What is a Summer Job Worth?
The Impact of Summer Youth Employment on Academic Outcomes

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Summer youth employment programs represent a policy lever with implications for youth development, employment, and education. This paper estimates the impact of New York City's Summer Youth Employment Program (SYEP) on educational outcomes in the following school year for a large sample of low-income high school students. The program provides jobs to youth ages 14-21, and due to high demand allocates slots through a lottery. Analyses focusing on 36,550 students who applied in 2007 indicate that SYEP produces small increases in attendance in the following school year, with larger increases for students who may be at greater educational risk; those ages 16 and older with low baseline school attendance. For this group, SYEP also increases the likelihood of attempting and passing statewide high school math and English exams. Findings suggest that although SYEP's explicit goals focus on workforce readiness rather than academics, the program fosters engagement and success in school.

Key words: education, employment, youth development

INTRODUCTION

Many cities offer large-scale publicly funded summer youth employment programs, among them New York City, Washington, DC, and Detroit. These programs aim to engage youth, many of whom would otherwise not be able to secure summer employment, in productive activities that provide new skills, income, and understanding of the workplace. Many summer youth employment program participants are high school students and, while educational attainment is often considered a prerequisite for labor market success, employment experiences themselves may also influence students' educational engagement and achievement. A considerable body of research has examined the relationship between employment and academic outcomes for high school students during the school year, when tradeoffs between work and school are a major consideration. Yet, surprisingly little research has examined the impact of work during the summer, or of summer youth employment programs specifically, on students' educational outcomes.

Research on summer jobs programs is especially salient in the current economic climate, in which the availability of summer employment for teens has decreased considerably and public funding for summer jobs has waxed and waned (Bellotti, Rosenberg, Sattar, Esposito, & Ziegler, 2010; Sum, McLaughlin, & Khatiwada, 2008). For example, in 2009 the American Recovery and Reinvestment Act (ARRA) provided a temporary influx of funding for summer jobs for low-income youth. However by 2012, with limited funding available to pay for summer jobs, the U.S. Department of Labor coordinated the "Summer Jobs +" program which sought companies and non-profit organizations to pledge to provide summer work experiences for youth nationwide.

Research on the relationship between summer youth employment and educational outcomes for high school students sits at the intersection between the youth development, employment, and education policy areas that serve these students. We have long known that experiences outside of school can have important implications for academic success, and that these experiences often differ widely by socio-economic status (Coleman et al., 1966; Lareau, 2003; Rothstein, 2004; Alexander, Olson, & Entwisle, 2007; Duncan, Ludwig, & Magnuson, 2007). Perhaps due to the stubborn persistence of the academic achievement gap despite continued school reform efforts, there appears to be renewed interest among some policy makers and researchers to consider influences outside of the classroom on educational success (Ladd, 2012; Duncan & Murnane, 2011). Further, research indicates that experiences during the summer may be one important contributor to the academic achievement gap that persists along racial and socio-economic lines (Alexander et al., 2007; Cooper, Nye, Charlton, Lindsay, & Greathouse, 1996).

This paper estimates the impact of summer work experiences on high school students' educational outcomes in the following school year using data from New York City's Summer

Youth Employment Program (SYEP). Each year SYEP serves tens of thousands of New York City public school students, as well as thousands of youth who are out-of-school or in other educational settings. SYEP is administered by the city's Department of Youth and Community Development (DYCD), which contracts with community-based organizations throughout the city to place and supervise youth in summer jobs in both the public and private sectors. Participants work and receive training for up to 25 hours per week for 7 weeks during the summer, and receive the state hourly minimum wage.

Due to high demand for jobs through SYEP, the city uses a lottery system in an effort to equitably allocate program slots, which allows for causal estimates of the relationship between summer jobs and academic success. This paper uses SYEP data for 36,550 program applicants in 2007 matched to education files from the NYC Department of Education (NYCDOE). Analyses estimate the impact of summer jobs on high school students' academic engagement and attainment in the following school year. Outcomes of interest include school attendance, statewide high school math and English exams attempted and passed, and scores on these exams in the year following application to SYEP. These analyses indicate that winning the SYEP lottery results in a small increase in attendance in the following school year. Increases are larger for students who may be at greater educational risk; those ages 16 and older with low baseline school attendance. For this group, SYEP also increases the likelihood of attempting and passing optional statewide high school math and English exams.

This paper first provides an overview of the relevant literature and policy context. Next, the paper describes the Summer Youth Employment program and lottery, followed by a description of the sample, data, and research strategy for this study. Finally, the paper presents results, robustness checks, discussion, and conclusions.

LITERATURE AND POLICY CONTEXT

To date, there has been relatively little rigorous evaluation of the impact of summer youth employment programs (LaLonde, 2003). A search of the peer-reviewed literature over the past two decades yields little if any research specifically examining the impact of summer youth employment programs on high school students' academic outcomes. Two reports published by Public/Private Ventures document the findings of experimental studies evaluating summer youth employment programs. An evaluation of a program providing remediation, training, and half-day summer jobs for low-income youth in five cities for two consecutive summers finds that compared to a control group that received only summer jobs, participants demonstrate short-term increases in reading and mathematics test scores but no difference in longer term educational and life outcomes (Walker & Viella-Velez, 1992). A second study, of a program in Philadelphia that helped students find summer jobs and provided academic mentors, finds increases in attaining summer employment but no effects on intermediate-term educational or employment outcomes (McClanahan, Sipe, & Smith, 2004). More recently, Curnan et al. (2010) evaluate the implementation of the American Recovery and Reinvestment Act's summer youth employment initiative but do not examine impacts on education.

Much of the research on high school student employment and its impact on academic and other outcomes has focused solely on work experiences during the school year. Marsh & Kleitman (2005) identify three prominent theories regarding the relationship between work and education during the school year. First, a zero sum theory posits that time devoted to work will necessarily detract from time devoted to school. Second, the threshold theory suggests that working up to a certain amount of hours during the school year may be beneficial but work beyond this threshold may be detrimental. A third theory suggests that work builds character and is therefore beneficial for education. Thus, work may foster behavioral traits associated with academic success such as responsibility, positive work habits, time management skills, determination, and self-confidence (Lillydhal, 1990; Mortimer, 2003), as well as increase financial resources. Indeed, research in this area has largely focused 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 & Pabilonia, 2009). 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).

Much of the early literature on teen employment largely concluded that work was beneficial for high school students, decreasing the likelihood of dropping out and improving academic performance (Entwisle, Alexander, & Olson, 2000; D'Amico & Baker, 1984). Subsequent research suggested that the amount of time that high school students work during the school year has implications for student outcomes. Much of this research suggests that working a moderate number of hours (i.e., less 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 a lot (i.e., more than 20 hours per week) has negative effects (Lillydhal, 1990; Stern & Briggs, 2001). However, these studies may not have adequately addressed the challenge of determining whether any perceived impacts of teen employment on academic outcomes could be due to unmeasured differences between students who choose to work and those who do not.

