THE EFFECTS OF YOUTH EMPLOYMENT: EVIDENCE FROM NEW YORK CITY LOTTERIES*

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Programs to encourage labor market activity among youth, including public employment programs and wage subsidies like the Work Opportunity Tax Credit, can be supported by three broad rationales. They may (i) provide contemporaneous income support to participants; (ii) encourage work experience that improves future employment and/or educational outcomes of participants; and/or (iii) keep participants "out of trouble." We study randomized lotteries for access to the New York City (NYC) Summer Youth Employment Program (SYEP), the largest summer youth employment program in the United States, by merging SYEP administrative data on 294,100 lottery participants to IRS data on the universe of U.S. tax records; to New York State administrative incarceration data; and to NYC administrative cause of death data. In assessing the three rationales, we find that (i) SYEP participation causes average earnings and the probability of employment to increase in the year of program participation, with modest contemporaneous crowdout of other earnings and employment; (ii) SYEP participation causes a modest decrease in average earnings for three years following the program and has no impact on college enrollment; and (iii) SYEP participation decreases the probability of incarceration and decreases the probability of mortality, which has important and potentially pivotal implications for analyzing the net benefits of the program. *JEL* Codes: J13, J45, J38, J21.

I. Introduction

Many policies attempt to support individuals' labor market prospects, including public employment and subsidized employment programs. Youth unemployment in particular remains

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stubbornly high following the Great Recession both in the United States—where the unemployment rate for 16–24-year-olds was 12.2% as of this writing in 2015—and throughout much of the world. In light of high youth unemployment, policy makers have increasingly scrutinized youth employment programs. City programs across the United States provide youth with summer jobs—the 50 most populous cities in the country have all had summer youth employment programs in the past five years—and the federal Work Opportunity Tax Credit (WOTC) subsidizes employment of summer youth employees. Although the literature typically finds that non–summer-employment active labor market programs for youth are ineffective in improving labor market, education, and risky behavior outcomes (e.g., Hollister, Kemper, and Maynard 1984; Couch 1992; Bloom et al. 1997; Cave et al. 1993; Hendra et al. 2011; see surveys in Stanley, Katz, and Krueger 1998; Heckman, LaLonde, and Smith 1999; LaLonde 2003; Card, Kluve, and Weber 2010), summer youth employment has "received relatively little attention from program evaluators" (LaLonde 2003, p. 532).

Programs to support summer youth employment are justified with various rationales. One rationale is that summer employment could provide income support to youth (and their families) through wages earned in the program. The website of the New York City (NYC) Department of Youth and Community and Development (DYCD), which runs the Summer Youth Employment Program (SYEP) that we analyze in this article, states that SYEP aims to "provide supplemental income to aid low income families."[1] Similarly, economic stimulus efforts often aim to increase contemporaneous net earnings and employment. A second rationale is that summer work experience could improve future employment outcomes by directly increasing human capital—the NYC DYCD also states that SYEP aims to "develop youth skills"—by encouraging youth to receive more schooling after participating in the program or by acting as a signal to potential future employers.[2] A third rationale for such programs is that they could help keep youth involved in socially productive

1. See http://usmayors.org/workforce/documents/2010-7-01USCOMWDCSYEPPresentation011910.pdf (accessed May 16, 2014).

2. See http://www.nyc.gov/html/dycd/html/resources/syep.shtml (accessed May 16, 2014).

activities or "out of trouble."[3] Keeping youth out of trouble during the summer could have immediate benefits through incapacitation or could place youth on a safer path leading to decreased incarceration or mortality rates later in life.

We investigate the empirical support for these three rationales by analyzing the SYEP program in the years 2005–2008. During these years, SYEP provided summer jobs to NYC youth aged 14 to 21, paid by the NYC government at a total cost of $236 million.[4] Each year, SYEP received more applications than the number of jobs available and randomly allocated spots in the program by lottery. We compare the outcomes of individuals who participate in SYEP because they were randomly selected to receive a job to the outcomes of those randomly not selected. We link SYEP administrative data on these lottery winners and losers to Internal Revenue Service (IRS) administrative data on the universe of U.S. federal tax data; to New York State (NYS) Department of Corrections and Community Services (DOCCS) administrative data on individuals incarcerated in New York State; and to NYC Department of Health and Mental Hygiene (DOH) administrative data on causes of death in NYC. In the four years of lotteries we study, there were 294,100 SYEP applications subject to the lottery, of which 164,641 won a job and 129,459 did not.

This context provides a promising setting for studying a youth employment program. The large scale of the program, the random assignment, and the accurate data allow us to estimate precise causal effects on earnings, the employment rate, college enrollment, mortality, and incarceration up to a decade after program participation. Our sample sizes are at least an order of magnitude (and in many cases two orders of magnitude) larger than other randomized studies. The ability to look precisely at mortality, which other studies have not been able to observe, will prove particularly interesting because it has important implications for the magnitude of program benefits. NYC SYEP is also the largest summer youth employment program in the United States and therefore represents a central, recent case study of

3. See http://nycfuture.org/events/event/summit-on-the-future-of-workforce-development-in-new-york-city (accessed May 16, 2014).

4. Except where otherwise noted, all dollar amounts reported are in real 2013 dollars.

U.S. summer youth employment programs and of youth employment programs more generally.

We find that SYEP participation increases earnings and employment in the year of the program. In a baseline specification, SYEP raises average earnings through the program by $1,085 in the year of program participation, lowers other earnings by a modest $209, and therefore raises net earnings by $876. Thus, crowdout of other earnings was 19.28% of the SYEP transfer in this year. We also estimate that on net, SYEP raises the probability of having any job by 71 percentage points in the year of the participation, with a 5 percentage point decrease in the probability of having a non-SYEP job.

We do not find that youth employment has a positive effect on subsequent earnings or on college enrollment. In each of the three years following SYEP participation, SYEP participation causes a modest *decrease* in earnings of around $100 a year. Starting in the fourth year following SYEP participation, SYEP participation has an insignificant impact on earnings. The negative earnings effect in those three years is observed primarily among youth who are relatively older and have some work experience, and participation had an insignificant impact on subsequent earnings among WOTC-eligible individuals. We also find that SYEP has no impact on college enrollment, with an extremely precise 95% confidence interval that rules out a positive or negative effect greater than 1/70 of a year of college. It is notable that even for this young group with typically little prior job experience, an employment program did not provide a path to greater future earnings.

Over the year of SYEP participation and the subsequent four years, participation on net raises average earnings by $535. Thus, SYEP on net transfers to youth, though with significant crowdout (54.12%) of other earnings. Crowdout of other earnings is small relative to likely lifetime earnings but is substantial relative to the size of the program.

Consistent with keeping youth out of trouble, SYEP participation decreases the probability of incarceration and decreases the probability of mortality. SYEP reduces the probability of incarceration by 0.098 percentage points, driven by a decrease among males. Although this effect is small in absolute terms, it represents a substantial 9.93% reduction relative to the baseline in the control group. The SYEP-induced decrease in mortality, also driven by males, is 0.073 percentage points, again small in percentage point terms but a substantial 17.97% of the baseline in the control group. Cause of death data show that SYEP prevents death from external causes.

The point estimates imply that by October 2014, around 83 lives were saved by the SYEP program from 2005 to 2008. Under standard cost-benefit analysis calculations, this implies benefits of $747 million. Past literature has typically found negative net benefits of active labor market programs for youth but has not examined the mortality outcome.[5] Like most previous work on such programs, we find that the effects on future earnings cannot justify the program in a cost-benefit analysis; in fact, we find that SYEP modestly reduces participants' subsequent earnings. Adding a new twist to previous work, our mortality results show a very large new source of benefits that could be pivotal to the analysis of the costs and benefits to society as a whole.

Amid the extensive literature on active labor market programs, the literature on summer youth employment contains only a few studies. Criminologist Sara Heller (2014) examines a randomized controlled trial ($N = 1{,}634$) and finds that a summer youth employment program, in some cases in combination with cognitive-behavioral therapy, greatly decreased violent crime arrests; had no significant impact on arrests for property, drug, or other types of crime; and had little impact on schooling. However, her paper does not examine (i) longer-term impacts past 16 months after the program or (ii) the impact on mortality, earnings, college enrollment, or incarceration.[6] Our finding of negative effects on earnings in the years subsequent to the program echoes some of the findings in a more recent literature about temporary employment programs (not specifically for youth), such as Autor and Houseman (2010), Card and Hyslop (2005), or many "Work First" programs (Bloom and Michalopoulos 2001).

The article is structured as follows. Section II describes the policy environment. Section III describes our empirical specification. Section IV describes the data we use. Section V discusses the first stage and the validity of the lottery. Section VI discusses our results on earnings and employment. Section VII presents results

5. Job Corps shows negative net benefits for the full sample, with positive net benefits only for the older-youth subgroup (Schochet, Burghardt, and McConnell 2008; Lee 2009). Studies of WOTC have focused on the take-up of the program (Hamersma 2003) or on those eligible for WOTC through long-term welfare receipt.

