Are High-Quality Schools Enough to Close the Achievement Gap? Evidence from a Bold Social Experiment in Harlem*

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Abstract

Harlem Children's Zone® (HCZ) is arguably the most ambitious social experiment to alleviate poverty of our time. We provide the first empirical test of the causal impact of HCZ on educational outcomes, with an eye toward informing the long-standing debate whether schools alone can eliminate the achievement gap or whether the issues that poor children bring to school are too much for educators to overcome. We implement two identification strategies. First, we exploit the fact that HCZ charter schools are required to select students by lottery when the demand for slots exceeds supply. Second, we use the interaction between a student's home address and cohort year as an instrumental variable. Both approaches lead us to the same story: Harlem Children's Zone is enormously effective at increasing the achievement of the poorest minority children. Taken at face value, the effects in middle school are enough to reverse the black-white achievement gap in mathematics and reduce it in English Language Arts. The effects in elementary school close the racial achievement gap in both subjects. Harlem Gems and The Baby College®, the only two community programs in HCZ that keep detailed administrative data, show mixed success. We conclude by presenting three pieces of evidence that high-quality schools or high-quality schools coupled with community investments generate the achievement gains. Community investments alone cannot explain the results.

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At nine months old, there are no detectable cognitive differences between black and white babies (Fryer and Levitt, 2006b). Differences emerge as early as age two, and by the time black children enter kindergarten they are lagging whites by 0.64 standard deviations in math and 0.401 in reading (Fryer and Levitt, 2004).[1] On every subject at every grade level, there are large achievement differences between blacks and whites (Campbell, Hombo, and Mazzeo, 2000; Neal, 2006,) that continue to grow as children progress through school.[2] Even accounting for a host of background factors, the achievement gap remains large and statistically significant (Jencks and Phillips, 1998).[3]

There have been many attempts to close the achievement gap. Early childhood interventions such as Head Start, Nurse-Family Partnership, and the Abecedarian Project boost kindergarten readiness, but the effects on achievement often fade once children enter school (Currie and Thomas, 1995; Krueger and Whitmore, 2001; Anderson, 2008).[4] More aggressive strategies that place disadvantaged students in better schools through busing (Angrist and Lang, 2004) and school choice plans (Rouse, 1998; Krueger and Zhu, 2002; Cullen, Jacob, and Levvit, 2005; Hastings, Kane, and Staiger, 2006; Deming, 2009b), have also left the racial achievement gap essentially unchanged.[5] There are several successful charter schools and charter-management organizations, but the bulk of the evidence finds only modest success (Hanushek, Kain, Rivkin, and Branch, 2005; Hoxby and Rockoff, 2004; Hoxby and Murarka, 2007).[6]

School districts have tried an array of strategies to close the achievement gap, including smaller schools and classrooms (Achilles, Nye, Zharias, and Fulton, 1993; Nye, Fulton, Boyd-Zaharias, and Cain, 1995; Krueger, 1999; Krueger and Whitmore, 2001; Jepsen and Rivkin, 2002), mandatory summer school

[1]Early measures of math and reading achievement are more accurately described as measures of math and reading readiness. The ECLS-K reading test, for example, includes questions designed to measure basic skills (print familiarity, letter and word recognition, beginning and ending sounds, and rhyming sounds), vocabulary, listening and reading comprehension, knowledge of the alphabet, phonetics, and so on. The math test evaluates number recognition, counting, comparing and ordering numbers, solving word problems, and interpreting picture graphs.

[2]This fact was first established by Coleman, Campbell, Hobson, McPartland, Mood, Weinfeld, and York (1966). For more recent analysis, see Campbell et al. (2000), Carneiro and Heckman (2002), Fryer and Levitt (2004, 2006a), Neal (2006), or Phillips, Crouse, and Ralph (1998).

[3]Approximately two-thirds of the achievement gap cannot be explained by background characteristics.

[4] There is some evidence that Head Start, Perry Preschool and Nurse-Family Partnership may have positive long-term impacts on outcomes such as crime, high-school graduation, and labor-market outcomes (Currie and Thomas, 2000; Ludwig and Miller, 2006; Olds, 2006; Deming, 2009a).

[5] In a recent review of education policy focused on poor children, Jacob and Ludwig (2008) find that targeted investment in early childhood education, smaller class sizes, and bonuses for teachers in hard-to-staff schools all pass a cost-benefit analysis, but are unlikely to eliminate the racial and social class disparities in education outcomes by themselves.

[6] Abdulkadiroglu, Angrist, Cohodes, Dynarski, Fullerton, Kane, and Pathak (2009) is a notable exception, finding that students enrolled in oversubscribed Boston charter schools with organized lottery files gain about 0.17 standard deviations per year in ELA, and about 0.53 standard deviations per year in math. This may not be a representative sample of Boston Charter schools, however. The typical charter school applicant in Abdulkadiroglu et al. (2009) scores 0.173 and 0.277 standard deviations above typical Boston students on their fourth grade math and reading tests, and high school applicants score 0.089 and 0.179 standard deviations higher than average on their eighth grade math and reading tests.

(Jacob and Lefgren, 2004) merit pay for principals, teachers and students (Podgursky and Springer, 2007; Fryer, 2009a-e), after-school programs (Lauer, Akiba, Wilkerson, Apthorp, Snow, and Martin-Glenn, 2006; Redd, Cochran, Hair, and Moore, 2002), budget, curricula, and assessment reorganization (Borman and Hewes, 2003; Borman, Slavin, Cheun, Chamberlain, Madden, and Chambers, 2007; Cook, Hunt, and Murphy, 2000), and policies to lower the barrier to teaching via alternative paths to accreditation (Decker, Mayer, and Glaserman, 2004; Kane, Rockoff, and Staiger, 2008). These programs have thus far been unable to overcome the achievement gap in even the most reform-minded of districts.

The lack of progress has fed into a long-standing and rancorous debate among scholars, policymakers, and practitioners as to whether schools alone can close the achievement gap, or whether the issues children bring to school as a result of being reared in poverty are too much for even the best educators to overcome.[7] Proponents of the school-centered approach refer to anecdotes of excellence in particular schools or examples of other countries where poor children in superior schools outperform average Americans (Chenoweth, 2007). Advocates of the community-focused approach argue that teachers and school administrators are dealing with issues that actually originate outside the classroom, citing research that shows racial and socioeconomic achievement gaps are formed before children ever enter school (Fryer and Levitt, 2004, 2006b) and one-third to one-half of the gap can be explained by family-environment indicators (Phillips et al., 1998; Fryer and Levitt, 2004).[8] In this scenario, combating poverty and having more constructive out-of-school time may lead to better and more-focused instruction in school. Indeed, Coleman et al. (1966), in their famous report on equality of educational opportunity, argued that schools alone cannot treat the problem of chronic underachievement in urban schools.

Harlem Children's Zone is a 97-block area in central Harlem, New York, that combines reform-minded charter schools with a web of community services created for children from birth to college graduation that are designed to ensure the social environment outside of school is positive and supportive. This provides a unique laboratory to understand whether communities, schools, or a combination of the two are the main drivers of student achievement. The answer to this question is of tremendous importance for domestic policy as it goes to the heart of how communities and public goods should be allocated to alleviate racial and economic inequality.

[7] Interest groups such as the Education Equality Project (http://www.educationequalityproject.org/) and the Broader, Bolder Approach (http://www.boldapproach.org/) embody this debate.

[8] The debate over communities or schools often seems to treat these approaches as mutually exclusive, evaluating policies that change one aspect of the schools or a student's learning environment. While informative on the various partial derivatives of the educational production function, this is uninformative on the net effect of many simultaneous changes. The educational production function may, for example, exhibit either decreasing or increasing returns to various reforms. Smaller classes and more time on task matter more (or less) if the student has good teachers; good teachers may matter more (or less) if the student has a good out of school environment, and so on.

We use two separate statistical strategies to account for the fact that students who attend HCZ schools are not likely to be a random sample. First, we exploit the fact that HCZ charter schools are required to select students by lottery when the number of applicants exceeds the number of available slots for admission. In this scenario, the treatment group is composed of students who are lottery winners and the control group consists of students who are lottery losers. This allows us to provide a set of causal estimates of the effect of being offered admission into the HCZ charter schools on a range of outcomes, including test scores, attendance, and grade completion.

There are two important caveats to our lottery identification strategy. First, the HCZ middle school was not significantly oversubscribed in their first year of operation, and the HCZ elementary schools have never been significantly oversubscribed, making it more difficult to estimate the effect of being offered admission for these groups. Second, results from any lottery sample may lack external validity. The counterfactual we identify is for students who are already interested in charter schools. The effect of being offered admission to HCZ for these students may be different than for other types of students.

To complement the lotteries, our second identification strategy uses the interaction between a student's home address and her cohort year as an instrumental variable. This approach takes advantage of two important features of the HCZ charter schools: (1) anyone is eligible to enroll in HCZ's schools, but only students living inside the Zone are actively recruited by the HCZ staff; and (2) there are cohorts of children that are ineligible due to the timing of the schools' inception and their age. Our identification is driven by the between-cohort comparison of outcomes within the Zone, using the outcomes of children outside the Zone to control for natural year-to-year variation in test scores. If the interaction between a student's address and cohort only affects his or her outcome through its effect on enrollment in the charter school, this provides another set of causal estimates.

Both statistical approaches lead us to the same basic story. Harlem Children's Zone is enormously effective at increasing the achievement of the poorest minority children. Students enrolled in the sixth grade gain more than a full standard deviation in math and between one-third and one-half of a standard deviation in English Language Arts (ELA) by eighth grade. Taken at face value, these effects are enough to reverse the black-white achievement gap in mathematics (HCZ students outperform the typical white student in New York City and the difference is statistically significant) and reduce it in ELA. Students in the HCZ elementary school gain approximately one and three-quarters of a standard deviation in both math and ELA, closing the racial achievement gap in both subjects. HCZ students are also less likely to be absent and are somewhat more likely to matriculate on time. These results are robust across identification strategies, model specifications, and subsamples of the data. Surprisingly, students of all ability levels obtain roughly the same benefit from attending HCZ charters. The only meaningful difference we discovered between subsamples is that boys gain significantly more in math than girls.

The Baby College and Harlem Gems, the only community programs that collect detailed information on attendees, do not admit participants by lottery or admit in such a way that one can clearly identify their causal impact. Additionally, the IV strategy employed earlier does not have enough power in small samples. Least squares estimates show that enrollment in Harlem Gems is associated with scoring 0.227 to 0.232 standard deviations higher in math, and 0.238 to 0.318 higher in ELA among students who applied to the HCZ elementary school, but these results should be considered suggestive because they are measured imprecisely. We fail to find any empirical evidence that The Baby College is positively associated with achievement at age four or eight across multiple samples and datasets.

Beyond The Baby College and Harlem Gems, there are three additional pieces of evidence that suggest that the combined effects of the community programs alone are not responsible for the gains in achievement. First, our IV strategy purges the effects of the community bundle, yet is larger than the available lottery estimates. Second, students outside the Zone garner the same benefit from the schools as the students inside the Zone, suggesting that proximity to the community programs is unimportant. Third, the Moving to Opportunity (MTO) experiment relocated individuals from high-poverty to low-poverty neighborhoods (while keeping children in roughly similar schools) and showed small positive results for girls and negative results for boys (Sanbonmatsu, Kling, Duncan, and Brooks-Gunn, 2006; Kling, Liebman, and Katz, 2007). Additionally, and more speculative, there is substantial anecdotal evidence that Harlem Children's Zone was unsuccessful in the years before opening the charter schools. *We cannot, however, disentangle whether communities coupled with high-quality schools drive our results, or whether the high-quality schools alone are enough to do the trick.*

The paper is structured as follows. Section II provides a brief overview of Harlem Children's Zone. Section III introduces the data and econometric framework. Section IV presents estimates of the impact of four key elements of HCZ on educational outcomes. Section V discusses whether communities, schools, or both are most responsible for the results. Section VI concludes. There are two appendices: Appendix A outlines each program offered by Harlem Children's Zone. Appendix B is a data appendix that details our sample and variable construction.

II. Harlem's Children Zone

Harlem Children's Zone began in 1970 as an amalgam of after-school programs, truancy-prevention services, and anti-violence training for teenagers in schools, named the Rheedlen Centers for Children and Families. The disintegration of central Harlem during the crack epidemic of the 1980s and 1990s prompted some within the organization to question their piecemeal methodology – several programs, but no comprehensive strategy. In response, the president of Rheedlen, Geoffrey Canada, created the Harlem Children's Zone to address all the problems that poor children were facing – from crumbling apartments to failing schools and violent crime to chronic health problems – through a "conveyor belt" of services from birth to college. 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. Figure 1 provides an aerial map of Harlem Children's Zone and its expansion path.

HCZ offers a number of programs, which we have partitioned into "community" investments and "school" investments. Community programs are available to anyone living near HCZ, and serve over 7,400 children and over 4,100 adults. School programs are only available to the approximately 1,300 students who attend the HCZ charter schools.

Community Investments

HCZ has over 20 programs designed to help and empower individuals in their 97 blocks. These investments include early childhood programs (Head Start, e.g.), public elementary-, middle- and high-school programs (i.e. karate, dance, after-school tutoring), a college-success office, family, community and health programs, foster-care prevention services, and so on. Appendix A provides a description of all programs run by Harlem Children's Zone. 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 supportive adults. This is consistent with the vision articulated by scholars and policymakers who argue that communities, not schools, are responsible for the achievement gap. Below, we describe, in greater detail, two of the most ambitious community programs, and the only two for which it is possible to perform statistical analysis at this time.

