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

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ABSTRACT

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: (1) provide contemporaneous income support to participants; (2) encourage work experience that improves future employment and/or educational outcomes of participants; and/or (3) keep participants "out of trouble." We study randomized lotteries for access to New York City's Summer Youth Employment Program (SYEP), the largest summer youth employment program in the U.S., by merging SYEP administrative data on 294,580 lottery participants to IRS data on the universe of U.S. tax records and to New York State administrative incarceration data. In assessing the three rationales, we find that: (1) 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; (2) SYEP participation causes a moderate decrease in average earnings for three years following the program and has no impact on college enrollment; and (3) 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.

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1. Introduction

Many federal, state, and local policies attempt to support individuals' labor market prospects, including public employment and subsidized employment programs. In many cases, these programs focus on encouraging youth employment — often summer youth employment in particular. City programs across the country provide youth with summer jobs — the fifty most populous cities in the country have all had summer youth employment programs in the last five years — and the Work Opportunity Tax Credit (WOTC) subsidizes employment of summer youth employees. Expanded employment programs have also been used during times of high unemployment; for example, the American Recovery and Reinvestment Act (ARRA) provided states with $1.2 billion to give disadvantaged youth access to employment and training during the Great Recession, with a particular focus on summer employment (Bellotti *et al.* 2010). As youth unemployment remains elevated following the Great Recession, youth employment programs have received increased scrutiny from policy-makers. While literature typically finds that other (*i.e.* non-summer-employment) active labor market programs for youth have costs that outweigh their benefits (Stanley, Katz, and Krueger 1998; Heckman, Lalonde, and Smith 1999; Lalonde 2003), summer youth employment has "received relatively little attention from program evaluators" (Lalonde 2003, p. 532).

Programs to support summer 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 paper, states that SYEP aims to "provide supplemental income to aid low income families."[1] Similarly, stimulus efforts like ARRA summer youth spending aim to increase contemporaneous net earnings and employment (among other goals). A second rationale is that summer work experience could improve future employment outcomes, perhaps particularly for disadvantaged youth who would otherwise have low summer employment rates. For example, summer employment might lead to increases in human capital — the NYC DYCD also states that SYEP aims to

[1] See http://usmayors.org/workforce/documents/2010-7-01USCOMWDCSYEPPresentation011910.pdf. Accessed May 16, 2014.

"develop youth skills."[2] Summer employment could also raise future earnings by acting as a signal to potential future employers, or through the channel of educational attainment, if work experience translates into greater schooling. A third rationale for such programs is that they could help to keep youth involved in socially productive activities or "out of trouble."[3] Keeping youth out of trouble during the summer could help them avoid dangerous activities and could have benefits such as decreasing incarceration or mortality rates.

We investigate the empirical support for these three rationales by analyzing the SYEP program in the years 2005 to 2008, inclusive. 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 SYEP jobs available. In the face of this excess demand, SYEP randomly allocated spots in the program to applicants by lottery. We compare the outcomes of individuals who participate in SYEP because they were randomly selected to receive a job through SYEP, to the outcomes of individuals randomly not selected. We examine the effect of SYEP on observable outcomes suited to assess each of the rationales for summer youth employment programs. 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 NYS; and to NYC Department of Health and Mental Hygiene (DOH) administrative data on causes of death in New York City. In the four years of lotteries we study, there were 294,580 SYEP applications subject to the lottery, of which 164,977 won the lottery and 129,603 lost the lottery.

This context provides a particularly promising setting for studying a youth employment program. First, the large scale of the program, the random assignment, and the accurate data allow us to estimate precise causal effects, not only on earnings, the employment rate, and college enrollment, but also on rare outcomes including mortality and incarceration. Our sample sizes are at least an order of magnitude (and in many cases two

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[2] See http://www.nyc.gov/html/dycd/html/resources/syep.shtml; accessed May 16, 2014.
[3] See http://nycfuture.org/events/event/summit-on-the-future-of-workforce-development-in-new-york-city. Accessed May 16, 2014. Summer employment may also provide valuable goods through the services youth provide, though our data do not have information on these goods or their value.
[4] Except where otherwise noted, all dollar amounts reported are in real 2013 dollars. Subsequent to the years we examine, eligibility was expanded to age 24.

orders of magnitude) larger than the sample sizes of other randomized studies. The ability to look precisely at mortality, which other studies have not been able to observe, will prove particularly interesting since it will have important implications for the benefits of the program and offer a new take on previous results. The data allow us to estimate effects up to almost a decade after program participation. Second, the IRS and DOCCS administrative data allow us to examine a wide variety of outcomes, including contemporaneous and subsequent earnings, earnings by industry, college enrollment, incarceration, and mortality. Third, NYC SYEP is the largest summer youth employment program in the U.S. and therefore represents a central, recent case study of U.S. summer youth employment programs and an important example of youth employment programs more generally.

We find that SYEP participation increases earnings and employment in the year individuals are employed through the program. In a baseline specification, we estimate that in the year of SYEP participation, SYEP raises average earnings through the program by $1,085.34, lowers other earnings by a modest $208.87, and therefore raises net earnings in the year of participation by $876.26. Thus, crowdout of other earnings was 19.24 percent 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 only a five percentage point decrease in the probability of having a non-SYEP job.

At the same time, 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, we estimate that SYEP participation causes a modest *decrease* in earnings of around $100 per year. This effect is driven by those who had worked prior to SYEP participation and by those in the upper half of the earnings distribution; meanwhile, SYEP participation has an insignificant impact on subsequent earnings among WOTC-eligible individuals. Starting in the fourth year following SYEP participation, we find insignificant impacts of SYEP participation on earnings. We also find that SYEP has no impact on college enrollment, with an extremely precise 95-percent confidence interval that rules out a positive or negative effect greater than one one-hundredth of a year of college during the period we examine. It is notable that even for this young group with typically little prior job experience, and even during the Great Recession period that we examine separately, an employment program did not provide a path to greater future earnings.

Over the year of SYEP participation and the subsequent four years, SYEP participation on net raises average earnings by $536.53. Thus, SYEP succeeds on net in transferring money to youth selected for the program, though with significant crowdout of other earnings. The average decrease in total earnings in the year of SYEP participation and the subsequent four years is 54.02 percent of average SYEP earnings, representing an aggregate decrease of $104.03 million over these four subsequent years. The degree of crowdout of other earnings is small relative to likely lifetime earnings but is substantial relative to the size of the program.

Strikingly, consistent with the hypothesis that SYEP keeps youth "out of trouble," we find that SYEP participation decreases the probability of incarceration and decreases the probability of mortality. SYEP causes a decrease in the probability of incarceration of 0.10 percentage points, driven by a decrease among males. While this effect is small in percentage point terms, it represents a substantial 10.36 percent reduction relative to the baseline incarceration rate of 0.95 percent. The SYEP-induced decrease in mortality, also driven by males, is 0.08 percentage points, again small in percentage point terms but a substantial 19.92 percent of the 0.38 percent baseline mortality rate. Evidence from analyzing cause of death suggests that SYEP may prevent death by external causes. SYEP appears to put youth on a path that leads away from dangerous outcomes.

The point estimates suggest that by October 2014, around 86 lives were saved by the four years of the SYEP program from 2005 to 2008. Under standard cost-benefit analysis calculations that assign a value of $9 million to the lives of prime-aged males in real 2013 dollars (Viscusi and Aldy 2003), this would imply benefits of $773 million. Thus, the effects on mortality have the potential to change greatly one's view of the magnitude of the benefits of a youth employment program and how such benefits compare to its costs. Past literature has typically found negative net benefits of (non-summer-employment) active labor market programs for youth, but it has not examined this important outcome.[5] Like most previous

[5] Cost-benefit analyses typically show negative net benefits of youth active labor market programs more generally. See Hollister, Kemper, and Maynard (1984) and Couch (1992) on the National Supported Work Demonstration of 1975 to 1979, Bell and Orr (1994) on the AFDC Homemaker-Home Health Aide Demonstrations from 1983 to 1986, Bloom et al. (1997) on the National Job Training Partnership Act, Cave et al. (1993) on JobStart, and the other literature cited in the surveys above. A notable exception is Job Corps, an intensive multi-month training program in which participants receive room, board, and around 1100 hours of vocational training and academic instruction. Job Corps increases subsequent earnings and wages of youth while reducing crime (Schochet, Burghardt, and McConnel 2008; Lee 2009), though cost-benefit analyses show

work on active labor market programs for youth, 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, we also find a very large new source of benefits that could well be pivotal to the cost-benefit analysis.

Amid the extensive literature on active labor market programs (see the surveys cited above, as well as Card, Kluve and Weber 2010), the literature on summer youth employment is mostly several decades old and contains only a few studies. The most comparable study is the Youth Incentive Entitlement Pilot Project (YIEPP) demonstration of 1978 to 1980, which provided summer employment to disadvantaged youth and was evaluated using a between-city comparison of youth outcomes in Farkas et al. (1984).[6] 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, decreased violent crime arrests by 43 percent relative to the baseline rate, had no significant impact on arrests for property, drug, or other types of crime, and had little impact on schooling—but does not examine the impact on mortality, earnings, education, incarceration, total arrests, or costs relative to benefits. 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

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negative net benefits for the full sample, with positive net benefits only for the older-youth subgroup. Our research differs from this previous work by analyzing a summer youth employment program rather than a program providing long-term work, assisting in job search, or providing extensive training. (While SYEP includes a job-training curriculum, it is a small part of the program, comprising only 10 percent of the program hours.) Other, more recent randomized controlled trials on employment programs include the UK Employment Retention and Advancement demonstration (Hendra et al. 2011). Studies of the Work Opportunity Tax Credit have focused on the take-up of the program (Hamersma 2003) or focused on those eligible for WOTC through long-term welfare receipt rather than focusing on summer employment for youth in empowerment zones.

[6] Farkas et al. find that YIEPP raised employment and had no effect on schooling. However, Stanley, Katz, and Krueger (1998) conclude that "the short length of the follow-up combined with questions about the quasi-experimental design [of Farkas et al. (1984)] left many doubts about the validity and likely duration of the effect" (p. 12). Crane and Ellwood (1984) found that the Summer Youth Employment and Training Program, which provided minimum wage summer jobs and some remedial education to disadvantaged youth, increased overall employment during the summer of the program, but did not report on subsequent earnings and other outcomes. The Summer Training and Employment Program of the mid-to-late 1980s and early 1990s, which focused more on training, had positive short-run impacts on academic achievement but none after the program ended, and no impact on employment outcomes (Grossman and Sipe 1992). Stanley, Katz, and Krueger (1998) state that the evidence is "limited" and that "additional research would be helpful…[on] programs that provide government jobs or employment subsidies for disadvantaged youth."

(2010) on temporary-help job placements.[7]

The paper is structured as follows. Section 2 describes the policy environment. Section 3 describes our empirical specification. Section 4 describes the data we use. Section 5 discusses the first stage and the validity of the lottery. Section 6 discusses our results on earnings and employment. Section 7 presents results on college attendance. Section 8 shows results on incarceration. Section 9 discusses results on mortality. Section 10 concludes.

2. Policy Environment

During the years we study (2005 to 2008), SYEP provided NYC youth between the ages of 14 and 21 with paid summer employment for up to seven weeks in July and August.[8] Since the 1960s, NYC has been helping fund summer jobs for youth, and the NYC DYCD began running the program in 2003. Since 2005, DYCD has stored computerized records of applications, which were made available for this research. Because SYEP ran the program on its own, without researchers' involvement, we are evaluating an existing government program (as opposed to evaluating a randomized experiment designed by researchers).

SYEP places participants in entry-level jobs and pays them the NYS minimum wage for working up to 25 hours per week during the summer.[9] The NYC government funds the program and its administration, including the wages of SYEP participants in their summer jobs. In 2005 to 2008, the mean expenditure per SYEP participant per time participating in SYEP was $1,403 (including both wages paid to participants and administrative costs). During these years, more than half of SYEP funding came from NYC; over a third of funding was provided by NYS; and less than 10 percent was provided by the federal government through the Workforce Investment Act.

[7] "Work First" programs, especially for temporary jobs, in many cases lead to short-run increases in earnings but negative longer-run effects (see e.g. Bloom and Michalopoulos 2001 for a survey of literature on welfare and work policies). In related work, Card and Hyslop (2005) find temporary but fading effects of a welfare-to-work program. One interpretation of such results is that temporary jobs may interrupt an individual's career development, without providing a bridge to longer-term employment. It is perhaps surprising that we find a similar result in our young sample, where the vast majority of individuals have limited work experience.

[8] See, for example, the SYEP annual report from 2007 http://www.nyc.gov/html/dycd/downloads/pdf/syep_2007_annual_summary.pdf (accessed August 4, 2014). Subsequent to the lottery years we examine, eligibility was expanded to also include youth aged 22 to 24 and the number of weeks in the program was shortened to six.

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

There are various types of SYEP jobs, including jobs at summer camps, daycare centers, government agencies, hospitals, law firms, museums, and retail organizations. As we discuss later, nearly half of SYEP jobs are at summer camps or day care centers. In 2005 to 2008, 74.68 percent of the jobs were with non-profit, private sector firms; 10.95 percent were with for-profit, private sector firms; and 14.37 were with government entities. Thus, the program is typically closer to what previous literature has called a "Work Experience" program, in which individuals are given temporary private sector jobs, than to "Public Sector Employment" program, in which individuals are given a government job (e.g. Heckman, Lalonde, and Smith 1999; Lalonde 2003).

All NYC youth who are able to provide certain documentation are eligible to apply for SYEP. In order to be eligible, 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 18 (called a Blue Card for those 14-15 and a Green Card for those 16-17); proof of citizenship/alien status; proof of address; proof of family income; and a signed SYEP application. Males 18 and older are required to show proof of Selective Service registration.[10]

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 rather typically work for the employers to which providers match participants. During the summer, the providers also give participants approximately 17.5 hours of workshops on job readiness, career exploration, financial literacy and opportunities to continue education. In 2005 to 2008, the mean total number of employers at which SYEP participants worked in any given year is 5,290. In these years, the mean number of SYEP participants working for a given SYEP employer is 5.69.

In a given year, applicants to SYEP apply through a specific SYEP provider.[11] Individuals choose which provider to apply to; 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. They apply for the program online or at a

[10] See, for example, https://application.nycsyep.com/Images/SYEP_2014_Required%20Documents.pdf. Aliens are eligible for SYEP.

[11] SYEP providers are located in all five boroughs of New York City and include organizations such as the Catholic Charities Archdiocese and the Brooklyn Neighborhood Improvement Association.

SYEP provider during the application period, 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. There are 62 SYEP providers in our data. *Within* each provider, there is a lottery to determine which individuals are selected for SYEP. Thus, winning the lottery is random *conditional* on applying to a given provider.

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 SYEP 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. In order to fill the remaining slots, SYEP providers conducted subsequent lotteries. In each lottery, the number of winners was selected in order 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 s/he had not won a slot yet or had won a slot previously but did not accept it, s/he 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 (as is the dummy for winning the initial lottery). In our baseline specification, our instrument is a dummy for winning any of the lotteries.[12] In the 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).

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

[12] As shorthand, we sometimes refer to "winning any of the lotteries at a given provider in a given year" as "winning the lottery," and we refer to "losing all of the lotteries at a given provider in a given year" as "losing the lottery."

subsequent year). Winning or losing the lottery 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. Participating in SYEP in a given year also does not affect the probability of winning or losing the lottery in a subsequent year, conditional on applying in the subsequent year. For those unable to participate in SYEP, there are occasionally opportunities to participate in comparable government programs, though such opportunities are a small fraction of the size of SYEP.[13] For example, NYC runs a Young Adult Internship Program, but this program enrolls only 1,350 participants each year as opposed to the average of over 34,000 enrolled in SYEP per year in our data.

It is also worth briefly describing the NYC labor market and youth labor market during the period we study. Average wages vary widely across NYC: Manhattan has the highest average wage of any county in the U.S., whereas the average wage in Brooklyn is around three-quarters of the average wage in the U.S. The seasonally adjusted unemployment rate in NYC fell slightly from 6.0 percent in January 2005 to 4.7 percent in January 2008, before rising sharply during the Great Recession to a high of 10.5 percent in January 2010, and then slowly falling to 8.8 percent by December 2012. U.S. youth unemployment also rose sharply during the Great Recession: in January 2005, unemployment among 16-24 year-olds was 11.6 percent; it held fairly steady until January 2008, when it was 11.7 percent; it peaked at 19.5 percent in April 2010; and it slowly fell to 16.6 percent by December 2012.