6. See also Farkas et al. (1984), Crane and Ellwood (1984), Grossman and Sipe (1992), and McClanahan, Sipe, and Smith (2004).

on college enrollment. Section VIII shows results on incarceration. Section IX discusses results on mortality. Section X concludes. Additional results and discussion can be found in the Online Appendix.

II. Policy Environment

During the years we study (2005–2008), SYEP provided NYC youth aged 14 to 21 with paid summer employment for up to seven weeks in July and August.[7] Since 2005, DYCD has stored computerized records of applications, which were made available for this research. Because SYEP ran the program on its own, we are evaluating an existing government program (as opposed to a randomized experiment designed by researchers).

SYEP places participants in entry-level jobs and pays them the New York State minimum wage for working up to 25 hours a week during the summer.[8] In 2005 to 2008, the mean expenditure per SYEP participant per time participating in SYEP was $1,403 (including wages paid to participants and administrative costs).

SYEP provides youth with various types of jobs, including jobs at summer camps, daycare centers, government agencies, hospitals, law firms, and museums. Nearly half of SYEP jobs are at summer camps or daycare centers. In 2005–2008, 74.68% of the jobs were with nonprofit private sector firms; 10.95% were with for-profit private sector firms; and 14.37% were with government entities. Thus, the program is typically closer to a "work experience" program, in which individuals are given temporary private sector jobs, than to a "public sector employment" program, in which individuals are given a government job (e.g., Heckman, LaLonde, and Smith 1999). The jobs that participants perform vary widely across employers but typically involve low-skill tasks. As an example of the most common jobs—those at summer camps of community organizations—one large, representative community center employer had five types of jobs available: camp counselor (who leads activities with children like song,

7. See, for example, the SYEP annual report from 2007, available at http://www.nyc.gov/html/dycd/downloads/pdf/syep_2007_annual_summary.pdf (accessed August 4, 2014).

8. In the years of our data, the nominal state minimum wage rose from $6.00 an hour in 2005 to $6.75 an hour in 2006 to $7.15 an hour in 2007 and 2008. In 2014, it is $8.00 an hour. SYEP does not pay for overtime.

dance, and physical activities), group leader (who leads counselors), support staff (who assist camp staff in daily activities like distributing lunch), clerical aide/office assistant, and janitor assistant/custodian (personal correspondence with DYCD, March 17, 2015).

All NYC youth who provide certain documentation are eligible to apply for SYEP. Applicants must show proof of identity using an official picture ID; proof of employment authorization; proof of age; proof of Social Security number using a Social Security card; working papers for those under age 18 (a Blue Card for those aged 14–15 and a Green Card for those 16–17); proof of citizenship/alien status; proof of address; and proof of family income. Males 18 and older must show proof of Selective Service registration.[9]

SYEP is administered by community-based organizations called "providers," which contract with DYCD to place SYEP participants into worksites and administer the program. Participants typically do not work directly for providers, but work for the employers to which providers match participants. In 2005–2008, the mean number of SYEP participants working for a given SYEP employer was 5.69. Over the summer, providers give participants around 17.5 hours of workshops on job readiness, career exploration, financial literacy, and opportunities to continue education, or roughly 10% of the total hours in SYEP. During the years we study, this training component was decentralized across providers, was typically not considered a crucial component of the program, and generally was not costly for providers to deliver (personal correspondence with DYCD, March 17, 2015).

In a given year, applicants to SYEP apply through a specific SYEP provider. Individuals choose the provider to which they apply; applicants typically choose a provider located near their home. In a given year, an applicant applies to only one provider and is unable to apply to other providers at any point in that year. The application period is usually early April to mid-May of the program year. Since there are more applicants than available slots in each year, the individuals who are allowed to participate in SYEP are selected by lottery. Within each provider in each year, there is a lottery to determine which individuals are

9. See, for example, https://application.nycsyep.com/Images/SYEP_2014_Required%20Documents.pdf.

selected for SYEP. Thus, winning the lottery is random conditional on applying to a given provider in a given year.

In each year, SYEP selected applicants through a series of lotteries. In an initial lottery, SYEP randomly selected winners and losers, where the number of winners was chosen to match the number of jobs available. However, not all of the individuals selected through this initial lottery participated in SYEP. Selected individuals may have chosen not to participate or failed to prove eligibility to participate. To fill the remaining slots, SYEP providers conducted subsequent lotteries. In each lottery, the number of winners was selected to match the number of remaining jobs at the SYEP provider, until the number of SYEP enrollees approximately matched the number of available jobs. We obtained data from SYEP on both the winners and losers of the initial SYEP lottery and (separately) on the identities of those who won any of the lotteries in a given year and provider (as well as the identities of those who lost all lotteries in a given year and provider). For an applicant to a given SYEP provider, if a lottery occurred and he or she had not won a slot yet or had won a slot previously but did not accept it, he or she was automatically entered into the subsequent lottery. Individuals were not able to withdraw their applications after the application deadline, nor were they able to enter subsequent lotteries if they had not applied to the provider by the deadline. Since selection of individuals was random in every lottery conditional on reaching that lottery, the dummy for whether an individual won any of the lotteries is exogenous. In our baseline specification, our instrument is a dummy for winning any of the lotteries.[10]

In any given year, individuals not selected in any of these lotteries were officially not able to participate in SYEP in that year, though they remained eligible to apply to SYEP in a subsequent year. Winning or losing the lottery in a given year, or participating in SYEP in a given year, does not affect the probability of winning or losing the lottery in a subsequent year, conditional on applying in the subsequent year. The opportunities to

10. As shorthand, we sometimes refer to “winning (losing) any of the lotteries at a given provider in a given year” as “winning (losing) the lottery.” In the Online Appendix we show that the results are similar when our instrument is a dummy for winning the initial lottery (which is a slightly less powerful instrument).

participate in comparable government programs are small relative to the size of SYEP.[11]

Providers make the assignments of participants to employers, and to particular jobs within employers, based on two inputs. Applicants specify their skills and industry interests on their applications, and employers inform providers of restrictions on the type of participants they can hire (e.g., an employer might require high school graduates for certain jobs). The particular method for matching participants to jobs based on these two inputs varies across providers. Once a provider matched a participant to a job within an employer, the employer occasionally chose to reallocate the participant to a different job during the course of the summer.

III. Data

III.A. DYCD Data

The DYCD data on SYEP contain a number of key pieces of information that we use, including whether an individual won or lost any of the SYEP lotteries (including the subsequent lotteries); whether the individual participated in SYEP; which provider an individual applied to; the year the lottery was conducted; self-reported information on variables including gender, date of birth, race, and name; and Social Security number (SSN). The data include information on all SYEP applicants, regardless of whether they enrolled in SYEP. For SYEP participants, the data additionally include the industry the individual worked in through SYEP (in industry categories created by SYEP).

III.B. IRS Data

We merge the SYEP administrative data to IRS administrative data using SSN, which matches 99.6% of the SYEP applicants to the IRS data. It is not surprising that we obtain a very high match rate, as individuals were required to list their SSN and show their Social Security card (as well as the voluminous additional documentation listed above) to be eligible for SYEP. To include additional individuals who may have an incorrect SSN listed but have other information correct, we match the

11. See http://www.nyc.gov/html/dycd/downloads/pdf/Summer_Youth_Alternatives2014.pdf (accessed August 4, 2014).

remaining SYEP data to IRS data when name, gender, day of birth, month of birth, year of birth, and first or last four digits of the SSN all match. This allows us to match an additional 0.2 percentage point of the SYEP data to the IRS data, for a total match rate of 99.8%. The results are robust to other matching procedures.

The IRS data contain a wide variety of information, including name; date of birth; age; gender; the identity of family members; the Employer Identification Numbers (EINs) of their employer(s); the North American Industrial Classification System (NAICS) industry code of their employer; each individual's day, month, and year of death (if any); whether an individual's employer is a nonprofit; and whether the individual is enrolled in college. Our measure of an individual's annual earnings comes from W-2s, mandatory information returns filed with the IRS by employers for each employee for whom the firm withholds taxes and/or to whom remuneration exceeds a modest threshold.[12] Thus, we have data on W-2 earnings regardless of whether an employee files taxes. Having "any job" is defined as earning a positive amount, again as reported on the W-2 form. Mortality is observed for the full population, and college enrollment is observed on a mandatory information return; therefore neither depends on filing status. We use data on each of these variables in each year from 2004 to 2012 (inclusive). We winsorize earnings at \$100,000 (e.g., Chetty et al. 2011). Like most administrative data sets, the data lack information about the hourly wage, hours worked, or underground earnings.

SYEP participants receive a W-2 from the NYC government (rather than the employer they worked for through SYEP). Earnings from the NYC government is an extremely good measure of their earnings through SYEP. In principle, this measure could differ from their earnings through SYEP if the individual also held another job with the NYC city government, but mean NYC government earnings among nonparticipants is very small.

III.C. Incarceration Data

We also collected data from the NYS DOCCS on individuals incarcerated in a NYS prison in years up to and including 2013.

12. This measure does not include self-employment income, as reported on the 1099-MISC or 1040 Schedule C. We find negligible impacts of SYEP on these measures of self-employment income.

