The Medium-Term Impacts of High-Achieving Charter Schools*

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Abstract

Using survey data from the Promise Academy in the Harlem Children's Zone, collected for the purposes of this study, we estimate the effects of high-performing charter schools on human capital, risky behaviors, and health outcomes. Six years after the random admissions lottery, youth offered admission to the Promise Academy middle school score 0.279 (0.073) standard deviations higher on academic achievement outcomes, 0.067 (0.076) standard deviations higher on an index of academic attainment, and 0.313 (0.091) standard deviations higher on a measure of "on-time" benchmarks. Admitted females are 10.1 percentage points less likely to be pregnant in their teens, and males are 4.4 percentage points less likely to be incarcerated. We find little impact of the Promise Academy on self-reported health. These effects are larger than those expected from test score increases alone, implying that high achieving charter schools alter more than cognitive ability.

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"Education is the best provision for old age." $\sim$ Aristotle

1 Introduction

The typical charter school is no more effective at increasing test scores than the typical traditional public school (Gleason et al. 2010). Yet, an emerging body of research using admissions lotteries suggests that high-performing charter schools can significantly increase the achievement of poor urban students.[1] Students attending over-subscribed Boston-area charter schools score approximately 0.4 standard deviations (hereafter σ) higher per year in math and 0.2σ higher per year in reading (Abdulkadiroglu et al. 2011). Promise Academy students in the Harlem Children's Zone (HCZ) score 0.229σ higher per year in math and 0.047σ higher per year in reading (Dobbie and Fryer 2011). The Knowledge is Power Program (KIPP) schools – America's largest network of charter schools – and the SEED urban boarding school in Washington D.C. experience similar test score gains (Angrist et al. 2010, Tuttle et al. 2010, Curto and Fryer 2014).

An important open question is whether these increases in student achievement translate into comparable gains on medium-term outcomes such as high school graduation, college enrollment, drug-use, teen pregnancy, or incarceration. Charter advocates argue that high-performing charter schools are effective at implementing educational "best-practices" – frequent teacher feedback, data-driven instruction, an extended school day and year, and a relentless focus on achievement – which develop basic skills that lead to both gains on short-run state test scores and longer-term non-tested measures (Carter 2000, Thernstrom and Thernstrom 2004, Whitman 2008).[2] Conversely, critics argue that high-performing charter schools increase test scores through intense test prep (Haladyna, Nolen, and Hass 1991, Haladyna 2006), a paternalistic environment (Whitman 2008), strategic resource allocation, or blatant cheating, without instilling long-term or general knowledge in their students.

In this paper, we use data from the Promise Academy in the HCZ to test a "proof of concept"

[1] These findings are closely related to earlier work by Neal (1997), who finds that there are only small gains of Catholic school attendance for urban whites and non-urban students, but large gains for urban minorities. The larger benefits of Catholic school attendance for urban minorities is likely due to the poor public schools otherwise available to them. Grogger and Neal (2000) find similar results for educational attainment using alternative data sources.

[2] There is also evidence that students assigned to high test score value-add teachers are more likely to attend college, earn higher salaries as adults, and are less likely to become pregnant as teenagers (Chetty, Friedman, and Rockoff 2011). Additionally, attending a high-quality public school can reduce crime and increase college enrollment even when there is little impact on state test scores (Cullen, Jacob, and Levitt 2006, Deming 2011, Deming et al. 2014), perhaps due to the development of non-tested forms of intelligence or changes in social networks (Heckman and Rubenstein 2001, Heckman et al. 2006, Segal 2008, Whitman 2008, Chetty et al. 2011).

– whether the best practices used by high-performing charter schools can impact medium-term outcomes. Like many other high-performing charters, the Promise Academy largely adheres to the five tenets of effective charter schools identified by Dobbie and Fryer (2013). The school has an extended school day and year, emphasizes the recruitment and retention of high-quality teachers, uses extensive data-driven monitoring to track student progress and assign students to small group-tutoring sessions based on these data, and makes a concerted effort to change the culture of achievement (Dobbie and Fryer 2013). Appendix Table 1 provides evidence that suggests the Promise Academy charter school is emblematic of other successful charter schools, not an outlier.

Our identification strategy exploits the fact that the Promise Academy is required to select students by lottery when the number of applicants exceeds the number of available slots for admission. The treatment group is composed of youth who are lottery winners and the control group consists of youth who are lottery losers. This empirical strategy allows us to provide a set of causal estimates of the effects of the Promise Academy on a number of medium-term outcomes.

Outcomes for our analysis are measured using survey data collected from youth entered in the 2005 and 2006 Promise Academy sixth grade admissions lotteries for the purposes of this study. The survey included questions about educational achievement and attainment, risky behaviors, and health outcomes. We also administered the Woodcock-Johnson math and reading tests as an alternative measure of cognitive ability, and included questions on a number of potential mechanisms such as non-cognitive skills, social networks, risk aversion, and discount rates. We surveyed 407 out of 570 lottery entrants, a high response rate for survey studies on low-income urban youth (Cullen, Jacob, and Levitt 2006, Kling, Liebman, and Katz 2007, Rodriguez-Planas 2012). We augment the survey data with administrative data on high school test-taking from the New York City Department of Education (NYCDOE) and college enrollment data from the National Student Clearinghouse (NSC).

We find that the Promise Academy increases the quality, but not necessarily the quantity, of schooling. Six years after the admissions lottery, lottery winners outscore lottery losers by 0.281σ (0.083) higher on the no-stakes Woodcock-Johnson math exam, and by 0.115σ (0.083) on the Woodcock-Johnson reading exam. The latter is not statistically significant. On New York City's high school Regents exams, designed to measure mastery in core subjects, lottery winners pass approximately one additional exam, score 0.293σ (0.090) higher on exams taken by the majority of the sample, and are more than twice as likely to take and pass more advanced exams such as chemistry and geometry. Lottery winners are also 13.3 (4.8) percentage points more likely to

graduate on-time, 17.0 (5.1) percentage points more likely to enroll in college immediately after high school, and 8.9 (4.1) percentage points more likely to enroll in a four-year college, a 35 percent increase from the control mean.

Yet, youth who lose the lottery may eventually "catch up" in terms of educational attainment. While they are significantly more likely to graduate from high school in four years and enroll in college immediately thereafter, lottery winners are statistically as likely to graduate high school in six years (0.037 (0.045)) or to eventually enroll in college (0.055 (0.043)) – though due to lack of power, the 95% confidence interval contains estimates of modest size. Lottery winners and losers have almost identical numbers of total semesters enrolled in college (0.161 (0.123)). Combining our human capital variables into three index measures, we find that lottery winners increase their academic achievement by 0.279σ (0.073), increase their educational attainment by by 0.067σ (0.076), and are by 0.313σ (0.091) more likely to reach "on-time" benchmarks.

We also investigate risky behaviors and self-reported health. Female lottery winners are 10.1 (4.7) percentage points less likely to report being pregnant during their teenage years, a 59 percent drop from the control mean of 17 percent. Male lottery winners are 4.4 (1.7) percentage points less likely to be incarcerated, essentially a 100 percent drop from the control mean. Students who win the lottery to attend the Promise Academy report similar drug and alcohol use and criminal behavior as students who lose the lottery. An index measure of risky behavior that combines all four variables is negative and marginally significant (p-value= 0.06). We find no impact of the Promise Academy on asthma, obesity, or mental health. Lottery winners are 0.103σ (0.061) more likely to report healthy eating habits, but there is no impact on an index that combines all four health related variables.

We complement our main analysis with two robustness checks. First, we consider the extent to which differential sample attrition threatens our estimates. Lottery winners were 11.8 percentage points more likely to respond to our survey. If lottery losers who did not respond to the survey differ in some important way, this could invalidate our empirical design by creating unobserved differences between the treatment and control groups. To investigate this possibility, we: (a) compare treatment effects on the subsample of students who completed our survey and the larger set of students for which we have administrative data; (b) calculate Lee (2009) bounds; and (c) impute outcomes for youth who did not respond to the survey and estimate median regressions (similar to the classic work in labor market dropouts, see Brown 1984). In 36 out of 46 cases, are qualitative results are unchanged.

In our second set of robustness checks, we account for multiple-hypothesis testing by calculating two sets of p-values with algorithms that account for the probability of making one or more false discoveries when performing multiple hypothesis tests (Romano and Wolf 2005, Romano, Shaikh and Wolf 2008). The most conservative bounding procedures reduce some individual effects to statistical insignificance, but our main index findings remain essentially unchanged.

We conclude with a more speculative discussion on the potential mechanisms underlying our results. First, we investigate the empirical importance of the HCZ neighborhood programs and the Promise Academy school policies by separately estimating the effects on youth who are more or less likely to receive neighborhood benefits based on their home address. Consistent with Dobbie and Fryer (2011), we find no evidence that the neighborhood programs drive our results. Second, we consider the extent to which changes in test scores might explain the impact of the Promise Academy on non-test score outcomes. Using the cross-sectional relationship between test scores and non-test score outcomes reported by Chetty, Friedman, and Rockoff (2011), we find that only a portion of our estimated effects (roughly 2 percent for pregnancy and 15 percent for college enrollment) can be explained by the test score change. Third, we estimate the impact of the Promise Academy on a number of other possible mechanisms. We find little impact on survey-based measures of non-cognitive skills, social networks, or discount rates.

Our analysis has three important caveats. First, our primary source of data is from only one New York City charter school, which could differ from other high-performing schools in important ways that limit our ability to generalize the results. The inputs and impacts of the Promise Academy are similar to other high-performing charter schools and turnaround efforts that use congruent practices and have yielded similar results on state test scores (Angrist et al. 2010, Tuttle et al. 2010, Abdulkadiroglu et al. 2011, Abdulkadiroglu et al. 2014, Dobbie and Fryer 2011, Fryer 2014). Moreover, our results are strikingly consistent with research on the effectiveness of Catholic schools in increasing student attainment, achievement, college enrollment, and wages – especially among urban youth (Neal 1997, Grogger and Neal 2000). Strategically, we chose to obtain a higher response rate on a detailed face-to-face survey with lottery entrants from one school, as opposed to a lower rate with lottery entrants from multiple schools using online or other methods, in order to maximize the internal validity of our study. The cost of this face-to-face approach – roughly $2,150 per observation – necessitated the focus on a single school.[3]

[3]Interviewing a random subsample of lottery entrants from multiple schools proved to be infeasible, as there are not enough charter schools with a large enough alumni sample and binding admissions lotteries for a study with multiple high-performing schools in a single city. The additional cost of interviewing subjects in multiple cities would

Second, the survey respondents may not have truthfully answered our questions. In particular, it is plausible that Promise Academy students were directly or indirectly pressured to overstate the impact of the school. Arguing against this, results using administrative outcomes are even larger than the survey results.

Third, our analysis is necessarily limited to various medium-term outcomes. Longer-term outcomes, such as college graduation, earnings, and mortality, are not a part of our analysis due to the age of the lottery entrants.

The remainder of the paper is structured as follows. Section 2 provides a brief overview of the Harlem Children's Zone. Section 3 describes the data collected for this paper and our lottery-based research design. Section 4 estimates the impact of the Promise Academy on human capital, risky behaviors, and health. Section 5 discusses potential mechanisms. Section 6 concludes. There are four Web Appendices. Web Appendix A presents additional analyses to supplement the results in the text. Web Appendix B is a data appendix that details our sample and variable construction. Web Appendix C details the tracking and outreach efforts used to contact lottery entrants. Web Appendix D includes the full survey instrument.

2 Harlem Children's Zone

The Harlem Children's Zone consists of over 20 neighborhood and school programs meant to address the myriad problems that children from low income families face – housing, schools, crime, asthma, nutrition – through a "conveyor belt" of services from birth to college. The approach is based on the assumption that one must improve both communities and schools to have a long-term impact on disadvantaged youth. Starting with a 24-block area in central Harlem, the Zone expanded to a 64-block area in 2004 and a 97-block area in 2007.

Neighborhood Programs

The HCZ neighborhood programs serve as broad investments in community development. These programs include early childhood programs, K-12 tutoring, after-school programs, a college success office, family programs, health programs, a foster-care prevention program, a tax assistance program, and parenting interventions. Consistent with Wilson's (1987) theory of non-linear neighborhood effects and cycles of poverty, HCZ's vision is to create a "tipping point" in the neighborhood so that children are surrounded by an enriching environment of college-oriented peers and support-

have forced a much smaller survey population.

ive adults. HCZ neighborhood programs are available to anyone living near HCZ and reportedly serve more than 8,000 youth and 5,000 adults each year.