3. Data

We merge a number of administrative datasets, as described below. First, we link administrative data on SYEP lotteries that occurred in each year from 2005 to 2008 (inclusive) provided by the NYC DYCD to IRS administrative data on individuals in the universe of tax records.

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 the initial SYEP lottery; whether an individual

[13] See http://www.nyc.gov/html/dycd/downloads/pdf/Summer_Youth_Alternatives2014.pdf for a list of alternatives to SYEP (accessed August 4, 2014).

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). Our SYEP data include information on all SYEP applicants, regardless of whether they enrolled in SYEP or not.[14] For SYEP participants, the data additionally include the industry the individual worked in through SYEP (in industry categories created by SYEP).

IRS data

We merge these SYEP administrative data to IRS administrative data based on an individual's SSN, which matches 99.6 percent of the SYEP applicants to the IRS data. This represents a very high match rate that is substantially higher than many papers that have matched data on experiments to administrative tax data. It is not surprising that we obtain a high match rate, as individuals were required to list their Social Security number and show their Social Security card (as well as the voluminous additional documentation listed above) in order to be eligible for SYEP. In order to include additional individuals who may have an incorrect SSN listed but have other information correct, we match the 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. The extensive documentation of proof of identity required of applicants lessens the possibility that this information is inaccurate. 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 percent. As a robustness check, we show that the results are nearly identical when we match people only based on SSN. The results are also robust to other matching procedures.

The IRS data contain a wide variety of information including: name; date of birth; age; gender (IRS form SS-5 or W-7); individuals' earnings in each year from self-employment (form 1099-MISC) and from non-self-employment (form W-2) separately; the identity of their family members (if anyone is claiming them as a dependent, and the identities of other dependents, as listed on form 1040); family's earnings (form 1040); the

[14] Personal communication with Alan Cheng, Assistant Commissioner, Youth Workforce Development at DYCD and Director of SYEP from 2005-2010.

Employer Identification Numbers (EINs) of their employer(s) (form W-2); the North American Industrial Classification System (NAICS) industry code of their employer (Business Master File); each individual's day, month, and year of death (if any) (DM-1); whether an individual's employer is a non-profit (form 990); and whether the individual is enrolled in college (form 1098-T). We use data on each of these variables from 1999 to 2012 (inclusive). We winsorize earnings at $100,000 following previous literature using administrative data (*e.g.* Chetty *et al.* 2011). As youth earnings are typically low, this has a negligible effect on the estimates. Like most administrative datasets, the data lack information about the hourly wage, hours worked, or underground earnings. The data provide information by calendar year rather than at a finer temporal scale (*e.g.* quarterly information).

SYEP participants receive a W-2 from the NYC city government (as opposed to the employer they worked with through SYEP). Since we observe individuals' employers in the IRS data (which in the case of SYEP participants was the NYC city government), we are able to observe the amount of income (if any) that an individual received from the NYC city government. We consider this to be 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 annual earnings from the NYC city government among those not participating in SYEP was only $5.10, indicating that earnings in the NYC city government absent SYEP were negligible.

Cause of Death Data

To further investigate the observed effect on mortality, we collected data on causes of death of SYEP applicants. 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 2012 (the most recent year of data currently available to researchers). 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 609 unique DOH mortality episodes (occurring from 2005 to 2012 in NYC) to the SYEP data.

Incarceration Data

We also collected data from the NYS Department of Corrections and Community Supervision (DOCCS) on individuals incarcerated in a NYS prison in years up to and including 2013. Everyone who has been confined in NYS prison is listed in the database, except youthful offenders (18 or under at the time the offense was committed), those who have had their convictions reversed by a court, and certain previously incarcerated non-violent offenders who are covered by a special provision that eventually removes information on incarceration episodes for relatively minor crimes.[15] The restriction to youthful offenders is a particularly important limitation of the data in our context, because most SYEP applicants are 18 or younger at the time of SYEP application (though even those 18 or younger at the time of SYEP application may still have been subsequently incarcerated for offenses committed at age 19 and older). 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 on when first name, last name, day of birth, month of birth, and year of birth all match. 0.95 percent of SYEP applications, and 1.01 percent of SYEP applicants — a total of 2,004 SYEP applicants — match to the DOCCS data (note that SYEP applicants are distinct from SYEP applications since individuals can apply multiple times). Among SYEP applicants, the mean number of times incarcerated was 0.01; among those incarcerated, 93.79 percent were incarcerated once, 6.07 percent were incarcerated twice, and 0.14 percent were incarcerated three times.

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 year (i.e. the year after they enter into the SYEP lottery), Year 2 refers to the year

[15] Under this special provision, an offender's incarceration episode is removed from the database five years after an offender in this limited class completes his or her maximum term of imprisonment, or five years after each such offender completes his or her term of parole or post release supervision.

after Year 1, and so on. Year -1 refers to the year before the SYEP lottery in Year 0, Year -2 refers to the year before that, and so on.[16]

In the years we examine, SYEP gave "special slots" for disadvantaged groups (such as low-income, foster care, runaway/homeless, disabled, and court-involved youth) that were not selected by lottery. We therefore drop these applicants from our sample.[17] We also delete observations in which the same Social Security Number (SSN) is associated with multiple applications in a given year, which results in deleting approximately 1,000 observations per calendar year in Years -6 to 4 (and fewer in subsequent years).[18] The number of remaining observations in each year from Year -6 to Year 4 is 294,580, corresponding to 198,745 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.

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.[19] We return to this issue when we discuss our specification.

114,013 individuals participated in SYEP at some point. In any given year over the lottery years we study, around 3 percent of the eligible population in NYC applied to SYEP. Since we have complete IRS data as far forward as 2012, we observe everyone in these complete data until at least Year 4 (as the last lottery we observe is in 2008). The total number of observations used across all of the data is 3,613,727.

Summary statistics

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[16] For example, for individuals in the 2005 lottery, Year -1 refers to the Year 2004; Year 0 refers to the Year 2005; Year 1 refers to 2006; and so on. For individuals in the 2006 lottery, Year -1 refers to the Year 2005; Year 0 refers to the Year 2006; Year 1 refers to the Year 2007; and so on.

[17] SYEP also conducted a lottery in 2004, but DYCD did not keep records on SYEP lottery winners and losers. In the data starting in 2009, we are unable to distinguish which individuals were eligible for special slots at the time the initial lotteries were run and therefore do not use data on lotteries starting in 2009.

[18] The results are nearly identical when we include these individuals in the sample.

[19] As we discuss, we cluster our standard errors by provider to account for intra-cluster correlations.

The first column of data in Table 1 shows summary statistics for the full sample. "Main outcomes" refers to outcomes observed in Years 0 to 4. Given applicants' young ages, it is not surprising that mean total earnings in the sample are quite low compared to the general population — only $3,555.29. Mean NYC government earnings are $218.39, with the remainder coming from non-NYC government sources. 63 percent are employed in any job, and 50 percent have any non-NYC government job. 23 percent of the sample is enrolled in college in a given year. Median total earnings (shown in Table 3) rises from $939.18 in Year 0 to $2,473.01 in Year 4.

The "lagged outcomes" section examines outcomes in Year -1. In the year before entering the SYEP lottery, mean total earnings are small ($892.17), and 4 percent are enrolled in college, which is not surprising given their ages. 21 percent participated in SYEP in Year -1, so it is unsurprising to see modest SYEP earnings in Year -1 ($256.96 on average).

Turning to the demographics of SYEP applicants, they come on average from disadvantaged family backgrounds and are disproportionately minorities. Mean family income is low ($39,521.56 in Year -1).[20] 48 percent of SYEP applicants are black, far greater than their representation in NYC as a whole, whereas 13 percent of SYEP applicants are white, far lower than their representation in NYC as a whole.[21] Just under one-half (45 percent) are male. The mean age is 16.50. The vast majority, 93 percent, are U.S. citizens.

Appendix Table 1 shows the breakdown of SYEP jobs by industry, as reported by SYEP. As discussed above, SYEP participants are issued a W-2 by the New York City government, not by the firm at which they work during SYEP, and the 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. SYEP reports that much of the sample works at a day care or day camp (35.77 percent of the sample), at a camp outside of New York City (9.84 percent of the sample), in community/social service jobs (11.27 percent), arts and recreation jobs (10.50 percent), educational services jobs (8.65 percent), healthcare/medical jobs (8.58 percent), and government agency jobs (7.09 percent). While the SYEP industry classification is not based on the North American Industrial Classification

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

[21] It is not feasible to compare the percentages in a more precise way, because the U.S. Census race categories are different than the self-reported race categories on the SYEP application.

System (NAICS), the industries that SYEP lists are strongly suggestive of NAICS industry. Thus, in the industry analysis we conduct below, we use the descriptions provided by SYEP to develop a set of 2-digit NAICS codes that corresponds to the industries described by SYEP (the crosswalk is shown in the Appendix). This classification is imperfect because SYEP did not explicitly classify jobs into NAICS codes, but it will prove to be illuminating. To classify SYEP jobs as for-profit, non-profit, or government, we use data on this reported by the SYEP program. For jobs not through SYEP, we classify employed individuals as working at a non-profit 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.[22]

4. Empirical Strategy

Our empirical strategy exploits the random assignment of SYEP access through the lotteries. Since some of those selected for SYEP ultimately did not enroll in the program, we use winning the SYEP lottery as an instrument for participating in SYEP. Thus, a basic two-stage least squares specification is:

$$L_{ij} = \alpha_1 W_{ij} + X_j\alpha + u_{ij} \qquad (1)$$

$$E_{ij} = \beta_1 L_{ij} + X_j\beta + v_{ij} \qquad (2)$$

In this specification, E_{ij} represents an outcome (such as the level of earnings) of individual *i* that participated in SYEP provider lottery *j*. W_{ij} represents a dummy for whether the individual won the SYEP lottery or not. L_{ij} represents a dummy for whether the individual participated in SYEP. In the typical specification, X_j represents a vector of dummies for each of the SYEP providers interacted with the year the lottery occurred. Because individuals applied to providers and the lotteries were run at the provider level in each year of the lottery, we must control for dummies for each provider in each year of the lottery. In some specifications, we control for additional covariates observed in the IRS or SYEP administrative data. ε_{ij} is an error term. We cluster our errors at the level of the SYEP provider, which we view as a conservative choice. As we show in Appendix Table 6, clustering our standard errors at the individual level instead leads to nearly identical standard errors.[23] We interpret our coefficient β_1 as a local average treatment effect of SYEP among

[22] One limitation is that the NAICS code is missing for many government workers.
[23] Individuals who apply multiple times typically apply at the same provider (located near their home).

the compliers (i.e. those induced to participate in SYEP by winning the lottery). Because those who lost the lottery are officially ineligible to participate, this also represents the average treatment effect on the treated.

We typically investigate the results separately in different years; in other words, we run the specification above for each year of outcomes separately (e.g. we run one regression to estimate the effects in Year 0; we run a separate regression to estimate the effects in Year 1; and so on). In some cases, we examine the results across multiple years (e.g. we examine the effects of SYEP participation on total earnings in Years 0-4). In this case, we sum earnings across all of the years examined (in the example, we would sum earnings across Years 0-4) and run the regressions (1)-(2) above using this summed earnings variable as the outcome.[24]

It is possible that SYEP participation in Year 0 could affect the probability of participation in subsequent years. In this case, part of the effect of Year 0 SYEP participation on subsequent earnings we estimate could be mediated through the impact of SYEP on future SYEP participation.[25] The specification in (1)-(2) corresponds to what Cellini, Ferreira, and Rothstein call 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. As we discuss in the Appendix, we also estimate the effect of SYEP participation on earnings using the "dynamic" design of Cellini, Ferreira, and Rothstein (2010). In our context, this dynamic estimator effectively yields the effect of SYEP participation in Year 0 on earnings in any given year, while 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, both of which are of interest. As we show in the Appendix, SYEP participation in Year 0 only slightly affects the probability of SYEP participation in subsequent years (i.e. Years 1-4); thus, it is not surprising that the static and dynamic estimates prove to be similar (as the mediating role for effects on future participation is limited due to the small effect on future participation).

[24] Earnings is not indexed by time in regressions (1)-(2) above because we use earnings from only one period in any given regression.
[25] We use the term "subsequent earnings" to refer to earnings in the calendar years following the calendar year of SYEP participation.

In some cases, we investigate a binary dependent variable (e.g. a dummy for whether an individual has a job or is enrolled in college). In an instrumental variables model with a binary endogenous variable and a binary outcome, models such as a two-stage probit are in general inconsistent, and we run a linear probability model instead (Angrist 2001).[26] 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, this corresponds to defining the outcome as the probability that an individual has a job at any point in Years 0-4).

One assumption that this specification makes is that the effect of moving from zero times participating in SYEP to one time participating in SYEP is the same as the effect of moving from one time participating to two times, from two times to three times, and so on. We later separately run regressions for those who previously participated in SYEP different numbers of times, and we cannot reject that the results are the same across these groups. We also investigate a number of other specifications, such as using a SYEP lottery win as an instrument for the total number of times participating in SYEP, and estimate comparable results.

5. Preliminary Empirical Results

Validity of Randomization

Table 1 demonstrates the validity of the randomized design by comparing the characteristics of SYEP lottery winners and losers. Specifically, we run a "reduced form" 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 (total earnings, NYC government earnings, non-NYC government earnings, whether the individual has any job, whether the individual has a non-NYC government job, whether the individual is enrolled in college, total income of the family, and whether the individual participates in SYEP), and a number of demographic variables (number of family members, as well as dummies for race, gender, age, and whether the individual is a U.S. citizen). Consistent with the validity of the randomization, none of these eighteen variables is

[26] 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.

significantly related to treatment status. Moreover, for the outcome variables observed in the year before SYEP participation, we find insignificant estimates (with similar point estimates and small standard errors) in every other year prior to SYEP enrollment (Year -2, Year -3, Year -4, Year -5, and Year -6).[27] We also find that the probability that SYEP applicants match to the IRS data is balanced.

First stage

Appendix Table 2 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 (standard error 0.01), and the F-statistic is 4,188.68.[28] SYEP participation in Year 0 affects the probability of SYEP participation in Years 1 to 4 separately, though these effects on the probabilities of future participation are very small (3 percentage points or less).

6. Effects on Earnings

Main estimates of effects on earnings and probability of having a job

Table 2 shows our main estimates of the effect of SYEP participation on earnings and the probability of having a job. Row A shows the baseline instrumental variables specification for the year in which the individual enters the SYEP lottery. Column 1 shows the effect of SYEP participation on total earnings in Year 0. The point estimate of the coefficient β_1 is \$876.26 ($p<0.01$), as SYEP participation on average leads to a substantial

[27] As placebo tests, we estimate our IV specification (1)-(2) above when the dependent variable is measured in Year -6 to Year -1. When the dependent variable is total earnings, in Year -6 the coefficient on Year 0 SYEP participation is -8.93 and the standard error is 22.83; in Year -5 the coefficient on Year 0 SYEP participation is -13.33 and the standard error is 25.06; in Year -4 the coefficient on Year 0 SYEP participation is -19.92 and the standard error is 24.63; in Year -3 the coefficient on Year 0 SYEP participation is -31.98 and the standard error is 26.47; in Year -2 the coefficient on Year 0 SYEP participation is -38.80 and the standard error is 27.44; and in Year -1 the coefficient on Year 0 SYEP participation is -32.40 and the standard error is 29.46. Thus, in all of the years prior to Year 0, the effects are insignificantly different from zero, and the standard errors are small. In all of these years, the estimates are also small and insignificant, with small standard errors, when the dependent variable is any of the following: NYC government earnings; non-NYC government earnings; probability of having a job; or probability of having a non-NYC government job.

[28] Our empirical strategy could also be used to examine the effect of employment in Year 0 (through SYEP or other employers) on earnings. Winning the lottery causes an increase in the probability of Year 0 employment of 51 percentage points, smaller than the 73 percentage point increase in the probability of SYEP participation. If we were to use winning the lottery as an instrument for Year 0 employment, we would scale up the linear estimates by a factor of 1.43.

increase in earnings in the year of SYEP participation. The estimate is quite precise; the standard error is only \$25.08. Column 2 shows that SYEP participation causes earnings from the NYC government to increase by an average of \$1,085.34 (standard error \$10.14). Column 3 shows the effect on non-NYC government earnings, which is -\$208.87 (standard error \$24.83). Non-NYC government earnings fell by 19.24 percent of the increase in NYC government earnings, as SYEP crowds out some other earnings opportunities.[29] In Column 4, the dependent variable is the individual's probability of having positive earnings (including in both SYEP and non-SYEP jobs), which we refer to as the probability of having a job. SYEP participation raises the probability of having a job by 71 percentage points. Column 5 shows that SYEP participation lowers the probability of having a non-NYC government job by 5 percentage points in Year 0, indicating modest crowdout. To contextualize the magnitude of these impacts, Appendix Table 3 shows the mean and standard deviation of total earnings and a dummy for having a job among SYEP lottery losers in each year.