Some recent research on employment during high school has taken advantage of more sophisticated methods for addressing selection issues. Monahan, Lee, and Steinberg (2011) argue that “the impact of part-time employment on adolescent functioning remains unclear because most studies fail to adequately control for differential selection into the workplace” (p.96). Using propensity score matching Monahan et al. (2011) find that working more than 20 hours per week is associated with declines in school engagement and increases in substance abuse and delinquency, whereas working 20 hours or less has little or no effect. Rothstein (2007) finds that high school employment has a small negative effect on grade point average but these effects decrease when student fixed effects are included and become insignificant when hours worked are instrumented. Staff, Schulenberg, and Bachman (2010) find that the mere desire to work long hours is associated with poor academic performance, suggesting the influence of selection bias in estimating the relationship between intensive work and academic outcomes.

Still other recent research examines outcomes beyond high school. For instance, Lee and Orazem (2010) find that working more hours in high school does not affect academic performance, and increases the likelihood of high school graduation but decreases the likelihood of going to college. Painter (2010) finds that adolescent employment improves net worth and financial well-being as an adult. In addition, an experimental evaluation of the career academies high school model, which includes academics and career development opportunities, found no short-term impacts on educational outcomes but did find positive effects on longer term life outcomes (Kemple & Willner, 2008).

Also relevant to this study are evaluations of large publicly-funded employment programs for out-of-school youth, which focus on employment, earnings, and educational outcomes. Although these studies employ strong experimental research designs, their primary sample of interest is out-of-school youth rather than current students. In a randomized study of the National Job Corps program, Schochet, Burghardt, and McConnell (2008) find that the program increased receipt of GED and vocational certificates, as well as short-term earnings. In an experimental study of the National Job Training Partnership Act Title II-A Programs for out-of-school youth and economically disadvantaged adults, Bloom et al. (1997) find some positive effect on educational attainment for female high school dropouts but not for males. In both studies, however, the authors conclude that the benefits of these intensive programs did not exceed the costs for most youth. In an earlier article, "Why teens do not benefit from work experience programs," Foster (1995) argues that the employment programs of the 1960's, 70's, and 80's had little success because they were not appropriate for the populations that they targeted, which may have lacked the baseline level of experience or education needed to truly benefit.

Work during high school may also influence students' outcomes by keeping them away from harmful or unproductive situations. For instance, Jacob and Lefgren (2003) find that schools play an "incapacitating" role that keeps youth occupied and out of certain types of trouble when school is in session. Employment could play a similar role when school is out of session. Similarly, Anderson and Hughes (2009) find that unstructured time with peers is associated with greater delinquent behavior. On the other hand, Anderson and Hughes (2009) also find greater youth income to be associated with delinquent behavior.

Finally, differences in summer experiences have been found to have important implications for learning and the achievement gap between low and middle income students (Alexander et al., 2007; Cooper et al., 1996). However, most research in this area has focused on elementary school students, and relatively little rigorous research has examined the effect of summer experiences on academic outcomes for high school students. Overall, the existing research base regarding the influence of summer youth employment programs, or summer work experience more broadly, on high school students' educational success is quite limited. This study aims to address this gap in the literature by examining the impact of the randomly assigned offer to participate in New York City's SYEP on school attendance, attempting and passing statewide tests, and test scores in the following school year.

New York City's Summer Youth Employment Program (SYEP)

All New York City residents ages 14-21 are eligible for SYEP, although the program operates through community-based organizations (CBOs) that are located in primarily low-income communities in each of NYC's five boroughs. These CBOs serve as intake sites, and CBO staff supervise SYEP placements and deliver the program's training component. This study uses SYEP data for the summer of 2007. In 2007, 51 CBOs facilitated SYEP placements and the program had a total budget of $50.4 million in federal, state, local, and private funds. The program received 93,750 applications and served 41,650 youth, the majority of whom were students in public city high schools, although this is not a requirement for participation (NYC DYCD, 2011).

To apply to SYEP, youth complete an on-line or paper application, through which they choose one of the CBOs contracted by DYCD to provide program services. Youth may not apply to more than one CBO. Within every CBO applicant pool the number of applicants to SYEP exceeds the number of available slots. Therefore, DYCD conducts lotteries among the applicants to each specific CBO to determine participation in SYEP.[2] This lottery system effectively assigns the offer to participate or not participate in SYEP at random, providing a

[2] DYCD cross checks applications across all providers by applicant social security number and name to ensure that each youth submits only one application. For each provider, a central computerized system uses a random assignment algorithm to select applicants (using their random ID numbers) to fill the number of slots that each CBO is allocated by contract. The system does not use any applicant information other than the random ID number to determine selection into the program.

unique opportunity to evaluate the effects of the program on student outcomes. The lottery process creates a control group of youth who apply to SYEP but are not chosen, and who due to the random nature of the lottery we expect to be similar to the SYEP participants on both observable and unobservable characteristics.

SYEP's stated goals are to introduce and prepare youth for the world of work, reduce youth unemployment during summer months, and provide supplemental income to families (NYC DYCD, 2012). Participants work in a variety of entry-level jobs in the non-profit, private, and public sectors for up to 25 hours per week, for seven weeks from the beginning of July to mid-August, and receive the New York State minimum wage. Ten percent of participant hours are devoted to education and training on topics such as work readiness, financial literacy, career exploration, post-secondary education options, and health education. Work sites include a wide variety of public and private-sector settings, with the most common placements being summer camps and day care centers.

Although SYEP includes an educational and training component, it is primarily a workforce development program rather than an education program. Given this orientation, it is worth considering why we may expect to find impacts of SYEP on educational outcomes. Figure 1 outlines a theory of change for SYEP and mechanisms through which participation may improve educational and other outcomes.[3] Assignment to SYEP provides students with opportunities for a summer job, income, education and training. In the short-term SYEP may increase students' level of work experience, financial well-being, and understanding of the work world. SYEP may also promote social growth including characteristics such as confidence and responsibility. The job and training component may improve financial literacy and knowledge of

[3] This is the author's theory of change for how SYEP may influence educational outcomes, rather than a theory put forth by DYCD.

education and career pathways and requirements. These short-term impacts, not observable in this study, may in turn lead to increased academic effort and persistence as measured by school attendance rate, and test taking. This paper focuses on these medium-term outcomes which are observable given the available data, even as the mechanisms are not. Long-term outcomes, also not observable in the data for this study, could include increased high school graduation rates, enrollment in higher education, and workforce success.