The Baby College, a signature community initiative and the first program to be started, is a nine-week parenting workshop for expectant parents and those with children up to three years old. The curriculum, based on the work of T. Berry Brazleton, covers a broad range of subjects, including brain development, discipline, immunization, safety, asthma, lead poisoning, parental stress, and parent-child bonding (Brazelton, 1992). The College, held on Saturday mornings with weekly follow-up visits at home, is free of charge and offers parents a number of incentives to attend, including free breakfast, lunch, and daycare, as well as drawings for prizes such as gift certificates at local stores or a month's free rent. Anyone with children up to three years old is eligible to attend The Baby College, but tremendous effort is made to recruit parents from inside the Zone (Tough, 2008). Thirteen HCZ employees canvass apartment buildings, seek suggestions for good candidates from past Baby College participants, and visit laundromats, supermarkets and check-cashing outlets within the Zone in search of possible enrollees, including revisiting people who indicated they were interested, but never enrolled.

Another major community initiative is Harlem Gems, an intensive, all-day, pre-kindergarten program with a 4:1 child-to-adult ratio that focuses on kindergarten preparation. The curriculum is designed to increase socialization skills, build routines, and begin development of the pre-literacy and language skills students need in kindergarten. The Gems program incorporates a number of nontraditional subjects such as Spanish and French, and strongly encourages parents to volunteer at the school and become more involved in their child's education. Each Gems classroom employs a Family Worker, whose primary responsibility is ensuring that parents are active and involved. There are monthly parent meetings, book nights, and field trips that parents are encouraged to attend, and between one and five monthly parenting workshops on a wide range of topics, including some designed purely for parental involvement (e.g. a knitting club). [9]

School Investments

The Promise Academy charter schools began in the fall of 2004 with the opening of the Promise Academy 1 elementary and middle schools, followed in the fall of 2005 with the opening of the Promise Academy 2 Elementary School.[10] The Promise Academies will enroll a new kindergarten and sixth-grade cohort each year until it is a full K-12 set of charters.[11]

The Promise Academies have 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. Our rough estimate is that Promise Academy students that are behind grade level are in school for twice as many hours as a typical public school student in New York City. Students who are at or above grade level still attend the equivalent of about fifty percent more school in a calendar year.

Both schools emphasize the recruitment and retention of high-quality teachers and use a test-score value-added measure to incentivize and evaluate current teachers. The schools have had high turnover as they search for the most effective teachers: 48 percent of Promise Academy teachers did not

[9] There is also a Head Start version of Harlem Gems, which follows a similar model except that (1) children can enter the Head Start at three or four years of age, thus a sizable proportion of the participants receive two years of instruction, and (2) because of income guidelines, students in Head Start tend to come from families with lower socioeconomic status than that of Universal Pre-K participants. In all versions of the program, the students are most often referred from The Baby College or recruited from within the HCZ borders.

[10]Promise Academy 2 held both a kindergarten and first-grade lottery their first year, enrolling 40 students in each grade. After their first year of operation, Promise Academy 2 relocated, and in the process lost a number of students. To simplify our analysis and abstract from the issues created by this relocation, we focus our analysis on Promise Academy 1.

[11]In the fall of 2007, Promise Academy 1 did not enroll a new sixth-grade cohort, and expelled the entire existing eighth-grade cohort (the 2004 lottery cohort). HCZ was unhappy with the performance of the middle school to this point, and the decision was made to focus on the existing seventh- and eighth-grade cohorts in reforming the school. Our data, which were public months after the decision was made, shows that the 2004 cohort did quite well. In 2008, the lottery was moved back to fifth grade (Tough, 2008).

return for the 2005 – 2006 school year, 32 percent left before 2006 – 2007, and 14 percent left before 2007 – 2008. Each teacher has an annual meeting with Geoffrey Canada to discuss their performance, and is supported by myriad behind-the-scenes efforts to make sure their time is spent primarily on teaching and not administrative tasks.

The schools provide free medical, dental and mental-health services (students are screened upon entry and receive regular check-ups), student incentives for achievement (money, trips to France, e.g.), high-quality, nutritious, cafeteria meals, support for parents in the form of food baskets, meals, bus fare, and so forth, and less tangible benefits such as the support of a committed staff. The schools also make a concerted effort to change the culture of achievement, surrounding students with the importance of hard work in achieving success. These types of school policies are consistent with those that argue high-quality schools are enough to close the achievement gap.

Anecdotal Evidence of Success

A slew of anecdotal evidence suggests that the HCZ approach is working. For six straight years, 100 percent of pre-kindergarteners in the Harlem Gems program were school-ready. Eighty-one percent of parents who have attended The Baby College report reading to their child more often than they previously read to them. Older students in the HCZ charter schools typically outperform their peers at neighboring schools, and, most recently, three young women from the HCZ chess program won the national championship for their age group. In the latest progress report from the New York City Department of Education, the Promise Academies received an A.

One has to take the above evidence with a grain of salt – children who participate in HCZ are not a random sample of students. Children may enroll in the Zone's programs because they are already struggling or under-performing in public schools, because they are identified as being particularly at risk by a teacher or counselor, or because they or their parents are highly motivated to seek out the best opportunities. In other words, students served by HCZ are likely to be self-selected, either positively or negatively, and results that simply compare outcomes of HCZ children to the outcomes of other children in Harlem may be biased in one direction or another.

III. Data and Econometric Framework

We merge data from two sources: information from files at Harlem Children's Zone and administrative data on student demographics and outcomes of the approximately 1.1 million students enrolled in New York City Public Schools from the New York City Department of Education (NYCDOE).

The data from Harlem Children's Zone consist of enrollment files from The Baby College and Harlem Gems community programs and lottery results from the Promise Academy charter schools.[12] A typical student's data include her first name, last name, birth date, parents' or guardians' names, home address, and the program to which she is applying or attending. With the exception of The Baby College, the HCZ data are remarkably complete. The Baby College data are more sparse because almost 18 percent of children had not been born at the time of data collection! In these cases "expected named" and due date were collected.

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 status, behavior, attendance, and matriculation with course grades for all students and state math and ELA test scores for students in grades three through eight. The data also include a student's first and last name, birth date, and address. We have NYCDOE data spanning the 2003 – 2004 to 2007 – 2008 school years.[13]

The state math and ELA tests, developed by McGraw-Hill, are high-stakes exams conducted in the winters of third through eighth grade.[14] Students in third, fifth, and seventh grades must score proficient or above on both tests to advance to the next grade. The math test includes questions on number sense and operations, algebra, geometry, measurement, and statistics. Tests in the earlier grades emphasize more basic content such as number sense and operations, while later tests focus on advanced topics such as algebra and geometry. The ELA test is designed to assess students on three learning standards – information and understanding, literary response and expression, critical analysis and evaluation – and includes multiple-choice and short-response sections based on a reading and listening section, along with a brief editing task. [15]

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. Students with moderate disabilities or who are Limited English Proficient must take both tests, but may be granted special accommodations (additional time, translation services, and so on) at the discretion of school or state administrators. In our analysis the test scores are normalized to have a mean of zero and a standard deviation of one for each grade and year.

We also construct measures of absenteeism and matriculation using the NYCDOE data. Absenteeism is measured as the total number of absences a student accumulates during the school year.

[12]The exception being the 2004 middle-school cohort, for which the lottery numbers were lost and the 2006 elementary-school lottery cohort, which is still being compiled by HCZ staff.
[13] In August 2009, we will obtain 2009 statewide test scores and re-run our analysis.
[14] Sample tests can be found at http://www.emsc.nysed.gov/osa/testsample.html
[15] Content breakdown by grade and additional exam information is available at http://www.emsc.nysed.gov/osa/pub/reports.shtml

Matriculation is an indicator for whether a student is "on-time" given her expected grade. In our elementary-school sample a student's expected grade is imputed from her initial kindergarten year. If a student enters kindergarten in 2003 – 2004, she is on time if enrolled in first grade in 2004 – 2005, second grade in 2005 – 2006, and so on. If we do not observe an elementary student in kindergarten, we impute the expected grade from the student's birth date. In our middle-school sample a student's expected grade is imputed from her initial sixth-grade year.[16] If we do not observe a student in sixth grade, we impute an expected grade from the nearest observed grade.

The HCZ data were matched to the New York City administrative data using the maximum amount of information available. Match keys were used in the following order: (1) last name, first name, date of birth with various versions of the names (abbreviations, alternative spellings, hyphenated vs. non-hyphenated); (2) last name, first name, and various versions of the date of birth (most often the month and day reversed); (3) last name, first name, prior school, and prior grade with various likely adjustments to prior grade; (4) name, date of birth, and prior grade. Once these match keys had been run, the remaining data were matched by hand considering all available variables. Match rates were 86.5 percent for the Harlem Gems data (N=452), 92.6 percent for the winners of the kindergarten lottery (N=203), 89.2 percent for the losers of the kindergarten lottery (N=195), 90.5 percent for the winners of the middle-school lottery (N=200), and 85.5 percent for the losers of the middle-school lottery (N=352).[17] These numbers are comparable to the match rates achieved by others using similar data (Hoxby and Murarka, 2007).

Match rates for The Baby College were significantly lower. Recall, approximately 18 percent of the Baby College sample was not born yet, necessitating that we match on expected name and due date. Another 20 percent of The Baby College sample did not have age or birth date information. Finally, a number of Baby College participants were too young to be in our NYCDOE dataset. Within participants with complete information and of appropriate age, we were able to match 66 percent (N=614) of our sample. As Baby College participants have three to six years to attrite before we observe them in the NYCDOE data, this is consistent with our expected match rate.

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[16] In our sample, the majority of students has already been held back by sixth grade. As a result, a matriculation measure based on kindergarten enrollment is relatively uninformative.

[17] If nonrandom attrition is driving the difference in match rates in our middle-school sample, our results may be biased. We explore the sensitivity of our results by creating lottery-lottery losers to equalize the match rates for each cohort. These pseudo-students are assigned the highest test score observed in the control group for that year (about two standard deviations above the mean). The results of this exercise are available in Appendix Table 2. The estimated effect of attending the Promise Academy Middle School remain qualitatively similar, with ITT and TOT effects only slightly smaller than the main lottery results reported in Table 3. We also note that our IV estimates using the interaction between a student's cohort and address should be unaffected by the difference in match rates as the identification is driven by cross-cohort and in-Zone versus out-Zone comparisons only.

Using the student addresses provided by the NYCDOE, we also calculated the distance from each student's home to the nearest point on the boundary of the Harlem Children's Zone using arcGIS. When multiple addresses were available for a single student, we use the earliest available address.

Summary statistics for the variables that we use in our core specifications are displayed in Table 1. Students who entered the elementary- or middle-school lottery are more likely to be black, but all other variables, including eligibility for free or reduced-price lunch, are similar to the typical New York City student. Students enrolled in the school lottery score 0.287 standard deviations and 0.266 standard deviations below the typical New York City student in math and ELA respectively, but outscore the typical student living in the Zone by 0.186 in math (p-value = 0.001) and 0.062 in ELA (p-value = 0.289).

Econometric Approach to Estimating the Causal Impact of Harlem Children's Zone

The simplest and most direct test of any HCZ program would be to examine the outcome of interest regressed on an indicator for enrollment in the particular program (HCZ_i) and controls for basic student characteristics (X_i):

$$outcome_i = \alpha + \beta X_i + \gamma HCZ_i + \varepsilon_i.$$

If students select into the programs because of important unobserved determinants of academic outcomes, such estimates may be biased. To confidently identify the causal impact of HCZ enrollment, we must compare students with different enrollment statuses who would have had the same academic outcomes had they both been enrolled or not enrolled in the program. By definition, this involves a counterfactual we cannot observe.

In our analysis, we construct the counterfactual in two ways. First, we exploit the fact that HCZ charter schools are required to select students by lottery when demand exceeds supply, and, second, by using the interaction between a student's home address and cohort year as an instrumental variable.

New York law dictates that over-subscribed charter schools allocate enrollment offers via a random lottery. Restricting our analysis to students who entered the HCZ lottery, we can estimate the causal impact of being offered admission into the charter school by comparing average outcomes of students who 'won' the lottery to the average outcomes of students who 'lost' the lottery. The lottery losers therefore form the control group corresponding to the counterfactual state that would have occurred for students in the treatment group if they had not been offered a spot at the charter school.

Let Z_i be an indicator for a winning lottery number. The mean difference in outcomes between the lottery winners (Z_i = 1) and lottery losers (Z_i = 0) is known as the "Intent-to-Treat" (ITT) effect, and is estimated by regressing student outcomes on Z_i. In theory, predetermined student characteristics (X_i) should have the same distribution within the lottery winners and losers because they are statistically independent of group assignment. In small samples, however, more precise estimates of the ITT can often be found by controlling for these student characteristics (X_i). The specifications estimated are of the form:

(1) $$outcome_i = \alpha_1 + X_i\beta_1 + Z_i\gamma_1 + \varepsilon_{1i}$$

where our vector of controls, X_i, includes an indicator variable for gender, a mutually inclusive and collectively exhaustive set of race dummies, an indicator for free lunch, and predetermined measures of the outcome variable when possible (i.e. pre-lottery test scores).[18]

The ITT is an average of the causal effects for students who enroll in the charter school and those that do not. The ITT therefore captures the causal effect of being offered a spot in the HCZ charter school, not of actually attending.

Under several assumptions (that the treatment group assignment is random, lottery losers are not allowed to enroll, and winning the lottery only affects outcomes through charter school enrollment), we can also estimate the causal impact of actually enrolling in the HCZ charter school.[19] This parameter, commonly known as the "Treatment-on-Treated" (TOT) effect, measures the average effect of enrollment on lottery winners who choose to attend the HCZ charter school. The TOT parameter can be estimated though a two-stage least squares regression of student outcomes on enrollment (HCZ_i) with lottery status (Z_i) as an instrumental variable for enrollment:

(2) $$outcome_i = \alpha_2 + \beta_2 X_i + \gamma_2 HCZ_i + \varepsilon_{2i}$$

The TOT is the estimated difference in outcomes between students who actually enroll in the charter school and those in the control group who would have enrolled if they had been offered a spot.