Rows B through D of Table 2 show the effects in Years 1, 2, and 3. In each of these years, SYEP participation in Year 0 lowers total earnings modestly, by around \$100 ($p<0.05$) in each of the years separately[30]; raises NYC government earnings slightly ($p<0.01$); lowers non-NYC government earnings modestly ($p<0.01$); and lowers the probability of having a non-SYEP job slightly ($p<0.01$ in Years 1 and 2, and $p<0.05$ in Year 3). Column 4 shows that SYEP slightly raises the probability of having a job in Year 1. Since SYEP decreases total earnings but raises the probability of having a job, this suggests that SYEP leads individuals to earn less conditional on having a job. The confidence intervals are small throughout these estimates. Row E shows the estimated effects in Year 4, which turn insignificant (except for the small positive effect on NYC government earnings), though the confidence intervals continue to be quite small. We show later that the estimated effects are also insignificant in all subsequent years.

Row F shows the total effect in Years 0 through 4 (i.e. summing earnings over all of these years). The effect of SYEP on total earnings is positive and substantial (\$536.53). The effect on total earnings is only 45.98 percent as large as the average total of SYEP transfers

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[29] Some of this decrease in other earnings could have occurred after the summer of Year 0, *i.e.* it could reflect the effect of SYEP on earnings later in the calendar year (as opposed to the effect of SYEP on earnings in the summer of Year 0).

[30] Relative to mean earnings in the control group each year, these negative effects on earnings represent an earnings decrease by 4.44 percent, 2.90 percent, and 2.48 percent in Years 1, 2, and 3, respectively.

over this period ($1167.30), implying that the average decrease in other earnings is 54.02 percent 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, Row G shows the estimated effects in years 1 through 4 (i.e. the years in our data after SYEP participation in Year 0). The impact on total earnings in this period is negative and substantial (-$339.73, $p<0.05$). By contrast, the impact on the probability of having a job during this period is positive (increasing by one percentage point, $p<0.01$). As before, the estimates are precise.

Appendix Table 4 shows the estimates for years 5, 6, and 7. These regressions involve a smaller sample size than the main regressions because we are able to observe individuals only until 2012. Consequently, we cannot observe the 2008 SYEP cohort five or more years out; we cannot observe the 2007 SYEP cohort six or more years out; and so on. We show these results in the appendix because results from later years are not directly comparable to Years 4 and earlier, since the sample sizes differ. The estimates are nearly all insignificant. As we might expect from random chance, one estimate is significant, though it is not robust: the estimate for earnings in Year 7 is significant without controls in Panel A, but it becomes insignificant when we add controls in Panel B.[31] (All of the other results in the paper are robust to adding controls, as we discuss below.) Over Years 0-7, the results are similar to those over Years 0-4: SYEP participation raises total earnings summed over Years 0-7 by $560.87 ($p<0.05$), corresponding to a $1167.49 increase in NYC government earnings ($p<0.01$) and a $606.42 decrease in non-NYC government earnings ($p<0.01$). In order to investigate outcomes that are as long-term as possible, as a robustness check we have investigated the results using IRS data until 2013, as opposed to those in Appendix Table 4 that use IRS data through 2012. The 2013 data are currently incomplete because they do not yet represent the full universe of U.S. employment (though they likely reflect over 95 percent of U.S. employment). The results through 2013 show extremely similar results to those relying only on data through 2012; both with and without

[31] Specifically, we control for the following covariates: gender, dummies for race categories, citizenship, age, number of family members, individual's wage income in Year -1, individual's NYC government wages in Year -1, individual's non-NYC government wages in Year -1, number of employers in Year -1, a dummy for whether the individual had any job in Year -1, a dummy for whether the individual was enrolled in college in Year -1, a dummy for whether the individual was claimed on a return in Year -1, and family income in Year -1 if the individual was claimed.

controls, the estimated effects on earnings in Years 5, 6, 7 and 8 are all insignificant when we examine data through 2013.

Appendix 1 discusses a wide variety of robustness checks and 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 (2010); 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. Throughout these alternative specifications, we continue to find comparable results, as shown in Appendix Tables 5 through 8.

Effects on Quantiles

We examine the effect of SYEP on quantiles of earnings in Table 3.[32] For context, Panel D of Table 3 also shows the percentage of individuals earning zero in each year, which varies from 39.88 percent in Year 0 to 28.83 percent in Year 4, as well as quantiles of total earnings. Since a substantial percentage have zero earnings, it does not make sense to examine the effect of SYEP in lower quantiles of earnings such as the 25th percentile. Winning the SYEP lottery raises median earnings by $1,101.24 in Year 0 ($p<0.01$). The subsequent effect on median earnings varies across years, with a positive and significant effect in Year 1, a marginally significant smaller positive effect in Year 2, but a (smaller in absolute value) negative and marginally significant effect in Year 3. The table also shows the effect on higher quantiles of earnings, such as the 75th percentile and the 90th percentile, which are negative and significant in Years 1 to 3. Thus, the negative effect of SYEP on average subsequent earnings is driven primarily by negative effects in higher quantiles of earnings.[33]

[32] We use quantile regressions, rather than quantile instrumental variables regressions, because quantile IV regressions often did not converge. In those cases in which the quantile IV regressions did converge, the point estimates of the coefficients on SYEP participation were generally 10-40% larger than those on winning the SYEP lottery in the corresponding reduced form quantile regressions, with comparable standard errors.
[33] When we investigate the effect of SYEP on the variance of earnings, the regressions tend to show negative effects, which are significant at the 5% level in some specifications and not in others.

Heterogeneity

In Table 4, we return to our two-stage least squares specification and investigate heterogeneity in the effect of SYEP among subsamples distinguished by different individual characteristics.[34] Rows A and B show the effect of SYEP enrollment broken down by eligibility for WOTC support of summer youth employment (i.e. those aged 16-17 living in an Empowerment Zone).[35] The effect of SYEP on contemporaneous earnings (Column 1) is positive and substantial in both groups. The effect on earnings in subsequent years (Column 4) is insignificant for those eligible for the WOTC, but the WOTC-ineligible group shows more substantial and significant negative effects of SYEP on total earnings in Years 1-4.[36]

Turning to other heterogeneity analysis, the point estimate of the effect on total earnings in Years 1-4 is a bit less negative (though more statistically significant) in the below-median-family-income sample (Row C) than in the above-median sample (Row D), though these estimates are not significantly different. Note that in our sample, even those with above-median family income had very low median income relative to NYC as a whole. The effects are again similar between males and females (Rows E and F), with modestly more positive effects on total earnings among females than among males, though these effects are again not significantly different. The effect on earnings in Years 1-4 is much more negative among whites than among blacks, Latinos, and other races (Rows G through J).[37] Rows K and L show that SYEP participation has much less positive effects on earnings in Year 0 and more negative effects on earnings in Years 1-4 among those above the median

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[34] In the heterogeneity analysis, the effects on the probability of having a job typically track the effects on earnings (i.e. when SYEP increases (decreases) earnings significantly in a group and time period, the effect on the probability of having a job tends to increase (decrease) significantly). The only exception is in Year 1, when (as in Table 2) earnings fall even though total jobs increases in the full sample (and in several sub-groups).

[35] To be eligible for WOTC, the summer youth employee must be a 16-year-old or 17-year-old who works for the employer between May 1 and September 15, lives in an Empowerment Zone or Renewal Community, and has not worked for that employer before (see http://www.doleta.gov/business/incentives/opptax/eligible.cfm, accessed March 26, 2014)

[36] The estimated effects for the WOTC-eligible and ineligible groups are not significantly different.

[37] "Other races" refers to races other than whites, blacks and Latinos. As defined in the SYEP data (self-reported by applicants), the category of Latinos is mutually exclusive with the categories of whites, blacks, and other races. Note that these samples differ along other characteristics that are correlated with race, so it is not necessarily the case that the difference in results in these samples relates to the difference in race, as opposed to the difference in other characteristics that are correlated with race. (Of course, this point applies more broadly throughout our heterogeneity analysis.)

age of 16.25 than among those under this age (p<0.01).[38] This is interesting in light of the conclusion of previous studies of active labor market programs tend to be more effective for adults than for youth (Stanley, Katz, and Krueger 1998; Heckman, Lalonde, and Smith 1999; Lalonde 2003).

In Rows M and N, we find dramatically less positive (in Year 0) and more negative (in Years 1-4) effects among those who worked in Year -1 than among those who did not work in Year -1 (p<0.01 in both cases). In this sense, the reduction in subsequent earnings appears to be driven by those who previously worked, while we find no evidence that subsequent total earnings are significantly negatively affected in the group that did not previously work. Rows O and P examine the effects of the 2005-6 lotteries and of the 2007-8 lotteries, respectively. Looking at Years 1-4, we find an insignificant effect on total earnings in the 2005-6 lotteries but a substantial negative effect ($-523.10, $p<0.01$) in the 2007-8 lotteries.[39] These estimates are not significantly different, though they are nearly so at the 10% level ($p=0.13$). It is notable that even in the Great Recession, summer employment through SYEP did not have a positive effect—and in fact had a modest negative effect—on average future earnings.

Effects on type of job

We investigate the effect of SYEP on earnings in different industries. To simplify this exercise, we classify industries into two clusters: those in which the 2-digit industry is more prevalent (i.e. 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). As shown in Appendix Table 1, Cluster 1 contains arts and recreation, camp (out of city), community/social service, day care/day camp, educational services, and healthcare/medical, while Cluster 2 contains the remaining jobs (in financial services, legal services, cultural institution, real estate/property, retail, science and technology, and

[38] Age is measured on July 1 of the year in which the lottery occurs (Year 0). Age 16.25 generates a rough median split of the data by age. The results are also similar when we define the younger and older age groups differently (e.g. 14-18 and 19-21).
[39] When we examine the effects by year since lottery, for each of the lotteries separately, we again find no robust evidence of substantial differences in the estimates across lotteries (including no evidence of substantial differences across years of the business cycle), leading us to aggregate as we have shown.

"other").[40] This is a useful way of classifying the industries because it will allow us to examine whether SYEP led to a persistent increase in earnings in industries that SYEP typically places participants.[41] Columns 1-2 of Appendix Table 9 show that in Year 0, earnings in Cluster 1 rise (as would be expected through a mechanical effect since jobs provided by SYEP are more likely to be in Cluster 1 than jobs outside of SYEP) and earnings in Cluster 2 fall. In subsequent years, these effects persist: SYEP participation tends to continue to positively (negatively) affect Cluster 1 (Cluster 2) earnings and employment.[42]

Columns 3-5 of Appendix Table 9 show the effects on earnings and jobs in the for-profit, non-profit, and government sectors. The table shows that SYEP strongly raises earnings in the non-profit sector in Year 0 and subsequently continues to raise earnings modestly in the non-profit sector through Year 4 (with similar results for the probability of having a job). Earnings in the for-profit sector are lowered by SYEP by around $100 per year in Years 0, 1, 2, and 3. Meanwhile, SYEP increases earnings in the government sector in Year 0 but has a modest negative effect on government earnings in Years 3 and 4.

Interpreting the Earnings Results

While the negative effects on subsequent earnings are small relative to likely lifetime earnings, 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, our exploration of the mechanisms is necessarily tentative: we can say whether the predictions of our hypotheses are consistent with the data, but we cannot determine with certainty which hypotheses are correct. With these caveats in mind, we turn to examining our suggestive evidence on mechanisms.

As we discuss in greater detail in Appendix 2, we explore the possibility that effects are heterogeneous by job placement. Appendix Table 10 investigates the interaction between

[40] These are the industry names reported by SYEP; Appendix Table 1 lists the NAICS codes associated with the Clusters.
[41] Many other classifications of industries are certainly possible, although other similar classifications show similar results to those we report.
[42] 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 discussion in Appendix 2).

winning the SYEP lottery and the fraction of jobs in the SYEP provider that are in Cluster 1.[43] The regressions suggest that a Cluster 1 (Cluster 2) job placement has a negative (positive) effect on earnings both during and after SYEP, leading to a decrease (increase) in average earnings of $2,222.21 ($1,799.30) in Years 1-4. Thus, the evidence is consistent with the possibility that the effect of SYEP on Year 0 job type — in particular, the fact that SYEP tends to place individuals in Cluster 1 jobs like day care or day camp, as opposed to Cluster 2 jobs like law or finance — is a culprit for the negative effect on subsequent earnings. Nonetheless, we emphasize that these results are more suggestive than our main results: heterogeneity in the effects across providers could be driven by factors that happen to be correlated with the types of jobs in each provider.[44]

Not mutually exclusive with the above or other explanations, SYEP could crowd out jobs that could have led to greater future earnings.[45] As we discuss in Appendix 2, 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. Further, the subgroup analysis found more negative impacts for groups that were more likely to otherwise be working in Year 0 (i.e. older individuals, and those with a job in Year -1). Relatedly, SYEP decreases the probability that an individual continues working for a past employer (Appendix Table 11), raising the possibility that SYEP harms a participant's career development by interrupting career development with a past employer. Appendix 2 also discusses other potential explanations, including income effects, substitutability of leisure across years, and signaling; the evidence on these mechanisms is mixed, though we cannot rule them out.[46]

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[43] When estimating the effect of SYEP participation on total earnings in Years 0-4 in each provider separately, the standard deviation across providers of the estimated effects is $481.85 (with a mean effect of $536.53).

[44] In Table 5, we documented that SYEP slightly but persistently increases earnings in Cluster 1, while substantially and persistently decreasing earnings in Cluster 2. However, the effect of SYEP on subsequent job industry does not account for the effect of SYEP on subsequent earnings: when we estimate the effect of SYEP on the probability of working in each two-digit industry and calculate the mechanical effect of this industry pattern on earnings (using mean earnings in each industry and year among the control group), we find that this accounts for an insignificant fraction of the effect of SYEP on future earnings.

[45] It is worth noting that our results find only 19.24 percent earnings crowdout in Year 0, which may limit the potential quantitative importance of this explanation. In principle it is possible that SYEP participation in Year 0 could negatively affect future earnings relative to the counterfactual of having done no job in Year 0.

[46] The Appendix additionally briefly discusses other potential explanations, including SYEP-induced changes in the labor supply curve, and peer effects.

Finally, as discussed in the following section, we rule out the possibility that the decrease in subsequent earnings is a result of an increase in college enrollment.

7. Effects on College Enrollment

In principle, it is possible that SYEP could also have an impact on schooling decisions. This is particularly relevant because 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 (though our evidence in fact shows no increase in earnings through Year 8). Table 5 investigates the effect of SYEP enrollment on college enrollment.[47] Table 5 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).[48] Columns 1 through 5 show the effect on the probability of attending college in individual years from 0 to 4, and Columns 6 and 7 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. We find no significant impact throughout, with small standard errors. In Column 8, we estimate the effect of SYEP participation on total years enrolled in college in Years 0-4; the point estimate (in Row A, which estimates the regressions without controls) is -0.001, with a confidence interval that rules out an increase or decrease in total years of college greater than *one-hundredth of a year*. Thus, our estimates are extremely precise and rule out even a very small effect. As before, these estimates are nearly identical when we control for covariates (Row B), and they are very similar when we employ the dynamic specification of Cellini, Ferreira, and Rothstein (2010).[49]

In principle, it is also possible that SYEP could have an impact on individuals' decisions about whether to attend high school. This is particularly important because if SYEP has a positive impact on high school attendance, this could reduce individuals' earnings

[47] Our data lack a measure of whether individuals graduated from college.

[48] In these regressions investigating the 18 and older sample, when an individual participates in a SYEP lottery at an age below 18, we include data on this individual in our sample when the individual has reached age 18 and older.