School Programs

The Promise Academy largely adheres to the five correlates of effective schools identified by Dobbie and Fryer (2013). The Promise Academy has an extended school day and year with coordinated after-school tutoring and additional classes on Saturdays for children who need remediation in mathematics and English Language Arts skills. Promise Academy middle schoolers spent 1,785 hours in school during the 2010-2011 school-year, 46.1 percent more time than the typical New York City public school student and 11.8 percent more than the typical student in a high-performing New York City charter school (Dobbie and Fryer 2013). The Promise Academy also emphasizes the recruitment and retention of high-quality teachers and uses measures of test score value-added to incentivize and evaluate current teachers. The Promise Academy had high teacher turnover during the first three years of operation, with 48 percent of teachers not returning for the 2005-2006 school year, 32 percent not returning before 2006-2007, and 14 percent before 2007-2008. The Promise Academy also uses extensive data-driven monitoring to track student progress and differentiate instruction, with students who have not met the required benchmarks receiving small-group tutoring. Like other "No Excuses" charters, the Promise Academy makes a concerted effort to change the culture of achievement, stressing the importance of hard work, "grit," and persistence in achieving success. It is assumed that every student will enroll in college, with the goal of establishing college attendance as the default option.[4]

3 Data and Research Design

3.1 Data and Summary Statistics

We merge information from lottery files at the Harlem Children's Zone, survey data collected from lottery participants for the purposes of this study, administrative records on student demographics and outcomes from the New York City Department of Education (NYCDOE), and information on

[4]There are at least two potentially important differences between the Promise Academy and the typical high-performing New York charter school. First, the Promise Academy does not require parents or students to sign a behavioral contract, resulting in students that are more similar to the surrounding neighborhood than other charter schools. HCZ argues that only the most motivated and trusting parents are willing to sign even a non-binding contract. Second, Promise Academy students are exposed to a wide range of wrap-around services that are not available at most charter schools. The schools provide services such as free medical and dental services, student incentives for achievement, nutritious cafeteria meals, and parental engagement and support.

college enrollment from the National Student Clearinghouse (NSC).

Survey Data

We conducted in-person interviews with youth who entered the 2005 and 2006 sixth grade admissions lotteries. Students entered in the 2005 sixth grade lottery were finishing or had just finished 12th grade at the time of our survey, while students who entered the 2006 sixth grade lottery were finishing or had just finished 11th grade. Web Appendix B contains additional information on the coding of variables. Web Appendix C describes our tracking and survey administration, and Web Appendix D contains the full survey instrument and protocols used to administer the survey. This section summarizes the most relevant information from our Web Appendices.

From January 2012 through July 2012, we attempted to contact 570 Promise Academy lottery entrants using letters, phone calls, and home visits.[5] Using information from NYCDOE administrative data, internet searches of current addresses, and publicly available address records, we were able to successfully contact 501 of these lottery entrants. For the 69 lottery entrants that we were not able to contact, we exhausted all available leads before closing the sampling period. If we were unable to locate a working telephone, had gone in-person to each address and confirmed the family was not living there, spoke to neighbors or housing personnel about a forwarding address, used a central database search - and could not find a known way of contacting the family; we would consider it exhausted.[6]

Contacted youth were offered a financial incentive between $40 and $200 to participate in the study, with the amount increasing as the survey period progressed. Parents were also offered an additional cash incentive to review the consent form. Of the 501 lottery entrants we contacted, 407 agreed to participate in the study, 61 refused to participate in the study, and 33 were unable

[5]There were 599 unique entrants in the 2005 and 2006 Promise Academy admissions lotteries. We randomly selected 30 lottery losers to test and calibrate the survey instrument, leaving 189 lottery winners and 381 lottery losers in the potential survey sample after a duplicate row was discovered in the pretest sample. Results are identical including the pre-test respondents.

[6]Specifically, we worked through all contact leads provided to us initially for the sample in chronological order (most recent academic year to oldest). This included multiple telephone and address records from the NYCDOE for the listed parent/guardian. If we spoke with someone who said we had the wrong telephone number, we moved on to other contact information from the sample file. However, if the telephone line was not in service we would contact it periodically, as phone contracts came in and out of activation over time. Similarly, if we found that a household was no longer occupied by the family (confirmed vacant property, or if the current occupants confirmed the family no longer lived there) we spoke with neighbors and housing officials (for example: the building manager) to see if we could find forwarding information for the family. If we were unable to locate a family via telephone or in person at their listed household, we utilized an on-line central data base (Accurint), a purchased service that combines multiple public record data bases to provide known addresses, phone numbers and those of neighbors and relatives who were also called to see if they could provide additional information. Cell phones with intermittent service (including prepaid, disposable cell phones) and families that had moved several times since the initial lottery, posed challenges.

to participate due to distance, language barriers, health, incarceration, or another obstacle. We obtained a final survey response rate of 79.4 percent for lottery winners and 67.6 percent for lottery losers. Section 3.2 examines the differences between lottery winners and lottery losers who respond to our survey, finding no evidence of differential selection into our sample along observable characteristics or administrative outcomes.

The questionnaire, based largely on the comprehensive survey used to evaluate the Moving to Opportunity experiment (Kling, Liebman, and Katz 2007), took approximately 110 minutes to complete. The survey was designed to investigate three main outcomes: (1) human capital, (2) risky behaviors, and (3) health. We also asked about non-cognitive skills, peer networks, and economic preferences in order to assess potential underlying mechanisms.

Human capital is measured through the Woodcock-Johnson Broad Math and Reading tests, which is meant to augment the human capital measures available in the NYCDOE and NSC datasets.[7] The Woodcock-Johnson exams are designed to test general knowledge rather than the subject-specific skills emphasized on New York State tests. The assessments are designed to be appropriate for all grades and ability levels and to have a high degree of internal reliability.[8] The Woodcock-Johnson Broad Math score is composed of Applied Problems, Calculation, and Math Fluency subscores. The Applied Problems section consists of word problems read aloud to youth. The Calculation section tests computation skills ranging from arithmetic to Calculus. The Math Fluency section requires youth to answer as many simple questions as possible in three minutes. The Broad Reading score consists of Letter-Word Identification, Passage Comprehension, and Reading Fluency subscores. The Letter-Word Identification section tests pronunciation of increasingly difficult words. The Passage Comprehension questions require youth to identify a word or phrase that completes a sample sentence. The Reading Fluency section, like the Math section, requires youth to answer as many simple questions as possible in three minutes. Web Appendix B contains additional details on the Woodcock-Johnson and the administration of the tests.

[7]The Woodcock-Johnson Brief Battery that we use in our survey is an updated version of the Woodcock-Johnson Revised Battery administered as a part of the MTO evaluation. Accordingly, there is not perfect alignment between the sub-tests. We followed the advice of Woodcock-Johnson staff and administered the four sub-tests included in the MTO follow-up – Letter-Word Identification, Passage Comprehension, Applied Problems, and Calculation – in addition to the Math Fluency and Reading Fluency sections. Following Kling, Liebman, and Katz 2007, we omit the Writing sections to reduce the length of the survey. Treatment effects for each individual sub-test can be found in Web Appendix Table 1.

[8]Sanbonmatsu et al. (2006) analyze test results in the Panel Study of Income Dynamics Child Development Supplement and find that the internal reliability of the test is strong for a population similar to ours, with scores for eight to seventeen year-old black students showing a correlation between 0.5 and 0.6 with the same test taken five years earlier. In our sample, the correlation between students' Woodcock-Johnson scores and their eighth grade state test scores is approximately 0.6 in both math and reading.

Risky behaviors are measured through a series of questions on pregnancy, controlled substance use, and crime. For pregnancy, we ask female youth if they have ever been pregnant, even if no child was born. In our sample, 14.6 percent of females have been pregnant at some point. We measure criminal behavior using an indicator for being in jail or prison when we contacted the lottery entrant. We measure incarceration for all 501 individuals that we successfully contacted, regardless of whether they completed the survey. As a result, we have a higher effective response rate for our incarceration measure (87.8 percent) than for our other survey based measures.[9] We also constructed an index based on youth's self-reported criminal behaviors, such as theft, destruction of property, fighting, or carrying a gun. The reported incidence of these behaviors is relatively low. Twenty-two percent of control youth report having ever been in a serious fight, and 14.1 percent report having stolen an item worth less than $50. Rates of all other criminal behaviors we measure are less than ten percent. It is likely that youth in our sample also under-report participation in risky behaviors, and our estimates using these self-reported measures should be interpreted with this caveat in mind.[10] To measure drug and alcohol use, we construct a summary index based on whether a youth reports that she has consumed alcohol in the last 30 days, smoked marijuana in the last 30 days, or used hard drugs within the past year. 29.2 percent of control youth report consuming alcohol in the last 30 days, 22.0 percent smoking marijuana, and only one control youth reported using hard drugs.

We measure mental health using the K6 anxiety scale used in Kling, Liebman, and Katz (2007), standardized to have a mean of zero and standard deviation of one in the control group. Physical health is measured using an index based on indicators for self-reported poor health, having had an asthma attack in the past year, having a Body Mass Index (BMI) above the 95th percentile for the respondent's age and gender, and having reported chronic health problems. To investigate health risk factors, we ask about the number of times in the past week the youth has consumed foods such as fruits and vegetables, soft drinks, savory snacks, and fast food. We use these responses to create a nutrition index, reversing the sign on the unhealthy food variables. We also construct a health behavior index from questions about having a physical examination in the past year, the frequency

[9]Criminal records are available for offenders over the age of 18. As much of our sample is under that age, and crimes that were committed before one's 18th birthday will not show up in publicly available records regardless of current age, we rely on survey information as our primary source of incarceration outcomes. Of the three youth that were reported as incarcerated and were over 18 at the time of first contact, we were able to confirm one using common incarceration databases. No other youth in the sample were matched to records in these databases.

[10]There is evidence that youth in the Moving to Opportunity experiment significantly under-report participation in risky behaviors. Approximately one-third of the MTO control group had an arrest record at the time of the survey, but did not report ever having been arrested (Kling, Ludwig, and Katz 2005).

of light exercise, the frequency of vigorous exercise, and having a dental exam in the past year. These measures of health-related behavior are important to the extent that many ailments are not easily detected among teenagers. For instance, while black adults are one and a half times more likely to develop hypertension and diabetes than white adults (Lopes and Port 1995), the rates of these diseases among black and white youth are roughly the same (Liese et al. 2006). However, many risk factors for both hypertension and diabetes, such as childhood obesity and youth dietary patterns, are more prevalent in black youth.

The remainder of the survey explores three potential mechanisms that may explain any impacts of the Promise Academy. First, we explore the importance of non-cognitive skills by assessing self esteem, persistence, and locus of control. Second, we measure differences in peer networks by asking youth how important it is for their friends to study, stay in school, and attending class regularly, in addition to whether their friends use drugs, drink alcohol, smoke cigarettes, steal, fight, and join gangs. Finally, we measure changes in discount rates and risk aversion, both important determinants of decision-making in economic models.

Administrative Data

We augment our in-person survey data with administrative data from the Harlem Children's Zone, NYCDOE, and NSC. The data from the Harlem Children's Zone consist of lottery files from the 2005 and 2006 sixth grade lotteries. To ensure that all youth in the lottery have an equal chance of being admitted to the Promise Academy, we drop entrants with a sibling that received a winning lottery number in a previous year, as these entrants are automatically admitted. Entrants with a sibling entered in a Promise Academy in the same year are included in our analysis, although we control for the fact that these entrants have a higher probability of admission due to potential admission through sibling preference. Results are identical dropping all siblings. When youth enter more than one lottery, we only include them in the first lottery cohort. A typical student's data include her name, birth date, parents' or guardians' names, home address, and lottery outcome. Following Dobbie and Fryer (2011), we define lottery winners as youth who receive a winning lottery number or whose waitlist number was below the average highest number called across both years. These lottery winners are admitted to both the Promise Academy middle and high schools simultaneously.

Table 2 presents enrollment outcomes for our lottery sample. Sixty-three percent of lottery winners attend the Promise Academy for at least one year, with 42 percent of lottery winners

attending the Promise Academy high school for at least one year. The typical lottery winner attends the Promise Academy schools for 3.360 years, 3.084 more years than the typical lottery loser.

The NYCDOE data contain student-level administrative data on approximately 1.1 million students across the five boroughs of the NYC metropolitan area. The data include information on student race, gender, free and reduced-price lunch eligibility, behavior, attendance, matriculation for all students, state math and English Language Arts (ELA) test scores for students in grades three through eight, and Regents test scores for high school students. The data also include a student's first and last name, birth date, and address. We have complete NYCDOE data spanning the 2003-2004 to 2013-2014 school years, with test score and basic demographic data available from the 1999-2000 school year onwards. Thus, we observe all high school outcomes for both lottery year cohorts.

The state math and ELA tests are high-stakes exams conducted every year for third through eighth grade students. All public school students, including those attending charters, are required to take the math and ELA tests unless they are medically excused or have a severe disability. We normalize test scores to have a mean of zero and a standard deviation of one for each grade, subject, and year across the entire New York City sample.

Regents Exams are statewide subject examinations required for high school graduation. In order to graduate, students must score 65 or higher on Global History and Geography, U.S. History and Government, Comprehensive English, at least one math exam, and at least one science exam. To receive Advanced Designation, students must pass all of exams required for graduation, along with two additional math exams and a second science exam. We create two measures to capture general achievement on Regents. Our first measure is the total number of Regent exams passed. The second is the average score on the Living Environment, Global History, and Integrated Algebra exams standardized to have a mean of zero and standard deviation of one in the entire New York City sample. These are the only three Regents exams taken by over 70 percent of both lottery winners and lottery losers. If youth are missing one or two of these exams, we calculate the average using just the non-missing scores. Results are nearly identical dropping these observations or imputing missing values. Web Appendix Table 2 presents estimates on taking each exam, passing each exam, and exam score conditional on taking.