Everyone who has been confined in NYS prison is listed in the database, except those who were 18 or younger at the time the offense was committed, those who have had their convictions reversed by a court, and certain offenders who are covered by a special provision for relatively minor crimes. The exclusion of youthful offenders is a particularly important data limitation in our context, because most SYEP applicants are 18 or younger at the time of SYEP application, and therefore incarceration episodes that occurred due to crimes around the time of SYEP participation will be excluded from the data (even once they have reached age 19). We also do not observe those jailed in a local jail such as Riker's Island. In total, we observe 466,062 unique incarceration episodes in the DOCCS data. Since the DOCCS data do not include SSN, we match information from the DOCCS data to the SYEP administrative data when first name, last name, day of birth, month of birth, and year of birth all match. Of the SYEP applications, 0.95% match to the DOCCS data; of the SYEP applicants, 1.03% (a total of 2,048 SYEP applicants) match to the DOCCS data. Among those incarcerated, 93.36% were incarcerated once.

III.D. Cause of Death Data

Our main estimates on mortality use the IRS mortality data previously described, which cover the full U.S. population through October 2014. To further investigate the observed effect on mortality, we matched the SYEP data to NYC Department of Health and Mental Hygiene (DOH) administrative data on the cause of death for individuals who died from known causes in NYC from 2005 to 2013 (the most recent year of data currently available). Just as with the IRS data, we match SYEP earnings records to DOH data on the basis of SSN, first name, last name, and month, day, and year of birth. We match 620 unique DOH mortality episodes (occurring from 2005 to 2013 in NYC) to the SYEP data.

III.E. Data Setup

In the discussion that follows, we call "year 0" the year an individual applies to a SYEP lottery. In year 0, an individual participates in the SYEP program (if they win the lottery and take the SYEP job), or alternatively they do not participate. Year 1 refers to the following calendar year, year –1 refers to the year before year 0, and so on.[13] In the years we examine, SYEP gave "special slots" for disabled youth that were not selected by lottery. We drop these applicants from our sample. We also delete observations in which the same SSN is associated with multiple applications in a given year, thus deleting approximately 1,000 observations per calendar year in years –1 to 4 (and fewer in subsequent years). The number of remaining observations in each year from year –1 to year 4 is 294,100, corresponding to 198,454 individuals. The number of observations in each year of data is greater than the number of individuals because some individuals apply to SYEP in more than one year. A total of 113,698 individuals participated in SYEP at some point.

Because individuals can apply to SYEP in more than one year, our setup of the data follows the parallel setting in Cellini, Ferreira, and Rothstein (2010), in which treatment in a given year can affect the probability of treatment in a following year. Following their method, we stack multiple panels of data. In each panel, year 0 is defined as the year an individual participates in a lottery. Thus, an individual appears in multiple panels if she applied to SYEP multiple times.

In any given year over the lottery years we study, around 4% of the eligible population in NYC participated in SYEP. Since we have complete IRS data until 2012, we observe everyone until at least year 4 (as the last lottery we observe is in 2008).

III.F. Summary Statistics

The first column of data in Table I shows summary statistics for the full sample. Given applicants' young ages, it is not surprising that mean total earnings over years 0 to 4 are quite low compared to the general population—only \$3,555. Mean NYC government earnings over this period are \$218.[14] Sixty-three percent are employed in any job, and 50% have any non-NYC government job. Twenty-three percent are enrolled in college in a given year. Median total earnings (shown in Online Appendix Table 9) rises from \$936 in year 0 to \$2,470 in year 4. Turning

13. For example, for individuals in the 2005 lottery, year – 1 refers to 2004, year 0 refers to 2005, year 1 refers to 2006, and so on.

14. Mean NYC government earnings in years 0–4 are \$218—lower than its mean in year –1 (\$257). The mean in year –1 is higher because some of those who apply to SYEP in year 0 participated in previous years and the mean in years 0 to 4 is pulled down by year 0 applicants reaching ages with lower participation rates.

TABLE I

Treatment-Control Balance

(1) Variable	(2) Mean (std. dev.)	(3) Coeff. (std. err.) on treatment
Main outcomes (years 0–4)		
Total yearly earnings	3,555.18 (7,195.92)	—
NYC gov't yearly earnings	218.18 (474.76)	—
Non-NYC gov't yearly earnings	3,334.02 (7,253.66)	—
Has any job	0.63 (0.48)	—
Has any non-NYC gov't job	0.50 (0.50)	—
College enrollment	0.23 (0.42)	—
Lagged outcomes (year –1)		
Total earnings	889.27 (4,482.52)	−23.76 (21.57)
NYC gov't earnings	256.72 (495.72)	−1.81 (2.13)
Non-NYC gov't earnings	632.57 (4,470.96)	−21.96 (21.33)
Has any job	0.32 (0.47)	−0.0024 (0.0019)
Has any non-NYC gov't job	0.13 (0.34)	−0.0016 (0.0012)
College enrollment	0.04 (0.20)	0.00086 (0.00069)
Family income	39,526.34 (29,412.55)	−33.46 (127.23)
SYEP participation	0.21 (0.41)	−0.0014 (0.0018)
Race		
White	0.13 (0.33)	−0.0019 (0.0015)
Latino	0.27 (0.44)	0.00065 (0.0015)
Black	0.48 (0.50)	0.00073 (0.0017)
Other	0.12 (0.33)	0.00050 (0.0016)
Other variables		
Male	0.45 (0.50)	−0.0024 (0.0022)
Age	16.50 (1.63)	0.00055 (0.0086)
# Family members	4.31 (1.85)	−0.0020 (0.0068)
U.S. citizen	0.93 (0.25)	−0.00067 (0.00098)
SYEP-IRS match dummy	0.998 (0.05)	−0.00032 (0.00024)

Notes. The table shows summary statistics and demonstrates that there are no significant differences in covariates across the treatment and control groups. In column (2), we report means of variables, with standard deviations in parentheses. In column (3), we use OLS to regress the variable in question on a dummy for winning the SYEP lottery and provider-year fixed effects, and report coefficients and standard errors on the SYEP win dummy from this regression. The sample includes 294,580 observations for all variables, except in the case of measuring prior year SYEP participation (238,023 observations). Main outcomes are observed in years 0–4 (inclusive) and are observed at a yearly level (so that, for example, the mean of the "has any job" dummy refers to the probability that an individual has a job in a given year). Lagged outcomes are observed in the calendar year prior to the SYEP lottery in question. Family income refers to income from SYEP lottery participants' tax unit. All outcomes are derived from IRS data except gender, race, citizenship, age, and SYEP participation, which are derived from SYEP administrative data. — indicates that for the main outcomes, readers should refer to subsequent tables, which investigate the effect of SYEP on these outcomes in detail. For binary outcomes, we report the mean of a dummy that equals 1 if the characteristic is observed. "Match dummy" refers to a dummy variable that equals 1 if the individual was matched to tax records according to SSN or gender, date of birth, name, and first or last four digits of the SSN.

to the demographics of SYEP applicants, on average they come from disadvantaged family backgrounds and are disproportionately minorities. Mean family income is low ($39,526 in year –1).[15] Forty-eight percent of SYEP applicants are black, far greater than the share of NYC residents who are black, whereas 13% of SYEP applicants are white, far lower than the share of NYC residents who are white. Just under one-half (45%) are male. The mean age is 16.50. The vast majority, 93%, are U.S. citizens.

Online Appendix Table 1 shows the breakdown of SYEP jobs by industry, as reported by SYEP.[16] SYEP reports that much of the sample works at a day care or day camp (36.99%) or at a camp outside of New York City (10.59%). The SYEP industry classification is not based on the North American Industrial Classification System (NAICS), but we use the descriptions provided by SYEP to develop a set of two-digit NAICS codes that corresponds roughly to the industries described by SYEP (the crosswalk is shown in the Online Appendix). To classify SYEP jobs as for-profit, nonprofit, or government, we use data reported by SYEP. For jobs not through SYEP, we classify employed individuals as working at a nonprofit if their employer files a form 990, as working in the government if their NAICS code is 92, and as working for a for-profit otherwise.

IV. Empirical Strategy

Our empirical strategy exploits the random assignment of SYEP access through the lotteries. Since some of those selected for SYEP did not enroll, we use winning the SYEP lottery to instrument for participation. A basic two-stage least squares specification is:

$$P_{ij0} = \alpha_1 W_{ij0} + \mathrm{X}_j\alpha + u_{ij0} \tag{1}$$

$$E_{ijt} = \beta_1 P_{ij0} + \mathrm{X}_j\beta + v_{ijt} \tag{2}$$

Here E_{ijt} is a year-t outcome (such as the level of earnings in year t) of individual i that participated in SYEP provider lottery j.

15. The 2011 American Community Survey reports that mean U.S. household income is $69,821.

16. DYCD did not receive records of the EINs of the firms at which SYEP participants worked. Thus, we are limited to using the industry breakdown provided by SYEP.