[49] Perhaps unsurprisingly, we also find that SYEP's effects on college attendance cannot account for SYEP's effects on earnings: in our IV regression of earnings on SYEP participation, controlling for college makes a negligible, statistically insignificant difference to the coefficient on SYEP participation.

while they are of high school age. However, Leos-Urbel (2012) examined SYEP data from 2007 and found that winning the SYEP lottery decreased the probability that an individual attended high school the following school year, though this effect was small and significant only at the 10 percent level.[50] While we do not have data on high school attendance in our data, we can indirectly investigate whether this affect could drive our negative earnings results. To address this issue further using our data, in Appendix Table 12 we estimate the results using data on individuals too old to have still been in high school after the summer of SYEP (i.e. those older than 18 by the end of the calendar year in Year 0). Among this group, SYEP lowered total earnings in Years 1-4 by \$1,067.64 ($p<0.05$), a *more* negative point estimate than in the full population. Furthermore, we can attempt to infer whether high school attendance could have increased as a result of SYEP participation. If there were a significant positive impact on high school attendance or completion, then we might expect (1) a positive impact of SYEP on earnings several years later (because of the strong earnings returns to high school); (2) a larger negative impact on near-term earnings in the younger group than the older group (since the older group is less likely to still be of high school age during or after SYEP participation); and (3) an eventual positive impact on the probability of having a job. None of these predictions is observed in the data (in fact, (1) and (2) are the opposite of what we observe in the data). Moreover, one might expect an effect on high school attendance or graduation to translate into an effect on college enrollment, but we are able to rule out even very small impacts on this outcome. Thus, the range of evidence we have is not supportive of the hypothesis that the decrease in earnings is related to effects on high school or college enrollment.[51]

[50] The paper's main focus is on the correlation between SYEP participation and log days attending school *conditional* on attending school, but 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). The author stated (personal correspondence, 8/4/14) that he also estimated models for being present in school following SYEP for the sub-group that showed the largest increase in log days attended (those 16 or over with low prior attendance) and the coefficient on winning the SYEP lottery was not statistically significant.

[51] If SYEP decreases (increases) high school attendance, with no corresponding effect on earnings, this would decrease (increase) the net social cost of SYEP (holding constant the effect of SYEP on earnings).

8. Effects on Incarceration

Keeping kids "out of trouble" during the summer could lead them away from crime and thus reduce the probability of incarceration. In Table 6 we create a dummy representing whether an individual appears in the DOCCS incarceration 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 0.10 percentage points. This is a 10.36 percent reduction relative to the baseline incarceration rate of 0.95 percent. 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. The results are very similar when controlling for covariates (Column 2), when the dependent variable is the number of times incarcerated (Column 3), and with a probit (Column 4). In the probit specification, we regress the incarceration dummy directly on a dummy for winning the lottery and report marginal effects. (The first stage is extremely similar across subgroups, in all cases showing that winning the SYEP lottery increases the probability of participation by around 0.73 percentage points.)

We find important differences in the incarceration effect across subgroups. Recall that only individuals 19 and older when they commit a crime are included in our DOCCS incarceration data. 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. The reduction in the incarceration rate due to SYEP for this group is 0.46 percentage points ($p<0.05$), which is very large relative to the baseline rate of 0.85 percentage points. This represents a 54 percent reduction in incarceration and is substantial in absolute terms. 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 percent level (p=0.12). 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; and more among blacks and whites (particularly blacks) than among Latinos and other races. No notable differences were found across other subgroups shown in other tables (i.e. eligible vs. ineligible for WOTC, 2005-6 vs. 2007-8 lotteries, and below vs. above age 16.25).

9. Effects on Mortality

Paralleling the negative effects on incarceration, keeping kids "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 percent of the sample of SYEP applicants dies by October 2014, the latest data available to researchers at this time. In Table 7, we present a Cox proportional hazards model relating the daily hazard of death to whether an individual won the SYEP lottery (column 1). This model is "reduced form" in the sense that we directly regress the dependent variable on a dummy for winning the lottery, whereas our baseline specification in regressions (1)-(2) was two-stage least squares; to run a two-stage least squares model more analogous to our main specification, we create a dummy representing whether an individual has died by 2014 and separately estimate a linear probability IV model (Column 2). Finally, we show a "reduced form" probit (reporting marginal effects), in which the dependent variable is the same dummy (Column 3).

The results of all three specifications are similar in significance and implied magnitude, all showing that SYEP significantly reduces mortality.[52] The p-value is well below five percent: in the two-stage least squares regression, p=0.016, and in both the Cox and probit cases, $p=0.018$. The reduction in mortality parallels the reduction in incarceration. The IV specification shows that in the full population, SYEP reduces the probability of mortality by 0.08 percentage points. Relative to the baseline mortality rate, this represents a reduction in mortality of 19.92 percent. In combination with the number of SYEP participants, the estimates imply a reduction of 86 deaths by 2014 due to the SYEP program in years 2005 to 2008, paralleling the 112-person reduction in incarceration by 2013.

It is also possible to estimate the effect in various subgroups. While the small number of deaths prevent us from finding statistically significant differences in the treatment effect across groups, 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

[52] It is worth noting that the effect on mortality is not an important factor driving our main results on the negative effect of SYEP on subsequent earnings, for a number of reasons. First, the mortality effect is very small in absolute terms. Second, those additional individuals who have died in the control group (relative to the treatment group) have zero earnings, which should weakly reduce earnings in the control group and lead to more *positive* estimated effects of SYEP on earnings. Third, when we treat observations following mortality as missing observations, we obtain nearly identical results. Finally, when we calculate bounds on the estimates using the procedure of Lee (2009), the bounds are very tight and significant at the same significance levels shown in Table 2.

group than among the older group; among those who did not work prior to SYEP participation than among those who did work (paralleling the larger earnings crowdout among those who previously worked and those in the older group); in the 2005-2006 lotteries than among the 2007-2008 lotteries (which is unsurprising, given that we observe those in the 2005-2006 lotteries for longer and can therefore estimate more precise results and over a longer time frame); and among the WOTC-eligible group than among the WOTC-ineligible group. The estimates are similar among participants with family incomes below and above the median of SYEP applicants. All of the mortality results are nearly identical when we control for covariates (Appendix Table 13). They are also very similar when we perform our various other robustness checks.

Appendix Table 14 estimates the mortality effects by calendar year. We show the effect of SYEP on a dummy for whether an applicant died *by* a given year; thus, the effect *in* a given year can be calculated as the difference between the coefficient for that year and the previous year. The cumulative effect becomes significant at five percent in 2013 and 2014. In data on the full U.S. population from the Social Security Administration Actuarial life tables, the yearly death rate increases several-fold from the mid-teen years to the mid-twenties. Similarly, Appendix Table 14 shows that in our data, the death rate is generally substantially higher in later years than it is in earlier years. Thus, it is not surprising that we estimate larger effects on mortality in later years, as we are working with far more variation in these years. Appendix Table 15 considers the effects in the year since SYEP application and shows that similarly, significant effects occur in Years 7, 8, and 9.[53]

Cause of death

As a secondary analysis, it is potentially illuminating to investigate the particular causes of death that were affected by SYEP participation. One important limitation of our DOH cause of death data is that the IRS data indicate that many of the deaths (among both SYEP lottery winners and losers) occur in 2013 and 2014, whereas the DOH data only cover years through 2012 and are limited to people who die in NYC. Thus, we might expect to find

[53] 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.

substantially smaller effect sizes, estimated with substantially less statistical power, in the DOH analysis.

Given that our evidence suggests that SYEP "keeps kids 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.[54] These account for 70.61 percent of all deaths in our data by 2012. Among SYEP applicants in our sample, the most common cause of death is homicide, accounting for 50.08 percent of all deaths, and reflecting a much higher percentage than in the population as a whole in this age range.[55]

Appendix Table 16 shows our results. In Row A, we use the DOH data and find a reduction in deaths by 2012 by *any* cause that is significant at the 10 percent level, reflecting an 18 percent reduction in deaths relative to the control group. For comparison, in Row B we find similar very results in the IRS mortality data when a dummy for death by 2012 (multiplied by 100) is the dependent variable: the coefficient is very similar, and the estimate is also significant at the 10 percent level.[56] Given these marginally significant results, we might also expect marginally significant results when we examine specific causes of death.

Row C shows that SYEP causes a reduction in deaths from external causes (p=0.08), representing a 21 percent reduction relative to the control group. The point estimate is 81 percent as large as the point estimate in Row A of the effect on mortality by any cause. The point estimate in Row D shows that SYEP reduces the probability of death by homicide, representing a 23 percent reduction relative to the control group, but the estimate is slightly less significant (p=0.136). The point estimate of effect on the probability of death by homicide is 66 percent as large as the effect on mortality by any cause in Row A. The effect on death from homicide is significant at 5 percent in various subgroups, including those who had not previously worked. When we estimate the regression for non-homicide external causes or "natural" causes in Rows E and F, respectively, the point estimates are much closer to zero, represent a much smaller percentage reduction, and are far from statistically significant (p=0.56 and p=0.54, respectively).

[54] See, for example, http://www.nyc.gov/html/doh/downloads/pdf/vs/vs-population-and-mortality-report.pdf for the classification of deaths into "external" causes and "natural" causes and their prevalence in NYC. Natural causes represent all deaths other than external causes.
[55] See http://www.cdc.gov/injury/wisqars/pdf/10LCID_All_Deaths_By_Age_Group_2010-a.pdf
[56] It is not surprising to find a slight discrepancy between the DOH and IRS data results, because the IRS data cover all deaths whereas the DOH data cover only deaths in NYC.

We conclude that there is some evidence that SYEP reduces external causes of death. The evidence further suggests that external causes of death, and homicide in particular, account for most of the overall effect on mortality through 2012.

10. Conclusion

We investigate the effects of summer employment on youth by analyzing the New York City Summer Youth Employment Program, which randomly selected applicants for access to the program. We can now revisit the three broad rationales for programs that support summer youth employment: (1) transferring to youth; (2) raising future earnings, employment, or education; and (3) 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. In the year of program participation, we find that SYEP raises earnings substantially, with only modest (19.24 percent) crowdout of other earnings that is small relative to many results in other literature.[57] Overall, SYEP leads to a substantial and significant increase in earnings when we consider the sum of earnings in the year of SYEP participation along with the subsequent years—albeit with quite substantial crowdout (54.02 percent) over Years 0 to 4 combined.

We find that 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. Thus, we find no evidence that SYEP succeeds in raising subsequent earnings, employment, or education; on balance, we find the opposite. The impact on subsequent earnings is small relative to likely lifetime income, but it is substantial relative to the size of the SYEP transfer (36.13 percent of the transfer). It is notable that even for this young group with little or no prior job experience — and even during the Great Recession — an employment program does not provide a path to greater future earnings.

Finally, we find evidence that SYEP succeeds in the goal of keeping youth "out of trouble." SYEP leads to a 0.10 percentage point decrease in the rate of incarceration in state prison, which is small in percentage point terms but substantial relative to the baseline rate of 0.95 percent. Among those 19 and older at the time of SYEP participation — the group for

[57] For example, the meta-analysis of Card, Kluve, and Weber (2010) finds no systematic evidence that public employment programs raise net contemporaneous employment.

which we are able to observe essentially all incarceration episodes for crimes committed at the time of SYEP participation or later — the reduction in incarceration is quite substantial (0.46 percentage points) and over half of the baseline incarceration rate. Paralleling the effect on incarceration, SYEP reduces the mortality rate by 0.08 percentage points, which is again small in percentage point terms but large relative to the baseline (0.38 percentage points). Combined with the number of individuals in SYEP, the mortality point estimate implies that 85.93 lives were saved by SYEP in the time window of the program that we analyze. While SYEP has a modest negative effect on subsequent earnings for three years — which may at first appear to contrast with the reduction in incarceration and mortality — the reduction in incarceration and mortality parallels the typically positive effects on earnings found in the lower quantiles of the earnings distribution, which also suggest that SYEP improves the left tail of outcomes.

These effects on mortality are very important in a cost-benefit analysis. The earnings effects that we estimate prove to be modest relative to the program's benefits from reduced mortality. The value of a statistical life is estimated to be in the range of $9 million for prime-age workers (Viscusi and Aldy 2003, in $2013); if SYEP saved 85.93 lives, this would imply benefits of $773.38 million. It is clear that the point estimates imply that the mortality benefits will be large within any plausible range of the value of life.[58] Meanwhile, 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, we find that the reduction in incarceration due to the SYEP program in years 2005 to 2008 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.[59]

It is not possible for us to determine with certainty whether the benefits of the

[58] Viscusi and Aldy (2003) find that the lower end of the plausible range of the value of a statistical life is around $5.25 million ($2013), which would still imply very large SYEP mortality benefits of $451 million. For this group of younger workers, who have more years of life remaining than the typical prime-age worker does, the value of a statistical life could be higher than $9 million (Viscusi and Aldy 2003). On the other hand, Viscusi and Aldy also find that the value of life has a positive income elasticity, whereas SYEP participants typically have low income. Another caveat to these results is that the lower end of the confidence interval shows that SYEP saved only eight lives, but 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.

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

program outweigh the costs, as there are many costs and benefits we do not observe. For example, we do not observe the value of the goods produced by SYEP participants, the costs of SYEP participants' out-of-pocket expenses from participating in the program (such as commuting or childcare), the social cost of crimes not captured in the incarceration data (such as future incarceration episodes), the cost of other public programs that could have been affected by SYEP participation, and so on.

Nonetheless, it is clear that the $773.38 million in mortality benefits is substantial compared to plausible estimates of the various costs of the program. Due to the SYEP program in the years 2005 to 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, and assuming such administrative costs are bought at roughly competitive prices, the opportunity cost of these expenses is likely to be in roughly the same range; the opportunity cost of time of SYEP participants is unknown, but theory tells us it 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 is unknown but should equal the discounted accounting cost of SYEP, $236.4 million, multiplied by the marginal social cost of public funds.[60] What is clear, and what we can conclude from this exercise, is that while we cannot say with certainty whether SYEP's benefits outweigh its costs, it is certainly the case 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.

As in any empirical setting, our estimates are local to the group examined — in our case, SYEP participants. Thus, for example, our estimates apply to New York City, while effects may be different in other cities. Despite this caveat, our results may have important implications for other efforts to improve youth employment outcomes, including the Work Opportunity Tax Credit, other cities' summer employment programs, and other efforts to support summer youth employment.[61] Crucially, our results also demonstrate that effects on less-scrutinized outcomes such as mortality can be very important. Like most other studies of

[60] We discount to 2005 using a 3 percent real discount rate and express all dollar values in real 2013 terms.
[61] Our results are consistent with those of previous studies that find limited employment and earnings effects of subsidies for hiring disadvantaged groups like the WOTC and Welfare-to-Work Credits (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. It is also worth noting that for the WOTC-eligible group, we estimate a positive and insignificant effect of SYEP participation on subsequent earnings, and a positive and significant effect of SYEP participation on earnings in Years 0-4.

youth active labor market programs surveyed (Stanley, Katz, and Krueger 1998; Heckman, Lalonde, and Smith 1999; Lalonde 2003; Card, Kluve, and Weber 2010), 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. This is certainly true in our context, where in fact we find that SYEP modestly reduces participants' future earnings for three subsequent years. However, unlike other studies, we find a very large source of new benefits due to the program's reduction in mortality, which has a strong potential to be pivotal in assessing whether the program's benefits outweigh the costs. Other youth active labor market programs may or may not have such mortality benefits — data should be gathered to determine whether this is true — but it is worth noting that like SYEP, other youth programs have been found to keep youth out of trouble (for example, Schochet, Burghardt, and McConnell 2008 find that Job Corps reduces crime, and Heller 2014 finds that the Chicago summer youth program reduces violent crime arrests).

Our results also suggest that earnings crowdout may be minimized, and the reductions in mortality and incarceration can be maximized, by targeting such policies toward younger individuals, and/or those not previously working. Furthermore, the results show modest contemporaneous crowdout; if applicable in contexts such as the 2009 stimulus spending on summer youth employment, this suggests that summer youth employment spending increased net income, and net contemporaneous income and employment, of the youth employed through the program.