In addition to fostering behavioral traits associated with academic success, earning money during the summer could reduce the number of hours a student needs to work during the school year and in turn increase time devoted to academics. Further, knowledge of the work world gained through SYEP could potentially increase students' academic effort by demonstrating the value of a high school diploma or additional credentials. The program could also benefit students simply by keeping them occupied and away from harmful situations that they would otherwise be exposed to during the summer (Anderson & Hughes, 2009; Jacob & Lefgren, 2003). On the other hand, SYEP could negatively affect educational outcomes if the work experience that students gain during the summer leads them to work more during the school year and focus less on school, or forego education altogether in favor of employment. (SYEP jobs themselves are paid for by DYCD rather than the employer, so these jobs end when the summer is over.) While summer jobs have the potential to influence academic outcomes, whether this relationship is positive or negative may be uncertain. Further, impacts may vary by student characteristics such as age or level of academic engagement prior to SYEP, which are explored in the analysis below.

SAMPLE AND DATA

Data for this analysis come from two sources, SYEP files and NYCDOE student records. The sample for this study is limited to SYEP applicants who were current students in New York City public schools, the population represented in the education data used for this analysis. Therefore, this analysis excludes SYEP applicants who had dropped out of school, graduated from high school or completed a GED, or attended college at the time of application to SYEP. The sample also does not include students in private or parochial schools. Student-level SYEP and DOE files do not share a unique common student identification number, and were matched based on student name and birth date, with a success rate of approximately 70 percent.[4]

SYEP data include an indicator variable for whether or not the lottery assigned the student to SYEP. For those who did participate the data also include the type of SYEP work placement and number of hours worked. Variables from NYCDOE files include student demographics such as eligibility for free and reduced price lunch, race/ethnicity, gender, age, limited English proficiency (LEP), and special education (SPED) status. Data for this analysis also include school attendance, math and English Regents exams attempted and passed, and standardized Regents test scores. New York State offers two types of high school diplomas, a "Regents" diploma and a "Local" diploma. The Regents diploma is considered more rigorous and prestigious, as it requires students to pass a series of standardized Regents exams, which are not required for the Local diploma. Therefore, attempting these exams provide a measure of academic effort and expectations.

[4]Some proportion of the unmatched SYEP applicant records include observations for youth who were not New York City public school students, including students in private or parochial schools or public schools outside of New York City, and as such were not in the NYCDOE files. Therefore, the match rate for the sample of interest, students in New York City public schools, is likely considerably higher.

The study sample includes students who applied to SYEP for the summer of 2007 and were in New York City public schools in the school year before (SY 2007) and after (SY 2008). Impact analyses focus on the 36,550 students who were in grades 8-11 during the school year prior to applying to SYEP.[5] Students applied to SYEP through 51 CBOs located in each of the five New York City boroughs, and lotteries to determine participation were conducted for applicants to each CBO. Analyses verifying the random nature of the 2007 SYEP lottery include all New York City public schools students in the data who applied to SYEP, regardless of whether they were in school in the following year (n=47,453).

Table 1 provides descriptive statistics for SYEP lottery applicant pre-existing characteristics, averaged at the lottery level.[6] Students selected by the SYEP lottery appear to be almost identical to those not selected on observed characteristics, indicating that the lottery is in fact random. On average, almost 90 percent of SYEP applicants are eligible for free or reduced price lunch, demonstrating that the program serves an overwhelmingly low-income population. More than half of SYEP applicants on average are black, with black and Hispanic students accounting for more than 85 percent of applicants. Also, a slightly higher percentage of applicants are female than male (54 percent versus 46 percent). On average, lotteries accept just over half of applicants although there is considerable variation in lottery application acceptance rates. Finally, on average, lotteries include slightly more than 450 students in both the selected and not selected groups.

SYEP serves an urban population of youth that is predominantly low income. Compared to New York City public school students who were age-eligible for SYEP but did not apply,

[5] Analyses exclude students in ungraded special education, as well as the small number of students in grades below 8, or those in grade 12 at baseline and in school in the following year.

[6] This table presents lottery level averages across the 51 individual lotteries, rather than the simple average for SYEP lottery winners and losers across the city as a whole. Each individual lottery varies in size, make-up, and selection rate. Analyses of the impact of SYEP control for these between lottery differences by including lottery fixed effects.

applicants are slightly more likely to be eligible for free or reduced price lunch (90 percent versus 88 percent) and female (54 percent versus 48 percent), and considerably more likely to be black (53 percent versus 32 percent). A smaller percentage of SYEP applicants are Hispanic, Asian, or white compared to non-applicants. Also, a considerably lower percentage of SYEP applicants are categorized as limited English proficiency. Some of these differences between SYEP applicants and non-applicants may be due to work eligibility requirements for SYEP which exclude certain students, or may reflect the demographics of the communities targeted by the program.

RESEARCH DESIGN

Using data for New York City public school students who apply to SYEP, this paper estimates the impact of assignment to SYEP on multiple school outcomes in the following academic year. First, I estimate school attendance models for the entire year. I also estimate attendance models that take advantage of the bi-annual reporting of student attendance and unpack attendance in the fall and spring terms. These models are preferred, as they account for seasonality and student attendance trajectories over time. Models predicting school attendance use the log attendance rate as the dependent variable; alternate forms are tested in subsequent robustness checks.[7] Next, I estimate models of the probability of attempting math and English Regents tests, followed by models estimating passing these tests, and standardized test scores.[8]

As noted above, participation in SYEP is determined through a lottery among applicants to each of the CBO providers. Assuming that each SYEP lottery is random and that there is no

[7] Log attendance is used to avoid issues of heteroskedasticity and extreme skew in order to more closely meet the assumptions of ordinary least squares regression (Engberg et al 2012). The log attendance rate is calculated as: log((days attended in semester/91 days per semester) x 100). Observations with zero days attended are excluded from the analysis.

[8] Models for attempting and passing Regents include all SYEP applicants. Models for test scores necessarily include only those who attempted exams.

differential attrition, within any individual lottery a simple comparison of means on the outcome of interest between those assigned to SYEP and those not should provide an unbiased estimate of the Intent-to-Treat (ITT) where the treatment is participation in SYEP. However, across each of the CBO lotteries both the characteristics of the applicants and the selection rates may differ. Therefore, models estimating the overall impact of SYEP pool all lotteries and include a lottery fixed effect. Preferred models include student characteristics in order to increase precision, although conclusions about SYEP do not change with inclusion of student characteristics. These models also include school fixed effects to control for the influence of time-invariant school characteristics on educational outcomes. For all models, standard errors are clustered at the lottery level. Finally, I test for the presence of heterogeneous treatment effects through separate models stratified by subgroups based on pre-existing characteristics including school attendance rate in the school year prior to SYEP and age.