W_{ij0} is a dummy for whether the individual won the SYEP lottery or not in year 0. P_{ij0} is a dummy for whether the individual participated in SYEP in year 0. Because individuals applied to providers and the lotteries were run at the provider level in each year of the lottery, we control for a vector X_j of dummies for each provider in each year of the lottery. In some specifications, we control for additional covariates. u_{ij0} and v_{ijt} are error terms. We cluster our errors by SYEP provider, which we view as a conservative choice. There are 59 providers in our data. Clustering our standard errors at the individual level instead leads to nearly identical standard errors (Online Appendix Table 6). We interpret our coefficient β_1 as a local average treatment effect of SYEP among the compliers (i.e., those induced to participate in SYEP by winning the lottery).[17]

We typically investigate the results separately in different years, running our specification for each year t of outcomes separately. In some cases, we examine the results across multiple years (e.g., examining the effect of SYEP on total earnings in years 0–4). In this case, we sum earnings across all of the years examined (e.g., summing earnings across years 0–4) and run our specification with this summed earnings variable as the outcome.

It is possible that SYEP participation in year 0 could affect the probability of applying to SYEP or the probability of accepting the SYEP job conditional on winning the lottery—and thus could affect SYEP participation—in subsequent years. In this case, part of our estimate of the effect of year 0 SYEP participation on subsequent earnings (defined as earnings in calendar years following the calendar year of SYEP participation) could be mediated through the impact of SYEP on future SYEP participation. In the terminology of Cellini, Ferreira, and Rothstein (2010), the specification in equations (1)–(2) is a “static” specification, in which we estimate the total effect of year 0 SYEP participation on earnings in a given year, including effects that are mediated through the channel of the effect of SYEP participation in year 0 on SYEP participation in subsequent years. In the Online Appendix, we also estimate the effect of SYEP participation on

17. Because lottery losers are officially ineligible to participate, this also should represent the average treatment effect on the treated. However, in very rare cases (1.67% of the sample), lottery losers participated in SYEP, for example, because after running all of the lotteries, providers still had remaining slots available and allocated remaining slots in the program to lottery losers.

earnings using the "dynamic" design of Cellini, Ferreira, and Rothstein. In our context, this dynamic estimator effectively yields the effect of SYEP participation in year 0 on earnings in any given year, removing the effect that operates through the channel of the effect of year 0 SYEP participation on subsequent SYEP participation. These two objects of study reflect different conceptual experiments of interest. However, since SYEP participation in year 0 only slightly affects the probability of SYEP participation in subsequent years (years 1–4), the static and dynamic estimates prove to be similar.

In some cases, we investigate a binary dependent variable, like a dummy for whether an individual has a job. In an instrumental variables model with a binary endogenous variable and a binary outcome, models such as a two-stage probit are generally inconsistent, and we run a linear probability model instead (Angrist 2001).[18] When we examine a binary variable pooled across years (e.g., probability of having a job, years 0–4), we define the variable as the probability that the outcome occurs at any point during those years (in the example, the outcome is the probability that an individual has a job at any point in years 0–4).

V. Preliminary Empirical Results

V.A. *Validity of Randomization*

Table I demonstrates the validity of the randomized design by comparing the characteristics of SYEP lottery winners and losers. We run a "reduced-form" ordinary least squares (OLS) regression of characteristics of SYEP applicants on a dummy for winning the lottery and provider-by-year fixed effects. We examine outcomes in the year prior to applying to SYEP and a number of demographic variables. Consistent with the validity of the randomization, none of these variables is significantly related to treatment status. Though not tabulated, we also find insignificant estimates in every other year prior to SYEP enrollment. The probability that SYEP applicants match to the IRS data is also balanced. A joint test of significance of all coefficients on treatment across all variables shows $p = .59$.

18. The coefficients in the linear first-stage and reduced-form regressions are typically nearly identical to the marginal effects in the probit reduced form and first stage, and also to those in a bivariate probit.

TABLE II
EFFECT OF SYEP LOTTERY WIN IN YEAR 0 ON SYEP PARTICIPATION IN EACH YEAR

	(1) SYEP participation	(2) F-statistic
A. Year 0	0.73	4,183.66
	(0.011)***	
B. Year 1	0.031	81.26
	(0.0034)***	
C. Year 2	0.011	47.68
	(0.0016)***	
D. Year 3	0.0031	10.80
	(0.00093)***	
E. Year 4	0.0015	6.42
	(0.00059)***	

Notes. The table shows the results of OLS regressions in which SYEP participation in year 0 and subsequent years is related to SYEP lottery win in year 0, controlling for provider-by-year fixed effects. The table shows marginal effects and standard errors on the SYEP participation dummy. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

V.B. First Stage

Table II shows the first stage—the effect of winning the SYEP lottery in year 0 on SYEP participation in year 0—as well as the effect of winning the SYEP lottery in year 0 on SYEP participation in subsequent years. For year 0 participation, the coefficient on the dummy for winning the SYEP lottery is 0.73, and the F-statistic is 4,183.66. As the take-up rate is 73%, the local average treatment effect (LATE) estimates of the effect of year 0 participation will generally be 1.37 ($=\frac{1}{0.73}$) times as large as the intent-to-treat (ITT) estimates.[19] SYEP participation in year 0 affects the probability of SYEP participation in years 1 to 4 separately, though these effects are very small (3 percentage points or less). Specifications (1)–(2) restrict the first stage to be the same across all providers in all years of the lottery; Online Appendix Table 20 shows that our key results are extremely similar when we allow the first stage to be different across providers or provider-years.

19. Our empirical strategy could also be used to examine the effect of employment in year 0 (through SYEP or other employers) on earnings. In this case, we would scale up the linear estimates by a factor of 1.43.

V.C. Comparison of Compliers and Never-Takers

Online Appendix Table 2 compares predetermined characteristics between lottery winners who participated (compliers) and lottery losers who did not participate (never-takers). Compliers have lower average earnings and a lower probability of being enrolled in year –1, are younger, are more likely to be black, and are more likely to be U.S. citizens.

VI. Effects on Earnings

VI.A. Main Estimates of Effects on Earnings and Probability of Having a Job

Table III shows our main estimates of the effect of SYEP participation on earnings and the probability of having a job. The point estimate of the effect of SYEP participation on total earnings in year 0 is \$876 ($p < .01$), as SYEP participation on average leads to a substantial increase in earnings in the year of SYEP participation. This represents a near doubling of earnings relative to the control group mean. SYEP participation causes year 0 earnings from the NYC government to increase by an average of \$1,085 ($p < .01$). SYEP participation reduces year 0 non-NYC government earnings by \$209 ($p < .01$), or 19.24% of the increase in NYC government earnings.[20] SYEP participation raises the probability of having a job in year 0 (including in both SYEP and non-SYEP jobs) by 71 percentage points. SYEP participation lowers the probability of having a non-NYC government job by 5 percentage points in year 0, indicating modest crowdout. It is notable that crowdout of non-NYC government earnings was 19.28%, whereas crowdout of other jobs was only 5 percentage points. Evidently, conditional on having a job in year 0, SYEP jobs tend to be lower-earning jobs.[21]

In each year from year 1 to year 3, SYEP participation in year 0 lowers total earnings modestly, by around \$100 ($p < .05$). Relative to mean earnings in the control group each year, these

20. Some of this decrease in other earnings could have occurred in year 0 after the summer of year 0.

21. The coefficient on the SYEP participation dummy when total number of jobs is the dependent variable is similar to the effect of SYEP participation on the probability of having a job, suggesting that the effect on holding multiple jobs is not responsible for this reduction in earnings conditional on having a job. We do not have data on hours worked to determine how hours compare in SYEP jobs and other jobs.

TABLE III
Effect of SYEP Participation on Earnings and Employment

	(1) Total earnings	(2) NYC gov't earnings	(3) Non-NYC gov't earnings	(4) Any job	(5) Any non-NYC gov't job
A. Year 0	875.89	1,085.38	−209.27	0.71	−0.048
	(25.08)***	(10.14)***	(24.84)***	(0.0063)***	(0.0035)***
	[1,151.54]	[1,111.93]	[39.61]	[0.30]	[0.27]
B. Year 1	−100.14	45.90	−146.04	0.012	−0.018
	(40.08)**	(5.01)***	(40.09)***	(0.0034)***	(0.0026)***
	[2,239.47]	[2,035.43]	[204.05]	[0.53]	[0.40]
C. Year 2	−94.04	23.25	−117.30	0.0045	−0.0097
	(42.05)**	(3.33)***	(42.42)***	(0.0031)	(0.0027)***
	[3,244.08]	[3,103.31]	[140.77]	[0.60]	[0.52]
D. Year 3	−111.01	8.19	−119.21	−0.00060	−0.0051
	(44.43)**	(2.08)***	(44.27)***	(0.0023)	(0.0023)**
	[4,469.13]	[4,378.43]	[90.71]	[0.66]	[0.62]
E. Year 4	−35.39	4.46	−39.85	0.0013	−0.00034
	(44.82)	(1.58)***	(45.24)	(0.0022)	(0.0022)
	[5,939.09]	[5,884.90]	[54.18]	[0.72]	[0.69]
F. Years 0–4	535.31	1,167.18	−631.67	0.089	−0.0058
	(173.14)***	(15.10)***	(173.78)***	(0.0033)***	(0.0022)***
	[17,043.31]	[16,514.01]	[529.32]	[0.89]	[0.83]
G. Years 1–4	−340.59	81.80	−422.39	0.010	−0.0027
	(154.54)**	(9.51)***	(154.96)***	(0.0021)***	(0.0021)
	[15,891.76]	[15,402.07]	[489.71]	[0.88]	[0.82]

