The Potential of Urban Boarding Schools for the Poor: Evidence from SEED*

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Abstract

The SEED schools, which combine a "No Excuses" charter model with a five-day-a-week boarding program, are America's only urban public boarding schools for the poor. We provide the first causal estimate of the impact of attending SEED schools on academic achievement, with the goal of understanding whether changing both a student's social and educational environment through boarding is an effective strategy to increase achievement among the poor. Using admission lotteries, we show that attending a SEED school increases achievement by 0.211 standard deviations in reading and 0.229 standard deviations in math, per year of attendance. Subgroup analyses show that the eff ects are entirely driven by female students. We argue that the large impacts on reading are consistent with dialectical theories of language development.

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1 Introduction

The racial achievement gap is an empirical fact that manifests itself in every American school district, at every level of schooling, and on nearly every academic assessment. In 2011, the National Assessment of Educational Progress (NAEP), which measures students' levels of proficiency in reading and math, reported that 42 percent of white fourth grade students and 16 percent of black fourth grade students are proficient in reading.[1] In math, 52 percent of white fourth grade students and 17 percent of black fourth graders are proficient. There is not one school district in NAEP in which more than twenty-one percent of black eighth graders are proficient in reading or math (National Center for Education Statistics, 2011).

There have been many attempts to close the achievement gap, including early childhood interventions; smaller schools and classrooms; mandatory summer school; merit pay for principals, teachers, and students; ending social promotion; using "smart" technology; and policies to lower the barrier to teaching via alternative paths to accreditation.[2] Yet, these policies have not substantially reduced the gap in even the most reform-minded districts. There is enthusiasm for charter schools – publicly funded schools that operate outside the direct control of local school districts – but the bulk of the evidence suggests that they perform roughly the same as traditional public schools (Zimmer, Gill, Booker, Lavertu, Sass, and Witte, 2009; Gleason, Clark, Tuttle, Dwoyer, and Silverberg, 2010).[3]

The lack of progress in closing the racial achievement gap has caused many to question whether schools alone can increase achievement among the poor or whether the challenges children bring to

[1]NAEP is a nationally representative set of assessments administered every two years to fourth, eighth, and twelfth graders that cover various subject areas, including mathematics and reading. Individual schools are first selected for participation in NAEP in order to ensure that the assessments are nationally representative, and then students are randomly selected from within those schools. Both schools and students have the option not to participate in the assessments. Tests are given in multiple subject areas in a given school in one sitting, with different students taking different assessments. Assessments are conducted between the last week of January and the first week of March every year.

[2]See Almond and Currie (2011) for a review of early childhood interventions; Krueger (1999), Krueger and Whitmore (2001), and Chetty, Friedman, Hilger, Saez, Schanzenbach, and Yagan (2011) for smaller classes; Bloom, Thompson, Unterman, Herlihy, and Payne (2010) for smaller schools; Jacob and Lefgren (2004) on summer school; Podgursky and Springer (2007) and Fryer (forthcoming) on merit pay; Fryer (2011) on incentives; Jacob and Lefgren (2009) on grade retention; Decker, Mayer, and Glaserman (2004) on Teach for America; and Kane, Rockoff, and Staiger (2008) on teacher certification programs

[3]There are, however, several charter schools and charter management organizations that have demonstrated marked success (Hoxby and Murarka, 2009; Abdulkadiroglu, Angrist, Dynarski, Kane, and Pathak, 2011; Angrist, Dynarski, Kane, Pathak, and Walters, 2010; Dobbie and Fryer, 2011; Gleason, Clark, Tuttle, Dwoyer, and Silverberg, 2010). Raymond (2009) estimates that 17 percent of charter schools outperform traditional public schools.

school as a result of being reared in dysfunctional families and failing communities are too much for all but the best educators to overcome. Consider the case of Washington, D.C.: 24.4 percent of blacks live in poverty, 23 percent of black children are reared in a two-parent household, 7.4 percent of black women will give birth while they are still teenagers, and nearly 50 percent of black men between the ages of 18 and 35 are under Criminal Justice supervision (Lotke, 1998).[4] Brooks-Gunn, Duncan, and Maritato (1999) argue that children who grow up in these types of circumstances tend to score lower than children from more affluent families on assessments of health, cognitive development, school achievement, and emotional well-being. In this scenario, combating poverty, having more constructive out-of-school time, or minimizing negative social interactions with a student's home environment will lead to better and more-focused instruction in school and increased student achievement.