The HCZ data were matched to the New York City administrative data using name and date of birth. We were able to match 95.8 percent of lottery winners to the NYC data (N=189), and

95.1 percent of lottery losers (N=410). Our match rates and attrition are similar to previous work using charter lottery data (e.g. Hoxby and Muraka 2009, Angrist et al. 2010, Angrist et al. 2011, Curto and Fryer 2014, Dobbie and Fryer 2013, Abdulkadiroglu et al. 2011). Additional information on the match rates and attrition for each lottery cohort are available in Table 1, with additional details on the match procedure available in Web Appendix B.

To explore the impact of HCZ attendance on college outcomes, we also match the lottery admissions records to information on college attendance from the National Student Clearinghouse (NSC), a non-profit organization that maintains enrollment information for nearly every college and university in the country. The NSC data contain information on enrollment and degrees granted for each college that a student attends. The Promise Academy lottery data were matched to the NSC database by NSC employees using each student's full name, date of birth, and high school graduation date. Youth who are not matched to the NSC database are assumed to have never enrolled in college, including one (unknown) student whose record was blocked by her school.

From the NSC data, we construct three meaures designed to measure college enrollment and persistence. First, we measure whether a student ever enrolls in college. We also construct a measure of "on-time" enrollment by investigating whether a student is enrolled in college the fall after graduating high school. Finally, we measure the number of semesters a student has enrolled in college. This measure is created by counting the number of fall and spring semesters we observe enrollment information for a student in the NSC data.

Columns 1 through 4 of Table 2 present summary statistics for baseline characteristics for our lottery sample and two comparison populations. We report separate sample means for all NYC students who were enrolled in 5th grade in the 2004-2005 or 2005-2006 school year, all such students who live in the HCZ neighborhood, lottery winners, and lottery losers. Eighty-four and a half percent of lottery entrants are black, compared to 32.8 percent of NYC fifth graders and 63.7 percent of neighborhood fifth graders. Promise Academy lottery entrants under-perform the City average on math and ELA tests by roughly a quarter of a standard deviation. Lottery entrants score marginally higher than their neighbors, but the difference is not significant. Taken together, these summary statistics suggest that the Promise Academy serves a disproportionately black population whose academic performance is similar to students in their geographic area.

3.2 Research Design

We estimate two empirical models – Intent-To-Treat (ITT) effects and Local Average Treatment Effects (LATEs) – which provide a set of causal estimates of the impact of attending a high-performing charter school on medium-term outcomes. The ITT estimates measure the causal effect of winning the Promise Academy admissions lottery by comparing the average outcomes of youth who 'won' the lottery to the average outcomes of youth who 'lost' the lottery:

$$outcome_i = \mu + \gamma X_i + \pi Z_i + \sum_j \nu_j Lottery_{ij} + \sum_j \phi_j Lottery_{ij} * 1(sibling_i) + \eta_i \qquad (1)$$

where Z_i is an indicator for winning an admissions lottery, and X_i includes controls for gender, race, 5th grade special education status, eligibility for free or reduced-price lunch, receipt of Limited English Proficiency (LEP) services, and a quadratic in two prior years of math and ELA test scores. $Lottery_{ij}$ is an indicator for entering the middle school lottery in year j, and $1(sibling_i)$ indicates whether student i had a sibling enter a Promise Academy lottery in the same year. Equation (**??**) identifies the impact of being *offered* a chance to attend the Promise Academy, π, where the lottery losers form the control group corresponding to the counterfactual state that would have occurred for youth in the treatment group if they had not been offered a spot in the charter school.

Under several assumptions (that the lottery outcomes are random, that winning the lottery has a monotonic impact on Promise Academy enrollment, and that being selected affects outcomes through its effect on Promise Academy enrollment), we can also estimate the causal impact of *attending* the Promise Academy. This parameter, commonly known as the Local Average Treatment Effect (LATE), measures the average effect of attending the Promise Academy on youth who attend the school as a result of winning the admissions lottery (Angrist and Imbens 1994). The LATE parameter can be estimated through a two-stage least squares regression of student achievement on Promise Academy attendance (PA_i), using the lottery offer Z_i as an instrumental variable for the first-stage regression. The second-stage equations for the two-stage least squares estimates therefore take the form:

$$outcome_i = \mu + \gamma X_i + \pi PA_i + \sum_j \nu_j Lottery_{ij} + \sum_j \phi_j Lottery_{ij} * 1(sibling_i) + \eta_i \qquad (2)$$

and the first stage equation is:

$$PA_i = \alpha + \delta X_i + \lambda Z_i + \sum_j \theta_j Lottery_{ij} + \sum_j \iota_j Lottery_{ij} * 1(sibling_i) + \kappa_i \quad (3)$$

where λ measures the impact of the lottery offer on the probability of attending the Promise Academy. We estimate equations (**??**) and (**??**) using an indicator for having ever attended the Promise Academy and a continuous variable measuring the number of years at the Promise Academy.

There is a powerful first-stage effect of winning the lottery on Promise Academy enrollment on both the extensive and intensive margins. Appendix Table 2 presents formal first stage estimates. Columns 1 to 3 report results for having ever attended the Promise Academy and columns 4 to 6 present results for the number of years attended. All regressions control for lottery cohort and sibling fixed effects. Lottery winners are 55.7 (3.8) percentage points more likely to attend the Promise Academy compared to lottery losers. Lottery winners also attend the Promise Academy for 3.033 (0.240) more years compared to lottery losers. Baseline math and reading scores, gender, free lunch status, and LEP status are not significantly related to Promise Academy attendance. Black students attend for 0.539 (0.209) more years compared to non-black students, and special education students attend for 1.012 (0.332) fewer years compared to non-special education students.

Appendix Figure 1 plots the distribution of the number of years of Promise Academy attendance by lottery cohort and lottery status. Only 6.6 percent of lottery losers attend the Promise Academy for at least one year, compared to 63 percent of lottery winners. Thirty-eight percent of lottery winners attend the Promise Academy for the maximum number of years possible, compared to only 2.4 percent of lottery losers.

One potential threat to a causal interpretation of our estimates is that the Promise Academy admissions offer is not random ($E[\eta_i|Z_i] \neq 0$). We evaluate this possibility in column 5 of Table 2 by examining observed differences between lottery winners and lottery losers in the NYCDOE data. Lottery winners are 9.0 percentage points less likely to be female in the NYC sample. There are no other statistically significant differences between lottery winners and lottery losers, and a joint F-test that all coefficients are equal to zero has a p-value of 0.648.

A second threat to our interpretation of the estimates is that lottery entrants may have selectively responded to our survey. In particular, one may be concerned that lottery winners were 11.8 percentage points more likely to respond (see Table 1). If lottery losers who did not respond to our

survey differ in some important way, this could invalidate our empirical design by creating unobserved differences between the treatment and control groups. Following our results from column 5, column 8 of Table 2 reports the difference between lottery winners and lottery losers in our survey sample. Lottery winners in the survey sample are 11 percentage points less likely to be female. There are no other statistically significant differences between lottery winners and lottery losers in the survey sample, and a joint F-test that all differences are equal to zero has a p-value of 0.388. These results suggest that the lottery offer is also random in our survey sample.

A final concern is that our survey sample may differ from the initial sample of lottery entrants. For example, higher-achieving youth may be more likely to respond to our survey in either the lottery winner and lottery loser groups. In this scenario, our estimates using survey outcomes may not be representative of the entire sample of lottery entrants. We investigate selection into the survey sample in two ways: (1) correlating survey response with baseline characteristics for lottery winners and lottery losers, and (2) correlating survey response with observed administrative outcomes for lottery winners and lottery losers.

Panel A of Table 3 explores selection into our survey sample further by reporting results from a series of regressions of an indicator for survey response on baseline characteristics. The sample is restricted to lottery entrants in the survey pool who we are able to match to the NYCDOE data. All regressions include cohort fixed effects, an indicator for having a sibling in the same lottery, and a sibling-by-cohort interaction. Column 1 reports regression results for the pooled sample of lottery entrants. The coefficients are all small and statistically insignificant, and a joint F-test of all of the listed variables are equal to zero has a p-value of 0.915. These results suggest that observable baseline characteristics are not systematically associated with survey response.

Panel B of Table 3 reports results correlating survey response with administrative outcomes that are available for both respondents and non-respondents. By examining survey response along realized outcomes, we are able to determine whether survey response differs by changes in outcomes not predicted by baseline characteristics. The administrative outcomes available for this test include eighth grade math scores, eighth grade ELA scores, and college enrollment. In the pooled sample, a one σ increase in eighth grade math scores is associated with a 6.2 (2.8) percentage point increase in the probability of response, and college enrollment is associated with a 12.6 (3.9) percentage point increase in survey response.

To test whether this positive selection into the survey sample differs by lottery status, columns 2 and 3 present results for lottery winners and lottery losers separately and column 4 reports the

difference between the two groups. Consistent with the results from Table 2, there is nearly identical selection into the survey sample among lottery winners and lottery losers. None of the individual differences are statistically significant, and a joint F-test on the null that all three differences are equal to zero yields a p-value of 0.672. Thus, while there is positive selection into our survey sample based on realized outcomes, there is no evidence that lottery winners and lottery losers differentially select into the survey sample.[11]

4 Analysis

4.1 Main Outcomes

Below, we provide a series of estimates of the impact of the Promise Academy in the Harlem Children's Zone on human capital outcomes, risky behaviors, and physical and mental health.

A. Human Capital

Dobbie and Fryer (2011) find that Promise Academy students gain 0.229σ in math and 0.047σ in ELA per year on the required state exams. To provide evidence on whether these state test score gains reflect increases in general knowledge and skills, as opposed to test-specific skills, we estimate the impact of the Promise Academy on a number of alternative measures of human capital.

Panel A of Table 4 presents ITT and LATE estimates of the impact of the Promise Academy on various measures of academic achievement. Woodcock-Johnson results include lottery entrants who responded to the survey and complete the indicated Woodcock-Johnson test. Results are statistically identical restricting to respondents who answered every survey question. High school Regents results include lottery entrants who attend a NYC high school for at least one year, while college enrollment results include all lottery entrants. Throughout the paper, note that each regression using NYC administrative data as an outcome includes all available students for that outcome to maximize sample size. Each regression using survey data as an outcome includes

[11] Consistent with the results from Table 2, we also find no differential survey response between lottery winners and lottery losers on observable baseline characteristics. Columns 2 and 3 report results of the same regression estimated separately for lottery winners and lottery losers, and Column 4 reports the difference between lottery winners and lottery losers. Lottery winners eligible for LEP at baseline are 57.7 (25.2) percentage points less likely to respond to the survey compared to lottery losers eligible for LEP, and lottery winners missing a 5th grade ELA score are 68.6 (27.5) percentage points more likely to respond than lottery losers missing an ELA score. There are no other significant differences between lottery winners and lottery losers, however, and a joint F-test of the individual differences yields a p-value of 0.099. In results available upon request, we correlate survey response with predicted outcomes using baseline variables. Consistent with the results from Table 2 and Panel A of Table 3, there are no significant predictors of survey response among lottery winners or lottery losers.

all survey respondents for each outcome that were not part of the pilot survey.[12] Restricting all regressions to have a common sample does not qualitatively change our results.[13] Each regression controls for the demographic variables listed in Table 2, lottery cohort effects, sibling by lottery cohort effects, and a quadratic in 4th and 5th grade math and ELA scores. We report standard errors that are robust to arbitrary heteroskedasticity in all regressions.

The ITT estimates demonstrate that lottery winners score 0.281σ (0.083) higher than lottery losers on the math portion of the Woodcock-Johnson test, and 0.115σ (0.083) higher on the reading portion. The corresponding LATE estimates imply that youth who *attend* the Promise Academy for at least one year due to a winning lottery draw score 0.436σ (0.121) higher in math and 0.179σ (0.123) higher in reading. Attending the Promise Academy has the largest impact on Math Calculation, with Promise Academy students scoring 0.595σ (0.127) higher than they otherwise would have. Promise Academy students also score 0.338σ (0.153) higher in Math Fluency, and 0.321σ (0.139) higher on Letter-Word Identification (see Web Appendix Table 1). The estimated impacts on the other sub-test results are not statistically significant. Results are similar if we estimate LATEs with an instrument for the number of years at the Promise Academy.

Lottery winners also take more New York State Regents exams and score higher on the exams that most students take.[14] Lottery winners pass 1.228 (0.271) more Regents exams than lottery losers, a 32 percent increase from the control mean of 3.819 exams. On the three core exams that over 70 percent of lottery winners and lottery losers take – Living Environment, Global History, and Integrated Algebra – lottery winners score 0.293σ (0.090) higher than lottery losers. The gains are largest in Integrated Algebra, where lottery winners score 0.507σ (0.101) higher (see Web Appendix Table 2). Lottery winners are also 23.4 (4.9) and and 12.5 (4.2) percentage points more likely to take the more advanced Geometry and Chemistry exams, and, conditional on taking these exams, score 0.481σ (0.118) and 0.710σ (0.243) higher.

[12]While we put in significant resources into locating lottery entrants, we were constrained in our ability to ensure that subjects completed all portions of the survey. Because not all survey respondents completed every question, the available sample size varies from 395 for the Woodcock-Johnson Scores to 444 for the Risky Behavior Index. Throughout the paper, we report estimates including every respondent who has valid data for a particular outcome rather than imposing an ad hoc sample restriction to ensure that the sample sizes were consistent. Appendix Tables 3 and 4 report observation numbers for the largest possible sample for each outcome.