Notes. The table shows coefficients and standard errors on the SYEP participation dummy from two-stage least squares regressions (1)–(2) of earnings and employment outcomes on SYEP participation. Standard errors are in parentheses, and control group means are in square brackets. The instrument for whether an individual participated in SYEP is a dummy indicating that an individual won the SYEP lottery. Each row shows the results for a different year or set of years, and each column shows the results for a different outcome. Our measure of annual earnings comes from W2s, mandatory information returns filed with the IRS by employers for each employee. Having "any job" is defined as earning a positive amount of income, as reported on the W2 form. We control for SYEP provider-by-year dummies so that the estimates are driven by random variation in winning the SYEP lottery. In year 0, the control group mean and the effect on the jobs dummy do not add to 1; this is a consequence of the fact that there are separate lotteries in each provider in each year, with different numbers of observations in each lottery. The number of observations in each regression is 294,100, corresponding to 198,454 individuals. Standard errors are clustered at the level of the SYEP provider. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

negative effects on earnings represent earnings decreases of 4.47%, 2.90%, and 2.48% in years 1, 2, and 3, respectively. From years 1 to 3, SYEP participation raises NYC government earnings slightly ($p < .01$); lowers non-NYC government earnings modestly ($p < .01$); and lowers the probability of having a non-SYEP job slightly ($p < .01$ in years 1 and 2, and $p < .05$ in year 3). SYEP slightly raises the probability of having any job in year 1. This combination of results again suggests that SYEP leads individuals to earn less conditional on having a job. The effect on total earnings in year 4 turns insignificant, with a small confidence interval. These results are consistent with the Card, Kluve, and Weber (2010) meta-analysis findings that programs for youth, and programs involving subsidized jobs, often do not have positive impacts on labor market outcomes.

The effect of SYEP on total earnings in years 0 through 4 is positive and substantial ($535). The effect on total earnings is less than half of the average total of SYEP transfers over this period ($1,167); the average decrease in other earnings is 54.12% of average SYEP earnings. There is also a positive effect of 9 percentage points on the probability of having any job during these years. Finally, the impact on total earnings in years 1–4 is negative and substantial, but the impact on the probability of having a job during this period is small and positive.

Online Appendix Table 3 shows the "intent-to-treat" estimates. The coefficients are 73% as large as the IV estimates in Table III. When we remove the provider-year dummies and therefore do not rely on randomized variation (but control for all of the 13 predetermined covariates listed in Online Appendix Table 4), the estimates differ dramatically: SYEP participation is estimated to raise total future earnings (e.g., over years 1–4, SYEP is estimated to raise total earnings by $593, $p < .01$).

Our main specification examined years 0–4 to hold the sample size constant across years, and because the estimates turn insignificant beginning in year 4. Online Appendix Table 4 shows the estimates for years 5, 6, and 7. As we might expect from random chance, one estimate is marginally significant, though not robust: the estimate for earnings in year 7 is positive and marginally significant ($p < .10$) without controls, but it becomes insignificant when we add the controls. Over years 0–7, the results are similar to those over years 0 to 4.

Online Appendix I discusses a wide variety of variations on these basic results, including adding controls to the regressions; using the initial SYEP lottery as the instrument; including only individuals who match according to SSN; clustering at the individual level; the dynamic specification of Cellini, Ferreira, and Rothstein; investigating the effect of SYEP separately for those who had or had not previously participated in SYEP; using a SYEP lottery win as an instrument for the total number of times participating in SYEP; and estimating the effect on other family members' earnings. Online Appendix Tables 5 through 8 show that we continue to find comparable results throughout these alternative specifications. Online Appendix Table 9 shows that winning the SYEP lottery raises median earnings in year 0 and has generally positive effects on median earnings in subsequent years but negative effects in higher quantiles, the latter of which drives the negative earnings results in years 1 to 3.

Online Appendix Table 10 shows that SYEP has a more significant and negative effect on subsequent earnings among those ineligible for WOTC than among those eligible (i.e., ages 16–17 living in an Empowerment Zone [EZ]); among whites than among other race groups; among older SYEP participants than younger; and among those who worked in year –1 than among those who did not.[22] We find an insignificant effect of SYEP on total earnings in the 2005–2006 lotteries but a substantial negative and significant effect in the 2007–2008 lotteries.

VI.B. Effects on Type of Job

We investigate the effect of SYEP on earnings in different industries. As an illustrative exercise, we classify industries into two clusters: those in which the two-digit industry represents a greater percentage of total jobs among SYEP-provided jobs than among jobs held by the control group (Cluster 1), and industries in which the opposite is true (Cluster 2). Online Appendix Table 1 lists the industries in each Cluster. Table IV shows that SYEP participation leads to an increase in Cluster 1 earnings and employment both in year 0 and in subsequent years.[23] Table IV also shows that SYEP strongly raises earnings in nonprofit firms in year 0 and continues to raise these earnings modestly through year 4 (with similar results for the probability of having a job). Earnings in for-profit firms are lowered by SYEP by around $100 a year in years 0, 1, 2, and 3. SYEP increases earnings in government jobs in year 0 but modestly reduces government earnings in years 3 and 4.

22. An EZ is an area with particularly high poverty and/or emigration.

23. When we perform these regressions using the dynamic estimator of Cellini, Ferreira, and Rothstein (2010), we obtain very similar results, suggesting that the effect is not driven by SYEP participants reapplying to SYEP but instead by some stickiness in job choice (see the discussion in Online Appendix II).

TABLE IV

EFFECT OF SYEP PARTICIPATION ON EARNINGS AND EMPLOYMENT BY INDUSTRY AND JOB TYPE

	(1) Cluster 1	(2) Cluster 2	(3) For-profits	(4) Nonprofits	(5) Gov't
Panel A: Effects on total earnings					
A. Year 0	966.61	−92.08	−75.95	786.57	165.24
	(12.62)***	(24.97)***	(26.88)***	(28.29)***	(25.87)**
	[208.38]	[944.90]	[1,050.71]	[40.39]	[60.92]
B. Years 0–4	983.17	−444.51	−436.81	854.63	117.07
	(55.80)***	(164.64)***	(170.56)***	(38.33)***	(51.72)**
	[2,829.28]	[14,219.15]	[15,704.70]	[493.11]	[847.60]
C. Years 1–4	16.56	−352.43	−360.86	68.05	−48.17
	(52.88)	(146.92)**	(152.83)**	(16.88)***	(34.26)
	[2,620.90]	[13,274.24]	[14,653.99]	[452.72]	[786.68]
Panel B: Effects on having any job					
A. Year 0	0.81	0.046	0.043	0.67	0.16
	(0.0063)***	(0.0067)***	(0.0090)***	(0.022)***	(0.021)***
	[0.093]	[0.24]	[0.26]	[0.033]	[0.034]
B. Years 0–4	0.45	0.0085	0.011	0.51	0.13
	(0.0067)***	(0.0025)***	(0.0025)***	(0.016)***	(0.017)***
	[0.46]	[0.81]	[0.83]	[0.23]	[0.20]
C. Years 1–4	0.034	0.00065	−0.00018	0.040	0.0049
	(0.0039)***	(0.0022)	(0.0022)	(0.0039)***	(0.0029)*
	[0.44]	[0.79]	[0.82]	[0.21]	[0.18]

Notes. The table shows coefficients and standard errors on the SYEP participation dummy from IV regressions of earnings and employment outcomes on SYEP participation. The mean of the dependent variable in the control group is shown in brackets, below the standard error in parentheses. The instrument for whether an individual participated in SYEP is a dummy indicating that the individual won the SYEP lottery. Columns (1)–(2) show the results of IV regressions in which earnings (Panel A) or the probability of having a job (Panel B) in a given industry cluster and year are related to SYEP participation. Using DYCD's industry classification, Cluster 1 corresponds to industries that are overrepresented among SYEP lottery winners relative to SYEP lottery losers: arts and recreation, camp (out of city), community/social service, daycare/day camp, educational services, and healthcare/medical. We classify these as belonging to one of the following cluster of NAICS codes: 61, 62, 71, or 92. Cluster 2 corresponds to other SYEP classifications and NAICS codes. Columns (3)–(5) show the results of IV regressions in which earnings or the probability of having a job in a given sector (for-profit, nonprofit, or government) are related to SYEP participation. In years 0–4, mean yearly earnings in Cluster 1 is $681.08; in Cluster 2 is $2,875.31; in for-profits is $3,190.88; in non-profits is $187.45; and in government employers is $177.36. In years 0–4 (considering each year as a separate observation), the probability of employment in Cluster 1 and Cluster 2 is 25.47% and 46.56%, respectively. In years 0–4 (considering each year as a separate observation), the probability of employment in the for-profit, nonprofit, and government sectors is 48.94%, 7.63%, and 13.56%, respectively. See Gelber, Isen, and Kessler (2014) for estimates in each year from year 1 to year 4. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level. See other notes to Table III.