One potential strategy to minimize the gravitational pull of environments with negative externalities, which has yet to be tested, is coupling achievement-minded schools with a boarding program that ensures students have positive and nurturing interactions outside of school.[5] Theoretically, taking students away from their home environment and placing them in a boarding program could have one of three effects. If the environment that the typical student encounters in a boarding school is, on net, more positive than her home environment and the differences between them are correlated with academic achievement, then boarding schools will yield positive gains in

[4]Figures on poverty and family structure were obtained from the U.S. Census Bureau's 2006-2008 American Community Survey 3-Year Estimates. In Washington, D.C., 80 percent of white children are reared in a two-parent household. The percent of black women who will give birth as teenagers was estimated using data from the National Vital Statistics System. There were 908 births to black women aged 15-19 in Washington, D.C., in 2008. Based on data from the U.S. Census Bureau's 2006-2008 American Community Survey 3-Year Estimates, there are about 12,332 black women aged 15-19 in Washington, D.C. The corresponding birth rate among black women in Washington, D.C., aged 15-19 is about 7.4 percent. Being under Criminal Justice supervision is defined as being in prison or jail, being on probation or parole, being out on bond, or being sought on an arrest warrant.

[5]At least one residential program targeted to disadvantaged youths - Job Corps - has had considerable success, leading to a 15 percent increase in annual earnings, reduced dependence on welfare and public assistance by about 2 weeks per year, and a five-fold increase in the probability of obtaining a high school diploma. Job Corps is a program providing economically disadvantaged youths between ages 16 and 21 with basic education, vocational training, and other services in a residential setting. Its primary purpose is to improve the long-term productivity and lifetime earnings prospects of high school dropouts (Mallar, 1982). However, JOBSTART - a program intended to provide training and support similar to that of Job Corps, but in a less expensive, non-residential setting - has had statistically insignificant results. Cave, Bos, Doolittle, and Toussaint (1993) found that effects on earnings four years after the program were not statistically significant, there was little impact on youths' receipt of public assistance, and that while JOBSTART participants increased their educational attainment, this effect was mostly through receipt of the GED (General Educational Development) rather than completion of high school. This evidence suggests that residential programs could be more effective at delivering education and support to disadvantaged youths in urban areas than non-residential programs.

student achievement. If the boarding school environment is not more conducive to achievement, or if the new environment causes psychological or emotional distress or other behavioral responses that hinder a student's academic performance, then boarding schools may have a negative impact on achievement. Finally, if the positive and negative aspects of placing a student in a boarding program roughly balance out, or the differences in the home environment and the boarding school are not correlated with achievement (e.g., less television in boarding school), then the effects of boarding school will be negligible.

The SEED schools, located in Washington, D.C., and Baltimore, Maryland, are America's only urban public boarding schools for the poor. These schools combine a "No Excuses" charter school model with a five-day-a-week boarding program, which provides a rare laboratory to estimate the causal impact of attending an achievement-minded boarding school on student outcomes. The SEED schools serve students in grades six through twelve. Like other "No Excuses" charter schools (e.g., Knowledge is Power Program or Harlem Children's Zone), SEED schools have an extended school day, provide extensive after-school tutoring for students who need support, rely heavily on data to alter the scope, pace, and sequence of instruction, and maintain a paternalistic culture with high expectations. The middle school curriculum focuses on developing basic skills in reading and math, and the high school uses an intensive college-preparatory curriculum that requires all students to take the SAT or ACT college admissions test and apply to at least five colleges or universities in order to graduate.

To account for the fact that students who attend SEED schools may not be a random sample, we exploit the fact that SEED is required to select students by random lottery when applications exceed the available supply of admission slots. The treatment group is composed of students who won the lottery, and the control group is comprised of students who entered the lottery but did not win. This allows us to provide a set of causal estimates of the effect of being offered admission into SEED on student achievement.[6] The results we obtain are interesting and, in some cases, quite surprising. Our lottery estimates reveal that SEED schools are effective at increasing the achievement of the poorest minority children. Each year spent at SEED increases achievement by

[6]Our analysis focuses on the results from the SEED School of Washington, D.C., which has been in operation since 1998. The SEED School of Maryland has only been open since 2008. The first year of operation is usually the most difficult one for any school, and results tend to improve over time, so estimates of effect sizes from the first or second year of operation may not be representative of the effect sizes that one would expect from such a school once it is more established (Zimmer, Gill, Booker, Lavertu, Sass, and Witte, 2009).

0.211 standard deviations in reading and 0.229 standard deviations in math. Taken at face value, these effects are enough to close the black-white achievement gap in both subjects in four years. Estimated treatment effects are substantially larger for girls than boys in both subjects, but due to large standard errors we are only able to reject the null hypothesis of equality in reading. Students eligible for free or reduced price lunch also make more progress than ineligible students in reading. Treatment effects for special education and non-special education students are not statistically differentiable, though we are underpowered to detect small to modest differences.

The impact of SEED on student achievement is significantly larger than that of the average charter school – in fact SEED has one of the largest impacts on reading achievement in the literature. This is consistent with evidence presented by Charity, Scarborough, and Griffin (2004) and Rickford (1999) that shows that a students' familiarity with the Standard English dialect (as opposed to African American Vernacular English) is strongly correlated with reading test scores. If SEED students are more likely to speak non-standard English at home than at school, then a boarding program could result in increased reading gains.