I estimate the impact of SYEP on student outcomes using equation (1):

$$(1) Y_{it} = \beta_0 + \beta_1 SYEP_{it} + \beta_2 ST_{it} + \delta_t + \mu_{it}$$

Y is the outcome of interest (e.g., attendance, tests attempted, tests passed) for student *i* in time *t*. *SYEP* takes a value of 1 if, based on the lottery, the applicant was made an offer to participate in SYEP and 0 if he/she was not. *ST* is a vector of youth characteristics, including gender, race/ethnicity, free and reduced price lunch eligibility, limited English proficiency, special education status, and grade, δ is the vector of lottery and school fixed effects, and μ is the error term. β_1 is the primary coefficient of interest. Because participation in SYEP is determined by a lottery, β_1 should provide an unbiased estimate of the impact of assignment to SYEP.

For models estimating student attendance in the fall and spring terms rather than overall annual attendance, the data set is constructed as a panel with attendance observations in the fall and spring, before and after the SYEP lottery. These analyses use equation (2):

$$(2) Y_{it} = \beta_0 + \beta_1 Pre * Spring + Post * (\beta_2 Fall + \beta_3 Spring) + Post * SYEP_{it} * (\beta_4 Fall + \beta_5 Spring) + \beta_6 ST_{it} + \delta_t + \mu_{it}$$

Y is the log attendance rate for student *i* in time *t*. *Pre* is an indicator that takes a value of 1 in the school year prior to the SYEP lottery and 0 in the school year after the lottery. *Post* is the converse of *Pre*. *Fall* takes a value of 1 in the fall term and 0 in the spring term, and *Spring* is the converse. *Pre x Fall*, the first period observed, is the excluded reference term. β_4 and β_5 are the coefficients of interest, as they reflect the estimated effect of winning the SYEP lottery on school attendance in the following fall and spring terms, respectively. Other terms are as defined in (1) above.

RESULTS

Testing for the random nature of the lottery and selective attrition

First, it is necessary to verify that the SYEP lottery is in fact random. Table 2 presents separate models estimating the effect of winning the lottery on pre-existing student characteristics and outcomes, following the method used by Cullen, Jacob, and Levitt (2006).[9] If the SYEP lottery is random, the lottery outcome should not predict pre-existing student characteristics after accounting for differences between individual lotteries in composition and acceptance rates, which is accomplished by including lottery fixed effects. Models also include school fixed effects and standard errors clustered at the lottery level. These models indicate that the lottery is random. In all but one of these models the lottery indicator does not predict pre-

[9] Analyses presented use linear probability models. Logistic regression with marginal effects yields similar results.

existing student characteristics. One coefficient significant at the $p<0.10$ level would be expected by random chance when testing thirteen different outcomes.[10]

Even though the lottery appears to be random, estimates of the impact of SYEP on student outcomes could be biased if there is selective attrition, meaning that students assigned to SYEP are more or less likely to be enrolled in school and present in the education data in the following school year. Overall, within the sample of students in grades 8-11 in the school year prior to SYEP, 93.5 percent of those selected by the lottery were enrolled in the following school year compared to 93.4 percent of those not selected, suggesting that selective attrition is not a problem. To more rigorously test for selective attrition, Table 2 also includes models estimating the effect of the lottery indicator on the same pre-existing student characteristics and outcomes as above, limiting the sample to only those students who were enrolled in school in the school year following SYEP (Cullen et al., 2006). Again, the SYEP indicator does not significantly predict any student characteristics, suggesting that overall SYEP lottery winners and losers did not differentially attrit.

Estimating the effect of SYEP on school attendance

A primary outcome of interest in this analysis is school attendance. Table 3 presents the results of models estimating the impact of SYEP on school attendance rates (log) in the following school year, using equation (1). Model 1 is the simple unadjusted model and model 2 includes lottery fixed effects, school fixed effects, and a host of covariates. In the more controlled model (model 2), estimates indicate that SYEP increases attendance by about 1 percent ($p<0.05$), or about 1 to 2 school days.

[10]Also note that the sample gets considerably smaller when including test scores as a predictor because students do not take the math regents every year and thus many had not taken the test.

Table 4 presents models estimating the impact of SYEP on the school attendance rate (log) in the following fall and spring semesters, using equation (2). Model 1 is the unadjusted model and model 2 which is preferred includes lottery and school fixed effects and demographic controls. The positive and significant coefficients on *Post x Fall x SYEP* and *Post x Spring x SYEP* suggest that winning the SYEP lottery increases school attendance in the following year for lottery winners by about 1 percent in the fall and almost 2 percent in the spring semester. [11] The negative coefficients on *Pre x Spring*, *Post x Fall*, and *Post x Spring* indicate that compared to the first semester in the data set, the fall of the school year prior to SYEP, school attendance rates are lower in each following term for all applicants. Thus, on average attendance rates decrease as students get older and are lower in spring than fall. Considering all terms in the model together, we can conclude that attendance decreases less for SYEP lottery winners relative to lottery losers.

Although models 1-2 in Table 4 suggest that on average SYEP has a small positive effect on school attendance in the following school year, for students who were already high attending there is little possible room for improvement in attendance rates and ceiling effects could dampen the estimated effect of SYEP on attendance. Models 3-4 estimate the impact of SYEP on school attendance only for students who had an attendance rate below 95 percent at baseline, in the fall prior to the SYEP lottery. As expected, removing students with high attendance yields slightly larger estimates of the impact of SYEP on attendance. Model 4 is the preferred specification, which includes lottery and school fixed effects, and indicates a 1.6 percent increase in fall attendance and 2.7 percent increase in spring attendance for SYEP lottery winners who had attendance below 95 percent at baseline.

[11] Note: all reported statistically significant findings are at the $p<0.01$ level unless otherwise noted.

Finally, we may expect to find heterogeneous treatment effects based on student age. For instance, differences by age in students' level of skills and experience may influence their SYEP work experience itself as well as any resulting changes in behavior. Further, as students become older they tend to exert increased agency in decisions about their own school attendance and academic engagement. Table 5 presents attendance models by age, splitting the sample by whether the student was age 16 (the age at which students are no longer legally required to attend school) at the beginning of the school year following SYEP (NYC DOE, 2012). As above, the sample is further limited to those students with attendance below 95 percent in the fall term prior to SYEP, as students with high attendance had little room for improvement. For students below age 16, the preferred model (2), including lottery and school fixed effects and demographic controls, indicates no significant effect of SYEP on school attendance in the fall and a marginally significant increase in the spring. For students ages 16 and older, models 3 and 4, indicate significant effects of SYEP on attendance that are larger than the estimates presented in Table 4 for the SYEP applicant population as a whole. Model 4, the preferred specification, indicates that SYEP increases attendance by approximately 3 percent for this group, equating to 4-5 school days.