A key concern in estimating the TOT is how to account for students who leave the charter school. Over time, it is inevitable that students leave the HCZ charter schools, either through natural attrition or

[18] A student is income-eligible for free lunch if her family income is below 130 percent of the federal poverty guidelines, or categorically eligible if (1) the student's household receives assistance under the Food Stamp Program, the Food Distribution Program on Indian Reservations (FDPIR), or the Temporary Assistance for Needy Children Program (TANF); (2) the student was enrolled in Head Start on the basis of meeting that program's low-income criteria, (3) the student is homeless, (4) the student is a migrant child, or (5) the student is a runaway child receiving assistance from a program under the Runaway and Homeless Youth Act and is identified by the local educational liaison. A student is eligible for reduced-price lunch if family income is between 130 and 185 percent of federal poverty guidelines. Approximately 70 percent of our sample is eligible for free lunch, with another ten percent eligible for reduced-price lunch.

[19] The assumptions necessary to estimate the TOT generally hold for the Middle School lotteries. One to two students in each cohort enroll despite having "losing" lottery numbers due to clerical errors (private correspondence with HCZ), but otherwise the lottery is binding. These assumptions do not hold, however, for the kindergarten lotteries. The kindergarten lotteries were not significantly oversubscribed, and as a result, nearly every student was eventually offered a spot at the charter school. While we cannot estimate the ITT and TOT effects for these cohorts, we are able to use a student's position on the waitlist as an instrumental variable and estimate a local average treatment effect of attending the school. This is discussed further in the results section.

because they are asked to leave. First- (and second-) year attrition from the Promise Academy is 8.5 (17.6) percent for the elementary school and 15.6 (26.4) percent for the middle school, compared to 9.7 (17.0) percent for KIPP Star – a popular charter middle school two blocks from the western boundary of the Zone – and 16.4 (30.3) for the middle schools most demographically similar to HCZ. Demographically similar elementary schools all experience first-year attrition of over 40 percent. [20]

If attrition is nonrandom, the TOT estimates may be biased if we estimate the effect of current enrollment. We therefore let HCZ_i be an indicator for a student ever having been enrolled at the charter school, regardless of her current enrollment status. In the current paper, this will serve as a lower-bound estimate of the true effect. If we used the indicator as current enrollment (as others in the literature have done, see, for example, Abdulkadiroglu et al., 2009) we get TOT estimates that are approximately six percent higher than those reported.

To compliment the lotteries, our second statistical strategy exploits the interaction between a student's home address and cohort year as an instrumental variable. Two forces drive our identification. First, we compare outcomes between children living in the Zone who were eligible for its charter schools and students living in the Zone who were not eligible. For example, students who started kindergarten in 2003 were ineligible for the HCZ charter school, which began enrolling kindergarten students in 2004. As the 2003 cohort is likely to be quite similar to the 2004 cohort, they provide a plausible counterfactual. Second, we compare the outcomes of children living outside the Zone in the two cohorts to adjust for year-to-year shocks that may come about through broad city-wide reforms.

The basic logic and evidence for our IV strategy is illustrated in Figure 2, which plots the percentage of students enrolled in the HCZ charter schools and mean test scores for two successive cohorts[21]. In both panels, the vertical axis represents enrollment rates (the denominator is all children in the specified area) and the horizontal axis represents distance in meters to the nearest point on the Zone's border. The left panel is the 2004 cohort who are age-eligible for the Promise Academy; the right panel contains the 2003 cohort who was born a year too early and were already in kindergarten when the lottery commenced. Our identification strategy compares the outcomes of children in the Zone between the

[20] First-year attrition in elementary school is defined as the percent of students who leave between kindergarten and first grade. In the HCZ and public middle schools, attrition is defined as the percent of students who leave between sixth and seventh grades, and in KIPP Star it is defined as the percent that leave between fifth and sixth grades. Demographically similar public schools were chosen by minimizing the weighted difference in available demographic variables and previous test scores between sample schools and the Promise Academies. Weights were determined by regressing a previous cohort's demographic variables on test scores.

[21] In our elementary-school sample a student's cohort is imputed from her initial kindergarten year. If we do not observe an elementary student in kindergarten, we impute the expected grade from the student's birth date. In our middle-school sample a student's cohort is imputed from her initial sixth-grade year. If we do not observe a student in sixth grade, we impute an expected grade from the nearest observed grade.

eligible and ineligible cohorts, letting the age-ineligible students form the counterfactual of what would have occurred had the age-eligible children not enrolled in the charter school. We also compare children outside the Zone to adjust for year-to-year achievement shocks.

Let enrollment in HCZ programs be a function of student characteristics (X_i), home address ($inZone_i$), cohort year ($Cohort_i$), and the interaction between address and cohort year:

(3) $$HCZ_i = \alpha_3 + \beta_3 X_i + \gamma_3 inZone_i + \delta_3 Cohort_i + \eta_3 (inZone_i * Cohort_i) + \varepsilon_{3i}$$

The residual of this representative "first-stage" equation (3) captures other factors that are correlated with enrollment in HCZ that may be related to student outcomes. The key identifying assumptions of our approach is that (1) the interaction between address and cohort year is correlated with enrollment, and (2) the interaction between address and cohort year only affects student outcomes through its effects on the probability of enrollment, not through any other factor or unobserved characteristic.

The first assumption is econometrically testable. Appendix Table 1 presents results from representative first-stage regressions, where enrollment in the HCZ charter school at kindergarten and sixth grade are regressed on student race, lunch status, address, cohort year and the interaction between address and cohort year. For kindergarten enrollment, four of the seven interactions are significant at the one-percent level, one at the five-percent level and one at the ten-percent level. For sixth-grade enrollment, four of the six interactions are significant at the one-percent level with another significant at the ten-percent level. For both regressions, a joint F-test with the null that the interactions are jointly equal to zero is strongly rejected (p-value = 0.000).

The validity of our second identifying assumption – that the instrument only affects student outcomes through the probability of enrollment - is more difficult to assess. To be violated, the interaction between a student's address and cohort year must be correlated with her outcomes after controlling for the student's background characteristics, address and cohort year. This assumes, for instance, that parents do not selectively move into the Children's Zone based on their child's cohort. Given that all children, regardless of their address, are eligible for HCZ programs, this seems a plausible assumption. Motivated parents can enroll their children in the programs no matter where they live; the relationship between distance to the Zone and enrollment comes about primarily through increased knowledge about the programs or cost of attending, not eligibility. We also assume that shocks either affect everyone in a given cohort regardless of address, or affect everyone at a given address regardless of cohort. If there is a something that shifts achievement test scores for third graders living inside the

Children's Zone, but not third graders outside the Zone or fourth graders inside the Zone, our second identifying assumption is violated.[22]

Under these assumptions (and a monotonicity assumption that being born into an eligible cohort does not make a student less likely to enroll) we can estimate the causal impact of enrolling in the charter school. The identified parameter, commonly known as the local average treatment effect (LATE), measures the average effect of treatment for students induced into enrollment by the instrument (Imbens and Angrist, 1994). The LATE parameter is estimated though a two-stage least squares regression of student outcomes on enrollment (HCZ_i) with the interaction between address and cohort as an instrumental variable for enrollment.

V. The Impact of Four Key Elements of Harlem Children's Zone on Student Achievement

Promise Academy – Middle School

Figures 3-5 provide a visual representation of our basic results from Promise Academy – Middle School. Figure 3A plots yearly, raw, mean state math test scores, from fourth to eighth grade, for four subgroups: lottery winners in 2005, lottery losers in 2005, white students in New York City public schools and black students in New York City public schools.[23] Lottery winners are comprised of students who either won the lottery, were in the top ten of individuals on the wait list, or who had a sibling that is already enrolled in the Promise Academy. Lottery losers are individuals who lost the lottery and were eleven or below on the waiting list.[24] Technically, these represent ITT estimates.

In fourth and fifth grade, before they enter the middle school, math test scores for lottery winners, losers, and the typical black student in New York City are virtually identical, and roughly 0.75 standard deviations behind the typical white student.[25] Lottery winners have a modest increase in sixth grade, followed by a more substantial increase in seventh grade and dramatic gains by their eighth-grade year.

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[22] For our second identifying assumption to be valid, there should be no difference in the relationship between eligible and ineligible students living inside the Zone and the relationship between eligible and ineligible students living outside the Zone ($Eligible_{in}$-$Ineligible_{in}$= $Eligible_{out}$-$Ineligible_{out}$). Appendix Table 3 presents a partial test of this assumption. Columns 1 through 4 and 6 through 9 present basic demographic statistics for the IV sample. Columns 5 and 10 present p-values from a test with the null hypothesis that the difference between eligible and ineligible cohorts is the same for students in the Zone and out of the Zone. Ineligible students living outside the Zone are more likely to be eligible for reduced price lunch. There are no other statistically significant differences.

[23] The effect of receiving a winning lottery number is generally larger for students in the 2006 cohort, though we only observe sixth and seventh grade scores for these students and so decided not to show it in our figures. Figures from the 2006 cohort are available from the authors upon request.

[24] Given the size of the estimated treatment effect, our results are robust to many definitions of "lottery winner."

[25] This is similar in magnitude to the math racial achievement gap in nationally representative samples (0.882 in Fryer and Levitt, 2006b, and 0.763 in Campbell et al., 2000).

The TOT estimate, which is the effect of actually attending the Promise Academy Middle School, is depicted in panel B of Figure 3. As we directly observe test scores of lottery winners who enroll and we have a TOT estimate, we can estimate the mean test score for those in the control group who would have enrolled if they had won the lottery (Kling et al., 2007). The TOT results follow a similar pattern, showing remarkable convergence between children in the middle school and the average white student in New York City. After three years of "treatment," Promise Academy students have completely closed the achievement gap in math – they are outperforming their white counterparts by 0.173 standard deviations (p-value = 0.154). If one adjusts for gender and free lunch, the typical eighth grader enrolled in the HCZ middle school outscores the typical white eighth grader in New York City public schools by 0.432 standard deviations (p-value = 0.000). Put differently, the typical student upon entering the HCZ charter school was at the 20th percentile of the white distribution. After three years, the typical student is at the 55^{th} percentile.

Figure 4A plots yearly state ELA test scores, from fourth to eighth grade, for the 2005 cohort. Treatment and control designations are identical to figure 3A. In fourth and fifth grade, before they enter the middle school, ELA scores for lottery winners, losers, and the typical black student in NYC are not statistically different, and roughly 0.50 standard deviations behind the typical white student.[26] Lottery winners and losers have very similar ELA scores from fourth through seventh grade. In eighth grade, Promise Academy students distance themselves from the control group. These results are statistically meaningful, but much less so than the math results. The TOT estimate, depicted in panel B of Figure 4, follows an identical pattern with marginally larger differences between middle-school students and the control group. Adjusting for gender and free lunch pushes the results in the expected direction.

Interventions in education often have larger impacts on math scores as compared to reading or ELA scores (e.g. Decker et al., 2004; Rockoff, 2004; Jacob, 2005). This may be because it is relatively easier to teach math skills, or that reading skills are more likely to be learned outside of school. Another explanation is that language and vocabulary skills may develop early in life, making it difficult to impact reading scores later (Hart and Risley, 1995).

Thus far, we have concentrated solely on the average student. It is important to decipher which students benefit the most from being offered attendance to Promise Academy middle schools. The distributions of test scores, by treatment status, are plotted in Figures 5A and 5B. The modest gains in sixth-grade math appear to come primarily from the left tail of the distribution. By seventh and eighth grade, however, the lottery winner and lottery loser score distributions have noticeably separated at every point in the distribution. The gains are widespread across ability level and nearly every student gains

[26] This is smaller than the reading racial achievement gap in some nationally representative samples (0.771 in Fryer and Levitt, 2006b, and 0.960 in Campbell et al., 2000).

from the middle-school experience. In contrast, gains in eighth-grade ELA scores appear to come primarily from the left tail of the distribution.

Table 2 provides some concrete numbers, and, more importantly, standard errors to the figures just described. Columns 1 and 2 present ITT and TOT estimates of the 2005 cohort. Columns 3 through 6 provide identical estimates for the 2006 cohort and the pooled sample of 2005 and 2006 cohorts, respectfully. Each row represents a different outcome of interest, including math and ELA achievement scores from standardized statewide exams, an indicator for whether or not a student is scoring on grade level, absences, and whether or not a student matriculates on time.

A 2005 lottery winner scores 0.190 standard deviations higher in sixth-grade mathematics, 0.286 standard deviations higher in seventh grade, and 0.733 standard deviations in eighth-grade math relative to lottery losers. Lottery winners are not significantly more likely to be on grade level in mathematics in sixth grade, but are 22.9 percent more likely in seventh grade, and 42.0 percent in eighth grade. ELA achievement is strikingly similar between lottery winners and losers until their eighth-grade year, where admission into Promise Academy middle schools causes a 0.239 increase in ELA achievement. The effect of actually attending the Promise Academy – the TOT estimate – is proportionately larger. A 2005 enrollee scores 0.279 standard deviations higher in sixth-grade mathematics, 0.438 higher in seventh-grade math, 1.112 higher in eighth-grade math, and 0.363 higher in eighth-grade ELA relative to non-enrollees.

As previously stated, Promise Academy children spend nearly twice as much time in school as the typical public-school student. Despite this, lottery winners are absent less than the control group in every grade – 2.230 days in sixth grade, 5.267 days in seventh grade, and 6.253 days in eighth grade. There are no statistically significant differences in matriculation between the lottery winners and lottery losers. In most cases, the magnitude of the results in the 2006 cohort is bigger than the 2005 cohort. The pooled sample, mechanically, falls in between these two sets of estimates. The advantage of the pooled sample is that the standard errors are slightly lower.