But urban boarding schools are expensive. SEED spends almost $40,000 per student, per year – twice as much per pupil as the Washington DC public schools. A natural question is whether the investment has a positive return. Our lottery estimates suggest that attending the SEED school for one year is associated with a 3.8 percent increase in earnings (Chetty, Friedman, and Rockoff, 2012), a 1.0 to 1.3 percent decrease in the probability of committing a property or violent crime (Levitt and Lochner, 2001), and a 4.4 to 6.6 percent decrease in the probability of having a health disability (Auld and Sidhu, 2005; Elias, 2005; Kaestner, 2009). If SEED affects educational attainment as dramatically as achievement, the implied benefits are enormous (see, e.g. Card (1999) and Oreopoulos (2007)). The public benefits alone from converting a high school dropout to graduate are more than $250,000.[7] Unfortunately, however, calculating the non-test score benefits of attending SEED is difficult and, at this stage in the life-cycle of their oldest cohorts, premature. Whether or not the total benefits of attending SEED outweigh the costs can be known with the passage of time.

The next section of the paper presents some theoretical explanations for why urban boarding schools may (or may not) increase achievement among the poor. Section 3 discusses our data and

[7]See Appendix C for details of these calculations.

research design. Section 4 presents the results of our analysis, and the last section concludes. There are three online appendices. Appendix A details programs and services provided by SEED schools in both their academic and residential boarding programs. Appendix B is a data appendix that details our sample and variable construction. Appendix C provides further details and assumptions behind our cost-benefit calculations.

2 Conceptual Framework

SEED schools, like many other charter schools, can be interpreted as a change in the quantity and quality of inputs to the education production function. Ideally, students would be exposed to different bundles or intensities of inputs so we might better understand what elements of the production function are most important in increasing achievement. Unfortunately, since the input bundles do not vary significantly across SEED students, we cannot identify the production function without essentially assuming the result. Instead, we discuss the major hypotheses about how urban boarding schools might affect student achievement and attempt to connect these theories to anecdotal accounts of SEED practices.[8]

Potential Costs of Urban Boarding Programs

A large literature in sociology and psychology describes the potential costs of boarding schools, though much of the evidence is qualitative and should be interpreted with care. In this section, we highlight four potential channels: homesickness, stress, lack of positive parental support or input, and loss of identity (or what sociologists refer to as "double marginalization").[9]

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[8]Boarding schools have a long and controversial history as educational and socializing institutions in a variety of socioeconomic contexts around the world (Kahane, 1988). For instance, elite English and American boarding schools have been described by sociologists as conservative institutions aimed at preserving an existing social order (Kahane, 1988; Levine, 1980; Cookson and Persell, 1985; Zweigenhaft, 1992). In stark contrast, boarding schools also have a history as tools for assimilation for groups such as Native Americans (Adams, 1995; Ellis, 1996). In the late 1800s and early 1900s, Congress aggressively pushed to assimilate Native Americans through education, establishing 147 reservation day schools, 81 reservation boarding schools, and 25 off-reservation boarding schools with the explicit goal of inculcating Native American children with Protestant values (Adams, 1988).

[9]Other potential channels through which boarding schools may impose costs on students include: lack of parental supervision leading to engagement in adult behaviors, failure to develop independent decision-making ability as a result of overdependence on boarding school structure, or increased likelihood of substance abuse. Although there is evidence that a lack of parental supervision may make students more likely to engage in substance abuse and sexual activity (Barnes and Farrell, 1992; Chilcoat and Anthony, 1996; Dishion and McMahon, 1998), the effect of attending boarding school on this channel is unknown. There is also a lack of evidence of the effects of boarding schools on developing independence, or what effect this would have on achievement. While there is a literature on boarding schools as a trigger for increased substance abuse (Koss, Yuan, Dightman, Prince, Polacca, Sanderson, and Goldman,

If young students living away from home are homesick and as a result have difficulty concentrating or coping with academic work, then this could have adverse effects on student achievement (Fisher, Murray, and Frazer (1985)). In a study of Scottish boarding school students, Fisher, Frazer, and Murray (1986) found that approximately 70 percent reported being homesick at some point during their first year. Relatedly, Dick, Manson, and Beals (1993) suggest that adolescents are exposed to particularly high levels of stress as a result of social, physical, cognitive, and academic growth, and these stress levels can be exacerbated by sending a youth to boarding school, particularly if students lack familial support.

Lack of parental support and input is a third potential cost of boarding schools. If parental interactions such as discussing school-related activities at home each evening or getting help with homework contribute to academic success, then boarding schools may undercut academic achievement. If the boarding program results in parental detachment and less parental input, then the SEED schools may be less accountable to parents and school quality may be less than one would expect given other observable school inputs.