As noted above, we do not observe the social or non-pecuniary benefits of jobs; it is possible that there are externalities or non-pecuniary benefits associated with the work for non-profit organizations that youth typically do through SYEP and in subsequent jobs. In that light, it is worth noting that SYEP modestly raises average subsequent earnings in jobs at non-profit employers, while modestly lowering subsequent earnings in jobs at for-profit employers and lowering earnings in jobs with the government. As in other studies using administrative data, we do not observe earnings in the underground economy. However, we believe it is unlikely that observing underground earnings would dramatically impact our results. To overturn our finding that SYEP decreased total earnings in Years 1-4, SYEP would have had to lead participants to have higher 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); indeed, our other evidence shows that SYEP industries are "sticky" in the sense that SYEP raises non-profit earnings and lowers for-profit earnings in Year 0, and this effect persists into future years. It is possible that greater formal sector earnings through SYEP in Year 0 likewise leads individuals away from the informal sector, though we lack direct evidence on this issue. In addition, because we estimate a local average treatment effect, we are also unable to take into account any general equilibrium effects of SYEP.[62] It is also possible that SYEP jobs replace jobs that employers would have otherwise offered, which would create additional costs of the program — though we consider it unlikely that there is one-for-one displacement given that NYC pays for SYEP jobs.[63]

This paper leaves open a number of questions. As more years of data since SYEP participation accumulate, it would be possible to investigate even longer-term impacts of the program. The passage of time will also allow us to investigate more data on cause of death. In future work, we hope to overcome the obstacles that we have so far encountered in accessing data on arrests of SYEP applicants, in order to examine how SYEP affects this outcome. In order to develop more definitive evidence on the mechanisms through which the negative earnings effect operates, it would be of interest to use a randomized design to investigate whether the type of job into which an individual is placed has an effect on subsequent earnings. If the type of youth job matters for longer-term outcomes — as our suggestive evidence indicates — this has broader implications for understanding how initial job placements affects individuals' career trajectories.

[62] Crepon et al. (2013) find that positive effects of job assistance come at the expense of other labor market participants. While it is possible that such general equilibrium effects could arise in our context, 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 or the NYC youth labor market.

[63] Moreover, in another context such displacement effects might be expected to operate in part because a public employment program could increase wages faced by other employers; this is less worrisome in this context, in which wages are regulated to be at least the minimum wage.

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Table 1. *Experiment balance. Means and standard deviations of variables, and coefficients and standard errors from OLS regressions of pre-determined variables on the treatment dummy.*

(1) Variable	(2) Mean (SD)	(3) Coeff. (SE) on Treatment
Main Outcomes		
Total yearly earnings	3,555.29 (7,201.94)	--
NYC gov't yearly earnings	218.39 (474.92)	--
Non-NYC gov't yearly earnings	3,336.91 (7,259.75)	--
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	892.17 (4,489.23)	-23.65 (21.55)
NYC gov't earnings	256.96 (495.85)	-1.79 (2.13)
Non-NYC gov't earnings	633.37 (4,477.73)	-21.87 (21.32)
Has any job	0.32 (0.47)	-0.002 (0.002)
Has any non-NYC gov't job	0.13 (0.34)	-0.002 (0.001)
College enrollment	0.04 (0.20)	0.0009 (0.0007)
Family income	39,521.56 (29,412.29)	-36.99 (115.75)
SYEP participation	0.21 (0.41)	-0.001 (0.002)
Race		
White	0.13 (0.33)	-0.002 (0.001)
Latino	0.27 (0.44)	0.0007 (0.002)
Black	0.48 (0.50)	0.0007 (0.002)
Other	0.12 (0.33)	0.0005 (0.002)
Other variables		
Male	0.45 (0.50)	-0.002 (0.002)
Age	16.50 (1.63)	0.0006 (0.009)
# Family Members	4.30 (1.86)	-0.002 (0.007)
U.S. Citizen	0.93 (0.25)	-0.0007 (0.001)
SYEP-IRS Match dummy	0.998 (0.06)	0.0003 (0.0002)

Notes: The table shows summary statistics and demonstrates that there are no significant differences in covariates across the treatment and control groups. 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. 294,580 observations are included in the sample 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, DOB, name, and first or last four digits of the SSN. *** denotes significance at the 10% level; ** at the 5% level; and * at the 1% level.

Table 2. *Effect of SYEP participation on earnings and employment outcomes. The table shows coefficients and standard errors on the SYEP participation dummy from two-stage least squares regressions of earnings and employment outcomes on SYEP participation. The instrument for whether an individual participated in SYEP is a dummy indicating that an individual won the SYEP lottery.*

	(1) Total Earnings	(2) NYC gov't earnings	(3) Non-NYC gov't earnings	(4) Job dummy	(5) Non-NYC gov't job dummy
A) Year 0	876.26	1085.34	-208.87	0.71	-0.05
	(25.08)***	(10.14)***	(24.83)***	(0.006)***	(0.004)***
B) Year 1	-99.61	45.98	-145.59	0.01	-0.02
	(40.06)**	(5.01)***	(40.08)***	(0.003)***	(0.003)***
C) Year 2	-94.16	23.29	-117.45	0.004	-0.01
	(42.05)**	(3.33)***	(42.42)***	(0.003)	(0.003)***
D) Year 3	-110.81	8.22	-119.04	-0.0006	-0.005
	(44.43)**	(2.08)***	(44.26)***	(0.002)	(0.002)**
E) Year 4	-35.15	4.47	-39.62	0.001	-0.0003
	(44.82)	(1.58)***	(45.23)	(0.002)	(0.002)
F) Years 0-4	536.53	1167.30	-630.57	0.09	-0.006
	(173.10)***	(15.09)***	(173.75)***	(0.003)***	(0.002)***
G) Years 1-4	-339.73	81.96	-421.70	0.01	-0.003
	(154.51)**	(9.51)***	(154.94)***	(0.002)***	(0.002)

Notes: The table shows the results of two-stage least squares regressions (1)-(2) in which employment outcomes are related to SYEP participation. Each row shows the results for a different year or set of years, and each column shows the results for a different outcome. We control for SYEP provider-by-year dummies so that the estimates are driven by random variation in winning the SYEP lottery. The number of observations in each regression is 294,580, corresponding to 198,745 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.

Table 3. *Effect of SYEP participation on earnings outcomes by quantile. The table shows coefficients and standard errors on the SYEP participation dummy from quantile regressions of earnings outcomes on SYEP participation. The independent variable of interest is a dummy indicating that an individual won the SYEP lottery.*

	(1) Year 0	(2) Year 1	(3) Year 2	(4) Year 3	(5) Year 4
Panel A: Median regressions					
A) Total	1101.24	83.40	23.91	-41.87	-54.99
	(2.77)***	(18.83)***	(14.51)*	(22.09)*	(31.28)
B) NYC gov't	1077.87	--	--	--	--
	(1.45)***				
C) Non-NYC gov't	--	--	-54.50	-46.32	6.73
			(17.40)***	(24.58)*	(34.11)
Panel B: Regressions for 75th percentile					
D) Total	477.80	-134.90	-122.13	-140.45	7.59
	(2.72)***	(27.69)***	(39.96)***	(52.50)***	(60.44)
E) NYC gov't	1181.88	--	--	--	--
	(0.98)***				
F) Non-NYC gov't	-129.72	-177.46	-155.49	-145.19	2.42
	(11.03)***	(28.29)***	(44.33)***	(52.43)***	(61.04)
Panel C: Regressions for 90th percentile					
G) Total	88.76	-254.62	-188.26	-204.80	-9.35
	(37.01)**	(64.94)***	(71.74)***	(84.75)**	(105.87)
H) NYC gov't	1088.80	16.04	71.18	--	--
	(1.21)***	(1.02)***	(8.25)***		
I) Non-NYC gov't	-571.83	-288.38	-201.47	-365.84	-27.47
	(37.97)***	(66.53)***	(72.40)***	(87.35)***	(107.31)
Panel D: Descriptive statistics					
J) Percent earning 0	39.88	44.13	38.77	33.24	28.83
K) Median total	939.18	629.75	1,051.60	1,386.65	2,473.01
L) 75th total	1,353.58	2,006.57	3,947.14	6,196.07	8,674.60
M) 90th total	2,984.16	6,293.88	9,613.63	12,852.71	16,378.32

Notes: Each row investigates a different quantile and/or outcome variable. Panel A investigates the effect of SYEP on median earnings; Panel B investigates the effect on the 75th percentile of earnings; and Panel C investigates the effect on the 90th percentile. Rows A, D, and G investigate the effect on total earnings; B, E, and H investigate the effect on NYC government earnings; and C, F, and I investigate the effect on non-NYC government earnings. For context, Panel D shows descriptive statistics. "--" indicates that the quantile of earnings in question is zero, which implies that SYEP participation has no effect on the quantile in question. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level. See other notes to Table 2.

Table 4. *Effect of SYEP participation on earnings outcomes among subsamples. The table shows coefficients and standard errors on the SYEP participation dummy from IV regressions of employment outcomes on SYEP participation. The instrument for whether an individual participated in SYEP is a dummy indicating that an individual won the SYEP lottery.*

	Year 0			Years 1-4			N
	(1) Total Earnings	(2) NYC gov't earnings	(3) Non-NYC gov't earnings	(4) Total earnings	(5) NYC gov't earnings	(6) Non-NYC gov't earnings	
A) WOTC-eligible	842.21 (63.01)***	1089.02 (11.46)***	-246.66 (62.90)***	-18.73 (429.52)	57.84 (18.65)***	-72.56 (429.80)	32,248
B) WOTC-ineligible	879.04 (26.40)***	1084.97 (10.15)***	-205.71 (26.30)***	-384.76 (159.92)**	84.98 (9.90)***	-469.75 (160.54)***	262,332
C) Below-median inc.	888.81 (27.58)***	1086.79 (13.06)***	-197.91 (22.34)***	-317.61 (148.55)**	74.59 (11.35)***	-392.20 (150.49)***	147,291
D) Above-median inc.	860.00 (53.92)***	1083.95 (7.99)***	-223.60 (54.21)***	-382.17 (256.07)	89.24 (10.66)***	-471.43 (256.41)*	147,289
E) Males	855.87 (31.98)***	1079.51 (9.74)***	-223.53 (32.20)***	-365.47 (162.55)**	92.29 (11.61)***	-457.78 (166.49)***	132,692
F) Females	892.25 (38.69)***	1090.20 (10.76)***	-197.65 (39.03)***	-321.49 (243.72)	72.92 (10.46)***	-394.41 (244.62)	161,888
G) White	899.28 (70.66)***	1,195.37 (42.28)***	-296.09 (72.58)***	-1,242.35 (549.18)**	120.60 (20.56)***	-1,362.95 (554.96)**	37,172
H) Black	899.14 (34.88)***	1,073.35 (5.88)***	-174.02 (33.68)***	-122.69 (201.26)	72.27 (13.93)***	-194.93 (203.38)	142,627
I) Latino	803.95 (53.72)***	1,055.55 (8.59)***	-251.37 (52.00)***	-520.60 (329.05)	92.48 (10.71)***	-613.17 (327.03)*	79,095
J) Other races	944.36 (65.63)***	1,134.86 (34.09)***	-190.96 (57.07)***	-209.49 (296.54)	71.49 (15.19)***	-280.98 (300.37)	35,686
K) Older	688.58 (41.55)***	1068.93 (12.69)***	-380.13 (38.86)***	-726.62 (250.08)***	74.33 (9.36)***	-800.96 (251.94)***	147,260
L) Younger	1033.38 (27.50)***	1099.51 (8.84)***	-65.91 (28.03)**	-44.06 (158.11)	88.33 (11.35)***	-132.41 (155.13)	147,320
M) Work in Year -1	691.98 (80.32)***	1090.66 (15.00)***	-397.98 (82.92)***	-993.54 (419.67)**	56.70 (11.84)***	-1050.28 (422.72)**	94,855
N) No work in Year -1	966.08 (12.40)***	1083.04 (9.07)***	-116.96 (8.82)***	5.02 (101.75)	92.50 (10.79)***	-87.48 (102.42)	199,725
O) 2005-6 lotteries	903.03 (34.51)***	1068.96 (11.08)***	-165.84 (35.24)***	-61.98 (235.76)	77.44 (13.66)***	-139.45 (238.65)	116,079
P) 2007-8 lotteries	858.58 (36.17)***	1096.16 (10.62)***	-237.28 (35.07)***	-523.10 (195.88)***	84.94 (9.82)***	-608.04 (196.48)***	178,501

Notes: The table shows the results of regressions in which employment outcomes are related to SYEP participation. Each row shows the results for a different population. "Work in Year -1" indicates that the individual had positive earned income in Year -1. Below-median income refers to individuals with family income in Year -1 of $26,313 and below, and above-median income refers to individuals in families with higher income (where the median income refers to the median in Year -1 in the sample we investigate). The Older category is at least age 16.25, whereas the Younger category is below this age. The sample size is slightly different for above-median and below-median incomes, and for older and younger ages, because multiple individuals have the median value of income and age. See other notes to Table 2. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

Table 5. *Effect of SYEP participation on college attendance. 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.*

	(1) Year 0	(2) Year 1	(3) Year 2	(4) Year 3	(5) Year 4	(6) Years 0-4	(7) Years 1-4	(8) Total years of college
(A) No controls	0.001 (0.002)	0.003 (0.002)	-0.001 (0.002)	0.0005 (0.002)	-0.003 (0.002)	-0.004 (0.002)	-0.003 (0.002)	-0.001 (0.006)
(B) With Controls	0.0003 (0.001)	0.002 (0.001)	-0.002 (0.002)	0.0001 (0.002)	-0.004 (0.002)	-0.004 (0.002)*	-0.003 (0.002)	-0.003 (0.006)

Notes: The table shows the results of regressions in which a dummy for college attendance is related to SYEP participation. 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). Columns 6 and 7 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. Column 8 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. Row A shows the results without controls, and Row B shows the results with controls. See other notes to Table 2. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

Table 6. *Effect of SYEP participation on incarceration. Columns 1 and 2 show 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. In Column 3, the dependent variable is the number of times incarcerated. The instrument for whether an individual participated in SYEP is a dummy indicating that an individual won the SYEP lottery. Column 4 shows coefficients and standard errors from a probit regression of the incarceration dummy on the SYEP participation dummy.*

	(1) 2SLS, no covariates	(2) 2SLS, with covariates	(3) Times incarcerated	(4) Probit	(5) Prison dummy mean (x 100)
(A) Full population	-0.10 (0.05)**	-0.09 (0.04)**	-0.11 (0.05)**	-0.06 (0.03)**	0.95
(B) 19 and over	-0.46 (0.22)**	-0.49 (0.22)**	-0.44 (0.25)*	-0.39 (0.18)**	0.85
(C) 18 and under	-0.07 (0.05)	-0.06 (0.05)	-0.09 (0.05)*	-0.05 (0.03)	0.96
D) Below-median inc.	-0.14 (0.07)*	-0.14 (0.07)*	-0.13 (0.08)*	-0.09 (0.05)*	1.14
E) Above-median inc.	-0.05 (0.07)	-0.05 (0.06)	-0.08 (0.07)	-0.04 (0.04)	0.76
(F) Males	-0.22 (0.09)**	-0.22 (0.09)**	-0.24 (0.10)**	-0.16 (0.07)**	2.01
(G) Females	0.02 (0.02)	0.02 (0.02)	0.01 (0.02)	.03 (0.02)	0.08
(H) White	-0.15 (0.09)*	-0.15 (0.09)*	-0.16 (0.10)*	-0.12 (0.05)***	0.07
(I) Black	-0.16 (0.07)***	-0.16 (0.07)**	-0.19 (0.07)***	-0.13 (0.05)***	1.48
(J) Latino	0.05 (0.08)	0.05 (0.08)	0.04 (0.09)	0.03 (0.06)	0.70
(K) Other	-0.06 (0.10)	-0.06 (0.10)	-0.04 (0.11)	-0.16 (0.24)	0.33
L) Older	-0.07 (0.07)	-0.07 (0.07)	-0.08 (0.08)	-0.04 (0.04)	0.97
M) Younger	-0.12 (0.07)*	-0.11 (0.07)	-0.14 (0.08)*	-0.09 (0.05)*	0.93
(N) Prior work	-0.05 (0.09)	-0.04 (0.09)	-0.06 (0.09)	-0.03 (0.06)	0.79
(O) No prior work	-0.12 (0.05)**	-0.12 (0.05)**	-0.14 (0.05)***	-0.08 (0.03)**	1.02
P) 2005-6 lotteries	-0.12 (0.07)	-0.11 (0.08)	-0.15 (0.09)*	-0.09 (0.06)	1.15
Q) 2007-8 lotteries	-0.08 (0.06)	-0.08 (0.05)	-0.08 (0.06)	-0.05 (0.03)	0.82

Notes: Before running the regressions, we multiply the dependent variable by 100 so that coefficients show percentage point changes (for the reader's ease). Note that the probit specification runs "reduced form" regressions that regress the dependent variable directly on the dummy for winning the SYEP lottery, not an instrumental variables regression. The probit coefficients represent marginal effects, calculated at the mean. Adding controls to the specifications in Columns 3 and 4 yields nearly identical results. The group 19 and older consists of 24,809 observations, and the group 18 and younger consists of all remaining observations. See Tables 2 and 4 for other notes and information on samples. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