[13]This is for all non-incarceration regressions, given the incarceration indicator is only equal to one for people who did not take the survey.

[14]Selection into the Regents exams complicates the interpretation of these estimates. If, for example, the Promise Academy pushes weaker students to take harder Regents exams, then our results are likely to be too conservative. Consistent with this, Web Appendix Table 2 shows that lottery winners are at least as likely to take each exam except Comprehensive English, and are more likely to take and pass advanced subjects like Geometry, Physics, and Chemistry.

We attempt to summarize the impact of the Promise Academy on educational achievement using an index measure that combines all four individual achievement measures. We standardize each individual measure to have a mean of zero and a standard deviation of one in the control group. We then take the (unweighted) average of each standardized z-score measure. We include all youth with at least one non-missing outcome. Using this approach, the impact of winning the admissions lottery on an achievement index measure is 0.279σ (0.073), suggesting a large and precisely estimated impact of the Promise Academy on the *quality* of schooling.

Panel B of Table 4 presents ITT and LATE estimates of the impact of the Promise Academy on various measures of academic attainment. An important milestone of human capital development is high school graduation. The public benefits alone from converting a high school dropout to graduate is more than $250,000.[15] Lottery winners are 13.3 (4.8) percentage points more likely to graduate from high school in four years, a 23 percent increase from the control mean of 57.8 percent. Surprisingly, however, lottery winners are not statistically more likely to graduate in six years. Put differently, HCZ is effective at pushing students to graduate on-time but may not be more effective at getting them to graduate. It is too early to make more definitive conclusions on educational attainment, given our sample is still "attaining" schooling. It is possible has important impacts on college graduation or major choice in subsequent follow-ups. Indeed, using the cross-sectional partial correlations between our attainment outcomes (both on-time and eventual) and college graduation in the National Educational Longitudinal Survey of 1988, we predict a 10 percentage point increase in college graduation, nearly a 50% difference between lottery winners and losers.

Our final measure of human capital is college enrollment. Lottery winners are 17.0 (5.1) percentage points more likely to enroll in college immediately after high school graduation, a 51.7 percent increase from the control mean of 32.9 percent. Similar to the results for high school graduation, however, control students eventually catch up and make the treatment effects on college enrollment insignificant. Lottery winners are 5.5 (4.3) percentage points more likely to ever enroll in college.

[15] Using 2003 and 2004 Current Population Survey (CPS) data and the NBER TAXSIM, Rouse (2006) finds that present value lifetime earnings at age 20 of black male high school dropouts are $292,200 versus $601,800 for high school graduates–this means that the average black male dropout contributes $118,000 in income taxes over his lifetime versus $222,400 for a high school graduate. Accounting for property and sales taxes increases these figures by 5 percent. Overall, each additional black male high school graduate would produce a present value at age 20 of $167,600 in additional tax revenue. Using data from the 2002 Medical Expenditure Panel Survey (MEPS) combined with enrollment costs from the National Health Accounts (NHA), Levin et al. (2007) estimate that over the lifetime, each additional high school graduate would result in savings in public health costs with a net present value of $33,500 at age 20. Using data from the Bureau of Justice Statistics as well as FBI Uniform Crime Rate data, Belfield (2006) estimates that converting a black male high school dropout to a graduate is associated with criminal justice cost savings of $55,500. Taken together, this implies a public benefit of approximately $256,700 per new high school graduate.

Though statistically insignificant, the LATE estimate is roughly ten percentage points, a 25 percent increase over the control mean. The number of total semesters enrolled in college between lottery winners and lottery losers is small and statistically insignificant (0.161 (0.123)).

In Appendix Table 5, we demonstrate that lottery winners are also 8.9 (4.1) percentage points more likely to attend a four-year college and 2.8 (3.4) percentage points less likely to attend a two-year college. These results are consistent with the Promise Academy inducing at least some students to enroll in a four-year college instead of a two-year school. Appendix Table 5 also shows that lottery winners are 3.4 (2.9) percentage points more likely to enroll at a college where the average student has SAT scores of 1,000 points or higher (out of 1,600). These results suggest two more summary index measures: educational attainment and "on-time" benchmarks.[16] Our attainment index consists of three variables: high school graduate (after six years), college enrollment (ever enrolled), and number of semesters enrolled. Our "on-time" index is comprised of two variables: whether a student graduated from high school in four years and whether they enrolled in college immediately after graduation. Again, we include all youth with at least one non-missing outcome.

The impact of HCZ on the attainment index is modest in size, but statistically insignificant – evidence that the Promise Academy may not influence the *quantity* of schooling, though, again, students are still in the process of attaining schooling. The impact on the "on-time" index is 0.313σ (0.091).

To explore how much of the college enrollment results can be explained by high school graduation, we follow Neal (1997) and estimate results only for students who did not drop out of high school. Appendix Table 6 presents these results. Conditional on high school graduation, lottery winners are 15.1 (5.7) percentage points more likely to attend any college, a 25.5 percent increase from the control mean of 59.2 percent. Lottery winners are also 20.9 (6.1) percentage points more likely to attend a four-year college and 4.2 (5.6) percent less likely to attend a two-year college after conditioning on high school drop out status. Our results suggest that the college enrollment estimates are not driven by differences in high school graduation, but differences in the probability of enrolling in college conditional on graduating from high school. This is consistent with the fact

[16]We also constructed indices that weight the human capital variables according to their partial correlation with adult wages. Using the 1979 National Longitudinal Survey of Youth, we correlated adolescent test scores (Armed Forces Qualifying Test), college enrollment, and high school graduation with adult wages. Given the results of our partial correlations, we used weights of 40.0 percent, 40.5 percent, and 19.5 percent for adolescent test scores, college enrollment, and high school graduation, respectively, when constructing a weighted human capital index. The impact of winning the admissions lottery on a weighted human capital index is 0.270σ (0.071).

that HCZ puts tremendous emphasis on college enrollment vis-a-vis its college success office.[17]

B. Risky Behaviors

Panel A of Table 5 presents estimates of the impact of the Promise Academy on teen pregnancy, incarceration, self-reported drug and alcohol use, and self-reported criminal behavior. Pregnancy results include all female survey respondents, while self-reported results include all survey respondents who answered the relevant question. Results are statistically identical restricting the sample to respondents who answered every survey question. The incarceration results include the 233 male lottery entrants whom we successfully contacted, regardless of whether or not they completed a survey. We define incarceration as currently being in jail or prison when we contacted the lottery entrant. Following Table 4, each regression controls for the demographic variables listed in Table 2, lottery cohort effects, sibling by lottery cohort effects, and a quadratic in 4th and 5th grade math and ELA test scores. Standard errors have been adjusted to account for arbitrary heteroskedasticity.

Seventeen percent of female lottery losers report having been pregnant at some point. In comparison, 10.0 percent of minority women and 10.4 percent of low-income women in New York City schools give birth in their teens (Chetty, Friedman, and Rockoff 2011). Female lottery winners are 10.1 (4.7) percentage points less likely to report that they have ever been pregnant, a 59 percent reduction from the control mean.[18]

Four percent of male lottery losers were incarcerated during our sample period, compared to none of the male lottery winners. One female lottery loser and one female lottery winner were also incarcerated during our sample period.[19] In our ITT specification, male lottery winners are 4.4 (1.7) percentage points less likely to be incarcerated, essentially a one hundred percent decrease. To put this estimate in context, Deming (2011) finds being offered a spot at a student's first choice public school in Charlotte-Mecklenburg decreases the probability of spending at least 90 days in jail over the next five years by 10.7 percentage points for males in the highest risk quintile, an 81.1 percent drop.

[17] In our survey, HCZ students report having significantly more help writing their college essays and are given more time to visit college campuses.

[18] We also asked survey respondents about various self-reported sexual habits which might explain the effect on pregnancy. As the results in Panel D of Appendix Table 7 show, there are no detectable differences in these behaviors. Promise Academy youth are equally likely to have had sex, and are about as likely to have used a condom or another form of contraception during their most recent sexual experience, though we are under-powered to detect modest differences.

[19] In the 2011 Census of Juveniles in Residential Placement we calculate the incarceration rate as 0.72% for black males and 0.18% for black females in New York state.

In stark contrast, we find little evidence that the Promise Academy impacts self-reported drug and alcohol use or self-reported criminal behavior. Lottery winners are 0.020σ (0.067) less likely to report using drugs and alcohol, and 0.010σ (0.066) less likely to report criminal behavior, with neither estimate statistically significant. The results are similar if we estimate effects for males and females separately. There are at least three possible explanations for the positive impact on administrative outcomes and no effect on self-reported outcomes. First, our self-reported measures are likely biased downwards due to the fact that incarcerated youth are unable to respond to our survey. Second, there may be underreporting of risky behavior that masks a true treatment effect. For instance, youth in the MTO follow-up study under report criminal behavior by 15 to 20 percent, with treated youth only slightly less likely to self-report crime (Kling, Ludwig, and Katz 2005). Finally, it is possible that criminal behaviors are the same, but that lottery winners are less likely to be caught.

Following our human capital results in Table 4, we summarize the impact of the Promise Academy on risky behavior using an index measure that combines all four individual measures. Lottery winners are 0.135σ (0.072) less likely to engage in risky behavior according to our index measure. The result is driven by the incarceration and pregnancy results, as there is relatively little variation across students in the self-reported measures.

C. Health

Panel B of Table 5 presents estimates of the impact of the Promise Academy on healthy eating, mental health, physical health, and an index of surveyed health behaviors. Each regression includes all survey respondents who answered the indicated question, and follows the same specification as Panel A.

Lottery winners are 0.103σ (0.061) more likely to report healthy eating habits, yet these habits do not appear to have translated into improvements on any other health outcomes. Lottery winners self-report physical health that is 0.041σ (0.063) lower, with no discernible effects on asthma attacks, obesity, or self-reported health. Lottery winners also report mental health that is 0.032σ (0.104) lower than lottery losers. Our summary index of both physical and mental health is 0.031σ (0.057) higher for lottery winners as compared to lottery losers.

4.2 Robustness Checks

In this section, we explore the robustness of our results to two potential threats to validity: (1) differential attrition from the survey sample, and (2) type I errors due to multiple hypothesis-testing.

A. Attrition and Bounding

First, we consider the extent to which sample attrition threatens our estimates by calculating treatment effects on a common set of administrative outcomes for students who we can match to administrative data and the strict subset of students who we located and took our survey. Panels A1, B, and C of Table 6 presents these results for the administrative outcomes that are available for all lottery entrants. Column 1 presents standard ITT estimates using the full sample of lottery entrants as reported in column 2 of Table 4. Column 2 restricts the sample to lottery entrants in the survey sample to explore the extent of any attrition bias on these outcomes. If the survey sample yields significantly different effects than the administrative sample, there may be reason for worry. Column 4 of Table 6 reports the p-value on the difference between the coefficients in Columns 1 and 2 for each outcome.

The impact of being offered admission to the Promise Academy is similar in the full and survey samples across all of our administrative outcomes. The effect on the number of Regents exams passed is 0.045 higher in the survey sample, the effect on Regents scores is 0.015σ lower, and the impact on number of college semesters enrolled is 0.125 semesters higher. These results suggest that there is, at worst, small upwards bias in the survey sample, likely due to the fact that the first stage impact of a lottery offer on Promise Academy enrollment is approximately 11 to 15 percent larger in the survey sample (see Table 2).

Column 3 of Panels A1, B, and C reports the Lee (2009) bound for each administrative outcome. Each bound is calculated by dropping the fraction of the highest-achieving lottery winners necessary to equalize the response rate among lottery winners and lottery losers. Specifically, we drop the lottery winners with the highest residuals from our main estimating equation. In this worst case scenario, there is still a statistically significant effect of the Promise Academy on all of the administrative outcomes that were statistically significant in the full sample, with lottery winners scoring 0.279σ (0.073) higher on our achievement index, 0.067σ (0.076) on the attainment index, and 0.281σ (0.091) on the "on-time" index, compared to lottery losers. As expected, the Lee (2009) bounds are less than or equal to the true ITT estimates from column 1.

Panel A2 and Table 7 reports Lee (2009) bounds for our survey outcomes. None of the Lee (2009) bounds remain statistically significant for our survey outcomes, though due to large standard errors we cannot rule out that the bounds and the survey estimates are statistically identical. We are also unable to calculate a bound for incarceration, given there are no incarcerated males in the treatment group.

As a final check on the impact of attrition on our findings, we impute minimum values to equalize the response rate between lottery winners and lottery losers and then estimate median regressions. Our approach here is similar to that used in the labor market dropouts literature (Brown 1984). On all administrative outcomes and all but two survey outcomes, the results are qualitatively unchanged [not shown in tabular form].

B. Adjusting P-values for Multiple Hypotheses Testing

A second concern is that we are detecting false positives due to multiple hypothesis-testing. Appendix Table 8 presents results controlling for the Family-Wise Error Rate, which is defined as the probability of making one or more false discoveries – known as type I errors – when performing multiple hypothesis tests, using a step-down algorithm similar to those described by Romano and Wolf (2005) and Romano, Shaikh and Wolf (2008). For a given family of k-hypothesis tests, the algorithm uses resampling and stepdown methods to estimate the dependence structure of the test statistics and provides corrected p-values.