VI.C. *Interpreting the Earnings Results*

The negative effects on subsequent earnings are small relative to likely lifetime earnings, but it is worth considering the reasons behind the arguably surprising result that SYEP participation decreases earnings among a young group with little prior work experience, even during the Great Recession. Our randomized design is well suited to determine the program's causal impacts, but less equipped to determine the mechanisms that mediate these impacts. Thus, we can say only whether the predictions of our hypotheses are consistent with the data.

SYEP could crowd out jobs that could have led to greater future earnings.[24] As we discuss in Online Appendix II, we find that groups that experienced greater year 0 crowdout also experienced greater decreases in subsequent total earnings, as we would expect if crowdout of other experiences in year 0 leads to decreases in subsequent earnings. Furthermore, the subgroup analysis finds more negative impacts for groups that were more likely to otherwise be working in year 0—that is, older individuals and those with a job in year –1.[25] Relatedly, SYEP decreases the probability that an individual continues working for a past employer (Online Appendix Table 11), raising the possibility that SYEP harms a participant's career development with an existing employer.

Online Appendix Table 12 shows the interaction between winning the SYEP lottery and the fraction of jobs in the SYEP provider in Cluster 1. The regressions suggest that a Cluster 2 (Cluster 1) job placement increases (decreases) earnings both during and after SYEP, further suggesting that the effect of SYEP on year 0 job type is a culprit for the negative effect on subsequent earnings. However, heterogeneity in the effects across providers could instead be driven by other factors that happen to be correlated with the types of jobs in each provider.[26]

Online Appendix II discusses other potential explanations including income effects, time inseparability of leisure, signaling, changes in the labor supply curve, and peer effects; the evidence on these mechanisms is mixed, though we cannot rule them out. The next section shows that the decrease in subsequent earnings is not driven by college enrollment.

24. Our results find only 19.28% earnings crowdout in year 0, which may limit the potential quantitative importance of this explanation. In principle, however, it is possible that SYEP participation in year 0 could also negatively affect future earnings relative to the counterfactual of having no job in the formal sector in year 0.

25. These samples differ on average along many characteristics, so this evidence is merely suggestive.

26. When we regress earnings in years 1–4 on provider dummies interacted with the SYEP lottery win dummy, a joint F-test of equality of the coefficients across providers is not significant ($p = .33$).

VII. Effects on College Enrollment

In principle, SYEP could affect schooling decisions. Schooling is an investment that could lead individuals to decrease earnings in the years immediately after SYEP participation, as individuals focus on academics or enroll in college, but raise earnings in the slightly more distant future. Table V investigates the effect of SYEP participation on college enrollment.[27] The table reports results with the full sample for consistency with our other estimates, although the results are extremely similar when we limit the sample only to observations when individuals are 18 and over (as those under 18 rarely attend college). We find no significant impact throughout, with very small standard errors. When we estimate the effect of SYEP participation on total years enrolled in college in years 0–4, the point estimate is −0.000017, and the confidence interval rules out an increase or decrease in total years of college greater than 1/70 of a year. These estimates are nearly identical when we control for covariates (Online Appendix Table 13) or with the dynamic specification.

If SYEP had a positive impact on high school attendance, this could reduce individuals' earnings while they are of high school age. However, a range of evidence fails to support this hypothesis. Using SYEP data from 2007, Leos-Urbel (2012) found that winning the SYEP lottery slightly decreased the probability that an individual attended high school the following school year, though this effect was significant only at the 10% level.[28] Although we do not have data on high school attendance, we can indirectly investigate whether an effect on high school could drive our negative earnings results. Online Appendix Table 14 shows that among those older than 18, who are too old to have still been in high school after the summer of SYEP, SYEP decreased subsequent earnings much more than in the full population. Furthermore, if there were a significant positive impact on high school attendance or completion, then we might expect (i) a positive impact of SYEP

27. Our data lack a measure of whether individuals graduated from college.

28. That paper's main focus is on the correlation between SYEP participation and log days attending school conditional on attending school (and finds that participation is associated with a very small, 1% increase in days attending school conditional on attending school). However, it is difficult to interpret this correlation as the causal effect of SYEP participation on days attended because the sample attending school is selected (due to the negative effect of SYEP participation on the probability of high school attendance).

TABLE V
EFFECT OF SYEP PARTICIPATION ON COLLEGE ENROLLMENT

	(1) Coefficient (std. err.) on SYEP participation	(2) Control group mean
A. Year 0	0.0011	0.079
	(0.0015)	
B. Year 1	0.0029	0.15
	(0.0019)	
C. Year 2	−0.0012	0.24
	(0.0024)	
D. Year 3	0.00050	0.33
	(0.0021)	
E. Year 4	−0.0032	0.36
	(0.0024)	
F. Years 0–4	−0.0035	0.50
	(0.0025)	
G. Years 1–4	−0.0029	0.49
	(0.0025)	
H. Total years of college	−0.000017	1.17
	(0.0071)	

Notes. The table shows coefficients and standard errors on the SYEP participation dummy from two-stage least squares regressions of a college attendance dummy or total years of college on SYEP participation. The instrument for whether an individual participated in SYEP is a dummy indicating that an individual won the SYEP lottery. The results are similar if we limit the sample to those 18 years of age and older (because younger individuals are unlikely to go to college). Rows F and G investigate the impact of SYEP enrollment on the probability that an individual attends college at some point during years 0–4 and 1–4, respectively. Row H shows the effect of year 0 SYEP participation on the total number of years enrolled in college over years 0–4 cumulatively. The mean total number of years enrolled in college over years 0–4 is 1.17. Column (2) shows the mean of the dependent variable in the control group that lost the lottery. See other notes to Table III.

on earnings several years later; (ii) a larger negative impact on near-term earnings in the younger group than the older group; and an eventual positive impact on the probability of (iii) college enrollment and (iv) having a job. None of these predictions is observed in the data (in fact, i and ii are the opposite of what we observe in the data).

VIII. EFFECTS ON INCARCERATION

Keeping youth out of trouble during the summer could lead them away from crime and reduce the probability of incarceration. In Table VI, the dependent variable is a dummy for whether an individual appears in the DOCCS incarceration

TABLE VI
EFFECT OF SYEP PARTICIPATION ON INCARCERATION

	(1) 2SLS	(2) Control incarceration mean (×100)	(3) N	(4) p-value of test for equality
A. Full population	−0.098 (0.046)**	0.99	294,100	—
B. 19 and older	−0.48 (0.22)**	1.09	24,787	0.06
C. 16 to 18	0.0024 (0.069)	0.98	137,997	
D. Under 16	−0.16 (0.073)**	0.98	131,316	
E. Males	−0.22 (0.094)**	2.09	132,512	0.011
F. Females	0.023 (0.020)	0.078	161,588	
G. White	−0.15 (0.087)*	0.18	37,162	0.16
H. Black	−0.18 (0.073)**	1.45	142,468	
I. Latino	0.049 (0.081)	0.62	78,947	
J. Other	−0.068 (0.095)	0.33	35,523	
K. Prior work	−0.054 (0.091)	0.88	94,622	0.51
L. No prior work	−0.12 (0.050)**	1.04	199,478	
M. Emp. zone	−0.057 (0.080)	1.11	149,137	0.38
N. Not emp. zone	−0.14 (0.051)***	0.87	144,963	

Notes. Column (1) shows coefficients and standard errors on the SYEP participation dummy from two-stage least squares regressions of a dummy for incarceration in NYS on SYEP participation. Column (2) shows the mean of the dependent variable in the control group. Each row shows the results for a different population. We multiply the incarceration dummy by 100 so that coefficients show percentage point changes (for the reader's ease). "Emp. zone" refers to an Empowerment Zone. The final column reports p-values from tests of equality across the coefficients estimated across subgroups within a given category (e.g., across race groups, ages, or genders). *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

database. To parallel our main specification for employment, we estimate a linear probability two-stage least squares model.

In the full population (row A), we find that SYEP reduces the probability of incarceration by 0.098 percentage points.[29] This is a 9.93% reduction relative to the baseline incarceration rate in the control group of 0.99 percentage points. In combination with the number of SYEP participants, this implies that 112 fewer people were incarcerated by 2013 as a result of SYEP participation between 2005 and 2008. This result is notable in light of literature reviews that conclude that "work doesn't work" in reducing crime (Bushway and Apel 2012; Cook et al. 2014). Recall that only individuals 19 and older when they commit a crime that leads to incarceration in a NYS prison are included in our DOCCS incarceration data. In the full sample, the estimates thus incorporate possible effects on incarceration for future crimes (for both those under and over 19 at the time of SYEP participation) and on incarceration for crimes committed simultaneously with SYEP participation (only for those 19 and older at the time of SYEP participation). Online Appendix Table 15 shows that the results are very similar when controlling for covariates, when the dependent variable is number of times incarcerated, and with a probit.