Table 7. *Effect of SYEP on mortality. Column 1 shows hazard ratios and standard errors on a dummy for winning the SYEP lottery from a right-censored Cox proportional hazard model of time to mortality. Column 2 shows a two-stage least squares estimate using a linear probability model. Column 3 shows coefficients and standard errors from a probit regression.*

	(1) Cox	(2) 2SLS	(3) Probit	(4) Mortality dummy mean (x 100)
A) Full population	0.86 (0.05)**	-0.08 (0.03)**	-0.05 (0.02)**	0.38
B) WOTC-eligible	0.82 (0.16)	-0.10 (0.10)	-0.07 (0.07)	0.38
C) WOTC-ineligible	0.87 (.05)**	-0.07 (0.03)**	-0.05 (0.02)**	0.38
D) Below-median inc.	0.86 (0.09)	-0.09 (0.06)	-0.06 (0.04)	0.42
E) Above-median inc.	0.87 (0.07)*	-0.06 (0.04)*	-0.05 (0.03)*	0.34
F) Males	0.83 (0.06)**	-0.15 (0.06)**	-0.11 (0.04)**	0.61
G) Females	0.94 (0.12)	-0.02 (0.03)	-0.01 (0.02)	0.19
H) White	1.01 (0.32)	0.003 (0.08)	0.001 (0.05)	0.16
I) Black	0.91 (0.07)	-0.06 (0.05)	-0.05 (0.04)	0.52
J) Latino	0.72 (0.09)**	-0.14 (0.06)**	-0.10 (0.04)**	0.32
K) Other races	0.80 (0.22)	-0.05 (0.07)	-0.04 (0.05)	0.18
L) Older	0.95 (0.08)	-0.03 (0.05)	-0.02 (0.03)	0.40
M) Younger	0.80 (0.07)**	-0.10 (0.04)***	-0.09 (0.03)***	0.33
N) Work in Year -1	1.07 (0.13)	0.03 (0.06)	0.02 (0.04)	0.35
O) No work in Year -1	0.79 (0.06)***	-0.12 (0.04)***	-0.09 (0.03)***	0.39
P) 2005-6 lotteries	0.76 (0.06)***	-0.16 (0.05)***	-0.13 (0.04)***	0.47
Q) 2007-8 lotteries	0.96 (0.09)	-0.02 (0.04)	-0.01 (0.03)	0.32

Notes: Each row shows the results for a different population. We eliminate from the regressions those rare cases of individuals who died between applying to SYEP and the date of first participating in SYEP. See Tables 2 and 4 for other notes and information on samples. The Cox and probit specifications run "reduced form" regressions that regress the dependent variable directly on the dummy for winning the SYEP lottery, not an instrumental variables regression. The probit coefficients represent marginal effects, calculated at the mean. Column 4 shows the mean of the "mortality by 2014" dummy, multiplied by 100, in each group. So that readers can more easily interpret the results, we have also multiplied the dependent variable by 100 in Columns 2 and 3. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

Online Appendix 1: Other specifications

Robustness checks

Various robustness checks to the basic results on earnings in Table 2 yield extremely similar results. As we show in Appendix Table 5, all of the results are very similar to those in Table 2 when we perform the same specifications but additionally control for: gender, dummies for race categories, citizenship, age, number of family members, individual's wage income in Year -1, individual's NYC government wages in Year -1, individual's non-NYC government wages in Year -1, a dummy for whether the individual had any job in Year -1, a dummy for whether the individual had a non-NYC government job in Year -1, a dummy for whether the individual was enrolled in college in Year -1, a dummy for whether the individual was claimed on a return in Year -1, and family income in Year -1 if the individual was claimed on a return in Year -1.[i]

Appendix Table 6 shows a number of other specifications, all of which deliver results very similar to the baseline specification. First, in Panel A, we use the dummy for winning the *initial* SYEP lottery as the instrument, rather than our baseline where the instrument is a dummy for winning any of the SYEP lotteries in Year 0.[ii] Second, in Panel B, we only include individuals in the sample who match the SYEP data according to their SSN. Third, in Panel C, we cluster at the individual level rather than at the level of the provider, which leads to extremely similar standard errors and significance levels.

Dynamics

As shown in Appendix Table 2, SYEP participation in Year 0 slightly affects the probability of SYEP participation in future years (i.e. Years 1-4). Thus, some of the effects on earnings we observe are mediated through the impact of SYEP on future SYEP participation, though the small effect on future participation suggests a limited role for such a mechanism. To more precisely examine the extent to which this drives the results, we estimate the effect of SYEP participation on earnings using the "dynamic" specification of Cellini, Ferreira, and Rothstein (2010). In our context, this dynamic estimator effectively yields the effect of SYEP participation in Year 0 on earnings in any subsequent year, while removing the effect that operates through the channel of the effect of Year 0 SYEP participation on subsequent SYEP participation. By contrast, the instrumental variables estimates in Table 2, which we call the "static" estimates, estimate the effect of being employed through SYEP in Year 0 on the future path of earnings and employment, including the effect that works through future SYEP participation.

Following the "recursive" procedure of Cellini et al. (2010), we first estimate the coefficients in Table 2 and Appendix Table 2, showing the effect of Year 0 SYEP participation on subsequent earnings and on subsequent SYEP participation respectively, using the methods discussed above. Let β_τ^S represent the estimate of the effect of SYEP

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[i] Controlling for higher-order terms in income also has negligible affects on the results.

[ii] Unsurprisingly, these regressions have somewhat larger standard errors, but the estimates are still very significant and have small confidence intervals.

participation in Year 0 on earnings (or another outcome variable) in year τ from Table 2, and let π_t represent the effect of Year 0 SYEP participation on the probability of SYEP participation in year *t* (from Appendix Table 2). We calculate the dynamic effect β_τ^D in year τ as:

$$\beta_\tau^D = \beta_\tau^S - \sum_{t=1}^{\tau} \pi_t \beta_{\tau-t}^D \qquad (3)$$

We solve for the dynamic effects in each year using the recursive equation (3). Standard errors are obtained by the delta method. By contrast, the instrumental variables estimates in Table 2, which we call the "static" estimates and which represent β_τ^S in (3), estimate the effect of being employed through SYEP in Year 0 on the future path of earnings and employment. These two objects of study reflect different conceptual experiments, both of which are of interest.

Since Appendix Table 2 shows that SYEP participation in Year 0 has a small impact on the probability of future SYEP participation, it is not surprising that the dynamic estimator finds results that are similar to the static estimates. Appendix Table 7 shows that the effect on subsequent total earnings is somewhat more negative in the dynamic specification than in the static specification, particularly in the initial years. Nonetheless, it is worth noting that the estimated effect on non-NYC government earnings and on total earnings is generally similar to the estimated effect in the static specification in Table 2.

The small positive effect of SYEP participation on NYC government earnings in Years 1-4 in the dynamic specification indicates that average total wages conditional on SYEP employment in subsequent years must be increasing slightly over time, likely because of the rise in the minimum wage over time (and possibly because average hours worked in SYEP could have changed).

In our other analysis throughout the paper, we use the simpler static specification, though we note that throughout all of our specifications and outcomes we obtain comparable results in the dynamic version to those in the static analysis (which is unsurprising since the effect of Year 0 SYEP participation on subsequent SYEP participation is quite modest).

Appendix Table 8 shows specifications relating to the number of times the individual participated, or could have participated, in SYEP. Among applicants who were too young to have been eligible to participate in SYEP previously because they were 14 or younger in 2005 (115,337 applicants), the results are similar to our main sample.[iii] While we only have records from SYEP on lottery applications and SYEP participation starting in 2005, we can use IRS records on NYC government earnings in prior years as a proxy for prior SYEP participation (classifying individuals as having participated in SYEP in a given prior year when they had positive NYC government earnings in that year). Under this definition, we show the effect of SYEP participation separately for those who had

[iii] Among those 21 years old in the year of SYEP application, who are ineligible to participate in SYEP again, the sample size drops to 16,620, and the results are imprecise and insignificant (p>0.40).

previously participated in SYEP no times, one time, two times, three times, or four or more times. We also use winning the SYEP lottery as an instrument for the total number of times participating in SYEP between 1999 and Year 0 (inclusive) and show that this also yields comparable results.

Online Appendix 2: Discussion of Mechanisms

It is worth considering why SYEP participation reduced mean earnings for three years after participation in the program. In this Appendix, we consider a number of additional potential explanations, including: income effects; substitability of leisure across years; signaling; replacing work experience; job type; and effects on job transitions.

Income effects or substitutability of leisure

One potential explanation for the decrease in subsequent earnings relates to income effects. Getting a SYEP job leads to an average increase in earnings of $872.36 in the year of the SYEP job, which could in principle lead to increased leisure in subsequent years if leisure is a normal good. However, income effects cannot immediately explain the striking heterogeneity across groups that we find; for example, we would have to postulate that there is an income effect on the earnings of those who previously had a job, but not on the earnings of those who previously did not have a job. While it is possible that the income effects are heterogeneous in ways that track the heterogeneous findings across groups, this is an *ad hoc* — and not particularly parsimonious — explanation. Moreover, recall that SYEP leads individuals to earn less conditional on having a job. Consequently, such an income effect would have to operate in a manner that seems unexpected: it would have to decrease earnings even as it leads individuals to be equally or more likely to take a job.[iv]

In principle, another explanation for the results is that leisure could be substitutable across years, so that a decrease in leisure in Year 0 would have been associated with an increase in leisure in Year 1. However, this explanation runs into the same set of difficulties as the income effect explanation just explored.[v]

[iv] Moreover, as we discuss in further detail below, one of our robust findings is that individuals in groups that experienced larger increases in total earnings in Year 0 also experienced smaller earnings decreases in subsequent years. If an income effect were responsible for the results and were homogeneous across groups, we might have expected the opposite (assuming that leisure is a normal good). Note, however, that it is possible that in Year 0, the increase in income due to SYEP caused a decrease in non-SYEP earnings in Year 0 *subsequent* to SYEP participation (i.e. in the fall of Year 0).

[v] Again, leisure substitutability cannot immediately explain the striking heterogeneity across groups that we find; such an explanation would be *ad hoc*. Again, such leisure substitutability would not be consistent with the finding that the groups with larger increases in Year 0 total earnings tended to be those with smaller subsequent decreases in total earnings. And again, such leisure substitutability would be operating in a way that seems unexpected: it would have to decrease earnings even as it leads individuals to be more likely to take on a job. Again, a more satisfying hypothesis could explain both the effect of SYEP on the probability of having a job and the effect of SYEP on subsequent earnings.

Signaling

Another possible explanation is that employers use the information that an individual participated in SYEP in deciding whether to hire them. While winning the SYEP lottery is random conditional on applying, SYEP participation still contains information that employers could use. Those who apply for and enroll in SYEP may be those who have difficulty securing employment elsewhere. Thus, SYEP participants may be negatively selected relative to the population as a whole.[vi] Employers may therefore take the fact that an individual participated in SYEP as a negative signal of their productivity (in contrast to receiving a positive signal from the employment of an otherwise inexperienced worker, as in Pallais 2014).[vii] This would be consistent with some of the patterns across groups: groups with lower income on average (like blacks or younger individuals) tend to have negative effects on subsequent earnings that are smaller in absolute value, which we might expect if SYEP participation is interpreted less negatively in more disadvantaged groups (because more disadvantaged groups are less likely to have alternative options).

However, the signaling explanation is not immediately consistent with the difference in effects before and during the Great Recession. The point estimates of the effects on Years 1-4 earnings in the lotteries during the Great Recession (2007 and 2008) are more negative than those in the lotteries prior to the Great Recession (2005 and 2006). If employers were updating their expectation of individuals' productivity on the basis of SYEP participation, one might expect that employers would interpret SYEP participation more negatively when the individual participated before the Great Recession, than when the individual participated during the Great Recession (since it was more difficult to find other employment during the Great Recession). At the same time, recall that the estimates in 2005-2006 are insignificantly different from those in 2007-8, though barely so. Thus, while our evidence is not directly inconsistent with the signaling hypothesis, it also does not fully support the signaling hypothesis either.

Replacing valuable work experience

As noted in the main text, it is possible that SYEP harms individuals' future earnings because it affects the experiences that they gain in Year 0. If SYEP has a negative effect on subsequent earnings in part because it crowds out other, valuable employment experiences in Year 0, then we would expect that groups with more Year 0 crowdout would also show more negative effects of SYEP on subsequent earnings. This is borne out in the data.

[vi] Indeed, our data show that (unconditional on SYEP application) prior family income of SYEP participants is substantially lower than that of those who were eligible on the basis of being NYC residents but did not participate in SYEP.

[vii] SYEP has negative earnings effect for those who had previous employment, but no significant negative earnings effect for those who did not have previous employment. SYEP enrollment could be interpreted more negatively if an individual was previously employed than if the individual was not, for example because it indicates that the individual was unable to secure re-employment with the previous employer.

The groups for which the effect on total earnings in Year 0 is smallest are the groups for which the effect on non-NYC government earnings in Year 0 is particularly negative (i.e. groups for which the crowdout due to participating in SYEP is particularly large). In these groups, the effect on earnings in Years 1-4 also tends to be particularly negative, consistent with the hypothesis that such crowdout plays a role in explaining the negative effect of SYEP on subsequent earnings.[viii]

This pattern holds true for all the groups we analyze. First, the effect of SYEP participation on total earnings in Year 0 is highest in the 50th percentile, intermediate in the 75th percentile, and smallest in the 90th percentile. The effects on subsequent earnings are ordered the same way: the effect on earnings in Years 1-4 is sometimes positive in the 50th percentile, negative in the 75th percentile, and most negative in the 90th percentile. Similarly, the point estimate of the effect of SYEP participation on non-NYC government earnings in Year 0 among whites is particularly negative (-$295.99), is intermediate among Latinos ($-253.94), and is least negative among blacks and others (-$176.41 and -$194.20). This correlates with the effects on subsequent earnings estimated in each group, which is most negative for whites, intermediate for Latinos, and least negative for blacks and others. Likewise, the effect on non-NYC government earnings in Year 0 is more negative among the older SYEP applicants (-$384.56) than among the younger applicants (-$67.08), and there is a larger negative effect on subsequent earnings in the older group. Moreover, the effect on non-NYC government earnings in Year 0 is much more negative for those who worked prior to SYEP (-$401.29) than that among those who did not work prior to SYEP (-$117.76), and those who worked prior to SYEP showed the more negative effect on subsequent earnings. Finally, the Year 0 point estimate on non-NYC government earnings is much more negative in the 2007-8 lotteries than in the 2005-6 lotteries, and the effect of SYEP on future earnings is much more negative in the 2007-8 lotteries. It is also noteworthy that more negative subsequent impacts on earnings tend to occur in groups that are more likely to otherwise have a job in Year 0, like the older group or those who had a job in the prior year.

Effects of type of job

We investigate whether the type of SYEP job individuals are placed in has implications for their earnings. In Appendix Table 10, we show the following OLS regressions:

$$E_{ij} = \beta_0+\beta_1(W_{ij}*P_j)+\beta_2 W_{ij}+X_j\beta+v_{ij} \quad (4)$$

where E_{ij} represents the earnings of individual i in lottery j, W_{ij} is a dummy for winning the SYEP lottery, P_j is the percent of a provider's jobs that are in Cluster 1 (in a given lottery j), and X_j reflect dummies for each provider-lottery combination.[ix] Thus, the coefficient β_1 on the interaction term reflects whether individuals who win the lottery at providers with a greater proportion of Cluster 1 jobs have lower or higher earnings than

[viii] Nonetheless, as noted in the main text, we emphasize that each of these samples tends to differ on average along many characteristics (that are correlated across samples), and it could be that the effect on non-NYC government earnings in Year 0 is correlated across groups with the effect on subsequent total earnings for reasons unrelated to the hypothesis described above.

[ix] The IV version of this regression — using W_{ij} and $W_{ij}*P_j$ as instruments for SYEP participation and SYEP participation in provider P_j — shows directionally similar results with less statistical power.

those who win the lottery in providers with a smaller proportion of Cluster 1 jobs.[x] (If an individual applies to a provider with a greater proportion of Cluster 1 jobs, then winning the lottery is more likely to place the individual in a Cluster 1 job.)