Estimating the impact of SYEP on academic achievement

As noted above, high school students in New York State may choose between two types of diplomas; a "Local" diploma and a more rigorous and prestigious "Regents" diploma which requires passing multiple Regents exams.[12] Table 6 provides models for New York State

[12] Any specific Regent exam must be passed only once during high school, and there is no one designated grade in which students attempt any specific exam. For example, the vast majority of students take the English Regents in either 10th or 11th grade. Therefore, we do not expect all students pursuing a Regents diploma to attempt the English or math Regents exam in the school year following SYEP.

English and Math Regents exams for all SYEP applicants in the sample using equation (1).[13] All models include lottery and school effects, demographic controls, and clustered standard errors at the lottery level. Model 1 estimates the impact of SYEP on the likelihood that students attempt the English Regents exam.[14] Model 2 estimates the impact of SYEP on passing the English Regents exams for all SYEP applicants, and model 3 predicts z-scores for students that attempted the exam. Model 1 indicates that SYEP increases attempting the English Regents exam by about one percent ($p<0.10$). Model 2 does not suggest any significant impact of SYEP on the likelihood of passing the exam. Model 3 suggests a small significant negative impact of SYEP on English z-scores, which could be explained by increases in test taking among students with low academic proficiency. Models 4-6 suggest no significant impacts of SYEP on math Regents outcomes.

Table 7 presents the same models as Table 6 but limits the sample to students age 16 and over and with less than 95 percent attendance in the fall prior to SYEP, the sample that demonstrated positive effects on attendance and which may exert greater autonomy in decision making about academics. We may also expect to see larger effects for these older students because English and math Regents test taking, although not confined to one specific grade, is relatively rare in grade 9 and most prevalent in grades 10 and 11. Table 7 indicates a 2.9 percent increase in attempting the English Regents for this sample, a marginally significant ($p<0.10$) 1.7 percent increase in the probability of passing the exam, and no significant effect on test scores. For the math Regents, there is a marginally significant ($p<0.10$) 1.2 percent increase in attempting, a significant 1.3 percent increase in passing, and no effect on test scores. In interpreting the findings that on average SYEP increases the likelihood of passing Regents

[13] All models are linear probability models. Logistic regression with marginal effects yields similar results.
[14] This estimate includes all SYEP applicants in the sample, some of whom had previously passed this Regents Exam, although due to the random lottery this is distributed evenly across the treatment and control groups.

exams, note that these models include all SYEP applicants regardless of whether they attempted the exam. SYEP lottery winners who actually attempt the exams are not significantly more likely to pass. Thus, the increase in passing Regent exams can be attributed to increased attempts rather than increased performance on the exams among those who attempt them. These findings indicate that SYEP increases the number of students passing Regents exams overall. More concretely, these findings suggest that of the 7,533 students in this group who were selected by the SYEP lottery, an estimated additional 128 students passed the English Regents and 98 passed the math Regents because of SYEP.

Robustness Checks

This section includes multiple tests of the robustness of the findings for school attendance presented above. First, in the preferred attendance models using fall and spring term attendance, to address the potential influence of extreme values, the sample is trimmed to exclude the bottom 10 percent and top 10 percent of the sample based on attendance at baseline in the fall prior to SYEP.[15] This test does not meaningfully change the size or significance of the coefficients of interest. Second, to test for sensitivity to different functional forms of the attendance variable, rather than use the log attendance rate as the outcome of interest, I use attendance rate (0-100%) as the dependent variable. This does not meaningfully change the size or significance of the coefficients of interest.

In addition, although all tests indicate that the SYEP lottery is random, I estimate models including school attendance rate in the prior term (fall or spring) as an additional control. The coefficients of interest remain positive for both the fall and spring terms after SYEP but are statistically significant only for the spring term. Further, models restricting the sample to a balanced panel of students in all four terms (pre and post, fall and spring) yield coefficients of

[15] This yields a sample with attendance between 69 and 98 percent at baseline.

interest that remain positive for both terms after SYEP but are statistically significant only for the spring after SYEP.

Finally, as noted above, all impact estimates presented are Intent-to-Treat (ITT) estimates of the impact of being selected by the random SYEP lottery. However, not all students selected by the lottery actually participate. Of those assigned to SYEP by the lottery, 73 percent participate overall, and 71 percent of students ages 16 and over with attendance below 95 percent at baseline selected by the lottery actually participate.[16] Therefore, ITT estimates may underestimate the impact of actually participating in SYEP. Although the effect of actually participating in SYEP cannot be estimated experimentally, a quasi-experimental "Treatment on the Treated" (TOT) analysis can account for actual program participation. A simple "no-show" correction (Bloom, 1984) averages the estimated program impacts across actual participants rather than all treatment group members by dividing the impact estimate by the program participation rate. This approach assumes only that non-participants receive no benefit from the program. Accounting for participation rates among students selected by the SYEP lottery yields non-experimental estimates that are approximately 1.4 times greater than the ITT estimates reported here.

DISCUSSION

The results presented here indicate that SYEP has a positive impact on school attendance of 1 to 2 percent on average, or roughly 2-3 days. Impact estimates are larger for students who may be at greater educational risk; those age 16 or older who did not attend school at high rates in the prior school year. For these students, the average increase in attendance is approximately 3 percent, or 4-5 additional school days attended. In addition, results indicate that for this group,

[16] Among lottery winners who do accept the offer, participation is quite high with an average of 150 hours worked out of a maximum of 175 hours.

SYEP increases the probability of attempting and passing English and math Regents exams, although there is no significant effect on test scores. The increased probability of passing appears to be due to the increased probability of attempting the exams rather than improved test performance.

These estimated increases in attendance and in attempting and passing optional exams may appear small but they are not trivial. Viewed within the context of school attendance policy, the increases of 4-5 days attended for the group of older low-attending students represent about one-quarter of the 18 total days that New York City students may miss and still be promoted to the next grade. Rigorous research evaluating initiatives specifically aimed at boosting school attendance and preventing truancy is rather limited (Sutphen, Ford, & Flaherty, 2010). An experimental evaluation of a New York City program that provided incentive payments to high school students and their families for high rates of school attendance found a 3 percent increase in attendance (Riccio et al., 2010).[17] Nonetheless, influencing school outcomes is not a primary or explicit goal of SYEP. Rather, this study indicates that beyond its goals of providing low income youth with work experience and income during the summer, the program fosters increased academic engagement in the following school year. Finally, the program operates at a relatively modest cost of approximately $1,400 per participant, with the primary expense being participants' wages (NYC DYCD, 2012).