Table 3 presents instrumental variable results for our middle-school cohort. Our instrumental-variables strategy is most useful in examining programs that do not have oversubscribed lotteries, but it can also provide a useful robustness check of our lottery estimates, tests the external validity of our lottery estimates, and allows us to include the 2004 middle-school cohort whose lottery numbers were lost. A key assumption in our instrumental-variables analysis is that students outside the Zone provide an effective control for year-to-year variation (changes in the test, new community programs, and so on). If shocks are local, it is preferable to restrict our sample to students who live very close to the Zone. If shocks are more widespread, however, expanding the number of students in our sample will increase precision without biasing our results. We have no prior assumptions on the proximity of the shocks, so we experiment with a number of specifications, including samples that restrict to students within 800, 1600 and 2400 meters of the Zone. In practice, the results are qualitatively similar across the reported samples.[27]

Each entry in Table 3 is an estimate of the effect of being enrolled at the Promise Academy Charter School in sixth grade on the reported outcome, with standard errors clustered at the cohort level in parentheses. Columns 1 through 3 of Table 3 demonstrate that the effect on math achievement is large, positive, and increasing as children age. Point estimates range from 0.484 to 0.622 standard deviations for sixth-grade scores, 0.717 to 1.026 for seventh-grade scores, and 1.298 to 1.456 for eighth-grade scores. Consistent with these results, Promise Academy middle-school students are more likely to be at or above grade level. As before, the treatment effect is smaller for ELA scores. Point estimates range from 0.146 to 0.208 standard deviations for sixth grade, 0.006 to 0.136 for seventh grade, and 0.202 to 0.349 for eighth grade. None of the ELA estimates are statistically significant.

Let us put our estimates in perspective. Jacob and Ludwig (2008) report that three policies pass a simple cost-benefit analysis: lowering class size, bonuses for teachers for teaching in hard-to-staff schools, and early childhood programs. The effect of lowering class size from 24 to 16 students per teacher is approximately 0.22 standard deviations on combined math and reading scores (Krueger, 1999). A one-standard deviation increase in teacher quality raises math achievement by 0.15 to 0.24 standard deviations per year and reading achievement by 0.15 to 0.20 standard deviations per year (Rockoff, 2004; Hanushek and Rivkin, 2005; Aaronson, Barrow, and Sander, 2007; Kane and Staiger, 2008). The effect of Teach for America on student achievement is 0.15 standard deviations in math and 0.03 in reading (Decker et al., 2004). The effect of Head Start is 0.147 deviations in applied problems and 0.319 in letter identification on the Woodcock-Johnson exam, but the effects on test scores quickly fade in elementary school (Currie and Thomas, 1995; Ludwig and Phillips, 2007). An average charter school in New York City raises math scores by 0.09 standard deviations per year and ELA scores by 0.04 standard deviations per year (Hoxby and Muraka, 2007).[28] All these effects are a small fraction of the impact of being offered admission into HCZ charter schools.[29]

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[27] To improve precision we drop students with test scores greater than three standard deviations from the mean and students with more than 50 absences. Results are qualitatively similar if all students are included in the regression.

[28] Abdulkadiroglu et al. (2009) find effect sizes closest to our own, with students enrolled in a set of Boston charter schools gaining about 0.53 standard deviations per year in math and about 0.17 standard deviations per year in ELA.

[29] Jacob and Levitt (2003) use an algorithm for detecting teacher cheating to show there are serious cases of teacher or administrator cheating on high-stakes tests in four to five percent of Chicago elementary schools. While we do not have the question-by-question data necessary to run the Jacob-Levitt algorithm, we have the results of low-stakes interim test scores given by the charter schools for internal instruction purposes. Student performance on the low-stakes tests is comparable to the high-stakes tests.

Table 4 explores the sensitivity of our estimated treatment effects on a variety of subsamples of the data. We report only the TOT estimates from our lottery results and their associated standard errors. The first column of the table reports baseline results from the pooled regressions in Table 3. Columns 2 and 3 present coefficients for two mutually exclusive and collectively exhaustive subsamples (i.e. boys versus girls). The last column provides the p-value on a test of equality between the coefficients.

The most striking finding in Table 4 is that boys benefit substantially more in math than girls , yet have the same benefit as girls in ELA. In the vast majority of the program-evaluation literature, boys are much less likely than girls to absorb the benefits of treatment (e.g. Sanbonmatsu et al., 2006; Hastings et al., 2006; Anderson, 2008). One exception to this pattern is class size, which also appears to benefit boys more than girls (Krueger, 1999). Surprisingly, there are no other statistically meaningful differences among subsets including students who entered below or above the median test score, free lunch status, and students who live within 800 meters of the boundary of Harlem Children's Zone compared to students that do not.[30] In other words, every subgroup gains equally from attending.

Promise Academy -- Elementary School

The Promise Academy elementary-school lotteries, which admitted students in kindergarten for Fall 2004, 2005, and 2006, have never been significantly oversubscribed.[31] As a result, nearly all lottery "losers" are eventually offered admission into the charter school. This prevents us from estimating the ITT and TOT effects as cleanly as we did for the middle-school students. Instead, we use two statistical techniques. First, we construct IV estimates using a student's address interacted with cohort as an instrument similar to those presented in Table 3. Second, we use a student's waitlist number as an instrument in the sample of students who entered the lottery. Under a number of plausible identifying assumptions (a low waitlist number is correlated with enrollment, one's position on the waiting list is random, being higher on the waiting list does not make one less likely to enroll, and that a high waitlist position only influences outcomes through its effect on enrollment), we can identify the casual effect of attending the Promise Academy elementary charter school on student outcomes. Our variation in enrollment comes primarily from the timing of the enrollment offer. Students with a winning lottery number or a very low waitlist number were typically offered a spot at the HCZ school in the early summer, while students with a very high waitlist number are typically not offered a spot until late summer or early fall, after classes have already commenced.

[30] We also investigated the effects of being 400 and 600 meters from the Zone and obtained the same results. We chose 800 meters because it made the dichotomous split more evenly distributed.
[31] We are still waiting on Harlem Children's Zone to assemble the lottery files from the 2006 kindergarten cohort.

Table 5 presents estimates from our main IV strategy. The reported coefficient is an indicator of being enrolled at the HCZ charter school in kindergarten, which is instrumented for using the interaction between a student's address and cohort. Standard errors, clustered at the cohort level, are reported in parentheses. The effect of being enrolled at the elementary charter school on third-grade test scores -- the first year that children in New York take standardized exams -- is large and precisely estimated, with point estimates ranging from 1.906 to 2.039 standard deviations in math and 1.693 to 1.863 in ELA. This suggests that the HCZ elementary school impacts both math and ELA scores significantly, eliminating the race gap in both subjects. The relatively large gains in ELA are particularly noteworthy in light of our middle-school results, suggesting that deficiencies in ELA might be addressed if intervention occurs relatively early in the child's life. This is consistent with developmental research that shows that the critical period for language development occurs early in life, while the critical period for developing higher cognitive functions extends into adolescence (Nelson, 2000).

Table 6 presents a series of estimates for the elementary school lottery sample using a student's waitlist number as an instrumental variable.[32] The IV estimates suggest that children who attend the elementary school gain 0.749 standard deviations in math and 0.692 standard deviations in ELA by third grade, but imprecision makes definitive conclusions difficult. Given that only 32 students "lose" the lottery, this is not surprising. Students in the 2004 cohort have about 9 and a half fewer absences in first grade, and 16 fewer absences in second and third grades. All other estimates (on grade level in third grade, kindergarten and first grade absences, and matriculation) for the 2004 cohort are either small or estimated imprecisely. The pooled sample, however, confirms that Promise Academy students also have significantly fewer absences in first grade. Both specifications also suggest that students are somewhat more likely to matriculate on time.

The Baby College and Harlem Gems

The Baby College (the nine-week parenting program) and Harlem Gems (the preschool program) do not admit students in such a way that one can confidently identify their impact and our IV strategy employed earlier does not have enough power in such small samples.[33]

Table 7 fits least squares models using the sample of kids who entered the 2004 and 2005 kindergarten lottery. This estimate represents the partial correlation of attending Harlem Gems or The Baby College on our set of outcomes. To the extent that more motivated parents put their children in

[32] In the first stage regression, which is available from the authors upon request, receiving a winning lottery number or a high spot on the waitlist significantly increases a student's probability of enrollment.
[33] We only observe 65 Harlem Gem students with third-grade test scores, 24 of whom are enrolled in the Promise Academies and 41 who are not enrolled. Of those that were not in the Promise Academy, very few actually live in the Zone (five to six per cohort). Thus, we have no power in the first stage.

programs such as The Baby College and Harlem Gems, our correlations are an underestimate of the true effect. If, on the other hand, HCZ staff recruits less-motivated parents, our correlations are overestimates. As such, we strongly caution against any causal interpretation of the forthcoming estimates.

Columns 1 through 3 of Table 7 present a series of estimates using third-grade math scores as the dependent variable. Columns 4 through 6 use third-grade ELA scores, and columns 7 through 9 and 10 through 12 use the number of absences and an indicator for on-time matriculation in second grade as the dependent variable. Test scores are only available for the 2004 kindergarten cohort, while absences and matriculation results are available for both cohorts. Enrollment in Harlem Gems is associated with scoring 0.227 to 0.232 standard deviations higher in math, and 0.238 to 0.318 higher in ELA, though only the ELA result is statistically significant. These effect sizes are comparable to the effects of Head Start on early childhood test scores (Currie and Thomas, 1995; Ludwig and Phillips, 2007). Harlem Gems is also associated with fewer absences and a higher probability of on-time enrollment, but neither result is statistically significant. Enrollment in The Baby College is not associated with higher third-grade test scores or reduced absenteeism in second grade. The point estimate for on-time marticulation is positive, but not statistically significant.

The Baby College results from Table 7 are a bit surprising. An obvious explanation is that the effects of The Baby College, like many other early investments, fade by the time children are in third grade. To understand whether this is driving the null results, Table 8 shows the partial correlation between attending The Baby College and the Bracken School Readiness Assessment for the subset of children who attended the 2001-2007 cohorts of Harlem Gems. The Bracken test was administered before students attended Gems (three years before the test outcomes in Table 7), and has been normalized to have mean zero and a standard deviation of one. The raw correlation between attending The Baby College and Bracken Scores is 0.045 standard deviations (se = 0.111), which is displayed in column 1. Column 2 includes cohort fixed effects. Columns 3 through 5 add more demographic controls sequentially: age in months, parental education and household income. Controls for parental education include indicators for either parent having a high-school diploma or for either parent having more than high-school education. Both parents having less than a high-school education is the omitted category. Household income is reported in ten-thousand dollar increments up to 40 thousand dollars a year. Household income less than ten-thousand dollars a year is the omitted category. Adding these controls only makes the independent contribution of The Baby College on Bracken scores smaller and smaller. Accounting for our host of family demographic variables, attending The Baby College is associated with a 0.009 (se = 0.111) standard deviation gain in school readiness.[34]

Tables 7 and 8 fail to find any empirical evidence that The Baby College is positively associated with achievement at ages four or eight. There are a few potential explanations. Perhaps the most obvious is that our estimates are not causal and there may be thorny issues of selection that remain even after controlling for family demographics. Second, assessments of academic achievement may be the wrong outcomes to assess the effectiveness of a parenting program. It is quite possible that The Baby College is more likely to assist parents when kids are absorbing higher-order thinking and will help them engage with their children so they are less likely to engage in criminal activities, and so on. A final explanation is that The Baby College is not an effective parenting program. The Nurse-Family Partnership, one of the more successful early childhood programs with a parenting component, involves weekly visits from a registered nurse beginning in a mother's first trimester. These visits continue through a child's second birthday (though less frequently as a child ages), with over 50 total visits before mothers exit the program. In contrast, The Baby College uses paraprofessionals in a treatment that lasts only nine weeks. The answers to these hypotheses and more definitive conclusions about the true impact of The Baby College will be better understood with the passage of time, broader data collection, and planned experimentation.

VI. Communities, Schools, or Both?

HCZ students are exposed to a web of community services along with education innovations ranging from a longer school day and year to after-school programs to mental and physical health services. These programs fall into three broad categories: (1) community programs available to anyone in the 97 blocks that comprise the Zone; (2) student-family programs that are only available to HCZ students and their families, and (3) student programs that are only available to HCZ students. Thus far, we have loosely referred to (1) as the community programs and (2) and (3) as school programs. Let us consider each bundle in turn.

The community services – community centers, the truancy-prevention programs, the network of targeted programs such as the asthma and obesity initiatives, The Baby College, Harlem Gems, and so on – are available to any child in HCZ. These programs may plausibly affect student outcomes in any number of ways, from mitigating the physical and emotional barriers to success that Harlem students face, to providing a more supportive out-of-school learning environment.

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[34] Estimates from our main IV strategy using the interaction between a student's address and cohort year also suggest that the impact of The Baby College on third-grade test scores is zero, but the results are sensitive to small specification changes and have a weak first stage.

There are three pieces of evidence that suggest that the combined effect of the community bundle alone is not responsible for the gains in achievement. First, our main IV strategy compares children inside the Zone's boundaries relative to other children in the Zone that were ineligible for the lottery so the estimates are purged of the community bundle. Our IV estimates are larger than the lottery estimates, however, suggesting that communities alone are not the answer. Second, in our analysis of subsamples, children inside the Zone garnered the same benefit from the schools as those outside the Zone, suggesting that proximity to the community programs is unimportant. Third, the Moving to Opportunity experiment, which relocated individuals from high-poverty to low-poverty neighborhoods while keeping the quality of schools constant, showed null results for girls and negative results for boys (Sanbonmatsu et al., 2006; Kling et al., 2007). This suggests that a better community, as measured by poverty rate, does not significantly raise test scores if school quality remains essentially unchanged. Additionally, and more speculative, there is substantial anecdotal evidence that the Children's Zone program was unsuccessful in the years before opening the charter schools.[35] Indeed, the impetus behind starting the schools was the lack of test-score growth under the community-only model.