A fourth potential cost of urban boarding schools is one that may be particularly acute in urban areas: loss of identity. Arieli, Beker, and Kashti (2001) note that the risk of so-called "mainstreaming settings" – residential settings intended to introduce children from lower socioeconomic classes to the social and cultural mainstream of a society – is that they can confuse a child's sense of identity, a problem that sociologists have termed "double marginalization." In these circumstances, black students can develop an oppositional identity, view academic achievement as the prerogative of white people, and discourage their peers from striving for academic success, accusing them of "acting white" if they strive for success (Fordham and Ogbu, 1986; Fryer and Torelli, 2010).

Potential Benefits of Urban Boarding Schools

A complementary literature in sociology and psychology emphasizes the potential benefits of urban boarding schools including: placing students in safer, less volatile, and less stressful environments; minimizing negative parental and community interactions; and ensuring that students have positive adult role models, are provided with nutritious foods, and spend less time being idle. As stated in the Introduction, minority children are significantly more likely to be reared in a single

2003; Kleinfeld and Bloom, 1977), this is generally focused on Native American boarding schools, which students often attended unwillingly; its applicability to urban public boarding schools is doubtful.

female-headed household – 69 percent of black family households with children under the age of 18 in Washington, D.C., are single-mother households; for whites, this figure is 14 percent.[10] Many believe that until children's basic needs – security, stability, and frequent and positive parental interactions – are met, investments in education reform are futile. (Gonzales, Cauce, Friedman, and Mason, 1996; Ainsworth, 2002; Rothstein, 2004; Duncan and Magnuson, 2005; Brooks-Gunn and Markman, 2005). In this scenario, putting students in more stable environments will lead to greater focus in schools and increased academic achievement.

Perhaps equally important, boarding schools can be agents for delivering scholastic and social capital to their students. Many "No Excuses" charter schools desire to instill mainstream middle-class values and other non-cognitive skills into their students, as some posit that this type of education is essential for improving academic achievement among low-income students (Rosen, 1956; Mickelson, 1990; Whitman, 2008). Indeed, the slogan for the nation's largest network of charter schools is an explicit endorsement of the Protestant work ethic ("Work hard. Be nice."). In his analysis of six high-performing inner-city schools, Whitman (2008) argues that the success of "No Excuses" charter schools (SEED is one of the six schools profiled in the book) can be attributed to the fact that these schools paternalistically "micro-manage" their students' lives and teach them to act according to middle-class values. If this process of middle-class acculturation is a key ingredient to academic success, then a school equipped with a boarding program could be more effective at inculcating these values in its students.

Finally, the very nature of the boarding program ensures that students will spend much more time with their schoolmates in a structured, supervised setting. It is therefore plausible that boarding programs could intensify "peer effects" that result from attending school with a different set of students.

Anecdotal Evidence from SEED

We have identified at least eight potential channels through which an urban boarding program could potentially affect student achievement, positively or negatively. In an attempt to provide some insight into the possible mechanisms at play, we turn to narrative evidence of the environment at SEED.

SEED students are certainly not immune to homesickness, but it seems likely that it is less of

[10] Data obtained from the U.S. Census Bureau's 2006-2008 American Community Survey 3-Year Estimates.

a problem than at traditional boarding schools, since students return to their homes and neighborhoods on the weekends. What's more, while students spend less time at home with their parents and guardians, they receive different types of home inputs that may be good substitutes. Students are under adult supervision nearly 24 hours per day, and each SEED dorm is staffed by a Life Skills Counselor. Whitman (2008) describes these staff-members as "surrogate parents" and quotes a teacher who observes that SEED students receive more adult attention at school than they would have at home. Given 22 percent of lottery applicants live in dual-parent households this claim seems plausible.

The boarding program may also allow SEED to have a greater influence on certain character traits that could have important implications for learning. SEED's emphasis on non-cognitive skills is largely governed by its Habits for Achieving Life Long Success (HALLS) curriculum, which includes a detailed program of 200 lessons, including information about nutrition, etiquette, and social skills (Jones, 2009). The increased time at school and adult attention may make it easier for SEED to teach these skills than non-residential school. If these non-cognitive traits translate into education production – e.g. by increasing study skills, grit, or highlighting the importance of education and college – then they might result in academic gains above and beyond a similar non-residential school.

Finally, SEED is a safer physical environment than many students experience at home. The average crime rate for the zip codes inhabited by SEED applicants is higher than the D.C. average. SEED goes to great lengths to guarantee the physical safety of its students, even installing iron gates and walls after a suspected criminal entered a dormitory while fleeing the police during the 2002-03 school year (Whitman 2008). If a safer environment reduces stress or facilitates schoolwork in other ways, this could produce further achievement gains.