This paper employs a strong research design, made possible by the high demand for SYEP and the use of a lottery system to allocate program slots fairly. Nonetheless, it is important to consider the limitations as well as the generalizability of the study results.

[17]The Opportunity NYC conditional cash transfer program, provided $50 incentive payments for each month that students in grades 9-12 had a school attendance rate above 95 percent, as well as a host of additional incentive payments for other areas of student and parent behavior.

First, the data do not include information regarding the mechanisms responsible for the findings of increased academic engagement. Thus we do not know whether or not, for instance, SYEP increases students' confidence or self-esteem, nor are we able to parse out any effect of the income associated with SYEP from other changes related to the experience itself. Similarly, the data do not include information regarding the counterfactual experiences of those not assigned to SYEP, which may be important for understanding how SYEP influences student outcomes. The data also do not include information regarding the quality of the SYEP experience or any related variation therein. Finally, although this study provides causal estimates of the impact of participating in SYEP for one summer on outcomes in the following school year, it does not speak to longer-term impacts or the effect of SYEP participation over multiple summers.

Publicly-funded summer youth employment programs exist in many US cities, which are in many ways similar to the program studied here. These programs also provide summer jobs for low income urban youth, although there may be differences in implementation that influence participants' experiences.[18] Beyond differences in implementation, the extent to which findings from this study generalize to other settings may depend in part on the counterfactual environment, including the availability of other employment or productive activities during the summer. Data for this study are for 2007, just before the great recession. Thus, we may expect to find larger program effects in following years as students not assigned to the program likely had less access to other employment opportunities.

CONCLUSION

This study is one of the first to provide causal estimates of the effect of a large scale summer youth employment program on students' academic outcomes. Winning the SYEP

[18] For example, in Washington, DC, the summer youth employment program is administered through the Department of Employment Services and program slots are allocated on a first come first serve basis.

lottery increases attendance in the following school year, with larger increases for students that had lower attendance at baseline, and for students ages 16 and over. These findings suggest that within the current economic climate and policy context, reductions in the availability of summer jobs for teens and budget cuts for summer employment programs may be a cause for concern. Although intended primarily to provide work experience and income for low-income youth, summer youth employment programs may increase engagement in school, which may in turn promote future success in the labor market.

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Figure 1. SYEP Theory of Change

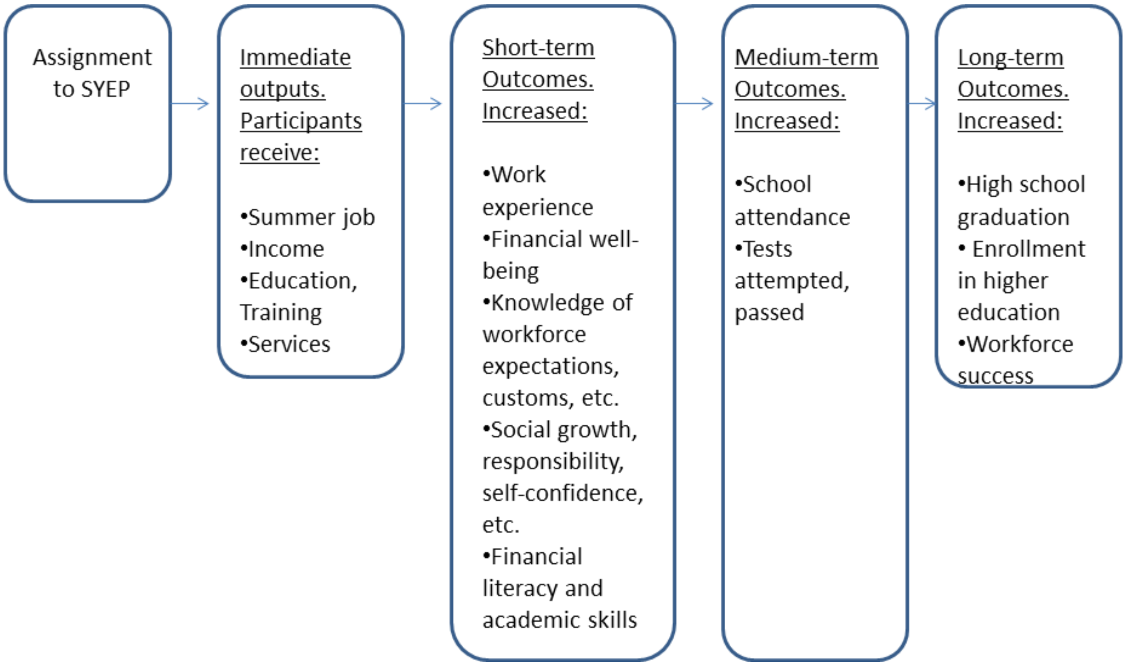

Table 1. SYEP Applicant Characteristics by Lottery Outcome, Lottery-Level Averages

	Selected	Not Selected
% Free Lunch Eligible	78.2	78.3
	(8.0)	(8.2)
% Reduced Price Lunch	11.2	11.2
	(4.4)	(3.9)
% Full Price Lunch	10.6	10.5
	(4.6)	(4.8)
% Black	55.1	54.6
	(25.7)	(25.7)
% Hispanic	31.0	31.4
	(22.5)	(22.7)
% Asian	9.2	9.4
	(17.7)	(17.5)
% White	4.7	4.6
	(9.1)	(8.4)
% Female	53.7	53.9
	(5.3)	(5.3)
% Limited English Proficiency	2.9	3.0
	(3.4)	(2.7)
% Age 16+	52.8	52.5
	8.3	8.7
% Special Education	16.7	16.3
	(6.5)	(6.3)
School Attendance Rate (pre)	82.7	82.2
	(2.9)	(3.2)
N Students	469	451
	(306)	(321)
% Selected by Lottery	52.4	
	(16.9)	
Total Lotteries	51	51
Total Students	24,179	23,274

Note: Standard errors in parentheses.

Table 2. Testing the Validity of the Lottery

	All Lottery Applicants		Applicants Enrolled in Public School Post-SYEP	
	Effect of Winning Lottery	SE	Effect of Winning Lottery	SE
Free	-0.001	(0.004)	-0.003	(0.004)
Reduced	-0.001	(0.003)	0.002	(0.003)
Full	0.001	(0.003)	0.001	(0.003)
Black	0.003	(0.004)	0.001	(0.004)
Hispanic	-0.002	(0.003)	-0.002	(0.003)
Asian	-0.001	(0.002)	0.001	(0.002)
White	-0.001	(0.001)	0.000	(0.001)
Female	-0.001	(0.004)	-0.000	(0.005)
LEP	-0.000	(0.002)	0.000	(0.003)
SPED	0.006	(0.005)	0.003	(0.005)
Lag Attendance Rate (log)	0.006*	(0.004)	0.005	(0.004)
Lag Zmath	-0.010	(0.013)	-0.006	(0.013)
Lag Zread	-0.006	(0.016)	0.004	(0.016)

* p<0.10, ** p<0.05, *** p<0.010

Note: Models include lottery and school fixed effects.