Appendix Table 8 confirms the robustness of our main findings. The p-values on the achievement and "on-time" indexes are 0.001 and 0.002, respectfully. after adjusting for multiple-hypothesis testing. Conversely, the p-value on the attainment index is 0.607 and the p-value on the risky behavior index rises from 0.062 to 0.175. We also presents results from the more conservative Holm step down method described in Romano, Shaikh and Wolf (2010), which controls the family-wise error rate without taking into account the dependence structure of the test statistics. The Holm method corrected p-values yield almost identical results.

5 Interpretation

5.1 Neighborhoods vs. Schools

In addition to the school investments typical of a high-performing charter school, Promise Academy students are exposed to a network of community services in the Harlem Children's Zone. The

community programs may plausibly impact future outcomes by providing a more supportive out-of-school learning environment.

To fix ideas, consider a model of education production where student achievement is a function of school inputs (s), community inputs (c), and a vector of other inputs such as parental involvement, student motivation, and so on ($\bar{x}$). For simplicity, we assume $f(s, c, \bar{x})$ is C^2 in all its arguments and additively separable. We use this simple apparatus to investigate three pieces of evidence to better understand the empirical importance of the HCZ neighborhood programs and the Promise Academy school investments.

A. Inside v. Outside HCZ

First, following Dobbie and Fryer (2011), we estimate treatment effects separately for youth living within 400 meters of the original 24-block Harlem Children's Zone (inside HCZ), who are more likely to receive neighborhood benefits, and youth living more than 400 meters away (outside HCZ), who are less likely to receive neighborhood benefits. Treatment effects for youth living inside the zone provide estimates of $\frac{\partial f}{\partial s} + \frac{\partial^2 f}{\partial s \partial c}$. Treatment effects for youth living outside the zone provide estimates of $\frac{\partial f}{\partial s}$. If the two estimates are similar, this implies $\frac{\partial^2 f}{\partial s \partial c} \approx 0$.

An important assumption in this approach is that youth who live inside HCZ are significantly more likely to participate in neighborhood programs, relative to youth who live outside the HCZ. To partially test whether address is associated with community program exposure, we collected administrative data from HCZ on participation in the neighborhood programs. The data consists of "sign-in" sheets maintained by six of the largest HCZ programs: the College Success counseling program, the Cut Above after-school program, the Employment and Technology Center, the Learn to Earn after-school program, the Peacemakers neighborhood safety program, and the Truce Fitness and Nutrition Center. Each data file includes the participant's name, date of birth, program, and date of participation, and spans the 2006 through 2009 fiscal years. We linked these data to the lottery files at HCZ using name and date of birth.

These "sign-in" data confirm that address is strongly associated with participation in neighborhood programs. Fifty-one percent of lottery winners living inside HCZ participated in at least one neighborhood program, as do 57 percent of lottery losers inside HCZ. In contrast, 23 percent of lottery winners and 31 percent of lottery losers living outside of HCZ participated in at least one neighborhood program. The original 24-block HCZ plus 400 meters is more predictive of program participation than the expanded 97-block HCZ, likely because the neighborhood programs are still

concentrated around the original HCZ. All of the reported results are consistent to alternative definitions of inside HCZ.

Table 8 presents these ITT estimates for youth living in and outside HCZ. We drop students with no baseline address information. Consistent with Dobbie and Fryer (2011), there are no statistically different effects by HCZ residence for any of our summary indices. Lottery winners living in HCZ have achievement scores that are 0.472σ (0.139) higher than lottery losers in the Zone, while lottery winners living outside the Zone have achievement scores that are 0.215σ (0.090) higher. We find similar results for both the attainment index and the "on-time" index – there is little differences between the treatment effect of being offered a chance to attend HCZ for students inside versus outside the Zone. Lottery winners in the Zone also are 0.200σ (0.160) less likely to engage in risky behaviors, and are 0.079σ (0.114) healthier than lottery losers in the Zone. In comparison, lottery winners out of the Zone are 0.123σ (0.088) less likely to engage in risky behaviors and 0.009σ (0.070) healthier than lottery losers out of the Zone.

Further, Appendix Tables 9 and 10 present estimates for the individual index components for students living inside and outside HCZ. There are only two statistically significant differences between the in and outside of HCZ treatment estimates for the 22 point estimates we consider, with many of the estimates larger for youth living outside of HCZ.

5.2 Test Scores and Later-Life Outcomes

We now consider the extent to which changes in test scores might explain the impact of the Promise Academy on non-test score outcomes. Specifically, we compare the reduced form estimates of the impact of the Promise Academy on non-test score outcomes to the effects implied by the cross-sectional relationship between test scores and non-test score outcomes in Chetty, Friedman, and Rockoff (2011) and the control group.

Chetty, Friedman, and Rockoff (2011) find that a one σ increase in math or ELA achievement is associated with a 5.6 percentage point increase in college attendance at age 20 for minorities, and a 5.2 percentage point increase for students from low-income families. A one σ increase in math or ELA achievement is also associated with a 1.2 percentage point decrease in teen pregnancy among both minority women and women from low-income families. Dobbie and Fryer (2011) show that the LATE of the Promise Academy on middle school test scoes is 0.229σ for math and 0.047σ for ELA. Using the average correlation across minorities and low-income families from Chetty, Friedman, and Rockoff (2011), these estimates imply that the test score effect alone would lead to a $(5.4 \cdot (0.229 +$

$0.047)) = 1.5$ percentage point increase in college enrollment and a $(1.2 \cdot (0.229 + 0.047)) = 0.3$ percentage point decrease in teen pregnancy. Using the LATE estimates in Tables 4 and 5, this implies that the eighth grade test score increase can explain $((1.5/9.8) \cdot 100) = 15.3$ percent of the college enrollment effect, and $((0.3/15.4) \cdot 100) = 1.9$ percent of the pregnancy effect.

We can perform a similar exercise using the correlations identified within the lottery losers. Following Chetty, Friedman, and Rockoff (2011), we estimate correlations based on math and reading scores from grades four through eight. We stack observations such that each row is a unique student-subject-grade combination, and identify the correlation between scores and outcomes after controlling for our full set of demographic variables and a cubic in previous year's test scores. The correlations that we identify are larger than those estimated by Chetty, Friedman, and Rockoff (2011). A one σ increase in math or ELA test scores is associated with a 12.2 percentage point increase in college enrollment, a 7.1 percentage point reduction in teen pregnancy, and a 1.5 percentage point reduction in the likelihood of being incarcerated. These correlations imply that the eighth grade test score increase can explain $(12.2 \cdot (0.229 + 0.047)/9.8 \cdot 100) = 34.4$ percent of the college enrollment effect, $(7.1 \cdot (0.229 + 0.047)/15.4 \cdot 100) = 12.7$ percent of the pregnancy effect, and $(1.5 \cdot (0.229 + 0.047)/7.5 \cdot 100) = 5.5$ percent of the incarceration effect. Large standard errors on the cross-sectional estimates means that we cannot rule out much larger and smaller impacts.

5.3 Other Mechanisms

Our results up until this point suggest that the Promise Academy investments drive the impact on non-test score outcomes, but that the impacts outcomes are significantly larger than what would be implied by the cross-sectional relationship between test scores and later outcomes. This section considers three additional mechanisms: (1) non-cognitive skills, (2) social networks, and (3) economic preference parameters.

A large body of evidence suggests that non-cognitive skills, such as self-esteem, locus of control (or more generally, attribution theory), and persistence, are correlated with later outcomes. Self-esteem is thought to influence teenage pregnancy and drug use (Stewart et al. 1995, Kalil and Kunz 1999, Cornelius et al. 2004), although there is considerable disagreement on these points (McGee and Williams 2000, Paul et al. 2000). Persistence, as measured through the 8-item scale we use in this paper, is associated with educational attainment and fewer career changes among adults and increased GPA and reduced grade retention among adolescents (Duckworth and Quinn 2009). Heckman et al. (2006) show that self-esteem and locus of control are related to earnings,

incarceration, and teen pregnancy. We test this mechanism by administering the Rosenberg self esteem index, which asks respondents to rate the extent to which they agree to a series of 14 statements such as "I certainly feel useless at times" and "At times, I think I am no good at all" (Rosenberg 1965). Youth were also asked to answer questions from the Rotter Locus of Control instrument, which measures the extent to which respondents believe they control events in their lives (Rotter 1966).

Panel A of Appendix Table 7 presents results of the impact of the Promise Academy on these non-cognitive skills. If anything, Promise Academy students report lower non-cognitive skills than the control group. Lottery winners score 0.121σ (0.110) lower on the Rosenberg self esteem index, and 0.249σ (0.115) lower on Duckworth and Quinn's (2009) short grit scale, though only the latter is statistically significant. Lottery winners have Locus of Control scores that are 0.046σ (0.107) higher, but the estimate is not statistically different than zero.

There are at least two possible explanations for the negative impact of the Promise Academy on measured non-cognitive skill. First, we measure non-cognitive skills using self-reports that are likely subject to reference bias, or the tendency for survey responses to be influenced by social context (e.g. Heine et al. 2002, 2008). For example, when asked to agree or disagree with a survey prompt such as "I am a hard worker," the respondent must implicitly compare him or herself to another hypothetical individual. A respondent with very high standards might consider a hard worker to be someone who studies four hours a night, while a respondent with very low standards might consider a hard worker to be someone who studies one hour a night. To the extent that the Promise Academy students hold themselves to a higher standard than they otherwise would have, our treatment will be biased downwards. Consistent with this idea, West et al. (2014) find that two high-achieving charter middle schools in Boston increase test scores, but decrease measures of conscientiousness, self-control, grit, and growth mindset. They find suggestive evidence that the decrease in measured non-cognitive skill is the result of reference bias, or the tendency for survey responses to be influenced by social context. The second possible explanation is that the Promise Academy has a negative impact on actual non-cognitive skill, and that the estimated treatment effects on human capital and risky behavior operate through some other mechanism.

The second mechanism we explore is the impact of the Promise Academy on traditional economic preference parameters such as risk aversion and discount rate. These measures are the common determinants of decision-making in economic models and have been linked to a variety of later outcomes (Borghans et al. 2008). Discount rates and risk aversion are measured by asking youth

to make choices through a fixed series of comparisons to infer an indifference point (Hardisty et al. 2011). For discount rates, youth were asked whether they would prefer that $40 be mailed to them later that day or for a larger amount to be mailed in one month. The amount was then varied until the student changed her answer or reached the extreme value of either $42 or $55. For risk aversion, youth were given a choice between a job that paid $600 with probability one and a second identical job that paid $1,200 with probability 0.5 and a value less than $600 with equal probability. The latter value was then altered until a student changed her answer or reached an extreme value of either $150 or $540. To maintain consistency with the rest of our results, we report results for both discount rate and risk aversion in standard-deviation units.

Winning the lottery to attend the Promise Academy has no detectable effect on discount rates. Lottery winners have discount rates that are only 0.021σ (0.110) higher.[20] Conversely, the Promise Academy does seem to alter risk aversion in its students, as lottery winners report 0.253σ (0.104) higher Pratt-Arrow measures than lottery losers.

The final mechanism we explore is the importance of changes in peer quality. A large literature suggests that outcomes are heavily influenced by one's peers (Sacerdote 2001, Fergusson et al. 2002, Boisjoly et al. 2006, Carrell et al. 2009, Deming 2011). We measure peer networks by asking youth about the attitudes of their peer group on crime and educational attainment. Academic peer quality was measured by asking youth to how important it is for their friends to study, stay in school, and attending class regularly. Risky behavior peer quality was measured by asking youth whether their friends use drugs, drink alcohol, smoke cigarettes, steal, fight, or are in a gang. We use these responses to create summary indices of peer networks.

Panel C of Appendix Table 7 presents results of the impact of the Promise Academy on peer quality. Lottery winners have peers that are 0.097σ (0.078) higher than lottery losers on our index measuring the relative importance of various academic activities in one's peer group, though the effect is not statistically significant. There is almost no difference between levels of risky behaviors in the networks of winners and losers, with an estimated point estimate of -0.010σ (0.069). Taken together, we interpret these results as suggesting that changes in peer quality are not driving our results, although we cannot rule out changes in other forms of social interaction.

[20] Over a third of the sample selected the highest discount rate category, preferring $40 now to $55 in one month, implying an annual discount rate of over 4,000 percent. We also find no impact of the Promise Academy on choosing the highest discount rate category, or choosing a rate above the median.

6 Conclusion

Investments in education reform are based, in part, on two important assumptions: (1) high quality schools can increase test scores, and (2) the well-known relationship between test scores and adult outcomes is causal. There is a growing consensus that the first assumption holds (Angrist et al. 2010, Abdulkadiroglu et al. 2011, Abdulkadiroglu et al. 2014, Dobbie and Fryer 2011, Neal 1997, Grogger and Neal 2000).

In this paper, we estimate the impact of attending the Promise Academy in the Harlem Children's Zone on a wide range of human capital decisions, risky behaviors, and health outcomes. Several stylized facts emerge. Youth randomly offered admission to the Promise Academy demonstrate large increases in academic achievement and are more likely to reach important benchmarks such as high school graduation or college enrollment, on time. Youth are statistically no more likely to increase their quantity of schooling, though many students are still in college and this conclusion may be altered with subsequent follow-ups. HCZ impacts important measures of risky behaviors such as teen pregnancy and incarceration, but has no impact on health behaviors. These data provide evidence that assumption two holds.