The bundle of student-family services, such as the provision of nutritious fruits and vegetables, pre-made meals, money and travel allowances to ensure kids get to school, and general advice on how to support their child's learning, are only available to HCZ students and their families. A simple test of this hypothesis is to compare outcomes of children in the Promise Academy charter schools to their siblings who are in regular public schools. If the student-family services provide inputs to the entire family, the siblings of the charter-school students will benefit.

We instrument a student's enrollment status with her lottery number.[36] Under several assumptions (that the treatment-group assignment is random, lottery losers are not allowed to enroll, and winning the lottery only affects outcomes through charter-school enrollment) we can estimate the causal impact of a student enrolling in the charter school on her sibling's outcomes.

Table 9 presents our primary sibling-peer results in the middle-school sample.[37] Each regression controls for the sibling's pre-lottery outcomes, gender, race and lunch status, as well as the HCZ student's

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[35]In *Whatever it Takes: Geoffrey Canada's Quest to Change Harlem and America*, Paul Tough writes, "A few years into the life of the Zone, Canada hit a snag. The problem was the schools. His original plan called for his staff to work closely with the principals of Harlem's local public schools, providing them with supplemental services like computer labs and reading programs…To Canada's surprise and displeasure, principals sometimes resisted the help, turning down his requests for classroom space or kicking out the tutors that the organization supplied. Even in the schools where the programs were running smoothly, they didn't seem to be producing results: the neighborhood's reading and math scores had barely budged" (Tough, 2008).

[36] The siblings sample is too small to execute our main instrumental variables strategy; the corresponding first stage is weak. The main IV results generally correspond to the results reported here, but are less precise and less robust to specification changes.

[37] Most siblings of the HCZ elementary students do not have valid test scores (e.g. are too young) or are also enrolled in the HCZ schools. We therefore concentrate our analysis on siblings of the middle-school cohort.

own enrollment in the Promise Academy. The reported coefficient is an indicator for whether the student was enrolled in the Promise Academy in sixth grade, using that student's lottery number as an instrument. Standard errors are clustered at the family level.[38] [39] [40]

The effect of a student's enrollment in HCZ appears to be relatively small on her sibling's test scores; siblings of charter-school students gain approximately 0.289 (se = 0.241) standard deviations in math and 0.133 (se = 0.198) in ELA, but measured so imprecisely that we cannot make firm conclusions. Comparing the standardized scores of students across grades imposes a number of functional form restrictions, so we also consider the effect of enrollment on a sibling's percentile score rank. Results are qualitatively similar, with siblings gaining 10.424 percentage rank points in math and 3.299 percentage rank points in ELA. There is a large and statistically significant effect of a student's enrollment on their sibling's absences, with siblings missing 8.922 fewer days on average. Taken at face value, this suggests that the student-family bundle of services decreases absences, but the impact on test scores is more modest.

We have provided some evidence that HCZ's success is unlikely to be driven by the bundle of community services, either directly or indirectly, and that the effects of the student-family programs on test-scores are, at best, modest. This suggests that either the Promise Academy charter schools are the main driver of our results or the interaction of the schools and community investments is the impetus for such success.

An important remaining question is why HCZ charters are so effective at educating the poorest minority students. With some trepidation, we will hazard a guess. With the available data it is impossible to disentangle which particular school input is driving the results, but it is likely not teacher incentives based on value-added alone (Fryer, 2009e), social workers alone (Kendziora, Jones, Brown, Osher, Rudolph, King, Trivedi, and Cantor, 2008), or student incentives alone (Fryer, 2009a; 2009b; 2009c). Each one of these initiatives may have a positive return on investment, but they are not likely to produce such dramatic results in isolation.[41]

It is plausible that high-quality teachers in the charter schools are responsible for the results as the estimates are within the variance of teacher quality in Rockoff (2004), Hanushek and Rivkin (2005), Aaronson et al. (2007), and Kane and Staiger (2008), which range from 0.15 to 0.24 standard-deviations

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[38]Results are qualitatively similar if we allow the effect of enrollment to vary by dosage.

[39]To address concerns that the subset of HCZ students with siblings may be different than the subset without siblings, we replicate out main results from Table 3 for both groups. Students with and without siblings experience nearly identical gains to HCZ enrollment, and are quite similar on observable characteristics.

[40] Siblings are defined as any two students who share the same last name and address at any point in time.

[41] There may also be important interactions between different elements of the HCZ school bundle. Analogous to the discovery of HAART that treats HIV/AIDS, each element of the bundle may not be effective individually but together one receives both the direct effect of the program and the interactions between elements of the bundle.

in math and 0.15 to 0.20 in reading. Recall, in their quest to find high-quality teachers, the teacher-turnover rate at the HCZ charter was 48 percent in their first year.

Second, a linear combination of good policy choices can explain the results. In their analysis of New York City charter schools, Hoxby and Murarka (2007) estimate the relationship between a series of school policy choices (time on task, e.g.) and the success of the charter school. Plugging in HCZ's combination of policies into the regression equation utilized in Hoxby and Murarka (2007) predicts yearly gains of 0.54 standard deviations, which would account for our results. Indeed, this estimate is larger than any other charter school in their sample, suggesting that HCZ has a unique blend of policies.

VII. Conclusion

The racial achievement gap in education is one of America's most pressing social concerns. The typical black seventeen year-old reads at the proficiency level of the typical white thirteen year-old (Campbell et al., 2000). On the Scholastic Aptitude Test, the gateway to America's institutions of higher learning, there is little overlap in the *distribution* of scores (Card and Rothstein, 2007). In the past decades, there has been very little progress in solving the achievement gap puzzle.

HCZ is enormously successful at boosting achievement in math and ELA in elementary school and math in middle school. The impact of being offered admission into the HCZ middle school on ELA achievement is positive, but less dramatic. High-quality schools or community investments coupled with high-quality schools drive these results, but community investments alone cannot.

We hope that our analysis provides a sense of optimism for work on the achievement gap. The HCZ model demonstrates that the right cocktail of investments can be successful. The challenge is to find lower-cost ways to achieve similar results in regular public schools. Future work in this vein is remarkably rich. HCZ is a bold social experiment and presents many opportunities to answer important questions that have evaded social scientists for decades.

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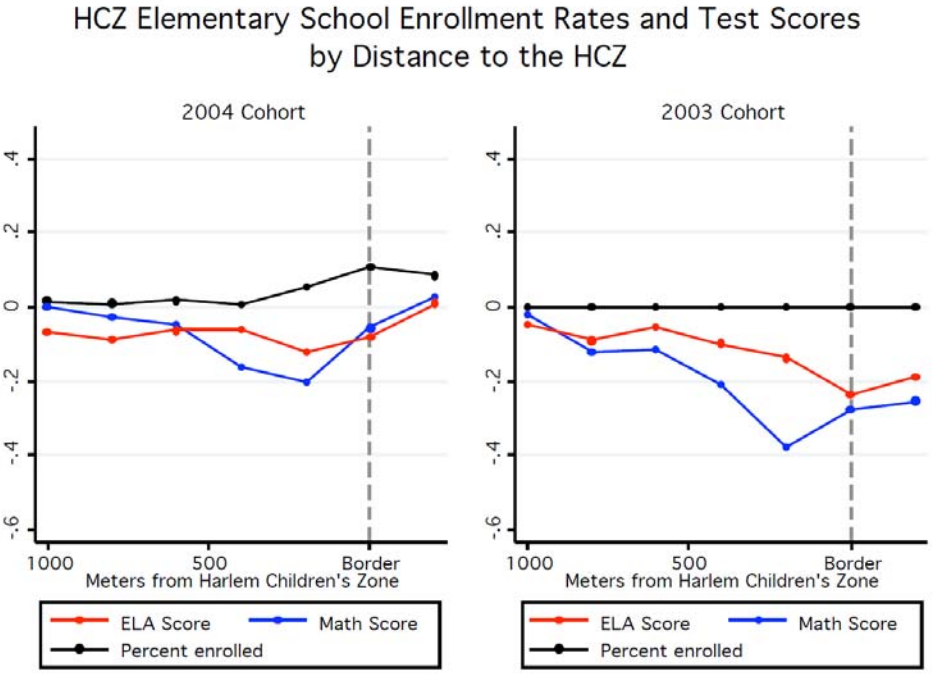
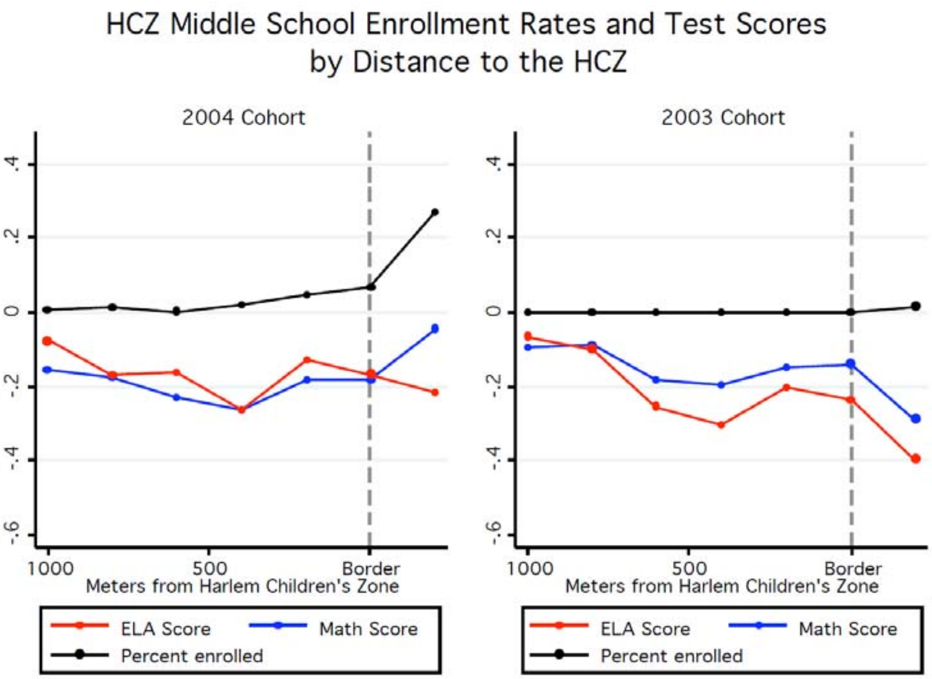
Figure 1
The Harlem Children's Zone

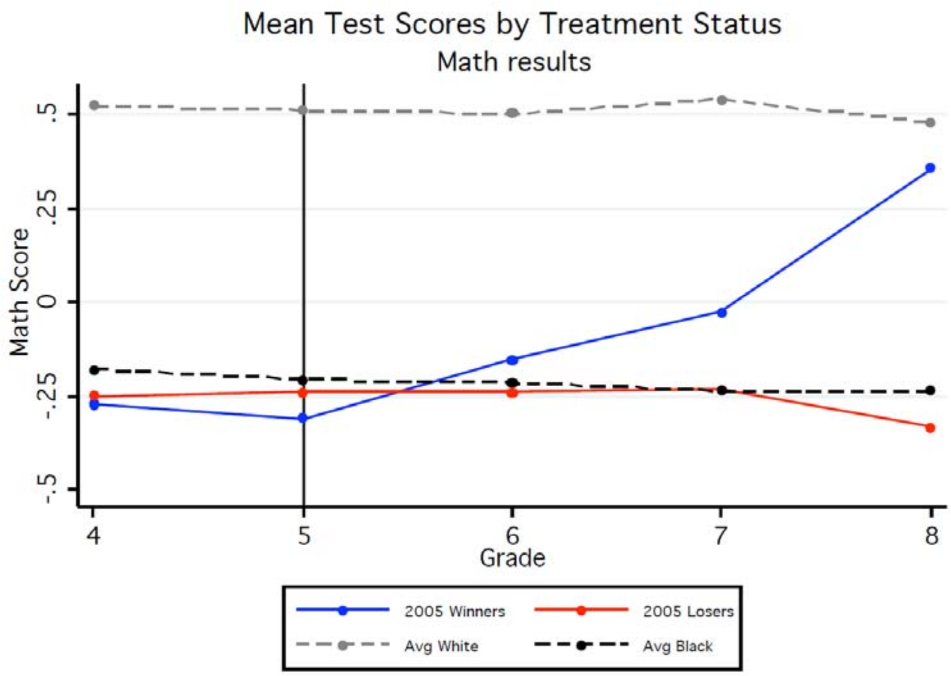
Figures 2A and 2B

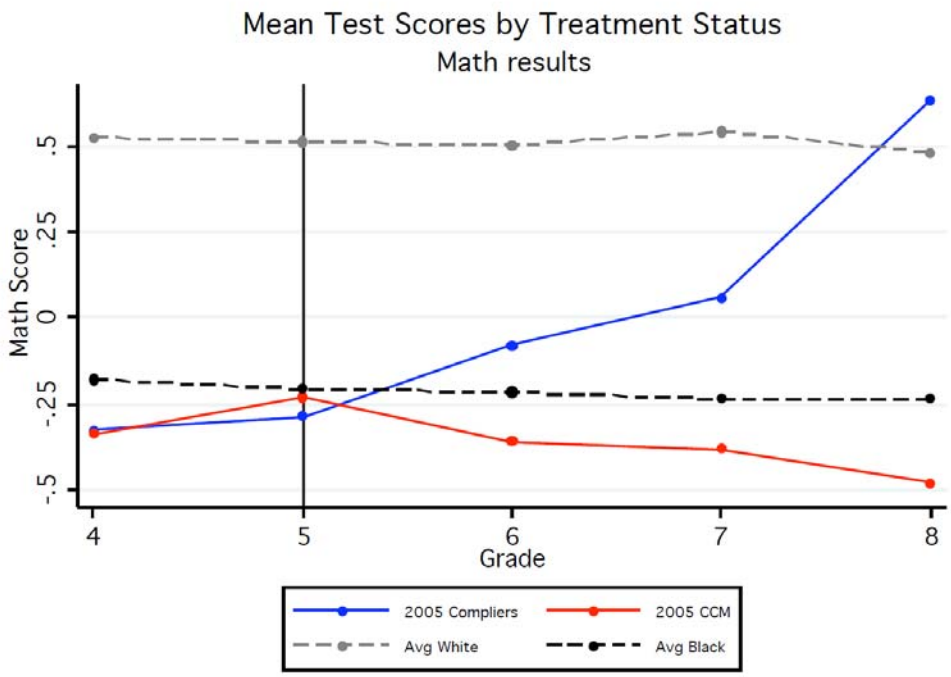




Notes: In our elementary-school sample a student's cohort is imputed from her initial kindergarten year. If we do not observe an elementary student in kindergarten, we impute the expected grade from the student's birth date. In our middle-school sample a student's cohort is imputed from her initial sixth-grade year.[1] If we do not observe a student in sixth grade, we impute an expected grade from the nearest observed grade. The HCZ is defined as the original 24-block Zone, ranging from 116th to 123rd Streets, 5th Avenue to 8th Avenue. Results are qualitatively similar if the Phase 2 or Phase 3 area is used.