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This paper's main goal is to produce credible causal estimates of the net impact of attending urban boarding schools on student achievement. The resulting "reduced form" estimates will likely reflect a number of the costs and benefits specified in this section.

3 Data and Research Design

We merge data from two sources: information from files at the SEED school and administrative data on student demographics and outcomes from the District of Columbia Public Schools (DCPS).[11]

The data from SEED consist of lottery files from the 2007 and 2008 lotteries. To ensure that all students in our lottery sample have an equal chance of being admitted to SEED, we drop students with a sibling already enrolled in SEED (they are guaranteed admission). Since siblings who apply together are more likely to get in (if one sibling wins the lottery then all siblings are allowed to enroll), we also include a dummy to indicate the presence of a sibling in the lottery, as well as the interaction of this dummy with year of application. Excluding the applicants with siblings in the same lottery yields results that are almost identical.[12]

A typical student's data from SEED's administrative files contains the applicant's cohort, first and last names, date of birth, whether and how the applicant was offered admission (immediately, off the waitlist, or not at all), whether the applicant already had a sibling attending SEED (and was therefore guaranteed admission), whether the applicant applied late to SEED (and was therefore simply added to the end of the waitlist and not included in the lottery), and, if applicable, date of withdrawal from SEED. The files also include demographic data such as sex, race, free lunch eligibility, special education status, English Language Learner status, and family background variables such as the student's living arrangement, parents' marital status, and parents' highest level of education (though the data fields for the latter two variables are quite sparse).

The SEED data were matched to administrative data from the District of Columbia Public Schools (DCPS) collected from 2005-06 through 2008-09 using the maximum 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 versus non-hyphenated)

2. Last name, first name, and various versions of the date of birth (most often the month and day reversed)

[11]Seventeen students in the SEED lottery have test scores from the Maryland Schools Assessment (MSA). We include these students in our main sample, though dropping them does not affect our conclusions. Results omitting the seventeen students are available from the authors upon request.

[12]No student entered both the 2007 and 2008 lotteries.

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.

In our final sample, the proportion of students for whom at least one achievement test score was matched is 95 percent for SEED lottery winners (N=129) and 92 percent for SEED lottery losers (N=92). Details of the match rates and attrition for each lottery cohort are reported in Table 1. Our match rates and attrition are similar to those from previous work using charter lottery data (Abdulkadiroglu, Angrist, Dynarski, Kane, and Pathak, 2011; Dobbie and Fryer, 2011; Hoxby and Murarka, 2009; Angrist, Dynarski, Kane, Pathak, and Walters, 2010).

The DCPS data contain student-level administrative data on approximately 45,000 students in each year. The data include information on student race, gender, free and reduced-price lunch eligibility, attendance, and math and reading achievement scores in grades three through eight and ten. The math and reading tests, extracted from the District of Columbia Comprehensive Assessment System (DC CAS), are administered each April to students in grades three through eight and ten. The DC CAS exams measure knowledge and skills in reading and math.

In Washington, D.C., all public school students, including those attending charters, are required to take the reading and math tests unless they are medically excused or have a severe disability. Students with moderate disabilities or who are English Language Learners 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.

Summary statistics for the variables that we use in our core specifications, as well as student living situation, are displayed in Table 2. Column 1 includes all students who attended seventh grade in DCPS in 2007-08 and 2008-09, and Column 2 restricts the sample to DCPS students who reside in "SEED neighborhoods," defined as those zip codes in which at least 5.8 percent (the median value in the DCPS seventh grade sample) of eligible students enter a SEED lottery. Columns 3 and 4 divide the sample into lottery winners and losers, respectively. Columns 5 through 6 report the difference in means (and the associated standard errors) between SEED applicants and the entire DCPS sample, and between SEED applicants and other students in SEED zip codes. The final column reports covariate differences, and their associated standard errors, between lottery winners and losers controlling for lottery fixed effects, sex indicator variables (since separate lotteries are held for males and females), and a contemporaneous sibling dummy, as well as the interactions of the sibling and gender dummies with year of application.

Every student in the SEED lottery sample is black. Males are more likely to be lottery winners than females, but this is due to the fact that the SEED school holds separate lotteries for males and females and receives more applications from females. Relative to the average DCPS student, SEED applicants have higher baseline test scores, but these differences are not significant. SEED applicants are significantly more likely to be eligible for free lunch and significantly less likely to be special education students. Relative to students in their own neighborhoods, however, SEED applicants have noticeably higher test scores – 0.216σ in reading and 0.243σ in math – and are about as likely to be free-lunch-eligible.