Moreover, the cross-sectional correlation between test scores and adult outcomes may understate the true impact of a high quality school, suggesting that high quality schools change more than cognitive ability. If true, the return on investment for high-performing charter schools could be much larger than that implied by the short-run test score increases.

A larger sample of schools, longer-term outcomes, and a better sense of the mechanisms generating the observed impacts are all ripe areas for future research.

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Table 1
An Accounting of the Sample

	Pooled		2005 Lottery		2006 Lottery	
	Winner	Loser	Winner	Loser	Winner	Loser
	(1)	(2)	(3)	(4)	(5)	(6)
Lottery Entrants	189	410	96	237	93	173
Matched To NYC Data	181	390	90	223	91	167
Match Rate	0.958	0.951	0.938	0.941	0.978	0.965
Survey Pool	189	381	96	222	93	159
Survey Respondents	150	257	76	145	74	112
Survey Response Rate	0.794	0.676	0.792	0.653	0.796	0.709

Notes: This table describes the match rate for Promise Academy lottery entrants to New York City administrative data and response rates for the in-person survey. The first row tabulates all students who entered the Promise Academy Middle School lottery in the Spring of 2005 or 2006, excluding students who were automatically admitted due to sibling preferences. The second row tabulates students whom we are able to match to New York City administrative data using the matching algorithm described in the text. The third row displays the percentage of students who are succesfully matched. Our survey pool includes all lottery entrants except for the group of randomly selected lottery losers that were used to test and callibrate the survey instrument during the Fall of 2011, along with any records that were discovered to be mistaken matches and/or duplicates during the survey process. The fifth row tabulates all students who completed our survey, and the sixth reports the percentage of the survey pool who responded.

Table 2
Summary Statistics

	All NYC	HCZ Area	Lottery Sample			Survey Respondents		
			Winners	Losers	Difference	Winners	Losers	Difference
Baseline Characteristics	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	0.489	0.490	0.448	0.538	−0.090*	0.450	0.560	−0.110*
White	0.140	0.018	0.011	0.005	0.006	0.000	0.008	−0.008
Black	0.328	0.637	0.860	0.825	0.035	0.866	0.812	0.054
Hispanic	0.395	0.320	0.117	0.160	−0.043	0.121	0.169	−0.048
Free Lunch	0.842	0.878	0.821	0.811	0.010	0.846	0.809	0.037
5th Grade Sp. Ed.	0.096	0.150	0.052	0.055	−0.003	0.056	0.058	−0.002
5th Grade LEP	0.104	0.082	0.035	0.041	−0.006	0.035	0.046	−0.011
5th Grade Math	0.025	−0.292	−0.248	−0.284	0.036	−0.258	−0.269	0.011
5th Grade ELA	0.020	−0.247	−0.242	−0.260	0.018	−0.274	−0.234	−0.040
Enrollment Outcomes								
Attended Promise Academy	0.001	0.040	0.633	0.066	0.567***	0.707	0.082	0.625***
Years in Promise Academy	0.004	0.172	3.360	0.276	3.084***	3.957	0.401	3.556***
Observations	154,988	1,311	181	390	571	150	257	407

Notes: This table describes summary statistics and balance tests for baseline observable data and post-lottery enrollment outcomes. Column (1) reports means for all New York City students enrolled in fifth grade during the Spring of 2005 or 2006. Column (2) reports means for New York City public school students enrolled in fifth grade during the Spring of 2005 or 2006 living within 400 meters of the original 24-block area of HCZ, ranging from 116th to 123rd Streets, 5th Avenue to 8th Avenue. Columns (3) through (5) report means and differences for all students who enter the sixth grade lottery. Columns (6) through (8) report the same information for students who respond to our in-person survey. Differences control for lottery year effects, indicators for having a sibling enrolled in the same lottery, and a sibling-year interaction term. 5th Grade Math and 5th Grade ELA indicate students' scores on the New York State Math and English Language Arts tests taken in the pre-lottery year. These scores are standardized to have mean zero and standard deviation one for the entire New York City sample by year. LEP denotes students who received special assistance due to Limited English Proficiency. Free Lunch indicates students who meet federal guidelines to receive free or reduced price lunch. See the Data Appendix for full variable definitions. ***, **, and * indicate statistically significant differences between lottery winners and losers with 99%, 95%, and 90% confidence, respectively.

Table 3
Youth Characteristics and Survey Response

	All Lottery Entrants	Lottery Winners	Lottery Losers	Difference
Panel A. Characteristics	(1)	(2)	(3)	(4)
Female	0.047	−0.022	0.078	−0.100
	(0.039)	(0.065)	(0.050)	(0.081)
Black	0.000	0.129	−0.040	0.168
	(0.050)	(0.095)	(0.061)	(0.112)
Free Lunch	0.026	0.112	−0.029	0.140
	(0.051)	(0.088)	(0.064)	(0.108)
5th Grade Sp. Ed.	0.012	0.058	0.014	0.044
	(0.092)	(0.120)	(0.117)	(0.167)
5th Grade LEP	0.067	−0.432*	0.145	−0.577**
	(0.120)	(0.223)	(0.123)	(0.252)
5th Grade Math	−0.014	0.017	−0.027	0.045
	(0.035)	(0.058)	(0.046)	(0.073)
5th Grade ELA	0.007	−0.052	0.042	−0.094
	(0.033)	(0.051)	(0.044)	(0.067)
Missing 5th Grade Math	−0.072	−0.453*	−0.026	−0.427
	(0.161)	(0.240)	(0.191)	(0.305)
Missing 5th Grade ELA	0.023	0.592***	−0.094	0.686**
	(0.135)	(0.225)	(0.161)	(0.275)
Missing Demographics	0.002	−0.122	0.031	−0.153
	(0.139)	(0.183)	(0.173)	(0.251)
	541	181	360	541
Panel B. Observed Outcomes				
Eighth Grade Math	0.062**	0.023	0.034	−0.011
	(0.028)	(0.039)	(0.039)	(0.055)
	452	157	295	452
Eighth Grade ELA	0.003	−0.056	0.002	−0.058
	(0.041)	(0.051)	(0.054)	(0.074)
	457	160	297	457
College Enrollment	0.126***	0.137**	0.082	0.055
	(0.039)	(0.062)	(0.052)	(0.081)
	541	181	360	541
p-value from Joint F-test Panel A	0.915	0.112	0.742	0.099
p-value from Joint F-test Panel B	0.002	0.012	0.391	0.672

Notes: This table reports the results of OLS regressions of an indicator for survey response on baseline characteristics and observed outcomes. The sample for eighth grade scores and college enrollment include lottery entrants not in the pre-test group who are matched to the NYC administrative data. Ever pregnant is restricted to females in the survey sample and incarcerated is restricted to males whom we successfully contacted. All regressions control for lottery-year indicators, indicators for having a sibling enrolled in the same lottery, and a sibling-year interaction term. Regressions in Panel B also control for the baseline demographic variables summarized in Table 2 and a quadratic of 4th and 5th grade math and ELA test socres. The final two rows report the p-value from a joint F-test of the null hypothesis that all coefficients in each Panel equal zero, estimated via seemingly unrelated regression in Panel B. Heteroskedasticity-robust standard errors are reported in parentheses. The number of observations is reported below the standard errors in Panel B. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

Table 4
The Impact of Attending the Promise Academy on Human Capital

	CM	ITT	LATE (Ever)	LATE (Years)
Panel A. Achievement	(1)	(2)	(3)	(4)
Woodcock Johnson Math	0.000	0.281***	0.436***	0.075***
	(1.000)	(0.083)	(0.121)	(0.020)
	243	386	386	386
Woodcock Johnson Reading	0.000	0.115	0.179	0.031
	(1.000)	(0.083)	(0.123)	(0.021)
	243	386	386	386
Regents Passed	3.819	1.228***	1.948***	0.325***
	(2.637)	(0.271)	(0.390)	(0.059)
	309	452	452	452
Regents Test Scores	−0.401	0.293***	0.459***	0.077***
	(0.833)	(0.090)	(0.132)	(0.021)
	296	437	437	437
Achievement Index	−0.032	0.279***	0.445***	0.078***
	(0.893)	(0.073)	(0.108)	(0.018)
	344	504	504	504
Panel B. Attainment				
Graduated (6 Years)	0.735	0.037	0.061	0.010
	(0.442)	(0.045)	(0.071)	(0.011)
	287	426	426	426
College Enrollment	0.422	0.055	0.098	0.018
	(0.494)	(0.043)	(0.074)	(0.013)
	410	599	599	599
Number of College Semesters Enrolled	1.046	0.161	0.287	0.053
	(1.426)	(0.123)	(0.210)	(0.038)
	410	599	599	599
Attainment Index	−0.010	0.067	0.119	0.022
	(0.887)	(0.076)	(0.130)	(0.024)
	410	599	599	599
Panel C. On Time				
Graduated (4 Years)	0.578	0.133***	0.221***	0.037***
	(0.495)	(0.048)	(0.074)	(0.012)
	303	446	446	446
Immediate College Enrollment	0.329	0.170***	0.282***	0.047***
	(0.471)	(0.051)	(0.076)	(0.012)
	304	448	448	448
On-Time Index	−0.001	0.313***	0.518***	0.086***
	(0.882)	(0.091)	(0.137)	(0.022)
	304	448	448	448

Notes: This table reports estimates of the effect of attending the Promise Academy. Column (1) reports the mean and standard deviation of each variable for the control group. Column (2) reports ITT estimates of the impact of winning the admissions lottery. Column (3) reports LATE estimates of the impact of ever attending the Promise Academy using a winning lottery number as an instrumnet. Column (4) reports LATE estimates of the impact of attending the Promise Academy for a year using a winning lottery number as an instrument. All regressions control for the baseline demographic variables summarized in Table 2, a quadratic of 4th and 5th grade math and ELA test scores, lottery-year indicators, indicators having a sibling enrolled in the same lottery, and a sibling-year interaction term. The sample for Regents results includes all lottery entrants who enroll at a NYC high school for at least one year. College enrollment and number of college semesters enrolled includes all lottery entrants. The sample for eighth grade scores, high school graduation, and immediate college enrollment includes lottery entrants who are matched to the NYC administrative data and have non-missing observations. All other outcomes are restricted to youth in the survey sample who answered the indicated qustion. Each index variable is restricted to youth with at least one non-missing outcome in that domain. Woodcock-Johnson scores come from the Brief Battery described in Web Appendix B. Regents passed equals the number of Regents exams with scores over 65 out of 100. Regents test scores is the mean of the standardized score on the Integrated Algebra, Living Environment, and World History exams, and includes all youth with at least one non-missing score. Graduated (6 years) is an idicator for whether or not a student graduated high school within 6 years of starting. Graduated (4 years) is an indicator for whether or not a student graduated high school within 4 years of starting. College enrollment is an indicator for whether a student ever enrollend in college. Immediate college enrollment is an idicator for whether or not a student enrolled in college the fall semester after graduating high school. The last row of each Panel is a summary index equal to the average of the standardized value of each of the preceding variables. Each standardized survey outcome is renormed using the mean and standard deviation of the control group. Standardized administrative outcomes are renormed using the mean and standard deviation of the entire NYC sample. Web Appendix B contains additional details on each variable. Heteroskedasticity-robust standard errors are reported in parentheses. The number of observations is reported below the standard error. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

Table 5
The Impact of Attending the Promise Academy on Risky Behaviors and Health

	CM	ITT	LATE (Ever)	LATE (Years)
Panel A. Risky Behaviors	(1)	(2)	(3)	(4)
Ever Pregnant (Female)	0.170	−0.101**	−0.154**	−0.027**
	(0.377)	(0.047)	(0.068)	(0.012)
	141	205	205	205
Incarcerated (Male)	0.042	−0.044**	−0.075**	−0.013**
	(0.201)	(0.017)	(0.030)	(0.005)
	144	233	233	233
Drug/Alcohol Index	−0.001	−0.020	−0.032	−0.006
	(0.692)	(0.067)	(0.103)	(0.018)
	256	406	406	406
Criminal Behavior Index	0.000	−0.010	−0.016	−0.003
	(0.618)	(0.066)	(0.101)	(0.018)
	257	407	407	407
Risky Behavior Index	0.053	−0.135*	−0.223*	−0.039*
	(0.895)	(0.072)	(0.116)	(0.020)
	288	444	444	444
Panel B. Health				
Nutrition Index	0.000	0.103*	0.165*	0.029*
	(0.572)	(0.061)	(0.095)	(0.016)
	257	407	407	407
Mental Health	0.000	−0.032	−0.051	−0.009
	(1.000)	(0.104)	(0.161)	(0.028)
	254	403	403	403
Physical Health Index	0.000	−0.041	−0.065	−0.011
	(0.599)	(0.063)	(0.098)	(0.017)
	257	407	407	407
Health Behavior Index	−0.001	0.025	0.040	0.007
	(0.499)	(0.052)	(0.080)	(0.014)
	257	407	407	407
Health Index	0.000	0.031	0.049	0.009
	(0.533)	(0.057)	(0.088)	(0.015)
	257	407	407	407