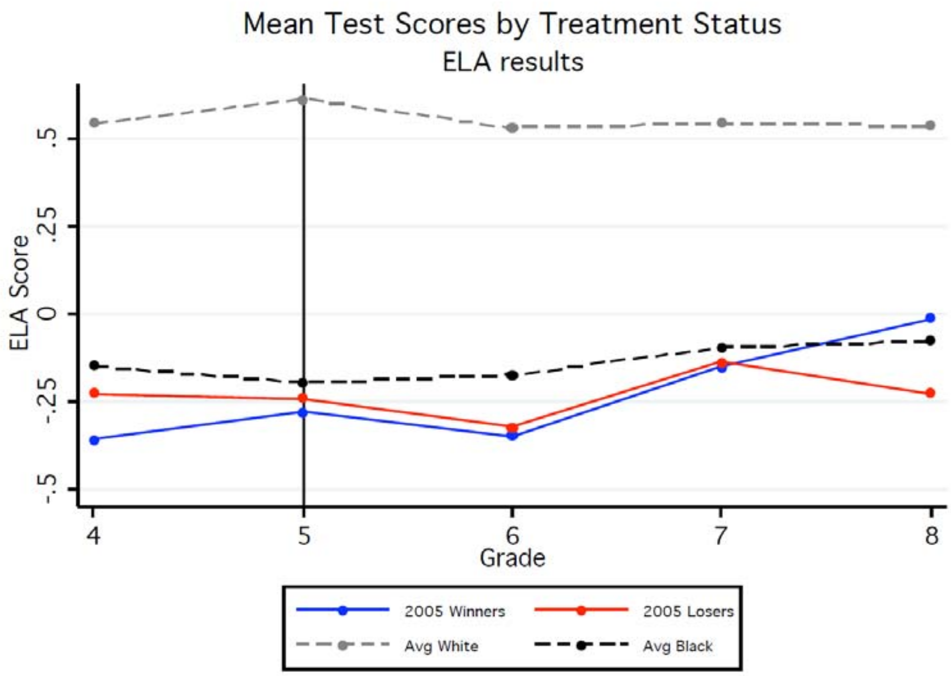
Figures 3A and 3B

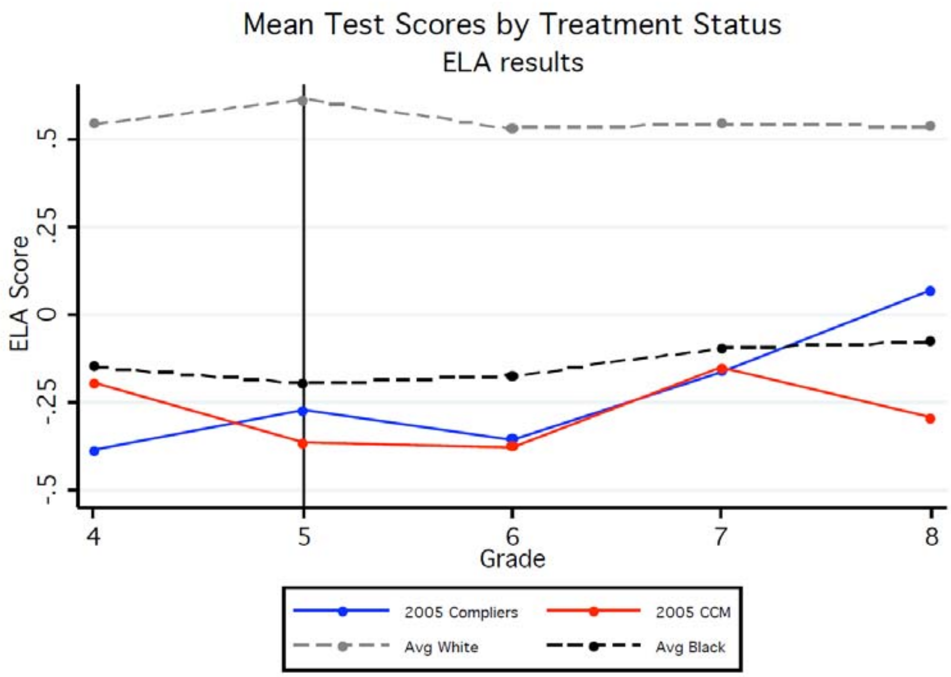




Notes: Lottery winners are students who receive a winning lottery number or who are in the top ten of the waitlist. Test scores are standardized by grade to have mean zero and standard deviation one in the entire New York City sample. The CCM is the estimated test score for those in the control group who would have complied if they had received a winning lottery number.

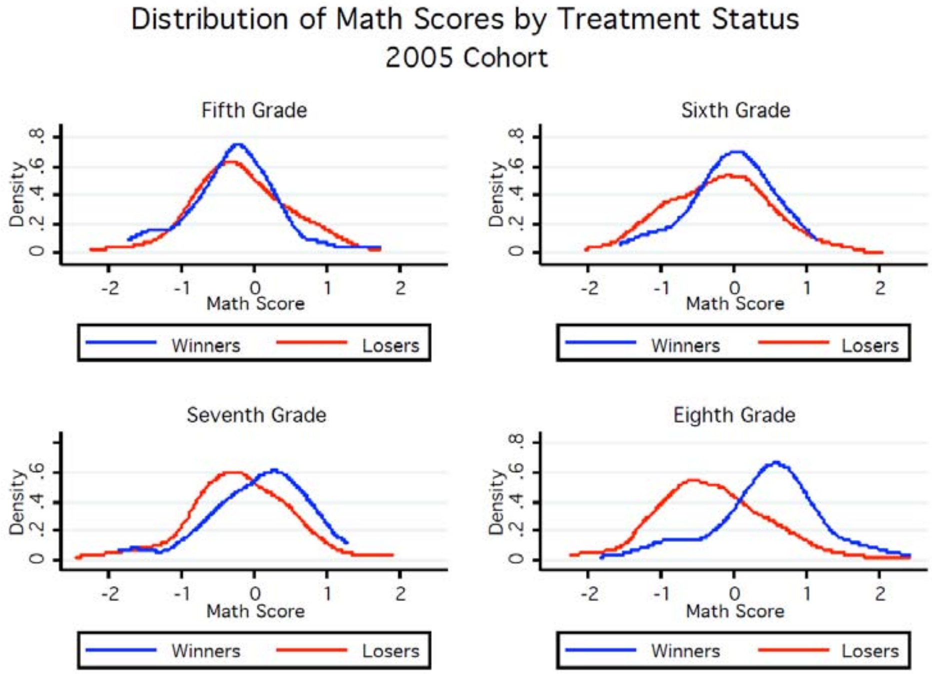
Figures 4A and 4B

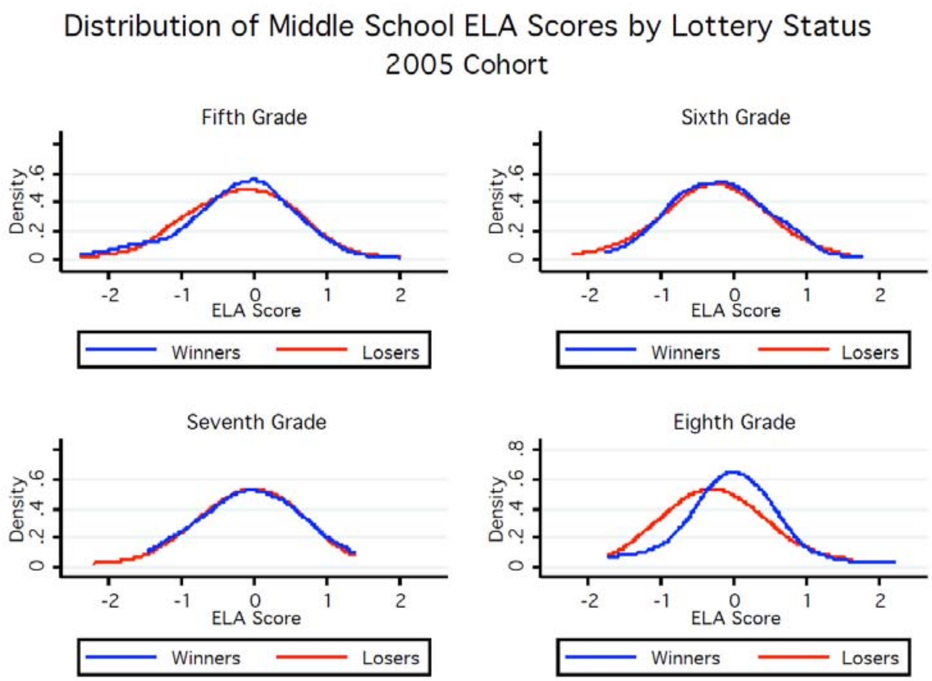




Notes: Lottery winners are students who receive a winning lottery number or who are in the top ten of the waitlist. Test scores are standardized by grade to have mean zero and standard deviation one in the entire New York City sample. The CCM is the estimated test score for those in the control group who would have complied if they had received a winning lottery number.

Figures 5A and 5B





Notes: Lottery winners are students who receive a winning lottery number or who are in the top ten on the waitlist. Test scores are standardized by grade to have mean zero and standard deviation one in the entire New York City sample.

Table 1
Summary Statistics of Lottery Sample

	Students in Lottery						Students living in designated areas			
	K – 3			6 – 8			K – 3		6 – 8	
Youth characteristics	Winners	Losers	p-value	Winners	Losers	p-value	HCZ	NYC	HCZ	NYC
Male	0.530	0.508	0.675	0.514	0.446	0.143	0.515	0.517	0.523	0.515
White	0.006	0.011	0.563	0.005	0.010	0.612	0.032	0.155	0.007	0.135
Black	0.818	0.768	0.244	0.849	0.827	0.529	0.711	0.297	0.755	0.326
Hispanic	0.127	0.166	0.299	0.141	0.160	0.556	0.229	0.399	0.230	0.396
Free Lunch	0.691	0.646	0.373	0.768	0.772	0.901	0.709	0.666	0.700	0.684
Reduced Lunch	0.149	0.122	0.444	0.108	0.099	0.757	0.096	0.079	0.098	0.101
5th Grade Math	--	--	--	-0.243	-0.301	0.381	--	--	-0.448	0.012
5th Grade ELA	--	--	--	-0.207	-0.272	0.374	--	--	-0.314	0.000
School characteristics										
Male	0.514	0.510		0.526	0.516		0.505	0.514	0.514	0.514
White	0.018	0.031		0.014	0.047		0.038	0.150	0.081	0.136
Black	0.768	0.701		0.800	0.572		0.648	0.302	0.542	0.324
Hispanic	0.191	0.232		0.168	0.349		0.278	0.400	0.336	0.395
Free Lunch	0.629	0.648		0.623	0.640		0.692	0.587	0.597	0.611
Reduced Lunch	0.108	0.102		0.109	0.079		0.082	0.066	0.083	0.087
5th Grade Math	--	--		-0.300	-0.277		--	--	-0.221	-0.014
5th Grade ELA	--	--		-0.275	-0.246		--	--	-0.161	-0.031
Charter	0.680	0.580		0.714	0.160		0.206	0.031	0.141	0.017
Observations	181	181		182	304		621	296093	417	223628

Notes: HCZ refers to the original 24-block area of the Harlem Children's Zone, ranging from 116th to 123rd Streets, 5th Avenue to 8th Avenue. NYC refers to the universe of New York City public-school students. Lottery winners are students who receive a winning lottery number or who are placed very high on the waitlist.