Table 3. School Attendance Rate (log) Models

	(1)	(2)
SYEP	0.022***	0.013**
	(0.008)	(0.005)
Demographic Controls	NO	YES
Lottery FE	NO	YES
School FE	NO	YES
R-sqr	0.001	0.137
N Lotteries	51	51
N Student	36,550	36,042

* p<0.10, ** p<0.05, *** p<0.010

Note: Demographic controls include Reduced Price Lunch, Full Price Lunch, Free Meal Status Missing, Hispanic, Asian, White, Female, LEP, SPED, Grade 8 (2007), Grade 9 (2007), Grade 10 (2007).

Table 4. Fall and Spring Term Level Attendance Rate (log) Models

	All Applicants		<95% Attendance in Fall Pre-SYEP	
	(1)	(2)	(3)	(4)
Post*Fall*SYEP	0.017***	0.011***	0.022***	0.016***
	(0.006)	(0.004)	(0.006)	(0.006)
Post*Spring*SYEP	0.024***	0.018***	0.033***	0.027***
	(0.007)	(0.006)	(0.008)	(0.008)
Pre*Spring	-0.111***	-0.111***	-0.117***	-0.119***
	(0.003)	(0.003)	(0.004)	(0.004)
Post*Fall	-0.084***	-0.080***	-0.092***	-0.088***
	(0.006)	(0.005)	(0.006)	(0.005)
Post*Spring	-0.195***	-0.193***	-0.231***	-0.230***
	(0.010)	(0.009)	(0.008)	(0.007)
Constant	4.459***	4.495***	4.392***	4.458***
	(0.005)	(0.008)	(0.003)	(0.009)
Demographics	NO	YES	NO	YES
Lottery FE	NO	YES	NO	YES
School FE	NO	YES	NO	YES
R-sqr	0.027	0.110	0.027	0.102
N Lotteries	51	51	51	51
N Observations	142,361	142,361	89,389	89,389
N Students	35,568	35,568	21,195	21,195

* p<0.10, ** p<0.05, *** p<0.010

Note: Demographic controls include free lunch eligibility, race/ethnicity, sex, special education status and English language learner status, and grade dummies.

Table 5. Fall and Spring Term Level Attendance Rate Models (log); Students with Attendance Below 95% in Prior Fall, by Age

	Age<16 & Attendance<95%		Age 16+ & Attendance<95%	
	(1)	(2)	(3)	(4)
Post*Fall*SYEP	0.006	-0.000	0.037***	0.028***
	(0.007)	(0.007)	(0.009)	(0.008)
Post*Spring*SYEP	0.024**	0.019*	0.042***	0.035***
	(0.010)	(0.011)	(0.013)	(0.013)
Pre*Spring	-0.106***	-0.107***	-0.125***	-0.127***
	(0.003)	(0.003)	(0.005)	(0.005)
Post*Fall	-0.070***	-0.067***	-0.109***	-0.105***
	(0.005)	(0.005)	(0.009)	(0.008)
Post*Spring	-0.199***	-0.197***	-0.258***	-0.258***
	(0.008)	(0.008)	(0.012)	(0.011)
Constant	4.419***	4.491***	4.373***	4.462***
	(0.003)	(0.057)	(0.005)	(0.011)
Demographics	NO	YES	NO	YES
Lottery FE	NO	YES	NO	YES
School FE	NO	YES	NO	YES
R-sqr	0.030	0.113	0.027	0.145
N Lotteries	50	50	51	51
N Observations	37,953	37,949	51,444	51,440
N Students	9,641	9,640	13,222	13,221

* p<0.10, ** p<0.05, *** p<0.010

Note: Demographic controls include free lunch eligibility, race/ethnicity, sex, special education status and English language learner status. Students categorized as age 16+ if student turns 16 prior to the beginning of the school year following SYEP.

Table 6. English and Math Regents Models, All SYEP Applicants

	English Regents			Math Regents		
	Attempted	Passed	Z-Score	Attempted	Passed	Z-Score
	(1)	(2)	(3)	(4)	(5)	(6)
SYEP	0.010*	0.004	-0.031**	0.005	0.004	0.006
	(0.005)	(0.005)	(0.012)	(0.004)	(0.004)	(0.023)
R-sqr	0.332	0.285	0.327	0.192	0.133	0.362
N Lotteries	51	51	50	51	51	50
N Student	36,122	36,122	11,013	36,122	36,122	7,597

* p<0.10, ** p<0.05, *** p<0.010

Note: All models include lottery fixed effects, school fixed effects, and demographic controls which include Reduced Price Lunch, Full Price Lunch, Free Meal Status Missing, Hispanic, Asian, White, Female, LEP, SPED, Grade 8 (2007), Grade 9 (2007), Grade 10 (2007).

Table 7. English and Math Regents Models, Students Ages 16+ with <95% Attendance Rate in Prior Fall

	English Regents			Math Regents		
	Attempted	Passed	Z-Score	Attempted	Passed	Z-Score
	(1)	(2)	(3)	(4)	(5)	(6)
SYEP	0.029***	0.017*	-0.022	0.012*	0.013***	0.054
	(0.010)	(0.009)	(0.020)	(0.006)	(0.005)	(0.046)
R-sqr	0.244	0.209	0.299	0.118	0.064	0.284
N Lotteries	51	51	50	51	51	50
N Student	13,183	13,183	4,902	13,183	13,183	2,207

* p<0.10, ** p<0.05, *** p<0.010

Note: This table includes students who were 16 or older prior to the beginning of the school year following SYEP. All models include lottery fixed effects, school fixed effects, and demographic controls which include Reduced Price Lunch, Full Price Lunch, Free Meal Status Missing, Hispanic, Asian, White, Female, LEP, SPED, Grade 8 (2007), Grade 9 (2007), Grade 10 (2007).

APPENDIX

Table A1. SYEP Student-Level Descriptive Statistics by Lottery and Application Status (percentages), 2007

	Selected	Not Selected	WIA[19]	Did Not Apply
Free Lunch	78.1	79.0	81.7	77.3
Reduced Price Lunch	11.2	10.9	9.3	10.4
Full Price Lunch	10.7	10.1	9.0	12.4
Black	51.4	55.2	50.5	32.3
Hispanic	31.8	30.5	33.1	39.7
Asian	11.1	10.7	10.3	14.3
White	5.7	3.5	6.1	13.6
Female	54.0	54.2	50.9	47.7
LEP	3.2	3.1	3.4	8.3
SPED	17.2	15.5	41.9	15.4
N Students	24,179	23,274	2,996	341,394

[19] The WIA column includes students who participated in SYEP through a Workforce Investment Act-funded program for identified as at risk of dropping out of school. All participants in this program were guaranteed a job through SYEP, therefore there was no lottery for these students and they are excluded from the analysis.