Lottery winners have slightly higher baseline reading and math scores, but the differences are not statistically significant. Free lunch status and special education status are also balanced between lottery winners and losers. There are two marginally significant differences between lottery winners and losers: (1) lottery winners are 9.7% (s.e. 4.5) less likely to be English Language Learners; and (2) lottery winners are less likely to live with other legal guardians. Although there are some differences between SEED lottery winners and losers on observable characteristics, the randomness of the lottery implies that these arise by chance. We correct for these imbalances as much as possible by including extensive controls in our main results.

To complement Table 2, Appendix Figure 1 shows the geographic distribution of treatment and control students across Washington, D.C., as well as census tract poverty rates. This map confirms that SEED treatment and control students are similarly distributed across space and are more likely to live in higher-poverty areas of the city.

3.1 Research Design

Our research design exploits the fact that oversubscribed charter schools in Washington, D.C., are required to admit students via random lottery. This allows us to provide a set of causal estimates of the effect of attending the SEED school. Let the effect of attending the SEED school on student achievement be a linear function of the number of years spent at the school $(SEED_{igt})$. We estimate this effect using the equation:

$$achievement_{igt} = \alpha_t + \beta_g + \delta' X_i + \rho SEED_{igt} + \epsilon_{igt} \tag{1}$$

where $achievement_{igt}$ denotes the test score of student i tested in grade g in year t, α_t and β_g are year-of-test and grade-of-test effects, and X_i is a vector of demographic controls, which include an indicator variable for sex, since separate lotteries were conducted for males and females, as well as baseline test scores in reading and math, free lunch eligibility, special education status, and English Language Learner status; ϵ_{igt} is an error term that captures random variation in test scores.[13]

The causal effect of attending SEED is ρ. If the number of years a student spends at SEED were randomly assigned, ordinary least squares estimates of equation (1) would capture the average causal effect of each year spent at SEED. Because students and parents selectively choose whether to enroll in SEED, however, these estimates are likely to be biased by correlation between school choice and unobserved characteristics related to student ability, motivation, or family background.

We identify ρ by comparing the 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 in the charter school. We define lottery winners as students who receive a winning lottery number or are offered admission off of the waitlist. Given the size of the estimated treatment effect, our results are robust to other definitions of "lottery winner."

Under several assumptions (that the treatment group assignment is random and that winning the lottery only affects outcomes through SEED enrollment), we can estimate the average effect of treatment for students induced into enrollment by the lottery offer. The parameter is estimated through a two-stage least squares (2SLS) regression of student outcomes on years of enrollment ($SEED_{igt}$) with the lottery offer as an instrumental variable for enrollment.

The first stage equations for IV estimation take the form:

[13] All students in the lottery sample are black, so race is not included as a covariate.

$$\begin{aligned} SEED_{igt} =&\zeta_t + \eta_g + \sum_j \mu_j lottery_{ij} + \sum_j \gamma_j lottery_{ij} * 1(female_i) \\ &\sum_j \phi_j lottery_j * 1(sibling_i) + \iota X_i + \pi Z_i + \kappa_{igt}, \end{aligned} \tag{2}$$

where the lottery indicators $lottery_{ij}$ – interacted with an indicator for gender $[1(female_i)]$ and for whether a student has a sibling entered into the same lottery $[1(sibling_i)]$ – control for which lottery the student entered. π captures the effect of the lottery offer (Z_i) on the number of years a student spends at SEED.

4 The Impact of Attending SEED Schools on Student Achievement

Table 3 reports lottery results for the pooled sample consisting of the 2007 and 2008 cohorts at the SEED School in Washington, D.C. We report first stage (Column 1), reduced-form (Column 2), and 2SLS estimates (Column 3). There are two panels: the top panel displays the results for reading scores and the bottom panel presents analogous results for math scores. Within each panel, we estimate three specifications of equation (1). The first contains no controls, the second controls for previous year's achievement test scores in both reading and math, and the third specification includes controls for free lunch eligibility, special education status, and English Language Learner status. The outcome variable is seventh grade test scores from both cohorts and eighth grade test scores for the 2007 cohort.

Lottery winners score 0.264σ (0.100) higher in reading and 0.388σ (0.117) higher in math in the raw data. Controlling for previous scores and demographic variables reduces these effect sizes to 0.201σ (0.086) and 0.218σ (0.082) in reading and math, respectively. The first stage coefficients are all less than one, which is consistent with other work on "No Excuses" charter schools (Abdulkadiroglu, Angrist, Dynarski, Kane, and Pathak, 2011). The 2SLS estimate, which captures the causal effect of attending the SEED school for one year for students induced into enrolling by the lottery offer, is 0.211σ (0.092) in reading and 0.229σ (0.085) in math after controlling for baseline scores and demographics.