Table 2
Middle-School Lottery Results

	2005 Cohort		2006 Cohort		Pooled Sample	
Dependent Variable	ITT	TOT	ITT	TOT	ITT	TOT
6^{th} Grade Math Score	0.190***	0.279***	0.253***	0.382***	0.225***	0.342***
	(0.065)	(0.095)	(0.080)	(0.118)	(0.049)	(0.074)
7^{th} Grade Math Score	0.286***	0.438***	0.347***	0.484***	0.313***	0.468***
	(0.077)	(0.117)	(0.078)	(0.105)	(0.055)	(0.080)
8^{th} Grade Math Score	0.733***	1.112***	--	--	--	--
	(0.104)	(0.139)				
6^{th} Grade Math on Level	0.076	0.112	0.138**	0.209**	0.127***	0.193***
	(0.057)	(0.083)	(0.059)	(0.089)	(0.041)	(0.062)
7^{th} Grade Math on Level	0.229***	0.351***	0.155**	0.217**	0.217***	0.324***
	(0.060)	(0.092)	(0.061)	(0.084)	(0.042)	(0.064)
8^{th} Grade Math on Level	0.420***	0.638***	--	--	--	--
	(0.059)	(0.083)				
6^{th} Grade ELA Score	0.014	0.021	0.014	0.021	0.024	0.037
	(0.072)	(0.106)	(0.079)	(0.120)	(0.053)	(0.081)
7^{th} Grade ELA Score	-0.008	-0.012	0.097	0.137	0.039	0.058
	(0.069)	(0.104)	(0.079)	(0.112)	(0.049)	(0.072)
8^{th} Grade ELA Score	0.239***	0.363***	--	--	--	--
	(0.081)	(0.122)				
6^{th} Grade ELA on Level	0.040	0.059	(0.019)	(0.029)	0.029	0.045
	(0.051)	(0.076)	(0.058)	(0.087)	(0.038)	(0.058)
7^{th} Grade ELA on Level	-0.015	-0.023	-0.049	-0.069	0.002	0.003
	(0.054)	(0.082)	(0.062)	(0.088)	(0.041)	(0.061)
8^{th} Grade ELA on Level	0.128**	0.194**	--	--	--	--
	(0.056)	(0.085)				
6^{th} Grade Absences	-2.230*	-3.243*	-5.369***	-8.319***	-4.094***	-6.224***
	(1.304)	(1.886)	(1.044)	(1.612)	(0.850)	(1.303)
7^{th} Grade Absences	-5.267***	-8.046***	-3.507	-5.133	-3.849***	-5.818***
	(1.701)	(2.564)	(2.344)	(3.426)	(1.394)	(2.074)
8^{th} Grade Absences	-6.253***	-9.474***	--	--	--	--
	(2.008)	(3.045)				
On-Time at 6^{th} Grade	--	--	--	--	--	--
On-Time at 7^{th} Grade	-0.015	-0.024	-0.005	-0.007	0.004	0.005
	(0.040)	(0.061)	(0.014)	(0.020)	(0.023)	(0.034)
On-Time at 8^{th} Grade	0.027	0.041	--	--	--	--
	(0.031)	(0.047)				
Observations: Treated	87	87	95	95	182	182
Control	182	182	122	122	304	304

Notes: Each regression controls for the gender, race, lunch status, and predetermined values of the dependent variable. Treatment is defined as either having a winning lottery number or being high on the waitlist. Test scores are standardized to have mean zero and standard deviation one by grade in the full New York City sample. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Table 3
Middle-School IV Results

Dependent Variable	800 Meters	1600 Meters	2400 Meters
6th Grade Math Score	0.622	0.484	0.550
	(0.662)	(0.646)	(0.573)
7th Grade Math Score	1.049*	1.026*	0.717
	(0.509)	(0.484)	(0.433)
8th Grade Math Score	1.369***	1.456***	1.298***
	(0.289)	(0.239)	(0.213)
6th Grade Math on Level	-0.184	-0.301	-0.226
	(0.189)	(0.199)	(0.244)
7th Grade Math on Level	0.361	0.200	0.029
	(0.230)	(0.297)	(0.245)
8th Grade Math on Level	0.739***	0.740***	0.620***
	(0.134)	(0.134)	(0.113)
6th Grade ELA Score	0.146	0.167	0.208
	(0.490)	(0.441)	(0.375)
7th Grade ELA Score	0.006	0.068	0.136
	(0.591)	(0.554)	(0.515)
8th Grade ELA Score	0.202	0.349	0.335
	(0.286)	(0.244)	(0.249)
6th Grade ELA on Level	0.098	0.059	0.132
	(0.239)	(0.182)	(0.181)
7th Grade ELA on Level	-0.439	-0.463	-0.446
	(0.314)	(0.291)	(0.278)
8th Grade ELA on Level	0.249	0.195	0.207
	(0.193)	(0.186)	(0.179)
6th Grade Absences	-3.382	-3.439	-3.456
	(4.433)	(4.508)	(5.242)
7th Grade Absences	-0.113	-0.294	0.600
	(4.576)	(5.527)	(5.442)
8th Grade Absences	-1.070	-1.058	1.039
	(5.947)	(5.298)	(4.712)
On-Time at 6th Grade	--	--	--
On-Time at 7th Grade	0.177	0.113	0.097
	(0.159)	(0.148)	(0.147)
On-Time at 8th Grade	-0.053	-0.109	-0.067
	(0.170)	(0.147)	(0.159)
Observations	7473	16286	23903

Notes: Each regression controls for the gender, race, lunch status, predetermined values of the dependent variable (when available), and whether the student lives within the 24-block Children's Zone. Enrollment in the HCZ charter school at sixth grade is the reported coefficient, which is instrumented for using interactions between the student's cohort and whether she lives within the 24-block Children's Zone. Standard errors are clustered at the cohort level. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Table 4
Middle-School Lottery Results by Subsample

Dependent Variable	Pooled	Boys	Girls	p-value
6th Grade Math Score	0.342***	0.297***	0.273**	0.880
	(0.074)	(0.106)	(0.117)	
7th Grade Math Score	0.468***	0.401***	0.464***	0.700
	(0.080)	(0.118)	(0.111)	
8th Grade Math Score*	1.112***	1.422***	0.841***	0.044
	(0.139)	(0.195)	(0.209)	
6th Grade ELA Score	0.037	0.011	0.085	0.648
	(0.081)	(0.122)	(0.106)	
7th Grade ELA Score	0.058	0.011	0.096	0.578
	(0.072)	(0.116)	(0.098)	
8th Grade ELA Score*	0.363***	0.317*	0.373**	0.822
	(0.122)	(0.170)	(0.179)	
Observations	486	228	258	

Dependent Variable	Pooled	Free lunch	Not on free lunch	p-value
6th Grade Math Score	0.342***	0.283***	0.289*	0.974
	(0.074)	(0.093)	(0.147)	
7th Grade Math Score	0.468***	0.421***	0.471***	0.782
	(0.080)	(0.097)	(0.154)	
8th Grade Math Score*	1.112***	1.052***	1.422***	0.298
	(0.139)	(0.158)	(0.316)	
6th Grade ELA Score	0.037	0.059	0.027	0.855
	(0.081)	(0.093)	(0.151)	
7th Grade ELA Score	0.058	0.040	0.102	0.725
	(0.072)	(0.085)	(0.156)	
8th Grade ELA Score*	0.363***	0.428***	0.043	0.210
	(0.122)	(0.136)	(0.273)	
Observations	486	375	111	

Dependent Variable	Pooled	Close to HCZ	Not close to HCZ	p-value
6th Grade Math Score	0.342***	0.215**	0.343***	0.281
	(0.074)	(0.098)	(0.097)	
7th Grade Math Score	0.468***	0.396***	0.463***	0.580
	(0.080)	(0.102)	(0.100)	
8th Grade Math Score*	1.112***	1.096***	1.166***	0.738
	(0.139)	(0.172)	(0.177)	
6th Grade ELA Score	0.037	0.090	0.018	0.539
	(0.081)	(0.106)	(0.094)	
7th Grade ELA Score	0.058	0.128	(0.003)	0.206
	(0.072)	(0.098)	(0.084)	
8th Grade ELA Score*	0.363***	0.356**	0.334**	0.903
	(0.122)	(0.149)	(0.157)	
Observations	486	237	249	

Notes: Each regression controls for the gender, race, lunch status, and predetermined values of the dependent variable. Regressions marked with a # are only the 2005 cohort; all other regressions are pooled across the 2005 and 2006 cohorts. The reported number of observations is for the pooled regressions. The reported coefficient is the effect of being enrolled in the Promise Academy in sixth grade interacted with the defining subsample variable, instrumented for using treatment status interacted with the defining subsample variable. Scores are standardized to have mean zero and standard deviation one by grade in the full New York City sample. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Table 5
Elementary-School IV Results

Dependent Variable	800 Meters	1600 Meters	2400 Meters
3rd Grade Math Score	1.906***	2.039***	1.994***
	(0.375)	(0.231)	(0.220)
3rd Grade Math on Level	0.600*	0.667**	0.574*
	(0.291)	(0.253)	(0.224)
3rd Grade ELA Score	1.693***	1.863***	1.827***
	(0.375)	(0.157)	(0.132)
3rd Grade ELA on Level	0.718*	0.788***	0.624***
	(0.305)	(0.121)	(0.113)
1st Grade Absences	2.806	4.378	6.089
	(2.825)	(3.536)	(5.429)
2nd Grade Absences	4.534	-0.414	-3.399
	(7.483)	(6.647)	(6.171)
3rd Grade Absences	-2.828	-3.575	-4.566*
	(3.788)	(2.882)	(2.197)
On-time at 1st Grade	-0.200*	-0.181	-0.117
	(0.094)	(0.122)	(0.144)
On-time at 2nd Grade	0.136	0.013	0.018
	(0.256)	(0.190)	(0.198)
On-time at 3rd Grade	-0.053	0.068	0.104
	(0.176)	(0.121)	(0.144)
Observations	6467	14182	21095

Notes: Each regression controls for the gender, race, lunch status, predetermined values of the dependent variable (when available), and whether the student lives within the 24-block Children's Zone. Enrollment in the HCZ charter school at kindergarten is the reported coefficient, which is instrumented for using the interaction between the student's cohort and whether she lives within the 24-block Children's Zone. Standard errors are clustered at the cohort level. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Table 6
Elementary-School Lottery Results

Dependent Variable	2004 Cohort	2005 Cohort	Pooled
3rd Grade Math Score	0.749	--	--
	(0.459)		
3rd Grade Math on Level	0.146	--	--
	(0.113)		
3rd Grade ELA Score	0.692	--	--
	(0.438)		
3rd Grade ELA on Level	0.126	--	--
	(0.233)		
1st Grade Absences	-9.592*	-9.047	-1.364
	(5.571)	(7.014)	(3.491)
2nd Grade Absences	-16.002***	-15.474	-11.104***
	(4.375)	(10.235)	(4.187)
3rd Grade Absences	-15.889***		
	(5.672)		
On-time at 1st Grade	0.064	0.164	0.089*
	(0.068)	(0.115)	(0.050)
On-time at 2nd Grade	0.152*	0.127	0.085*
	(0.085)	(0.105)	(0.050)
On-time at 3rd Grade	0.087	--	--
	(0.090)		
Observations	109	253	362

Notes: Each regression controls for the gender, race, and lunch status. Enrollment in the HCZ charter school at kindergarten is the reported coefficient, which is instrumented for using the student's position in the lottery and waitlist. Test scores are standardized to have mean zero and standard deviation one by grade in the full New York City sample. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Table 7
Harlem Gems and Baby College Results in the Kindergarten Lottery Sample

	Math Score in 3rd Grade			ELA Score in 3rd Grade			Total Absences in 2nd Grade			On-Time at 2nd Grade		
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Promise Academy	0.513***	0.448**	0.447**	0.365**	0.297*	0.275	-4.511***	-4.334***	-4.338***	0.009	0.005	0.005
	(0.178)	(0.184)	(0.186)	(0.168)	(0.173)	(0.172)	(1.451)	(1.465)	(1.467)	(0.017)	(0.017)	(0.017)
Harlem Gems		0.227	0.232		0.238	0.318*		(1.725)	(1.813)		0.034	0.031
		(0.176)	(0.184)		(0.165)	(0.170)		(1.925)	(2.016)		(0.023)	(0.024)
Baby College			-0.026			-0.430*			0.427			0.018
			(0.276)			(0.256)			(2.883)			(0.033)
Observations	109	109	109	109	109	109	362	362	362	362	362	362

Notes: Each regression controls for the gender, race, and lunch status of the student. Columns (1) – (3) use 3rd grade math score as the dependent variable. Columns (4) – (6) use ELA score. Columns (7) – (9) use total absences in second grade, and columns (10) – (12) use a student's on-time status at second grade. Second-grade outcomes are used so that we can pool the 2004 and 2005 kindergarten cohorts. Test scores are standardized to have mean zero and standard deviation one by grade in the full New York City sample. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Table 8
Baby College Results in the Harlem Gems Sample

Variable	(1)	(2)	(3)	(5)	(6)
Baby College	0.045	0.028	0.034	0.005	0.009
	(0.111)	(0.110)	(0.111)	(0.111)	(0.111)
Male	--	-0.055	-0.046	-0.075	-0.079
		(0.095)	(0.094)	(0.093)	(0.093)
Black	--	0.320**	0.311**	0.294**	0.265**
		(0.124)	(0.123)	(0.123)	(0.123)
Age (in months)	--	--	-0.036***	-0.038***	-0.037***
			(0.014)	(0.014)	(0.014)
Parent's have HS diploma	--	--		0.729**	0.667**
				(0.302)	(0.315)
Parent's have more than HS	--	--		0.969***	0.839***
				(0.275)	(0.295)
Parent income 10k to 20k	--	--	--	--	0.101
					(0.217)
Parent income 20k to 30k	--	--	--	--	-0.126
					(0.200)
Parent income 30k to 40k	--	--	--	--	0.492*
					(0.255)
Parent income more than 40k	--	--	--	--	0.382
					(0.248)
Observations	451	451	451	451	451

Notes: The sample is made up of all students ever enrolled in the original Harlem Gems program, the Uptown Harlem Gems Program, or the Head Start Gems Program, between 2001-02 and 2007-08. Note that after the 2005-06 school year, the original Harlem Gems program became part of the Promise Academies. The dependent variable for all regressions is the Bracken scale test score, which is administered before the Harlem Gems begins. Scores are standardized to have mean zero and standard deviation one. The omitted race category is non-black, which make up less than 15 percent of the sample and is comprised mostly of Hispanics and African students. The omitted parent income is less than $10,000 per year. The omitted education category is less than a high school diploma. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Table 9
Siblings Results in the Middle-School Lottery Sample

Dependent Variable	Pooled
Math Score#	0.289
	(0.241)
Math Percentile Rank#	10.424
	(7.167)
ELA Score#	0.133
	(0.198)
ELA Percentile Rank#	3.299
	(6.266)
Absences	-8.922**
	(3.659)
On-time	0.062
	(0.062)
Observations	361

Notes: Each regression controls for sibling gender, race, lunch status, predetermined values of the dependent variable, and the sibling's own attendance at the Promise Academy. The reported coefficient is an indicator for whether lottery student was enrolled at the Promise Academy Middle School in sixth grade, instrumented for using the student's lottery status. Test scores are standardized to have mean zero and standard deviation one by grade in the full New York City sample. Standard errors are clustered at the family level. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level. #Test score data is only available for grades 3 – 8, limiting the sample size to 178.