Notes: This table reports estimates of the effect of attending the Promise Academy. Column (1) reports the mean and standard deviation of each variable for the control group. Column (2) reports ITT estimates of the impact of winning the admissions lottery. Column (3) reports LATE estimates of the impact of ever attending the Promise Academy using a winning lottery number as an instrumnet. Column (4) reports LATE estimates of the impact of attending the Promise Academy for a year using a winning lottery number as an instrument. All regressions control for the baseline demographic variables summarized in Table 2, a quadratic of 4th and 5th grade math and ELA test scores, lottery-year indicators, indicators having a sibling enrolled in the same lottery, and a sibling-year interaction term. Ever pregnant is restricted to female entrants in the survey sample, and incarcerated is restricted to male entrants whom we successfully contacted. All other outcomes are restricted to youth in the survey sample who answered the indicated qustion. Each index variable is restricted to youth with at least one-nonmissing outcome in that domain. Incarceration is an indicator for being incarcerated during the survey period. The drug/alcohol index is the average of standardized indicators for having used marijuana in the past 30 days, having consumed alcohol in the past 30 days, and having used hard drugs in the past year. The criminal behavior index is the average of standardized indicators for haveing ever destroyed property, having ever stolen an item worth less than 50 dollars, having ever stolen an item worth more than 50 dollars, having ever committed any other type of property crime, having ever been in a serious fight, having ever carried a handgun, and having ever been a gang member. The nutrition index is equal to the average of standardized fruit and vegetable consumption, negated soft drink consumption, negated sugary snacks consumption, and negated fast food consumption. The mental health index is the standardized K6 Anxiety Scale, defined as the sum of a student's response on a five point Likert Scale to six statements assessing one's mental state. The physical health index is the average of (negated) standardized indicators for reporting poor health, chronic health problems, having had an asthma attack in the past year, and having a BMI in the 95th percentile or above. The health behavior index is equal to the average of standardized indicators having had a physical examination in the past year, reporting vigorous physical activity, reporting moderate physical activity, and having had a dental examination in the past year. The last row of each Panel is a summary index equal to the average of the standardized value of each of the preceding variables. Each standardized survey outcome is renormed using the mean and standard deviation of the control group. Web Appendix B contains additional details on each variable. Heteroskedasticity-robust standard errors are reported in parentheses. The number of observations is reported below the standard error. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

Table 6
Attrition and Bounding - Human Capital

	Admin ITT	Survey ITT	Lee Bound	p-value (1) = (2)	p-value Lee = TE
Panel A1. Achievement (Admin.)	(1)	(2)	(3)	(4)	(5)
Regents Passed	1.228***	1.273***	1.121***		
	(0.271)	(0.290)	(0.267)	0.909	0.778
	452	347	449		
Regents Test Scores	0.293***	0.278***	0.227**		
	(0.090)	(0.098)	(0.091)	0.908	0.602
	437	337	431		
Achievement Index	0.279***	0.279***	0.279***		
	(0.073)	(0.075)	(0.073)	0.997	1.000
	504	398	504		
Panel A2. Achievement (Survey)					
Woodcock Johnson Math		0.281***	0.071		
	—	(0.083)	(0.073)	—	0.057
		386	367		
Woodcock Johnson Reading		0.115	−0.090		
	—	(0.083)	(0.074)	—	0.065
		386	367		
Panel B. Attainment					
Graduated (6 Years)	0.037	0.078	0.011		
	(0.045)	(0.048)	(0.045)	0.542	0.675
	426	334	418		
College Enrollment	0.055	0.095*	0.055		
	(0.043)	(0.052)	(0.043)	0.552	1.000
	599	407	599		
Number of College Semesters Enrolled	0.161	0.286*	0.161		
	(0.123)	(0.149)	(0.123)	0.519	1.000
	599	407	599		
Attainment Index	0.067	0.157*	0.067		
	(0.076)	(0.089)	(0.076)	0.440	1.000
	599	407	599		
Panel C. On Time					
Graduated (4 Years)	0.133***	0.163***	0.123**		
	(0.048)	(0.052)	(0.048)	0.674	0.890
	446	344	442		
Immediate College Enrollment	0.170***	0.190***	0.148***		
	(0.051)	(0.056)	(0.051)	0.790	0.761
	448	345	443		
On-Time Index	0.313***	0.362***	0.281***		
	(0.091)	(0.098)	(0.091)	0.713	0.803
	448	345	443		

Notes: This table reports ITT estimates accounting for survey attrition. Column (1) reports ITT estimates in the administrative sample not subject to attrition bias. Column (2) reports ITT estimates in the sample of survey respondents. Column (3) reports Lee (2009) bounds by dropping lottery winners with the best residual outcomes until there is an equal survey response rate between lottery winners and lottery losers. Column (4) reports the p-value from a test that the coefficients in columns (1) and (2) are equal. Column (5) reports the p-value from a test that the coefficient in column (3) is equivalent to the Treatment Effect (TE) reported in Table 4. Note that the TE is the coefficient reported in column (1) for administrative variables and the cofficient reported in column (2) for survey variables. All regressions follow the specification and sample restrictions from Table 4. Heteroskedasticity-robust standard errors are reported in parentheses. The number of observations is reported below the standard error. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

Table 7
Attrition and Bounding - Risky Behaviors and Health

	Survey ITT	Lee Bound	p-value (1) = (2)
Panel A. Risky Behaviors)	(1)	(2)	(3)
Ever Pregnant (Female)	−0.101**	−0.073	
	(0.047)	(0.047)	0.670
	205	201	
Incarcerated (Male)	−0.044**		
	(0.017)	—	—
	233		
Drug/Alcohol Index	−0.020	0.082	
	(0.067)	(0.069)	0.293
	406	387	
Criminal Behavior Index	−0.010	0.070	
	(0.066)	(0.068)	0.402
	407	388	
Risky Behavior Index	−0.047	0.037	
	(0.062)	(0.065)	0.352
	407	388	
Panel B. Health			
Nutrition Index	0.103*	−0.022	
	(0.061)	(0.059)	0.139
	407	388	
Mental Health	−0.032	−0.209**	
	(0.104)	(0.103)	0.226
	403	384	
Physical Health Index	−0.041	−0.149**	
	(0.063)	(0.063)	0.226
	407	388	
Health Behavior Index	0.025	−0.078	
	(0.052)	(0.050)	0.154
	407	388	
Health Index	0.031	−0.083	
	(0.057)	(0.054)	0.148
	407	388	

Notes: This table reports ITT estimates accounting for survey attrition. Column (1) reports ITT estimates in the sample of survey respondents. Column (2) reports Lee (2009) bounds by dropping lottery winners with the best residual outcomes until there is an equal survey response rate between lottery winners and lottery losers. Column (3) reports the p-value from a test that the coefficients in Columns (1) and (2) are equal. All regressions follow the specification and sample restrictions from Table 5. Heteroskedasticity-robust standard errors are reported in parentheses. The number of observations is reported below the standard error. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

Table 8
The Impact of Attending the HCZ Promise Academy
Inside and Outside the Zone

	Inside Zone	Outside Zone	p-value
	(1)	(2)	(3)
Achievement Index	0.472***	0.215**	
	(0.139)	(0.090)	0.120
	134	334	
Attainment Index	−0.029	0.133	
	(0.162)	(0.091)	0.384
	147	381	
On-Time Index	0.260	0.391***	
	(0.198)	(0.106)	0.559
	120	302	
Risky Behavior Index	−0.200	−0.123	
	(0.160)	(0.088)	0.671
	122	314	
Health Index	0.079	0.009	
	(0.114)	(0.070)	0.603
	112	287	

Notes: This table reports ITT estimates for youth with baseline addresses inside and outside of the Harlem Children's Zone. Column (1) presents ITT estimates for youth living within 400 meters of the original 24-block Zone. Column (2) presents ITT estimates for youth living outside 400 meters of the original 24-block Zone, and Column (3) reports a p-value of a test that the two coefficients are equal. Students with no baseline address information are dropped. All specifications and variable definitions are identical to those in Tables 4 and 5. Heteroskedasticity-robust standard errors are reported in parentheses. The number of observations is reported below the standard error. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

Appendix Table 1
Characteristics of Charter Schools

	HCZ	NYC	
	Promise Academy	Above Median	All Middle Schools
Human Capital	(1)	(2)	(3)
Teacher Formal Feedback	3.00	4.21	2.84
Teacher Informal Feedback	12.50	14.08	8.39
Total Teacher Hours	45.00	57.08	54.68
Max Teacher Pay	11.00	9.08	8.55
Data Driven Instruction			
Number of Interim Assessments	9.00	3.90	2.83
Tracking Using Data	1.00	0.33	0.57
Parent Engagement			
Academic Feedback	13.50	12.67	10.25
Behavior Feedback	54.00	26.25	21.36
Regular Feedback	54.00	13.90	8.15
Tutoring			
High Quality Tutoring	0.00	0.17	0.07
Any Tutoring	1.00	0.83	0.79
Small Group Tutoring	0.00	0.20	0.18
Frequent Tutoring	1.00	0.60	0.45
Instructional Time			
+25% Increase in Time	1.00	0.83	0.64
Instructional Hours	7.50	8.25	8.04
Instructional Days	210.00	193.50	188.64
Culture			
High Expectations	0.00	0.83	0.50
School-wide Discipline	0.00	0.33	0.36
Traditional Inputs			
Small Classes	0.00	0.40	0.64
High Expenditures	1.00	0.75	0.67
High Teachers with MA	1.00	0.40	0.64
Low Teachers without Certification	0.00	0.20	0.45
Other Controls			
Wrap-around Service Index	0.62	-0.14	-0.09
No Excuses	1.00	0.75	0.50
Schools	1	5	13

Notes: This table reports results from a survey of 35 New York City charter schools administered by Dobbie and Fryer (2011b). Column (1) reports the mean of each variable for the Promise Academy Middle School. Column (2) includes all schools with entry in middle school grades (5th - 8th) whose average treatment effects on Math and ELA scores are above the median in the sample. Column (3) includes all Middle Schools in the sample with a tested grade in 2010-2011. See Dobbie and Fryer (2011b) for variable definitions and codings.

Appendix Table 2
First Stage Results for Various Specifications

	Ever Attended			Years Attended		
	(1)	(2)	(3)	(4)	(4)	(6)
Lottery Winner	0.557***	0.553***	0.556***	3.033***	3.009***	3.011***
	(0.038)	(0.038)	(0.037)	(0.240)	(0.238)	(0.234)
5th Grade Math		0.002	−0.004		0.037	−0.005
		(0.027)	(0.027)		(0.168)	(0.171)
5th Grade ELA		−0.020	−0.031		−0.148	−0.209
		(0.027)	(0.027)		(0.148)	(0.151)
Black			0.064			0.539**
			(0.040)			(0.209)
Female			0.009			0.013
			(0.028)			(0.170)
LEP			−0.018			0.395
			(0.067)			(0.342)
Special Education			−0.168***			−1.012***
			(0.062)			(0.332)
Free or Reduced Lunch			0.012			0.073
			(0.036)			(0.208)
Observations	599	599	599	599	599	599

Notes: This table reports first stage results for various specifications. All specifications include lottery-year indicators and indicators for having a sibling enrolled in the same lottery as controls. When appropriate, we also include indicators for missing variables to prevent attrition. Columns (1)-(3) use an indicator for if a student ever attended the Promise Academy as the dependent variable. Columns (4)-(6) use the number of years a student attended the Promise Academy as the dependent variable. The variable definitions are the same as those described in Table 2. Heteroskedasticity-robust standard errors are reported in parentheses. The number of observations is reported in the last row. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

Appendix Table 3
Maximum Observations - Human Capital

Panel A. Achievement	
Woodcock Johnson Math	395
Woodcock Johnson Reading	395
Regents Passed	452
Regents Test Scores	437
Achievement Index	504
Panel B. Attainment	
Graduated (6 Years)	426
College Enrollment	599
Number of College Semesters Enrolled	599
Attainment Index	599
Panel C. On Time	
Graduated (4 Years)	446
Immediate College Enrollment	448
On-Time Index	448

Notes: This table reports the number of non-missing observations for all of the variables reported in Table 4. Variable definitions are identical to those in Table 4.

Appendix Table 4
Maximum Observations - Risky Behavior and Health

Panel A. Risky Behaviors	
Ever Pregnant (Female)	213
Incarcerated (Male)	238
Drug/Alcohol Index	417
Criminal Behavior Index	418
Risky Behavior Index	444
Panel B. Health	
Nutrition Index	418
Mental Health	414
Physical Health Index	418
Health Behavior Index	418
Health Index	407

Notes: This table reports the number of non-missing observations for all of the variables reported in Table 5. Variable definitions are identical to those in Table 5.