The magnitudes of our estimates in math are similar to those from other "No Excuses" charter schools, which range from 0.26σ to 0.54σ (Abdulkadiroglu, Angrist, Dynarski, Kane, and Pathak, 2011; Angrist, Dynarski, Kane, Pathak, and Walters, 2010; Dobbie and Fryer, 2011). The magnitudes of the results in reading, however, are surprising. The literature has typically found treatment effects on reading for middle school-aged or older children, under a host of interventions, to be significantly smaller than in math (Decker, Mayer, and Glaserman, 2004; Abdulkadiroglu, Angrist, Dynarski, Kane, and Pathak, 2011; Angrist, Dynarski, Kane, Pathak, and Walters, 2010; Dobbie and Fryer, 2011; Hoxby and Murarka, 2009; Fryer, 2012).

One of the leading theories for this result is that reading scores are influenced by the language spoken during the time when students are outside of the classroom (Charity, Scarborough, and Griffin, 2004; Rickford, 1999). Charity, Scarborough, and Griffin (2004) argue that if students speak non-standard English at home and in their communities, increases in reading scores are difficult to effect – especially for older students. The surprising effect of SEED on reading scores is broadly consistent with this point of view.

Tables 4 and 5 explore the heterogeneity of our estimated treatment effects in a variety of subsamples of the data and report p-values for the differences in the treatment effects. Each table reports 2SLS estimates that include baseline scores and demographic controls. Table 4 partitions the data by sex, whether or not a student is eligible for free lunch, and special education status.

The most striking result is within the gender subgroups. Taken literally, the point estimates imply that our findings are driven entirely by the female lottery applicants. The 2SLS estimates for females (including controls for baseline scores and demographic characteristics) are 0.382σ in reading (-0.138σ for males) and 0.265σ in math (0.037σ for males). The difference between males and females is significant for reading, but we cannot reject the null hypothesis that the effects are the same for math.

However, it is important to note that we are under-powered to detect whether there are modest positive effects for males – even though it is interesting to note the similarities to the gender differences observed in the Moving to Opportunity (MTO) experiment (Kling, Liebman, and Katz, 2007), which suggests that removing students from their home environment have particularly bad effects for boys. We urge caution in over-interpreting a single subgroup finding, however, as our preferred interpretation is that this result is suggestive at best.[14]

[14] The MTO results persisted across multiple cities and rounds of follow-up, whereas we are just beginning to scrape the surface of research on the effects of urban boarding schools. Understanding the mechanisms that could be

Students eligible for free lunch experienced larger effects than students who are not eligible for free lunch; this difference is marginally insignificant for reading. Estimated effects are also slightly larger for students who are not in special education compared to students who are in special education, but large standard errors prevent sharp conclusions.[15]

Table 5 examines whether the effects of SEED on achievement differ as a function of a student's pre-treatment test score, both by examining the effects of SEED for students above and below the median of the previous year test score, and by estimating a model that adds the interaction between baseline score and an indicator for winning the SEED lottery as an additional instrument. The results suggest that lower-ability students benefit more from SEED. Students with below-median baseline scores gained 0.347σ (0.137) in reading, which is significantly different from the effects for students who are above the median when they enter SEED (-0.044σ (0.082)). Similarly, students with below-median baseline scores showed gains of 0.358σ (0.139) in math, compared to 0.162σ (0.117) for students with above-median baseline scores – but due to low power we are unable to distinguish between these two point estimates.

The estimates from the baseline score interaction model also suggest that SEED may have larger effects for lower-ability students, but, again, coefficients are too imprecisely estimated to make definitive conclusions. The interaction terms for reading and math are -0.126σ and -0.084σ, respectively, which suggest that a student who is 0.5σ below the mean in terms of ability (as measured by baseline test score) would gain an additional 0.063σ in reading and 0.042σ in math per year. These interaction term coefficients are very similar to those reported by Angrist, Dynarski, Kane, Pathak, and Walters (2010) for the effects of attending a "No Excuses" charter school in Lynn, Massachusetts, on students of lower baseline ability. Still, our estimates should be interpreted with caution given the lack of power.

A potential worry is that our lottery estimates use the sample of students for which we have post-lottery scores. If lottery winners and losers have different rates of selection into this sample, our results may be biased. Table 6 compares the rates of attrition of lottery winners and lottery losers. In the pooled sample, 86.1 percent of winners and 87.9 percent of losers have reading scores.

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driving these observations is likely a fruitful path for future research.

[15] One might expect students from more disadvantaged backgrounds to reap larger benefits from SEED attendance. To investigate this hypothesis, we partition our sample based on two factors: the crime rate in a student's home census tract and his/her primary caregiver. The results (shown in Appendix Table 1) show no evidence of significant heterogeneity along these dimensions.