Appendix Table 1
Middle School IV First Stage Results

Instruments	Elementary School	Middle School
2001 Cohort x Zone Interaction	-0.0004*	--
	(0.0002)	
2002 Cohort x Zone Interaction	0.0010**	-0.0006*
	(0.0004)	(0.0003)
2003 Cohort x Zone Interaction	0.0000	0.0042***
	(0.0002)	(0.0002)
2004 Cohort x Zone Interaction	0.0608***	0.0988***
	(0.0001)	(0.0003)
2005 Cohort x Zone Interaction	0.0237***	0.0409***
	(0.0002)	(0.0003)
2006 Cohort x Zone Interaction	0.0138***	0.0214***
	(0.0004)	(0.0001)
2007 Cohort x Zone Interaction	0.0339***	-0.0003
	(0.0003)	(0.0002)
Select Control Variables		
Living in the Zone	-0.0055**	-0.0024*
	(0.0022)	(0.0012)
2001 Cohort Dummy	0.0000	--
	(0.0001)	
	0.0000	0.0001*
2002 Cohort Dummy	0.0000	0.0000
	0.0006**	0.0003***
2003 Cohort Dummy	(0.0002)	0.0000
	0.0133***	0.0124***
2004 Cohort Dummy	(0.0003)	0.0000
	0.0151***	0.0136***
2005 Cohort Dummy	(0.0003)	(0.0001)
	0.0188***	0.0085***
2006 Cohort Dummy	(0.0004)	(0.0002)
	0.0142***	0.0002
2007 Cohort Dummy	(0.0005)	(0.0001)
	0.0000	
Constant	-0.0086**	-0.0053*
	(0.0030)	(0.0025)
Observations	28289	29537
F-test on instruments (p-value)	0.000	0.000

Notes: Each regression controls also for the gender, race, and lunch status of the student. Both samples include all students in the relevant cohorts that live within 1600 meters of the Zone. The dependent variable is enrollment at the HCZ charter school. Results are comparable for other distances, and if other control variables are included. Standard errors are clustered at the cohort level. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Appendix Table 2
Sensitivity of Middle-School Lottery Results

	2005 Cohort		2006 Cohort		Pooled Sample	
Dependent Variable	ITT	TOT	ITT	TOT	ITT	TOT
6^{th} Grade Math Score	0.152**	0.234**	0.253***	0.382***	0.190***	0.303***
	(0.068)	(0.101)	(0.080)	(0.118)	(0.051)	(0.079)
7^{th} Grade Math Score	0.235***	0.359***	0.347***	0.484***	0.268***	0.400***
	(0.081)	(0.122)	(0.078)	(0.105)	(0.056)	(0.083)
8^{th} Grade Math Score	0.598***	0.977***	--	--	--	--
	(0.122)	(0.164)				
6^{th} Grade ELA Score	-0.051	-0.081	0.014	0.021	-0.020	-0.032
	(0.076)	(0.122)	(0.079)	(0.120)	(0.054)	(0.087)
7^{th} Grade ELA Score	-0.022	-0.033	0.097	0.137	0.029	0.043
	(0.068)	(0.101)	(0.079)	(0.112)	(0.050)	(0.075)
8^{th} Grade ELA Score	0.224***	0.366***	--	--	--	--
	(0.079)	(0.128)				
Observations: Treated	87	87	95	95	182	182
Control	190	190	134	134	324	324

Notes: This table explores the sensitivity of our results to the difference in match rates between the Middle School lottery winners and losers by creating pseudo-lottery losers to equalize the match rates for each cohort. These pseudo-students are assigned the highest test score observed in the control group for that year (about two standard deviations above the mean). The number of observations refers to the number of real, not pseudo students. Each regression controls for the gender, race, lunch status, and predetermined values of the dependent variable. Treatment is defined as either having a winning lottery number or being high on the waitlist. Test scores are standardized to have mean zero and standard deviation one by grade in the full New York City sample. *** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level.

Appendix Table 3
Summary Statistics of IV Sample

	Elementary School Sample					Middle School Sample				
	In the Zone		Out of the Zone			In the Zone		Out of the Zone		
Youth characteristics	Eligible	Ineligible	Eligible	Ineligible	Difference	Eligible	Ineligible	Eligible	Ineligible	Difference
Male	0.505	0.521	0.517	0.519	0.021	0.530	0.522	0.517	0.525	0.015
					(0.021)					(0.048)
White	0.028	0.030	0.135	0.149	-0.004	0.003	0.022	0.132	0.126	-0.025
					(0.015)					(0.032)
Black	0.715	0.716	0.324	0.304	0.014	0.760	0.764	0.329	0.329	-0.005
					(0.019)					(0.045)
Hispanic	0.231	0.230	0.400	0.401	-0.009	0.230	0.202	0.398	0.409	0.039
					(0.021)					(0.047)
Free Lunch	0.706	0.703	0.697	0.670	0.025	0.707	0.685	0.683	0.713	0.051
					(0.020)					(0.044)
Reduced Lunch	0.093	0.098	0.096	0.082	0.028**	0.087	0.101	0.098	0.096	-0.017
					(0.012)					(0.028)
5th Grade Math	--	--	--	--	--	-0.444	-0.525	0.001	-0.084	-0.004
										(0.102)
5th Grade ELA	--	--	--	--	--	-0.315	-0.346	-0.014	-0.083	-0.038
										(0.103)
Observations	595	510	11502	11908	--	287	178	7052	3025	--

Notes: In the Zone refers to the original 24-block area of the Harlem Children's Zone, ranging from 116th to 123rd Streets, 5th Avenue to 8th Avenue. Out of the Zone refers to all addresses within 1600 meters of the original Zone. Columns 5 and 10 have p-values from a test of equality between the difference of eligible and ineligible cohorts in and out of the Zone. See text for details.

Appendix A: Complete List of Harlem Children's Zone Programs

COMMUNITY INVESTMENTS

Early Childhood

The Baby College

The Baby College offers nine-week parenting workshops to expectant parents and those raising a child up to three years old.

The Three Year Old Journey

The Three-Year-Old Journey works with parents of children who have won the HCZ Promise Academy charter school lottery. Held on Saturdays over several months, it teaches parents about their child's development, building language skills and parenting skills.

Get Ready for Pre-K

Get Ready for Pre-K involves children in small and large group activities that are designed to increase socialization skills, build routines, and provide exposure to their first classroom experience. The program has a particular focus on the development of pre-literacy skills. In order to prepare children for September entry into Harlem Gems Universal Pre-K or Head Start, four-year-old children attend Get Ready for Pre-K from 8 AM to 6 PM every day for six weeks during the preceding summer.

Harlem Gems

The Harlem Gems Universal Pre-K is an all-day pre-kindergarten program that gets children ready to enter kindergarten. Classes have a 4:1 child-to-adult ratio, teach English, Spanish and French, and run from 8 a.m. to 6 p.m. HCZ runs three pre-kindergarten sites, serving over 250 children.

The Harlem Gems Head Start follows the same model as the Universal Pre-K but has a few important differences: 1) children can enter the Head Start at three or four years of age, thus a sizable proportion of the participants receive two years of instruction in the program, (2) because of income guidelines, students in Head Start tend to come from families with lower socioeconomic status than that of Universal Pre-K participants, and (3) while lead teachers at the UPK have master's degrees, Head Start teachers have bachelor's degrees.

Public Elementary School

Harlem Peacemakers

Harlem Peacemakers, funded in part by AmeriCorps, trains young people who are committed to making their neighborhoods safe for children and families. The agency has Peacemakers working as teaching assistants in seven public schools and the HCZ Promise Academy Charter School.

Public Middle School

TRUCE Fitness and Nutrition Center

TRUCE Fitness and Nutrition Center offers free classes to children in karate, fitness and dance. Participants also learn about health and nutrition, as well as receiving regular academic assistance. The program is focused on developing middle-school youth, grades 5-8.

A Cut Above

A Cut Above is an after-school program that helps students in the critical-but-difficult middle-school years. Supporting students who are not in the Promise Academy charter school, it provides academic help, leadership development, as well as high-school and college preparation.

Public High School

TRUCE Arts & Media

TRUCE (The Renaissance University for Community Education) does youth development through the arts and media, working with youth in grades 9-12 on academic growth, career readiness as well as fostering media literacy and artistic ability.

Employment and Technology Center

The Employment and Technology center teaches computer and job-related skills to teens and adults.

Learn to Earn

Learn to Earn is an after-school program that helps high-school juniors and seniors improve their academic skills, as well as preparing them for college and the job market.

College

The College Success Office

The College Success Office supports students who have graduated from high school and HCZ programs. It helps them get into the most appropriate college, then assists them throughout their college years.

Family, Community and Health Programs

Community Pride

Community Pride organizes tenant and block associations, helping many hundreds of tenants convert their city-owned buildings into tenant-owned co-ops.

Single Stop

Single Stop offers access to a wide variety of services - from counseling to financial advice to legal consultations - at several locations each week.

The HCZ Asthma Initiative

The HCZ Asthma Initiative works closely with asthmatic children and their families so they can learn to manage the disease and lessen its effects.

The Obesity Initiative

The Obesity Initiative is a multi-pronged program to help children and their families reverse the alarming trend toward obesity and its health effects.

The Beacon Center Program

The Beacon programs turn school buildings into community centers, offering programs during the afternoon, evening and weekend. They offer programs for youth and adults from education to the arts to recreation. Each summer, they offer all-day camp so children have a safe, enriching place to spend their time instead of hanging out on the street.

Foster Care Prevention Services

Foster Care Prevention programs work to stabilize and strengthen families so that their children are not placed in foster care.

The Family Development Program
The Family Development Program serves 120 families and specializes in access to mental-health professionals who collaborate with caseworkers to support therapeutic interventions.

The Family Support Center
The Family Support Center serves 90 families, and specializes in providing crisis-intervention services, referrals, advocacy, as well as groups on parenting and anger management.

The Midtown Family Place
The Midtown Family Place has 45 families and is based in Hell's Kitchen. It provides counseling, referrals and advocacy, as well as an after-school and summer program for children ages 5-12, a literacy program, and a food pantry.

Project Class
Project CLASS (Clean Living and Staying Sober) serves as many as 50 families. It specializes in providing referrals to drug- and alcohol-abuse programs, as well as creating, implementing and monitoring drug- treatment service plans. It also includes the Babies Initiative, which is offered to 20 families with children ages five and under who are at immediate risk of being put in foster care. This intensive program works to get family members whatever services they need in order to stabilize.

Truancy-Prevention
Truancy-Prevention has 90 families with at-risk children, and conducts groups on domestic violence, groups on parenting called the Parenting Journey, as well as a group for teen-agers.

SCHOOL INVESTMENTS

<u>Promise Academy Charter Schools</u>

Now in its fourth year, Promise Academy 1 currently serves kindergarten, first, second, third, seventh, and eighth-grade students, providing a comprehensive college-preparatory educational program, with an extended school day and school year. Promise Academy 2 is in its third year and currently serves kindergarten, first, second and third-grade students. Both Promise Academy 1 and Promise Academy 2 will serve children from kindergarten through twelfth grade, bringing a strong focus on literacy and mathematics (over two hours of literacy instruction and over 90 minutes of mathematics instruction each day) within a safe, structured and personalized environment. Each of the academies will be divided into four smaller "schools" (primary, elementary, middle, and high school) that will emphasize personalized relationships between students, teachers and families.

The academic day runs from 8 AM until 4 PM, approximately 20 percent longer than the vast majority of surrounding public schools. Students also have the opportunity to participate in after-school programming from 4 PM – 6 PM. The academic year consists of 210 days of school, an increase over the 180 days required by law, which includes a 25-day mandatory summer program.

In 2006, a health clinic opened in the Promise Academy 1 middle-school building so the students could get free medical, dental and mental-health services. The Harlem Children's Health Project is a partnership of the Children's Health Fund, the Mailman School of Public Health at Columbia University, New York-Presbyterian Hospital and HCZ. In addition, the clinic works with the elementary schools to identify children's unmet health needs and to facilitate necessary care.

Appendix B: Data Appendix

Attendance

The attendance variable is the yearly total of absences reported by the Department of Education.

Cohort Group

In our elementary-school sample a student's cohort is imputed from her initial kindergarten year. If we do not observe an elementary student in kindergarten, we impute the expected grade from the student's birth date. In our middle-school sample a student's cohort is imputed from her initial sixth-grade year.[1] If we do not observe a student in sixth grade, we impute an expected grade from the nearest observed grade.

Distance to the Zone

Student addresses were geocoded using a free geocoding utility. The distance from the nearest boundary of the Harlem Children's Zone was calculated using ArcGIS, with addresses within the Zone assigned the distance to the nearest boundary multiplied by negative one.

Lottery Treatment

Students in the lottery sample are considered "treated" if they receive a winning lottery number or are placed high enough on the waitlist to be offered a spot at the charter school. The one exception is the 2004 kindergarten lottery, where all students were offered a spot. In this case, "high" is defined as being high enough on the waitlist to be offered a spot before the start of the academic year.

Lunch Status

Students are defined as being eligible for either free lunch, reduced-price lunch, full-price lunch or as having missing lunch.

On Time

For each elementary student an expected grade was computed using the student's birthdate and the relevant mandated school entry date. For each middle-school student, we use the first year that the student was enrolled in fifth grade to create expected grade. A student is defined as on time if his or her reported grade is at or above his or her expected grade.

Race

The dataset contains a number of race variables. Students are defined as being black, Hispanic, or other (which includes missing).

Siblings

Sibling groups were formed using reported last names and addresses in each year.

Test Scores

Test scores and test grade level are reported by the New York City Department of Education. All scores are standardized to have mean zero and standard deviation one in each grade and year. The student is assigned the standardized score from the first year he or she is tested in that grade.