The results in Panel A of Appendix Table 10 show that being in a Cluster 1 job negatively impacts subsequent total earnings (significantly in Year 3 and Year 4, as shown in Column 1), and that this is driven by negative effects on earnings in Cluster 2 (as shown in Column 3). Thus, the regressions demonstrate that placing people in jobs in SYEP-type industries (i.e. Cluster 1 industries) has a substantial negative impact on earnings in other industries, while having no significant impact on earnings in SYEP-type industries. This is evidence that SYEP affects future earnings in part because it affects the type of job that individuals take in future years.

In Panel B of Appendix Table 10, we investigate the coefficient on the main effect of winning the SYEP lottery. This coefficient reflects the hypothetical impact of winning the SYEP lottery *in a provider that only places individuals into Industry Cluster 2.* Intriguingly, the effect on total earnings is positive and significant in many cases (specifically in Year 0, Year 3, Year 4, Years 0-4, and Years 1-4), and it is positive and insignificant in the remaining cases. The point estimates are substantial (several hundred dollars in the case of individual years, and several times larger in the case of Years 0-4 or Years 1-4 combined). If SYEP has a positive effect on earnings when individuals are in Industry Cluster 2, then SYEP could improve outcomes of its lottery winners by increasing the fraction of SYEP jobs that are in Industry Cluster 2.[xi]

Nonetheless, we reiterate the important caveat that heterogeneity in the effects across providers could be driven by factors that happen to be correlated with the types of jobs in each provider. In this case, these results could not be interpreted as the causal effects on earnings of Cluster 1 vs. Cluster 2 jobs.

Effects on employment transitions

It is also possible that SYEP harms individuals' career development by increasing employment transitions and interrupting experience with past employers.[xii] Youth could take the SYEP job rather than continuing with an existing employment relationship. For example, a youth who had worked at a summer job in the previous summer might choose not to return to the same employer if a SYEP job were available. Appendix Table 11 shows that SYEP has such an effect in Year 0 (among those who did not participate in

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[x] We also explored regressions in which we interacted winning the lottery with the percent of jobs at the provider that were private not-for-profit, private for-profit, or government jobs. We found no significant differences in the effects of SYEP across these groups.

[xi] We also note the caveat that when we control for all other available demographics interacted individually with W_{ij} (which adds many controls and should therefore reduce the efficiency of the estimates), the coefficients β_1 and β_2 above are reduced in significance and substantially reduced in magnitude, although we robustly estimate that β_1 is negative and substantial and that β_2 is positive and substantial.

[xii] Card and Hyslop (2005) examine a related issue when investigating the dynamic effects of the Self Sufficiency Project in Canada.

SYEP in Year -1), though this effect is small. In Year 1, the estimate is also negative but barely significant, and in subsequent years the estimates turn insignificant.

Employment transitions are associated with lower earnings in a cross-section of individuals, and when we use the size of this cross-sectional association, we find that the effect of SYEP on job transitions can account for 38 percent of the impact of SYEP on earnings in Years 1 to 4. However, we emphasize that the cross-sectional association between job transitions and earnings is not causal and is therefore subject to omitted variable bias. We also note that like other channels explored in this appendix, this channel is not mutually exclusive with others we have explored.[xiii]

Other potential explanations

It is also worth mentioning a number of other possible explanations for the results. First, it is possible that SYEP caused the labor supply curve to shift to the right: SYEP could lead individuals to be more willing to accept low-paying jobs of the sort SYEP offers.[xiv] Second, SYEP participants could be exposed to a peer group that has negative effects on their future earnings. We find no evidence for such a channel: when we interact winning the lottery with measures of peer group characteristics (including family income, gender, race, or age), the interactions are generally insignificant.

[xiii] If this is the primary explanation for negative effects on future earnings, we also would not immediately expect the variation across provider industries that we find.

[xiv] If the demand curve shifted to the left, we would expect a decrease in earnings and a decrease in hours worked. If the supply curve shifted to the right, we would expect an increase in hours worked and could observe a decrease or increase in earnings. Table 2 shows that total jobs increase in Year 1, although some of this increase is due to an increase in SYEP jobs, which do not reflect labor demand. The dynamic estimates effectively remove the influence of SYEP jobs and show that total jobs decrease in Year 1. While we lack a measure of hours worked for the full sample, employment may be a reasonable proxy for hours worked given the absence of other proxies.

<u>Appendix Tables (to be placed online)</u>

Appendix Table 1: *Industry breakdown*

SYEP-Reported Job Type	Percent of sample	Cluster
Arts and recreation	10.50	1
Camp (out of city)	9.84	1
Community/social service	11.27	1
Cultural institution	1.30	2
Day care/day camp	35.77	1
Educational services	8.65	1
Financial services	0.20	2
Government agency	7.09	1
Healthcare/medical	8.58	1
Hospitality/tourism	0.12	1
Legal services	0.19	2
Other	3.92	2
Real estate/property	1.13	2
Retail	1.26	2
Science and technology	0.18	2

Notes: The table shows the percentage of SYEP participants in each industry, as classified by SYEP administrative records. The "Cluster" column shows whether we classify a SYEP-reported industry designation into North American Industrial Classification System (NAICS) codes 61, 62, 71, and 92 (comprising Cluster 1), or into other NAICS codes (comprising Cluster 2). Cluster 1 consists of industries that are overrepresented in SYEP jobs relative to the control group's industry distribution.

Appendix Table 2: *Effect of SYEP lottery win in Year 0 on SYEP participation in Year 0 and future years. The table shows marginal effects and standard errors on the SYEP participation dummy from OLS regressions of subsequent SYEP participation on Year 0 SYEP participation.*

	(1) SYEP Participation	(2) F-statistic
A) Year 0	0.73	4188.68
	(0.01)***	
B) Year 1	0.03	81.90
	(0.003)***	
C) Year 2	0.01	48.16
	(0.002)***	
D) Year 3	0.003	10.89
	(0.0009)***	
E) Year 4	0.001	6.45
	(0.0006)***	

Notes: The table shows the results of regressions in which SYEP participation in Year 0 and subsequent years is related to SYEP participation in Year 0, controlling for provider-by-year fixed effects. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

Appendix Table 3. *Mean and standard deviation of employment outcomes among SYEP lottery losers.*

	(1) Total Earnings	(2) Job dummy
A) Year 0	1,153.07	0.30
	(4,951.26)	(0.46)
B) Year 1	2,241.12	0.53
	(5,749.12)	(0.50)
C) Year 2	3,246.27	0.60
	(6,700.48)	(0.49)
D) Year 3	4,471.66	0.66
	(7,778.43)	(0.47)
E) Year 4	5,941.65	0.72
	(9,091.84)	(0.45)
F) Years 0-4	17,053.76	0.89
	(29,463.98)	(0.31)
G) Years 1-4	15,900.7	0.88
	(25,844.26)	(0.32)

Notes: See notes to Table 1. Summary statistics for Years 0-4 or Years 1-4 are calculated using totals across all of the years in question.

Appendix Table 4: *Effect of SYEP participation on earnings and employment outcomes in Years 5 to 7. The table shows coefficients and standard errors on the treatment dummy from IV regressions of earnings and employment outcomes on SYEP participation, controlling for covariates. The instrument for whether an individual participated in SYEP is a dummy indicating that an individual won the SYEP lottery.*

	(1) Total Earnings	(2) NYC gov't earnings	(3) Non-NYC gov't earnings	(4) Job dummy	(5) Non-NYC gov't job dummy	Number of obs.	Number of individuals
Panel A: Without controls							
A) Year 5	9.93	0.85	9.08	0.001	-0.0004	200,711	147,577
	(52.05)	(1.18)	(52.02)	(0.002)	(0.002)		
B) Year 6	-70.77	-0.23	-75.84	-0.002	-0.0007	116,079	94,662
	(95.44)	(1.48)	(105.50)	(0.003)	(0.003)		
C) Year 7	271.19	-1.70	272.88	-0.004	-0.003	56,557	56,557
	(137.74)**	(1.97)	(137.95)**	(0.005)	(0.005)		
Panel B: With controls							
D) Year 5	-3.96	0.92	-4.88	0.0008	-0.001	200,711	147,577
	(46.41)	(1.23)	(46.36)	(0.003)	(0.003)		
E) Year 6	-74.12	-0.26	-70.52	-0.002	-0.001	116,079	94,662
	(95.99)	(1.48)	(95.62)	(0.003)	(0.003)		
F) Year 7	167.10	-1.64	168.74	-0.004	-0.004	56,557	56,557
	(129.29)	(1.96)	(129.45)	(0.005)	(0.005)		

Notes: The table shows the results of regressions in which employment outcomes are related to SYEP participation. See other notes to Table 2. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

Appendix Table 5: *Effect of SYEP participation on earnings and employment outcomes with controls. The table shows coefficients and standard errors on the treatment dummy from IV regressions of earnings and employment outcomes on SYEP participation, controlling for covariates. The instrument for whether an individual participated in SYEP is a dummy indicating whether an individual won the SYEP lottery.*

	(1) Total Earnings	(2) NYC gov't earnings	(3) Non-NYC gov't earnings	(4) Job dummy	(5) Non-NYC gov't job dummy
A) Year 0	903.65	1085.25	-181.38	0.71	-0.05
	(16.26)***	(10.11)***	(12.92)***	(0.007)***	(0.003)***
B) Year 1	-74.83	46.14	-120.97	0.01	-0.02
	(23.51)***	(4.90)***	(24.09)***	(0.003)***	(0.003)***
C) Year 2	-72.63	23.38	-96.01	0.004	-0.01
	(28.25)***	(3.34)***	(29.02)***	(0.003)	(0.002)***
D) Year 3	-92.94	8.23	-101.19	-0.0009	-0.005
	(31.57)***	(2.13)***	(31.43)***	(0.002)	(0.002)**
E) Year 4	-22.70	4.46	-27.16	0.0009	-0.0007
	(36.76)	(1.57)***	(37.13)	(0.002)	(0.002)
F) Years 0-4	640.61	1167.46	-526.64	0.09	-0.006
	(100.67)***	(14.83)***	(100.32)***	(0.003)***	(0.002)***
G) Years 1-4	-263.05	82.21	-345.26	0.01	-0.003
	(97.59)***	(9.38)***	(98.98)***	(0.002)***	(0.002)

Notes: The table shows the results of regressions in which employment outcomes are related to SYEP participation. The table is identical to Table 2 in the main text, except that we control for the following covariates: gender, dummies for race categories, citizenship, age, number of family members, individual's wage income in Year -1, individual's NYC government wages in Year -1, individual's non-NYC government wages in Year -1, number of employes in Year -1, a dummy for whether the individual had any job in Year -1, a dummy for whether the individual was enrolled in college in Year -1, a dummy for whether the individual was claimed on a return in Year -1, and family income in Year -1 if the individual was claimed. Controlling for any subset of these covariates yields extremely similar results. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

Appendix Table 6: *Effect of SYEP participation on earnings and employment outcomes, robustness tests. The table shows coefficients and standard errors on the SYEP participation dummy from IV regressions of employment outcomes on SYEP participation.*

	(1) Total Earnings	(2) NYC gov't earnings	(3) Non-NYC gov't earnings	(4) Job dummy	(5) Non-NYC gov't job dummy
Panel A. Initial lottery					
A) Year 0	869.61 (37.58)***	1116.22 (9.28)***	-246.48 (37.82)***	0.70 (0.006)***	-0.06 (0.004)***
B) Years 1-4	-448.31 (170.92)***	81.44 (12.33)***	-529.76 (169.44)***	0.01 (0.002)***	-0.0005 (0.002)
Panel B. Match only on SSN					
C) Year 0	876.96 (25.17)***	1085.60 (10.15)***	-208.42 (24.89)***	0.71 (0.006)***	-0.05 (0.004)***
D) Years 1-4	-336.85 (155.60)**	81.90 (9.53)***	-418.77 (156.00)***	0.01 (0.002)***	-0.003 (0.002)
Panel C. Cluster by individual					
E) Year 0	876.26 (26.62)***	1085.34 (10.44)***	-208.87 (26.70)***	0.71 (0.002)***	-0.05 (0.002)***
F) Years 1-4	-339.73 (142.93)**	81.96 (5.31)***	-421.70 (143.54)***	.01 (0.002)***	-0.003 (0.002)

Notes: The table shows the results of regressions in which employment outcomes are related to SYEP participation. In Panel A, the instrument for whether an individual participated in SYEP is a dummy indicating that an individual won the *initial* SYEP lottery. In Panel B, the regressions are identical to those in the baseline specification in Table 2, except that we include people only if their SSN matches between the SYEP and IRS data. The sample size in Panel B is 293,908. In Panel C, we cluster the standard errors at the level of the individual. In all of these cases, the results are similar to the main results in Table 2. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

Appendix Table 7. *Effect of SYEP participation on earnings and employment outcomes, dynamic specification. The table shows coefficients and standard errors on the SYEP participation dummy from IV regressions of employment outcomes on SYEP participation using the dynamic IV estimator of Cellini, Ferreira, and Rothstein (2010). The instrument for whether an individual participated in SYEP is a dummy indicating that an individual won the SYEP lottery.*

	(1) Total Earnings	(2) NYC gov't earnings	(3) Non-NYC gov't earnings	(4) Job dummy	(5) Non-NYC gov't job dummy
A) Year 0	876.26	1085.34	-208.87	0.71	-0.05
	(25.28)***	(10.22)***	(25.03)***	(0.006)***	(0.004)***
B) Year 1	-126.40	12.79	-139.20	-0.009	-0.02
	(39.84)***	(2.29)***	(39.71)***	(0.003)***	(0.003)***
C) Year 2	-104.93	9.95	-114.88	-0.004	-0.009
	(42.46)**	(2.08)***	(42.49)***	(0.003)	(0.003)***
D) Year 3	-110.61	4.19	-114.81	-0.003	-0.005
	(43.65)**	(1.39)***	(43.53)***	(0.002)	(0.002)**
E) Year 4	-31.96	2.51	-34.47	0.0004	-0.000006
	(43.81)	(1.32)*	(44.18)	(0.002)	(0.002)
F) Years 0-4	502.36	1114.79	-612.24	--	--
	(170.95)***	(11.54)***	(171.24)***		
G) Years 1-4	-373.90	29.44	-403.37	--	--
	(152.24)**	(4.52)***	(152.33)***		

Notes: This table employs the dynamic IV estimator of Cellini, Ferreira, and Rothstein (2010), as described in the text. We perform the dynamic estimate of the effect on earnings in Years 0-4 (or 1-4) by summing the coefficients estimated in the dynamic specification from Years 0 to 4 (or 1-4, respectively). "--" indicates that we do not perform the estimates from Years 0-4 and 1-4 for the probability of having a job; we cannot add these coefficients across Rows A through E (as in the case of the earnings estimates) because the probabilities are not independent. See other notes to Table 2. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

Appendix Table 8. *Effect of SYEP participation on earnings outcomes by SYEP participation history. The table shows coefficients and standard errors on the SYEP participation dummy from IV regressions of earnings outcomes on SYEP participation. The instrument for whether an individual participated in SYEP is a dummy indicating that an individual won the SYEP lottery.*

	(1) Rule out prior SYEP participation (SYEP data)	(2) No prior SYEP participation (IRS data)	(3) Prev. participated once (IRS data)	(4) IV for number of times participated	(5) Total earnings of other family members
A) Year 0	701.33	651.88	671.18	948.49	-41.75
	(37.83)***	(28.46)***	(24.79)***	(27.82)***	(187.10)
B) Year 1	-50.74	-54.87	-125.56	-103.28	33.57
	(38.30)***	(32.22)*	(38.37)***	(41.54)***	(180.08)
C) Year 2	-90.18	-69.50	-78.79	-95.60	1.61
	(40.45)**	(33.85)***	(54.92)	(42.75)**	(175.93)
D) Year 3	-98.76	-79.88	-76.49	-111.75	-10.72
	(40.46)**	(36.64)***	(80.74)	(44.87)**	(175.09)
E) Year 4	-70.58	-18.20	-41.19	-35.29	-32.78
	(47.92)	(43.29)	(93.05)	(44.96)	(182.58)
N	115,337	202,717	60,437	294,580	294,580

Notes: Column 1 shows results for applicants who were young enough that they never could have been eligible to participate in SYEP previously, because they were 14 or younger in 2005. In Columns 2 and 3, we use IRS records on NYC government earnings in prior years as a proxy for prior SYEP participation (classifying individuals as having participated in SYEP in a given prior year 1999 or after when they had positive NYC government earnings in that year). We show results for those who had previously participated no times or one time in Columns 2 and 3, respectively. For those participating two or more times, the sample sizes are much smaller, and the results are insignificant and uninformative. In Column 4, we use winning the SYEP lottery as an instrument for the total number of times participating in SYEP between 1999 and Year 0 (inclusive). In Column 5, we examine the effect on the total earnings of other family members. See other notes to Table 2. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

