apply in subsequent years is not. Thus, students who do not have access to alternate activities or means of finding employment might be more likely to apply for an additional year of SYEP participation. Or, more motivated students may apply year after year, and may benefit more from SYEP. Additionally, the decision to apply to SYEP for a second or third year may be due to a positive experience after the first year of SYEP.

Given these two channels, a finding of positive or stronger effects for multiple participators could be because there are increasing returns to participants for each year students participate, or simply because the estimates reflect the self-selection of students who are most likely to benefit from SYEP in any year. For policymakers, it may not necessarily be crucial to distinguish the two types of effects, at least for some types of policy questions. A finding of a large causal effect on multiple participators, regardless of the mechanism, may indicate that SYEP's decision to allow repeat participators is simply beneficial.

9.2 *Effect sizes*

How large are the effect sizes we estimate? One simple way to measure the effect sizes is to compare them to differences in the same outcomes by salient socioeconomic differences—the disparity in outcomes between white and black students and the disparity between poor (free lunch eligible) and not-poor students. As an example, our intent-to-treat estimate that SYEP improves the likelihood of passing any exam at the 65 threshold by 0.7% is roughly 38% the size of the black–white gap of 1.9% and 14% of the poverty gap of 5.3%.[29] The average effects on the treated group (TOT) are even larger. If allocated only to the disadvantaged group, SYEP would close the race gap in pass rates by almost 49% and the poverty gap by almost 19% with similar effects on the number of exams taken.

9.3 *What does SYEP cost to provide?*

We can obtain a rough estimate of the direct cost of the program as the sum of the wages paid to participants, administrative costs, and the costs for additional program features,

them to characterize the subgroups that benefit, which we explicitly do not attempt. Although the program and outcomes studied in that paper differ from those examined here, we interpret the findings as additional evidence of substantial heterogeneity in the treatment effects of summer programs.

[29] See Appendix Tables B.7–B.8.

such as education components. Drawing on features and experiences from SYEP and other social programs, we estimate each of these factors as follows. SYEP participants are paid New York State minimum wage, set at \$8.75 per hour. Program participants generally work 25 hours per week for 6 weeks, or 150 hours. Thus, payments to SYEP participants may be as high as \$1312.50. Estimates of administrative overhead costs vary, although 15% is commonly used by local governments.[30] Finally, the cost of the supplementary education and training will likely vary by provider or CBO, but previous work has estimated the per participant cost of an educational program at \$650 (Schwartz and Leos-Urbel (2014)).

Taken together, we estimate a cost of slightly more than \$2150 per participant—less than 15% of annual per pupil education spending in NYC. To be clear, this is an estimate of the budgetary cost, that is, the direct outlays paid by the government or funder of the program, the majority of which is essentially a transfer to (predominately low-income) youth participants. Although a comprehensive cost-benefit analysis is outside the scope of this paper, much of the program costs may be offset by the value of work provided to organizations that youth work for and the communities they work in, as well as by the value of the associated improvement in participants' educational outcomes (see, e.g., Chetty, Friedman, and Rockoff (2014)).

10. Conclusions

We use the randomized lottery design of the SYEP to estimate that participation in SYEP has a small positive effect on a variety of test taking and passing outcomes for New York City high school students. The effects of SYEP on test taking are considerably larger for students who had participated in SYEP in prior years, compared to those applying for the first time. This suggests that there may be dosage effects associated with SYEP participation and/or those students most likely to benefit from the program self-select by applying to SYEP for multiple years. Regardless, this analysis indicates that allowing participation in summer jobs programming for multiple years pays dividends for some high school students well beyond the paycheck itself. Indeed, the benefits of this relatively low-cost intervention are likely to substantially exceed the costs, suggesting SYEP may be an important addition to the toolkit for policymakers seeking to improve academic outcomes for high school students. Additional work exploring the persistence of the effects beyond high school, the spillover effects for peers and communities and, in a different vein, the heterogeneity in impacts across job placements and features, is clearly warranted to provide guidance to policymakers adopting summer youth employment programs across the country.

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[30] This is, for example, the overhead rate that the California Department of Education allows for public after-school programs.

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