Appendix Table 5
The Impact of Attending the Promise Academy on College Outcomes

	CM	ITT	LATE (Ever)	LATE (Years)
	(1)	(2)	(3)	(4)
College Enrollment	0.422	0.055	0.098	0.018
	(0.494)	(0.043)	(0.074)	(0.013)
	410	599	599	599
Immediate College Enrollment	0.329	0.170***	0.282***	0.047***
	(0.471)	(0.051)	(0.076)	(0.012)
	304	448	448	448
Number of College Semesters Enrolled	1.046	0.161	0.287	0.053
	(1.426)	(0.123)	(0.210)	(0.038)
	410	599	599	599
Two Year College	0.190	−0.028	−0.050	−0.009
	(0.393)	(0.034)	(0.059)	(0.011)
	410	599	599	599
Four Year College	0.254	0.089**	0.158**	0.029**
	(0.436)	(0.041)	(0.069)	(0.012)
	410	599	599	599
1000+ SAT College	0.105	0.034	0.061	0.011
	(0.307)	(0.029)	(0.049)	(0.009)
	410	599	599	599

Notes: This table reports estimates of the effect of attending the Promise Academy on college quality. Column (1) reports the mean of each variable for the control group. Column (2) reports ITT estimates of the impact of winning the admissions lottery. Column (3) reports LATE estimates of the impact of ever attending the Promise Academy using a winning lottery number as an instrument. Column (4) reports LATE estimates of the impact of attending the Promise Academy for a year using a winning lottery number as an instrument. All regressions control for the baseline demographic variables summarized in Table 2, a quadratic of 4th and 5th grade math and ELA test scores, lottery-year indicators, indicators having a sibling enrolled in the same lottery, and a sibling-year interaction term. Colleges that we cannot match to SAT or ACT data are coded as zero. Heteroskedasticity-robust standard errors are reported in parentheses. The number of observations is reported below the standard error. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

Appendix Table 6
The Impact of Attending the Promise Academy on College Outcomes - High School Graduates Only

	CM	ITT	LATE (Ever)	LATE (Years)
	(1)	(2)	(3)	(4)
College Enrollment	0.592	0.151***	0.232***	0.035***
	(0.493)	(0.057)	(0.082)	(0.012)
	211	316	316	316
Immediate College Enrollment	0.474	0.195***	0.301***	0.045***
	(0.501)	(0.061)	(0.086)	(0.013)
	211	316	316	316
Number of College Semesters Enrolled	1.483	0.434**	0.669**	0.099**
	(1.510)	(0.180)	(0.263)	(0.039)
	211	316	316	316
Two Year College	0.275	−0.042	−0.065	−0.010
	(0.448)	(0.056)	(0.083)	(0.012)
	211	316	316	316
Four Year College	0.346	0.209***	0.322***	0.048***
	(0.477)	(0.061)	(0.086)	(0.013)
	211	316	316	316
1000+ SAT College	0.147	0.073	0.113*	0.017*
	(0.355)	(0.045)	(0.066)	(0.010)
	211	316	316	316

Notes: This table reports estimates of the effect of attending the Promise Academy on college quality for the sample of students that graduated high school. Column (1) reports the mean of each variable for the control group. Column (2) reports ITT estimates of the impact of winning the admissions lottery. Column (3) reports LATE estimates of the impact of ever attending the Promise Academy using a winning lottery number as an instrument. Column (4) reports LATE estimates of the impact of attending the Promise Academy for a year using a winning lottery number as an instrument. All regressions control for the baseline demographic variables summarized in Table 2, a quadratic of 4th and 5th grade math and ELA test scores, lottery-year indicators, indicators having a sibling enrolled in the same lottery, and a sibling-year interaction term. Colleges that we cannot match to SAT or ACT data are coded as zero. Heteroskedasticity-robust standard errors are reported in parentheses. The number of observations is reported below the standard error. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

Appendix Table 7
Impacts of the Promise Academy on Possible Mechanisms

	CM	ITT	LATE (Ever)	LATE (Years)
	(1)	(2)	(3)	(4)
Panel A. Non-Cognitive Measures				
Self Esteem Index	0.000	−0.121	−0.195	−0.033
	(1.000)	(0.110)	(0.173)	(0.030)
	255	402	402	402
Grit Index	0.000	−0.249**	−0.393**	−0.068**
	(1.000)	(0.115)	(0.178)	(0.031)
	250	397	397	397
Locus of Control	0.000	0.046	0.075	0.013
	(1.000)	(0.107)	(0.167)	(0.029)
	254	397	397	397
Panel B. Discount Rates and Risk Aversion				
Discount Rate	0.000	0.021	0.034	0.006
	(1.000)	(0.110)	(0.171)	(0.029)
	257	403	403	403
Risk Aversion	0.000	0.253**	0.407**	0.070**
	(1.000)	(0.104)	(0.162)	(0.028)
	256	403	403	403
Panel C. Social Networks				
Academic Activities in Social Network	−0.003	0.097	0.152	0.027
	(0.754)	(0.078)	(0.119)	(0.021)
	252	397	397	397
Risky Behaviors in Social Network	0.001	−0.010	−0.015	−0.003
	(0.574)	(0.069)	(0.105)	(0.019)
	252	397	397	397
Panel D. Sexual Behaviors				
Ever Had Sex	0.644	−0.011	−0.017	−0.003
	(0.480)	(0.051)	(0.080)	(0.014)
	253	397	397	397
Condom Use	0.809	−0.043	−0.069	−0.012
	(0.395)	(0.057)	(0.087)	(0.015)
	162	254	254	254
Ever Been Pregnant	0.170	−0.101**	−0.154**	−0.027**
	(0.377)	(0.047)	(0.068)	(0.012)
	141	205	205	205

Notes: This table reports the estimates of the effect of attending the Promise Academy on mediating outcomes. Column (1) reports the mean and standard deviation of each variable for the control group. Column (2) reports ITT estimates of the impact of winning the admissions lottery. Column (3) reports LATE estimates of the impact of ever attending the Promise Academy using a winning lottery number as an instrument. Column (4) reports LATE estimates of the impact of attending the Promise Academy for a year using a winning lottery number as an instrument. All regressions control for the baseline demographic variables summarized in Table 2, a quadratic of 4th and 5th grade math and ELA test scores, lottery-year indicators, indicators for having a sibling enrolled in the same lottery, and a sibling-year interaction term. The sample includes lottery entrants in the survey sample. Results for condom and contraceptive use are restricted to students who report having ever had sex. Self Esteem is constructed from students' response to ten self-evaluative questions from Rosenberg (1965). Grit is measured by the eight-quetion Short Grit Scale developed by Duckworth and Quinn (2009). Locus of Control is constructed from students' levels of agreement with four pairs of questions developed by Rotter (1966) and adapted for the NLSY. Academic Activities in Social Network is the average of standardized measures of the importanace of studying to friends, the importance of education to friends, the importance of attending class to friends, and the importance of getting good grades to friends. Risky Behaviors in Social Networks is the average of standardized indicators for a youth's friends using drugs, smoking cigarettes, having stolen an item worth less than 50 dollars, having stolen an item worth more than 50 dollars, getting in fights, carrying a handgun, or being in a gang. Condom Use is an indicator for using a condom during the last time the student had sexual intercourse. Other Contraceptive Use is an indicator for using a non-condom form of contraception. All standardized variables are standardized to have mean zero and standard deviation one in the control group. See Web Appendix B for additional information on each variable. Heteroskedasticity-robust standard errors are reported in parentheses. The number of observations is reported below the standard error. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

Appendix Table 8
Main Estimates with Familywise-Error-Rate-Controlled p-values

	ITT Estimate	Uncorrected p-value	StepM Corrected p-value	Holm Corrected p-value
	(1)	(2)	(3)	
Achievement Index	0.279	0.000	0.001	0.001
	(0.073)			
Attainment Index	0.067	0.378	0.607	0.756
	(0.076)			
On-Time Index	0.313	0.001	0.002	0.003
	(0.091)			
Risky Behavior Index	−0.135	0.062	0.175	0.185
	(0.072)			
Health Index	0.031	0.589	0.607	0.756
	(0.057)			

Notes: This table reports ITT estimates correcting for multiple-hypothesis testing. Column (1) reports ITT estimates following the specification described in Tables 4 and 5. Column (2) reports the unadjusted p-value. Columns (3) and (4) report p-values controlling for the Familywise Error Rate, the probability of at least one false rejection. Specifically, Column (3) uses the StepM method described in Romano, Shaikh, and Wolf (2008) with 10,000 bootstrapped samples and k = 1. Column (4) uses the Holm stepdown method described in Romano, Shaikh, and Wolf (2010). Standard errors reported in parentheses are robust to arbitrary heteroskedasticity.

Appendix Table 9
The Impact of Attending the Promise Academy on Human Capital Inside and Outside the Zone

	Inside Zone	Outside Zone	p-value
Panel A. Achievement	(1)	(2)	(3)
Woodcock Johnson Math	0.328**	0.243**	
	(0.155)	(0.096)	0.641
	108	270	
Woodcock Johnson Reading	0.131	0.116	
	(0.150)	(0.102)	0.935
	108	270	
Regents Passed	2.204***	0.918***	
	(0.564)	(0.326)	0.049
	122	298	
Regents Test Scores	0.635***	0.212*	
	(0.144)	(0.111)	0.021
	115	291	
Achievement Index	0.472***	0.215**	
	(0.139)	(0.090)	0.120
	134	334	
Panel B. Attainment			
Graduated (6 Years)	0.020	0.058	
	(0.103)	(0.052)	0.741
	116	286	
College Enrollment	−0.029	0.098*	
	(0.095)	(0.053)	0.247
	147	381	
Number of College Semesters Enrolled	0.028	0.260*	
	(0.238)	(0.153)	0.412
	147	381	
Attainment Index	−0.029	0.133	
	(0.162)	(0.091)	0.384
	147	381	
Panel C. On Time			
Graduated (4 Years)	0.093	0.189***	
	(0.105)	(0.055)	0.417
	120	300	
Immediate College Enrollment	0.157	0.196***	
	(0.104)	(0.061)	0.747
	120	302	
On-Time Index	0.260	0.391***	
	(0.198)	(0.106)	0.559
	120	302	

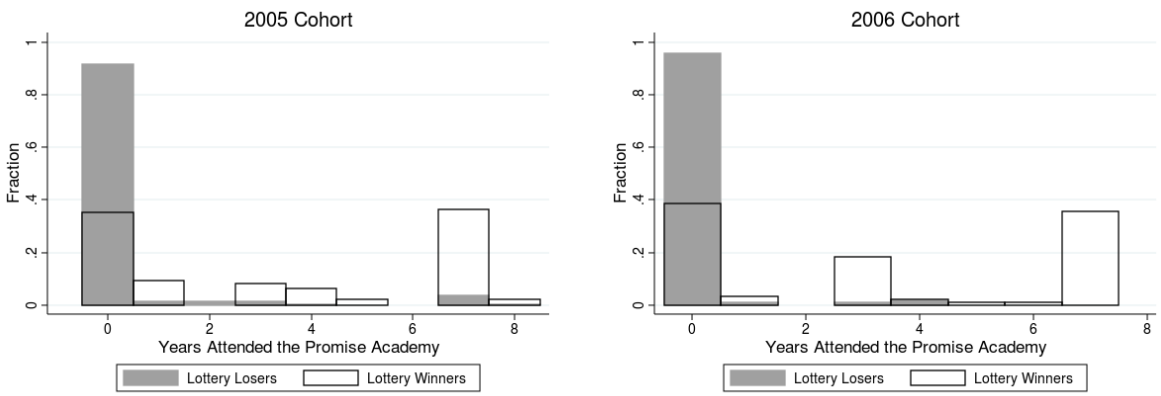
Notes: This Table reports ITT estimates for youth with baseline addresses inside and outside of the Harlem Children's Zone. Column (1) presents ITT estimates for youth living within 400 meters of the original 24-block Zone. Column (2) presents ITT estimates for youth living outside 400 meters of the original 24-block Zone, and Column (3) reports a p-value of a test that the two coefficients are equal. All specifications and variable definitions are identical to those in Table 4. Heteroskedasticity-robust standard errors are reported in parentheses. The number of observations is reported below the standard error. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

Appendix Table 10
The Impact of Attending the Promise Academy on Risky Behaviors and Health Inside and Outside the Zone

	Inside Zone	Outside Zone	p-value
Panel A. Risky Behaviors	(1)	(2)	(3)
Ever Pregnant (Female)	0.023	−0.085	
	(0.133)	(0.059)	0.460
	65	136	
Incarcerated (Male)	−0.046	−0.048**	
	(0.040)	(0.023)	0.960
	54	175	
Drug/Alcohol Index	−0.029	−0.007	
	(0.128)	(0.077)	0.884
	112	286	
Criminal Behavior Index	−0.019	−0.009	
	(0.147)	(0.076)	0.953
	112	287	
Risky Behavior Index	−0.200	−0.123	
	(0.160)	(0.088)	0.671
	122	314	
Panel B. Health			
Nutrition Index	0.088	0.074	
	(0.129)	(0.071)	0.923
	112	287	
Mental Health	0.250	−0.114	
	(0.190)	(0.125)	0.110
	111	284	
Physical Health Index	0.002	−0.051	
	(0.110)	(0.079)	0.694
	112	287	
Health Behavior Index	−0.048	0.058	
	(0.106)	(0.063)	0.394
	112	287	
Health Index	0.079	0.009	
	(0.114)	(0.070)	0.603
	112	287	

Notes: This Table reports ITT estimates for youth with baseline addresses inside and outside of the Harlem Children's Zone. Column (1) presents ITT estimates for youth living within 400 meters of the original 24-block Zone. Column (2) presents ITT estimates for youth living outside 400 meters of the original 24-block Zone, and Column (3) reports a p-value of a test that the two coefficients are equal. All specifications and variable definitions are identical to those in Table 5. Heteroskedasticity-robust standard errors are reported in parentheses. The number of observations is reported below the standard error. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

Appendix Figure 1
Years of Promise Academy Attendance



Notes: These figures plot the number of years lottery winner and lottery losers attend the Promise Academy. See text for details.