We find important differences in the incarceration effect across subgroups. Although we do not observe the timing of the crime committed, we find that SYEP causes a dramatic reduction in the incarceration rate among those who are 19 or older in the summer they participate in SYEP, among whom both incapacitation effects and effects on future behavior are possible. The reduction in the incarceration rate due to SYEP for this group is 0.48 percentage points ($p < .05$) — a very large (44.2%) reduction relative to the baseline rate of 1.09 percentage points in the control group. In the group 18 and under when they participate in the program, the estimated effect is smaller and not quite statistically significant at the 10% level ($p = .13$). The point estimates also suggest that SYEP reduces incarceration more among males than among females, more among those without prior work experience than those with prior experience, more among blacks and

29. If the treatment observations had better data quality than the control observations, this would bias us toward estimating a positive effect of SYEP on incarceration—the opposite of our finding.

whites (particularly blacks) than among Latinos and other races, and more among those outside EZs than those in them—though the treatment effects are only significantly different across subgroups in comparing males and females. The effects are significantly different across providers ($p < .01$). Online Appendix Table 16 shows the results for subgroups within the 19-and-older group.

IX. Effects on Mortality

Paralleling the negative effects on incarceration, keeping youth out of trouble during the summer could lead them down a safer path, and in extreme cases could even keep them alive. We observe in the IRS data that 0.38% of the sample of SYEP applicants die by October 2014. In Table VII, we create a dummy representing whether an individual has died by 2014 in the IRS data and again estimate a linear probability two-stage least squares model.[30]

In the full population, SYEP reduces the probability of mortality by 0.073 percentage points ($p < .01$). This represents a reduction in mortality of 17.97% relative to the baseline rate. In combination with the number of SYEP participants, the estimates imply a reduction of 83 deaths by 2014 due to the SYEP program in years 2005 to 2008.

The small number of deaths prevent us from finding statistically significant differences in the treatment effect across all groups, but the absolute value of the point estimate is larger among males than among females; among Latinos, blacks, and other races than among whites; among the younger group than among the older group; among those who did not work prior to SYEP participation than among those who did work; and among those living in EZs relative to other areas. These effects need not operate through a reduction in incarceration, and those whose lives were saved could differ from those kept from incarceration; nevertheless, the subgroups that show larger mortality effects typically, but not always, correspond to the subgroups that show an incarceration effect.

30. Online Appendix Table 19 additionally shows that we obtain similar results with controls, with a Cox proportional hazard model, and with a probit.

TABLE VII
EFFECT OF SYEP PARTICIPATION ON MORTALITY

	(1) 2SLS	(2) Control mortality mean (×100)	(3) *N*	(4) *p*-value of test for equality
A. Full population	–0.073 (0.031)**	0.41	293,761	—
B. Males	–0.14 (0.06)**	0.66	132,351	0.062
C. Females	–0.016 (0.032)	0.19	161,410	
D. White	0.0030 (0.076)	0.19	37,150	0.38
E. Black	–0.058 (0.048)	0.52	142,278	
F. Latino	–0.14 (0.055)**	0.35	78,848	
G. Other races	–0.054 (0.074)	0.20	35,485	
H. Older	–0.025 (0.047)	0.42	147,008	0.15
I. Younger	–0.11 (0.040)***	0.40	146,753	
J. Work in year –1	0.041 (0.059)	0.36	94,610	0.039
K. No work in year –1	–0.12 (0.043)***	0.43	199,151	
L. Emp. zone	–0.090 (0.044)**	0.41	148,959	0.62
M. Not emp. zone	–0.059 (0.044)	0.40	144,802	
N. By year 4	–0.013 (0.022)	0.20	294,238	0.0070
O. By year 9	–0.21 (0.084)**	0.69	56,557	

Notes. Column (1) shows coefficients and standard errors on the SYEP participation dummy from two-stage least squares estimate using a linear probability model. Each row reports the results for a different population (rows B–M) or a different time period (rows N and O). We eliminate from the regressions those rare cases of individuals who died prior to participating in SYEP, which explains the difference in the sample size between the full sample here and elsewhere, and is uncorrelated with winning the lottery. Column (2) shows the mean of the mortality dummy in the control group, multiplied by 100, in each group or time period. So that readers can more easily interpret the results, we have multiplied the dependent variable by 100. In rows N and O, we show the effect of SYEP on a dummy for whether an applicant died *by* a given year. See Tables III and VI for other notes and information on samples. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

We also show the effect of SYEP on a dummy for whether an applicant died by year 4 or year 9 (Online Appendix Tables 17 and 18 show the effects separately by each year).[31] The cumulative effect is insignificant by year 4 but becomes substantial and significant by year 9. In data on the full U.S. population from the Social Security Administration actuarial life tables, as well as in our data, the yearly death rate increases several-fold from the mid-teen years to the mid-twenties. Thus, it is not surprising that we estimate larger effects on mortality in later years. These later mortality benefits indicate lasting effects of the program. The effects are insignificantly different across providers ($p = .22$).

IX.A. Cause of Death

As a secondary analysis, we investigate the particular causes of death that were affected by SYEP participation using DOH data. The DOH data only contain deaths in NYC and only cover years through 2013, whereas many of the deaths in the IRS data (among both SYEP lottery winners and losers) occur in 2014. Thus, we might expect to find smaller effect sizes, estimated with less statistical power, in the DOH analysis.

Given that our evidence suggests that SYEP keeps youth out of trouble, we may be particularly interested in the effect of SYEP on the probability of death by external causes, which include homicide, suicide, accidents, and other extrinsic causes.[32] These account for 69.66% of all deaths in our data by 2013. Among SYEP applicants in our sample, the most common cause of death is homicide, accounting for 47.52% of all deaths, and reflecting a much higher percentage than in the population as a whole in this age range.[33]

Table VIII shows our results. The DOH data suggest that SYEP causes a reduction in deaths from external causes, representing a 20% reduction relative to the control group, although

31. As death records enter the IRS data more promptly than do earnings data, we have complete mortality data for the 2005 cohort through October 2014, which is in year 9, whereas we have complete earnings and other data for the 2005 cohort only through year 7.

32. See http://www.nyc.gov/html/doh/downloads/pdf/vs/vs-population-and-mortality-report.pdf for the classification of causes into these categories. Natural causes represent all deaths other than external causes.

33. See http://www.cdc.gov/injury/wisqars/pdf/10LCID_All_Deaths_By_Age_Group_2010-a.pdf.

TABLE VIII

EFFECT OF SYEP PARTICIPATION ON MORTALITY BY CAUSE OF DEATH

	(1) 2SLS	(2) Percent of deaths by 2013
Panel A: Death from any cause comparison between DOH and IRS data		
A. Any cause (DOH)	−0.050	100
	(0.024)**	
B. Any cause (IRS through 2013)	−0.059	100
	(0.028)**	
Panel B: Specific causes of death from DOH data		
C. External causes (DOH)	−0.039	69.66
	(0.021)*	
D. Homicide (DOH)	−0.024	47.52
	(0.018)	
E. Nonhomicide external causes (DOH)	−0.015	22.15
	(0.013)	
F. Natural causes (DOH)	−0.011	30.34
	(0.014)	

Notes. Each row shows the results of a different regression where a dummy for a different cause of death is the dependent variable in our two-stage least squares, linear probability model (1)–(2). So that readers can more easily interpret the results, we have multiplied the dependent variable by 100. The results are comparable with Cox or probit models; we show a two-stage least squares model here to show results that are comparable to the IV results in our other tables. It is not surprising to find a slight discrepancy between the DOH and IRS data results in rows A and B, because the IRS data cover all deaths, whereas the DOH data cover only deaths in NYC. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

this estimate is significant only at 10% ($p = .060$).[34] The point estimate is 78% as large as the point estimate of the effect on mortality by any cause in the DOH data. The point estimate shows that SYEP reduces the probability of death by homicide, representing a 17% reduction relative to the control group, but the estimate is insignificant at conventional levels ($p = .18$). The point estimate of effect on the probability of death by homicide is 49% as large as the effect on mortality by any cause in row A. The effect on death from homicide is significant at 10% in various subgroups, including those who had not previously worked. When we estimate the regression for nonhomicide external causes or "natural" causes the point estimates are much smaller and are insignificant ($p = .25$ and $p = .44$, respectively).

34. Again, if treatment observations had better data quality (i.e., were more likely to match) than control observations, this would bias us toward estimating that SYEP increases mortality—the opposite of our findings.

To interpret the effect on dying in NYC, and of independent interest, we investigate whether SYEP has an effect on the probability that participants remain in NYC. Among those who work in year 4, 88.88% work in NYC. Regressing a dummy for working in a NYC zip code in year 4 on the SYEP participation dummy (instrumented as usual) shows a coefficient of −0.0028 on the SYEP dummy ($p = .101$, insignificant at conventional levels). SYEP likewise has no significant effect on the probability of working in NYS (relevant to the incarceration results above).

X. Conclusion

We investigate the effects of summer employment on youth by analyzing the NYC SYEP from 2005 to 2008, which used a random lottery to select applicants for access to the program. We can now revisit the three broad rationales for programs that support summer youth employment: (i) transferring to youth; (ii) raising future earnings, employment, or education; and (iii) keeping youth "out of trouble."