Appendix Table 9: *Effect of SYEP participation on earnings and employment outcomes by industry and job type. The table shows coefficients and standard errors on the SYEP participation dummy from IV regressions of earnings and employment outcomes on SYEP participation. The instrument for whether an individual participated in SYEP is a dummy indicating that the individual won the SYEP lottery.*

	(1) Cluster 1	(2) Cluster 2	(3) For-profits	(4) Non-profits	(5) Gov't
Panel A: effects on earnings					
A) Year	966.59	-91.70	-75.56	786.65	165.13
0	(12.62)***	(24.96)***	(26.88)***	(28.29)***	(25.87)**
B) Year	31.41	-128.86	-134.36	33.20	1.62
1	(13.46)**	(35.92)***	(38.54)***	(4.80)***	(8.27)
C) Year	16.81	-110.13	-100.73	9.99	-3.35
2	(13.64)	(39.84)***	(39.97)**	(4.64)**	(8.16)
D) Year	-8.55	-101.74	-97.21	4.15	-18.02
3	(17.59)	(45.32)**	(46.57)**	(6.11)	(9.55)*
E) Year 4	-22.83	-11.12	-27.91	20.82	-28.32
	(23.57)	(40.64)	(45.94)	(9.92)**	(12.85)**
F) Years	983.43	-443.54	-435.77	786.65	117.06
0-4	(55.80)***	(164.61)***	(170.53)***	(28.29)***	(51.72)**
G) Years	16.84	-351.85	-360.21	68.16	-48.07
1-4	(52.89)	(146.90)**	(152.81)**	(16.88)***	(34.26)
Panel B: effects on dummy for having a job					
A) Year	0.81	0.05	0.04	0.67	0.16
0	(0.006)***	(0.007)***	(0.009)***	(0.02)***	(0.02)***
B) Year	0.03	-0.01	-0.01	0.03	0.003
1	(0.003)***	(0.003)***	(0.002)***	(0.003)***	(0.002)
C) Year	0.01	-0.005	-0.006	0.01	0.001
2	(0.003)***	(0.003)	(0.003)**	(0.002)***	(0.002)
D) Year	0.004	-0.003	-0.005	0.005	-0.0003
3	(0.002)	(0.003)	(0.003)**	(0.001)***	(0.001)
E) Year	0.004	0.0003	0.0008	0.004	-0.001
4	(0.002)*	(0.002)	(0.003)	(0.001)***	(0.001)
F) Years	0.45	0.008	0.01	0.51	0.13
0-4	(0.007)***	(0.002)***	(0.003)***	(0.02)***	(0.02)***
G) Years	0.03	0.0006	-0.0002	0.04	0.005
1-4	(0.004)***	(0.002)	(0.002)	(0.004)***	(0.003)*

Notes: 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, day care/day camp, educational services, and healthcare/medical. We classify these as belonging to one of the following cluster of NAICS codes: 61, 62, 71, and 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, non-profit, 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 percent and 46.56 percent, respectively. In Years 0-4 (considering each year as a separate observation), the probability of employment in the for-profit, non-profit, and government sectors is 48.94, 7.63, and 13.56 percent, respectively. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

Appendix Table 10: *Effect of SYEP provider industry mix on earnings outcomes. The table shows the results of OLS regressions in which the dependent variable is earnings (where the particular type of earnings in question is shown in the column heading). The independent variables are: 1) a variable formed by interacting a dummy for winning the SYEP lottery with the percent of the provider that is in Industry Cluster 1; 2) a dummy for winning the SYEP lottery; and 3) dummies for each provider-lottery combination. Panel A shows coefficients on the variable formed by interacting a dummy for winning the SYEP lottery with the percent of the provider that is in Industry Cluster 1. Panel B shows coefficients and standard errors on the dummy for winning the SYEP lottery.*

	(1) Total Earnings	(2) Total Earnings in Cluster 1	(3) Total Earnings in Cluster 2	(4) Total NYC gov't earnings	(5) Total non-NYC gov't earnings
Panel A: interaction term					
A) Year 0	-407.30	905.01	-1325.21	118.94	-526.17
	(219.90)*	(98.65)***	(172.97)***	(124.66)	(201.77)***
B) Year 1	-473.15	-40.71	-439.82	-43.44	-429.73
	(304.07)	(105.07)	(245.44)*	(48.50)	(288.15)
C) Year 2	-483.10	-108.85	-378.96	4.84	-487.94
	(306.31)	(99.45)	(271.40)	(34.73)	(293.13)
D) Year 3	-622.77	-28.06	-604.60	-17.06	-605.57
	(240.40)***	(158.76)	(232.93)***	(24.33)	(238.78)***
E) Year 4	-643.19	61.95	-688.00	-13.72	-629.47
	(296.82)**	(177.52)	(230.35)***	(11.53)	(298.94)**
F) Years	-2629.51	789.34	-3436.59	49.56	-2678.88
0-4	(1086.95)**	(472.85)	(937.57)***	(139.15)	(1106.98)**
G) Years	-2222.21	-115.66	-2111.38	-69.38	-2152.71
1-4	(961.25)**	(447.10)	(811.91)**	(91.13)	(945.14)**
Panel B: main effect					
A) Year 0	1011.71	-126.13	1150.93	680.23	331.57
	(202.92)***	(90.74)	(157.82)***	(115.26)***	(186.71)*
B) Year 1	365.40	60.33	310.13	76.14	289.28
	(281.69)	(96.18)	(225.75)	(45.22)*	(267.83)
C) Year 2	376.07	112.30	267.88	12.49	363.58
	(282.07)	(88.53)	(250.90)	(32.78)	(269.62)
D) Year 3	491.07	19.55	481.37	21.57	469.37
	(219.59)**	(143.80)	(218.62)**	(22.37)	(217.80)**
E) Year 4	566.75	-73.60	624.16	15.90	550.86
	(269.55)**	(160.16)	(212.78)***	(10.77)	(271.33)**
F) Years	2811.01	-7.55	2834.46	806.32	2004.66
0-4	(1005.44)***	(426.76)	(875.69)***	(131.53)***	(1025.63)*
G) Years	1799.30	118.59	1683.53	126.09	1673.09
1-4	(888.05)**	(401.58)	(760.78)**	(85.45)	(873.25)*

Notes: See notes to Appendix Table 9. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

Appendix Table 11: *Effect of SYEP participation on job transitions. The table shows coefficients and standard errors on a dummy for participating in SYEP, from a two-stage least squares regression. The instrument for participating in SYEP is whether an individual won the SYEP lottery. The dependent variable is the fraction of employers that an individual worked at in Year -1 that the individual still worked at in a given year.*

	(1) Coefficient (standard error)	(2) Mean of dependent variable
A) Year 0	-0.03	0.47
	(0.008)***	
B) Year 1	-0.01	0.21
	(0.007)*	
C) Year 2	-0.004	0.13
	(0.005)	
D) Year 3	-0.003	0.09
	(0.004)	
E) Year 4	0.004	0.06
	(0.003)	

Notes: The table shows the results of IV regressions in which the dependent variable is the fraction of employers that an individual worked at in Year -1 that the individual still worked at in a given year. The independent variable is a dummy for participating in SYEP, and the instrument is a dummy for winning the SYEP lottery. All regressions control for provider-year dummies. The second column shows the mean of the dependent variable. All regressions have 38,808 observations; the sample size is smaller than the main sample because the sample is limited to individuals who had a non-SYEP job in Year -1. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

Appendix Table 12. *Effect of SYEP participation on earnings and employment outcomes for those 18 years and older. The table shows coefficients and standard errors on the SYEP participation dummy from two-stage least squares regressions of earnings and employment outcomes on SYEP participation. The instrument for whether an individual participated in SYEP is a dummy indicating that an individual won the SYEP lottery. The sample is limited to those 18 years of age or older in the year of SYEP participation.*

	(1) Total Earnings	(2) NYC gov't earnings	(3) Non-NYC gov't earnings	(4) Job dummy	(5) Non-NYC gov't job dummy
A) Year 0	535.44	1039.67	-504.02	0.40	-0.07
	(73.63)***	(15.83)***	(71.65)***	(0.01)***	(0.007)**
B) Year 1	-329.11	45.42	-374.53	0.002	-0.02
	(122.02)**	(5.09)***	(123.34)***	(0.006)	(0.008)**
C) Year 2	-287.46	20.27	-307.72	-0.001	-0.007
	(132.09)**	(4.12)***	(133.17)**	(0.006)	(0.007)
D) Year 3	-311.43	10.10	-321.53	0.00004	-0.005
	(158.26)**	(4.18)***	(159.04)**	(0.006)	(0.006)
E) Year 4	-139.65	8.38	-148.03	0.003	0.001
	(172.48)	(3.23)***	(173.76)	(0.005)	(0.005)
F) Years 0-4	-532.20	1123.83	-1655.82	0.06	-0.006
	(588.31)	(20.28)***	(591.56)	(0.006)***	(0.004)
G) Years 1-4	-1067.64	84.16	-1151.80	0.006	-0.001
	(528.85)**	(12.05)***	(533.68)**	(0.004)*	(0.004)

Notes: The table is identical to Table 2, except that the sample is limited to those 18 years or older by the end of the calendar year of SYEP participation. The sample size is 72,432.

Appendix Table 13. *Effect of SYEP on mortality with controls. The first column shows hazard ratios and standard errors on a dummy for winning the SYEP lottery from a right-censored Cox proportional hazard model of time to mortality. The second column shows a two-stage least squares estimate using a linear probability model. The third column shows coefficients and standard errors from a probit regression. All regressions control for covariates.*

	(1) Cox	(2) IV	(3) Probit
A) Full	0.87	-0.07	-0.05
population	(0.05)**	(0.03)**	(0.02)**
B) WOTC-	0.82	-0.10	-0.07
eligible	(0.16)	(0.10)	(0.07)
C) WOTC-	0.87	-0.07	-0.04
ineligible	(.05)**	(0.03)**	(0.02)**
D) Below-	0.86	-0.09	-0.05
median inc.	(0.09)	(0.06)	(0.04)
E) Above-	0.88	-0.06	-0.05
median inc.	(0.07)	(0.04)	(0.03)
F) Males	0.84	-0.15	-0.06
	(0.06)**	(0.06)**	(0.03)**
G) Females	0.95	-0.01	-0.01
	(0.12)	(0.03)	(0.04)
H) White	1.02	0.004	-0.003
	(0.32)	(0.08)	(0.09)
I) Black	0.92	-0.06	-0.03
	(0.07)	(0.05)	(0.03)
J) Latino	0.73	-0.14	-0.10
	(0.09)**	(0.06)**	(0.04)**
K) Other	0.80	-0.05	-0.07
races	(0.22)	(0.07)	(0.09)
L) Older	0.95	-0.03	-0.01
	(0.08)	(0.05)	(0.03)
M) Younger	0.79	-0.12	-0.08
	(0.07)***	(0.04)***	(0.03)***
N) Work in	1.09	0.04	0.03
Year -1	(0.13)	(0.06)	(0.04)
O) No work	0.79	-0.11	-0.08
in Year -1	(0.06)***	(0.04)***	(0.03)***
P) 2005-6	0.76	-0.16	-0.10
lotteries	(0.06)***	(0.05)***	(0.03)***
Q) 2007-8	0.96	-0.015	-0.01
lotteries	(0.09)	(0.04)	(0.03)

Notes: Appendix Table 13 runs the same specification as Table 7, except that Appendix Table 13 also controls for the covariates listed in Appendix Table 5. See Table 7 for notes and further information on samples.

Appendix Table 14. *Effect of SYEP on mortality by calendar year. The table shows estimates of the effect of SYEP participation on mortality using a two-stage least squares, linear probability model.*

	(1) 2SLS	(2) 2SLS, with controls	(3) Mortality dummy mean (x 100)
A) 2005	0.008	0.008	0.01
	(0.009)	(0.009)	
B) 2006	-0.004	0.004	0.02
	(0.01)	(0.01)	
C) 2007	-0.0007	-0.0005	0.03
	(0.01)	(0.01)	
D) 2008	-0.02	-0.02	0.06
	(0.01)*	(0.01)*	
E) 2009	-0.004	-0.004	0.10
	(0.02)	(0.02)	
F) 2010	-0.01	-0.01	0.16
	(0.02)	(0.02)	
G) 2011	-0.03	-0.03	0.21
	(0.03)	(0.03)	
H) 2012	-0.04	-0.04	0.27
	(0.02)*	(0.02)*	
I) 2013	-0.06	-0.06	0.34
	(0.03)**	(0.03)**	
J) 2014	-0.08	-0.07	0.38
	(0.03)**	(0.03)**	

Notes: Each row shows the results for a different calendar year. We show the effect of SYEP on a dummy for whether an applicant died *by* a given year; thus, the effect *in* a given year can be calculated as the difference between the coefficient for that year and the previous year. Column 1 shows the results of our two-stage least squares specification (1)-(2). Column 2 shows the results of this specification when we add the controls listed in Appendix Table 5. Column 3 shows the mean of the dependent variable (i.e. the dummy measuring the probability of mortality by each year, relative to year of SYEP participation). So that readers can more easily interpret the results, we have multiplied the mortality dummy by 100. The results are comparable with Cox or probit models; we show a two-stage least squares (linear probability) model here in order to show results that are comparable to the IV results elsewhere in the paper. We use data through October 2014. See Table 7 for other notes and information on samples. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

Appendix Table 15. *Effect of SYEP on mortality by year since lottery. The table shows estimates of the effect of SYEP participation on mortality using a two-stage least squares, linear probability model.*

	(1) 2SLS	(2) 2SLS, with controls	(3) Mortality dummy mean (x 100)
A) Year 0	0.002	0.002	0.01
	(0.005)	(0.005)	
B) Year 1	-0.0001	0.0001	0.04
	(0.01)	(0.01)	
C) Year 2	-0.004	-0.004	0.08
	(0.02)	(0.02)	
D) Year 3	-0.01	-0.01	0.14
	(0.02)	(0.02)	
E) Year 4	-0.01	-0.01	0.19
	(0.02)	(0.02)	
F) Year 5	-0.03	-0.02	0.25
	(0.02)	(0.02)	
G) Year 6	-0.03	-0.03	0.30
	(0.03)	(0.03)	
H) Year 7	-0.10	-0.10	0.38
	(0.04)***	(0.04)***	
I) Year 8	-0.15	-0.14	0.44
	(0.05)***	(0.05)***	
J) Year 9	-0.22	-0.22	0.55
	(0.09)***	(0.09)***	

Notes: Each row shows the results for a different year relative to the year of SYEP participation. We show the effect of SYEP on a dummy for whether an applicant died *by* a given year; thus, the effect *in* a given year can be calculated as the difference between the coefficient for that year and the previous year. Column 1 shows the results of our two-stage least squares specification (1)-(2). Column 2 shows the results of this specification when we add the controls listed in Appendix Table 5. Column 3 shows the mean of the dependent variable (i.e. the dummy measuring the probability of mortality by each year, relative to year of SYEP participation). So that readers can more easily interpret the results, we have multiplied the mortality dummy by 100. The results are comparable with Cox or probit models; we show a two-stage least squares (linear probability) model here in order to show results that are comparable to the IV results elsewhere in the paper. See Table 7 for other notes and information on samples. Because the data extend until 2014, we observe lotteries from all four years (2005, 2006, 2007, and 2008) only until Year 6, implying that sample sizes are not constant across Years 6 to 9; see Appendix Table 4 for sample sizes in the relevant sets of lotteries. *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level.

Appendix Table 16. *Effect of SYEP on mortality by cause of death. The table shows estimates of the effect of SYEP participation on specific causes of death through 2012 using a two-stage least squares, linear probability model.*

	(1) 2SLS	(2) Percent of deaths by 2012
Panel A: death from any cause comparison between DOH and IRS data		
A) Any cause (DOH)	-0.037	100
	(0.021)*	
B) Any cause (IRS through 2012)	-0.043	100
	(0.024)*	
Panel B: specific causes of death from DOH data		
C) External causes (DOH)	-0.030	70.61
	(0.018)*	
D) Homicide (DOH)	-0.024	50.08
	(0.016)	
E) Non-homicide external causes (DOH)	-0.0055	20.53
	(0.095)	
F) Natural causes (DOH)	-0.0071	29.39
	(0.011)	

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 2SLS 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 in order to show results that are comparable to the IV results elsewhere in the paper. 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.