Table A2. Fall and Spring Term Level Attendance Rate (log) Models, Trimming at Top and Bottom of Sample by Attendance Rate in Fall Pre-SYEP

	Trimmed 5% at Bottom of Attendance in Fall Pre-SYEP (Includes 50-100% attendance rates)		Trimmed 10% at Bottom of Attendance in Fall Pre-SYEP (Includes 69-98% attendance rates)	
	(1)	(2)	(3)	(4)
Post*Fall*SYEP	0.012**	0.008**	0.012***	0.008***
	(0.004)	(0.003)	(0.004)	(0.002)
Post*Spring*SYEP	0.022***	0.018***	0.021***	0.017***
	(0.007)	(0.005)	(0.006)	(0.004)
Pre*Spring	-0.130***	-0.130***	-0.126***	-0.126***
	(0.003)	(0.003)	(0.003)	(0.003)
Post*Fall	-0.089***	-0.089***	-0.085***	-0.084***
	(0.005)	(0.004)	(0.005)	(0.004)
Post*Spring	-0.200***	-0.201***	-0.191***	-0.191***
	(0.009)	(0.008)	(0.009)	(0.008)
Constant	4.494***	4.572***	4.504***	4.554***
	(0.004)	(0.019)	(0.003)	(0.015)
Demographics	NO	YES	NO	YES
Lottery FE	NO	YES	NO	YES
School FE	NO	YES	NO	YES
R-sqr	0.045	0.112	0.056	0.113
N Lotteries	51	51	51	51
N Observations	140,140	140,140	120,083	120,083

* p<0.10, ** p<0.05, *** p<0.010

Table A3. Fall and Spring Term Level Attendance Rate (0-100%) Models

	All Applicants		<95% Attendance in Fall Pre-SYEP	
	(1)	(2)	(3)	(4)
Post*Fall*SYEP	0.859**	0.493***	1.137***	0.731***
	(0.327)	(0.157)	(0.301)	(0.207)
Post*Spring*SYEP	0.986**	0.637***	1.291***	0.920***
	(0.376)	(0.223)	(0.348)	(0.301)
Pre*Spring	-8.560***	-8.597***	-8.324***	-8.433***
	(0.129)	(0.132)	(0.140)	(0.142)
Post*Fall	-4.007***	-3.994***	-3.317***	-3.360***
	(0.263)	(0.192)	(0.214)	(0.196)
Post*Spring	-10.027***	-10.138***	-10.004***	-10.151***
	(0.393)	(0.312)	(0.280)	(0.260)
Constant	87.384***	89.206***	82.100***	85.610***
	(0.445)	(0.339)	(0.276)	(0.348)
Demographics	NO	YES	NO	YES
Lottery FE	NO	YES	NO	YES
School FE	NO	YES	NO	YES
R-sqr	0.048	0.182	0.041	0.146
N Lotteries	51	51	51	51
N Observations	147,412	147,412	93,502	93,502

* p<0.10, ** p<0.05, *** p<0.010

Table A4. Fall and Spring Attendance Including Attendance in Prior Term

	All Applicants	<95% Attendance in Fall Pre-SYEP
	(1)	(2)
Post x Fall x SYEP	0.002	0.005
	(0.004)	(0.005)
Post x Spring x SYEP	0.012***	0.020***
	(0.004)	(0.006)
Post x Fall	0.142***	0.155***
	(0.005)	(0.007)
Post x Spring	-0.018***	-0.040***
	(0.004)	(0.005)
Lag Log Attendance	0.821***	0.799***
	(0.015)	(0.017)
Constant	0.691***	0.769***
	(0.065)	(0.076)
Demographic Controls	YES	YES
Lottery FE	YES	YES
School FE	YES	YES
R-sqr	0.400	0.381
N Lotteries	51	51
N Observations	108,070	68,444

* p<0.10, ** p<0.05, *** p<0.010

Table A5. Fall and Spring Term Level Attendance Rate (log) Models, Balanced Panel

	All Applicants		<95% Attendance in Fall Pre-SYEP	
	(1)	(2)	(3)	(4)
Post*Fall*SYEP	0.011**	0.006	0.015***	0.010*
	(0.005)	(0.004)	(0.005)	(0.006)
Post*Spring*SYEP	0.024***	0.020***	0.034***	0.030***
	(0.007)	(0.005)	(0.008)	(0.008)
Pre*Spring	-0.108***	-0.108***	-0.115***	-0.115***
	(0.003)	(0.003)	(0.004)	(0.004)
Post*Fall	-0.066***	-0.064***	-0.069***	-0.066***
	(0.005)	(0.004)	(0.005)	(0.005)
Post*Spring	-0.200***	-0.198***	-0.241***	-0.239***
	(0.010)	(0.009)	(0.008)	(0.008)
Constant	4.472***	4.514***	4.410***	4.483***
	(0.005)	(0.008)	(0.003)	(0.009)
Demographics	NO	YES	NO	YES
Lottery FE	NO	YES	NO	YES
School FE	NO	YES	NO	YES
R-sqr	0.020	0.105	0.020	0.095
N Lotteries	51	51	51	51
N Observations	134,572	134,572	84,788	84,788
N Students	33,643	33,643	21,197	21,197

* p<0.10, ** p<0.05, *** p<0.010

SYEP Data Matching Description

SYEP files were matched to NYCDOE files using the SYEP applicants' name and date of birth by an independent consultant (in order to maintain student anonymity). SYEP files included 73,752 applicants and excluded applicants who had indicated on the SYEP application that they had left high school before finishing, graduated from high school or completed a GED, or attended college. Of these, 74.5 percent were successfully matched to a NYCDOE student ID number. Determining the exact "success" rate for the matching process is impossible because the unmatched records include an unknown number of youth who were not New York City public school students, but may have been students in private or parochial schools, or public schools outside of New York City. The match rate only for SYEP applicants who were NYCDOE students is likely considerably higher. Education variables were available for 94.5 percent of those applicants matched to a NYCDOE ID number. The final sample of 36,550 students excludes: duplicate observations for students who appeared to submit multiple SYEP applications; a subgroup that participated in SYEP through a school year program that guaranteed summer jobs and did not use a lottery; and students in ungraded special education, or grade 7 or 12 in the school year prior to SYEP.