We find support for the first rationale. SYEP increases contemporaneous employment and net earnings and transfers net income to participants. SYEP shows modest (19.24%) crowdout of other contemporaneous earnings, which is small relative to most previous results (Card, Kluve, and Weber 2010). Crowdout of earnings in the year of SYEP participation along with the subsequent years is more substantial (54.12%).

We find no evidence in favor of the second rationale. On balance, we find the opposite: SYEP lowers subsequent earnings for three years following SYEP participation, has little impact on the probability of future employment, and has no impact on college enrollment. The impact on subsequent earnings is small relative to likely lifetime income, but it is substantial relative to the size of the SYEP transfer in year 0 (31.38% of the transfer).

Finally, we find that SYEP succeeds in the goal of keeping youth out of trouble. SYEP leads to decreases in incarceration and mortality rates that are small in percentage point terms but large relative to the baseline rates. The reductions in incarceration and mortality parallel the typically positive effects on lower quantiles of the earnings distribution, suggesting that SYEP improves the left tail of outcomes, and the effects on higher quantiles of earnings suggest modest negative effects on the right tail. Similarly, the mortality and incarceration reductions typically appear strongest in the more disadvantaged groups, whereas the earnings crowdout typically appears largest for less disadvantaged groups. The stronger incarceration results in the 19 and older group, for which we observe results for contemporaneous criminal activity suggest that a substantial portion of the effect on crime could operate through incapacitation (preventing youth from engaging in crime during the summer they are working) or other near-term effects on criminal activity. By contrast, the mortality effects are significant only several years after SYEP participation—suggesting that in this case youth are kept out of trouble by putting them on a path that affects them years after the program.

The mortality effects have large benefits: the value of a statistical life (VSL) is estimated to be in the range of $9 million for prime-age workers in real 2013 dollars (Viscusi and Aldy 2003), implying benefits of $747 million for the estimated 83 lives saved by SYEP.[35] It is clear that the mortality benefits will be large within any plausible range of the value of life, although there is some uncertainty about the exact value of the benefits.[36] The reduction in incarceration has more modest aggregate benefits: combining Donohue's (2009) estimates of the per crime cost of an Index I crime with our estimates of the reduction in incarceration, the reduction in incarceration corresponds to a $4.66 million net benefit to society using Donohue's upper-end estimates of the benefits per crime and a $1.03 million net benefit using Donohue's lower-end estimates.[37] Nonetheless, it is possible that the effect on crime could have much larger implications for the cost-benefit analysis if we incorporated the effect on prevalent crime outcomes that are unobserved in our data. It is illustrative to note that there were 59 times more total reported violent and

35. The lower end of the confidence interval shows that SYEP saved only eight lives, but all of the effects (including the earnings effects) are estimated with error—and the point stands that SYEP has mortality benefits that are likely quite large and therefore have the potential to be pivotal in the cost-benefit analysis.

36. The lower end of the plausible range of the VSL is around $5.25 million (Viscusi and Aldy 2003), which would still imply very large SYEP mortality benefits of $436 million. For SYEP participants, who have more years of life remaining than the typical prime-age worker does, the VSL could be higher than $9 million (Viscusi and Aldy 2003). On the other hand, the value-of-life estimates typically have positive income elasticities, whereas SYEP participants typically have low income.

37. Index I crimes include willful homicide, forcible rape, robbery, burglary, aggravated assault, larceny over $50, motor vehicle theft, and arson.

property crimes in NYS in 2012 than the number of newly incarcerated individuals in a NYS prison (FBI 2012; Carson and Golinelli 2013). This suggests that the benefits of the impact on crime could be many times as large as the benefits above of reduced incarceration, if the impact on other crime outcomes is comparable to the impact on incarceration.[38]

There are many costs and benefits we do not observe, and it is not possible to determine with certainty whether the benefits of the program outweigh the costs. For example, we do not observe the value of the goods produced by SYEP participants, the cost of other public programs, and so on. We are also unable to take into account any general equilibrium effects of SYEP.[39] It is possible that SYEP jobs displace jobs that employers would have otherwise offered, creating additional costs of the program—though we consider it unlikely that there is one-for-one displacement given that NYC pays for SYEP jobs.[40] We also do not observe externalities or nonpecuniary benefits of jobs. We do not observe underground earnings, but we believe it is unlikely that this would dramatically affect our results. To overturn our finding that SYEP decreased total earnings in years 1–4, we would have to posit that SYEP raised underground earnings in these years. If anything, one might instead expect SYEP to push individuals into the formal sector, as SYEP itself is in the formal sector and our other evidence shows that SYEP industries are "sticky."

Nonetheless, it is clear that the $747 million in mortality benefits is substantial compared to plausible estimates of the

38. However, Heller (2014) estimates a significant impact on violent crime arrests (for which eventual incarceration is more likely) but no impact on other arrests, raising the possibility that the impacts on other crime outcomes are not commensurate with the impact on incarceration. On the other hand, among the 19-and-older group, for which we have the best data, the incarceration benefits are much larger relative to the costs.

39. Crépon et al. (2013) find that positive effects of job assistance come at the expense of other labor market participants. It is possible that such general equilibrium effects could arise in our context, but note that SYEP reduced individuals' subsequent earnings (which is unlikely to have come at the expense of others), and that SYEP is small relative to the entire NYC labor market and even the NYC youth labor market.

40. In another context, a public employment program could increase the equilibrium wage and therefore cause displacement, but in our context wages are regulated to be at least the minimum wage. The crime or mortality impacts could also have general equilibrium effects, such as displacement by other crimes (e.g., Yang 2008).

costs of the program. Due to the SYEP program in 2005–2008, the discounted value of the reduction in non-SYEP earnings is \$99.8 million; the discounted administrative costs of SYEP are \$50.4 million, which is equal to the opportunity cost of these expenses if they are bought at competitive prices; theory tells us that the opportunity cost of time of SYEP participants should have been less than the discounted transfers to SYEP participants, or \$186.0 million; and the deadweight cost of the taxes raised to fund SYEP equals the discounted accounting cost of SYEP, \$236.4 million, multiplied by the marginal social cost of public funds.[41] Although we cannot say with certainty whether SYEP's benefits outweigh its costs, it is clear that SYEP's mortality benefits are very large, and that they have a strong potential to be pivotal in determining whether the program's benefits outweigh its costs. Our results also suggest that earnings crowd-out may be minimized, and the mortality reductions maximized, by targeting SYEP toward groups with weaker alternative job opportunities, such as younger individuals.[42]

As in other empirical settings, our estimates are local—in our case, to the SYEP compliers. However, our results may have important implications for other efforts to improve youth employment outcomes, including the Work Opportunity Tax Credit.[43] Indeed, the most salient difference between our study and previous work appears to be driven by our ability to observe mortality, as opposed to differences in sample characteristics. Like previous studies (e.g., Hollister, Kemper, and Maynard 1984; Couch 1992; Cave et al. 1993; Bloom et al. 1997), we find that SYEP did not increase future earnings and that earnings effects on their own could not justify the program's costs in a cost-benefit analysis. SYEP applicants are on average younger than in the programs

41. We discount to 2005 using a 3% real discount rate and express all dollar values in real 2013 terms.

42. In the younger group, the mortality gains are valued at \$594 million; incarceration gains between \$1 million and \$3 million; the discounted earnings reduction at \$29 million; discounted administrative costs at \$25 million; the maximum opportunity cost of time is \$75 million; and the deadweight cost is \$100 million multiplied by the marginal social cost of public funds. The incarceration benefits may be largest in the oldest group, though that is difficult to judge given the data limitations. The results also suggest larger net benefits in EZs than other areas.

43. Our results are consistent with previous studies finding limited earnings effects of subsidies for hiring disadvantaged groups like the WOTC (Hamersma 2003). If the WOTC leads to no significant positive impact on earnings, then the direct fiscal impact is not offset by changes in later tax receipts.

studied in this previous work, and older SYEP applicants show the biggest negative earnings impacts. This suggests that we could observe even more negative earnings effects if our sample had the same age distribution as programs studied in this previous work.[44] Unlike previous studies, we find that the mortality reduction implies a very large source of new benefits, which has a strong potential to be pivotal in the cost-benefit analysis. SYEP participants tend to be disadvantaged, but unlike the programs analyzed in this previous work, which targeted youth with a record of delinquency or crime and/or youth who had dropped out of high school, SYEP does not include or exclude youth based on criminal records. The fact that the net benefits of SYEP tend to be largest in disadvantaged groups could suggest that the net benefits could be more positive if SYEP were targeted at such groups, thus reinforcing our conclusions.

Other active labor market programs for youth may or may not have such mortality benefits, but it is worth noting that like SYEP, other youth programs have been found to keep youth out of trouble (Schochet, Burghardt, and McConnell 2008; Heller 2014). Data should be gathered to determine whether mortality benefits appear in other contexts.

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Supplementary Material

An Online Appendix for this article can be found at QJE online (qje.oxfordjournals.org).

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44. The racial composition of youth are similar between these other programs and SYEP.

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