A simple test for selection bias is to investigate the impact of the lottery offer on the probability of entering our lottery sample. The results of this test are reported in Columns 3 through 5 of Table 6 – the difference is statistically zero. Similarly, 86.1 percent of winners and 86.4 percent of losers have math scores, and this difference is also statistically zero. This suggests that differential attrition is not likely to be a concern in interpreting the results [16]

5 Discussion

Our lottery estimates reveal that SEED is effective at increasing achievement among poor minority students. Students who enroll in SEED increase their achievement by 0.211σ in reading and 0.229σ in math, *per year.* Thus, SEED schools have the power to eliminate the racial achievement gap in four years.

Let us put the magnitude of our estimates in perspective. The effect of lowering class size from 24 to 16 students per teacher is approximately 0.22σ (0.05) on combined math and reading scores (Krueger, 1999). While a one-standard deviation increase in teacher quality raises math achievement by 0.15σ to 0.24σ per year and reading achievement by 0.15σ to 0.20σ per year (Rockoff, 2004; Hanushek and Rivkin, 2005; Aaronson, Barrow, and Sander, 2007; Kane, Rockoff, and Staiger, 2008), value added measures are not strongly correlated with observable characteristics of teachers, making it difficult to identify the best teachers ex ante. The effect of Teach for America, one attempt to bring more skilled teachers into poorly performing schools, is 0.15σ in math and 0.03σ in reading (Decker, Mayer, and Glaserman, 2004). The effect of Head Start is 0.147σ (0.103) in applied problems and 0.319σ (0.147) in letter identification on the Woodcock-Johnson exam, but the effects on test scores fade in elementary school (Currie and Thomas, 1995; Ludwig and Phillips, 2007).

[16] To provide further evidence that attrition is not driving our results, we also conduct bounding exercises motivated by Manski (1995), Juhn (2003), and Lee (2009). To implement the former approaches, we sort attriters into groups with identical demographic information and baseline test deciles, and then calculate the 25th and 75th percentile of scores for DCPS students with the same observable characteristics. We can then recalculate our treatment effects, assuming that treatment attriters would have scored at the 25th percentile and control attriters at the 75th percentile within these groups (this spread is equivalent to a -0.70σ treatment effect in math and a -0.62σ effect in reading.) Appendix Table 2 shows the results of this exercise. Unsurprisingly, our results are smaller but qualitatively similar to our main specification – 0.142σ (0.084) in reading and 0.159σ (0.080) in math. Lee (2009) proposes an alternative approach to bounding the attrition effect that involves dropping certain students based on rates of *differential* attrition. As Table 6 shows, treatment students are at most 2 percent more likely to attrite; dropping the lowest-performing 2 percent of the control sample also does not affect our results.

These effect sizes are a small fraction of the impact of attending SEED. An emerging literature on "No Excuses" charter schools finds effect sizes closest to our own.[17] Abdulkadiroglu, Angrist, Dynarski, Kane, and Pathak (2011) and Angrist, Dynarski, Kane, Pathak, and Walters (2010) find effect sizes similar to ours, with students enrolled in a set of Boston area "No Excuses" charter middle schools gaining about 0.4σ per year in math and 0.1σ per year in reading. Dobbie and Fryer (2011) report that the impact of attending the Harlem Children's Zone's middle schools is 0.26σ in math and 0.05σ in reading. The key difference is that SEED schools increase reading scores more than the typical "No Excuses" charter.

As the Obama administration and other governments around the U.S. decide whether and how to use urban boarding schools as a model to increase achievement among the poor, cost is an important consideration. At the SEED School in Washington, D.C., about \$39,275 is spent per pupil per year, compared to \$20,523 per student in District of Columbia Public Schools (DCPS).[18] Therefore, a natural question arises for policymakers: is the extra \$18,752 per student per year a good investment?

Taken at face value, the achievement gains of SEED students will translate into improved life trajectories. Our lottery estimates suggest that attending the SEED school for one year is associated with a 3.8 percent increase in earnings (Chetty, Friedman, and Rockoff, 2012), a 1.0 to 1.3 percent decrease in the probability of committing a property or violent crime (Levitt and Lochner, 2001), and a 4.4 to 6.6 percent decrease in the probability of having a health disability (Auld and Sidhu, 2005; Elias, 2005; Kaestner, 2009). If SEED affects educational attainment as dramatically as achievement, the implied returns are dramatic (e.g. Card, 1999; Philip Oreopoulos, 2007). The public benefits alone from converting a high school dropout to graduate are more than \$250,000.[19] Moreover, results from Chetty, Friedman, Hilger, Saez, Schanzenbach, and Yagan (2011) suggest that long term benefits of a high quality education may operate through non-test score channels we do not observe in this paper.

We hope that our analysis provides a sense of optimism for work on the achievement gap. Evidence from SEED along with recent results in Abdulkadiroglu, Angrist, Dynarski, Kane, and

[17] The fact that "No Excuses" charter schools coupled with a boarding option increases achievement similar to "No Excuses" charter schools without boarding is consistent with the evidence on neighborhood effects described in Kling, Liebman, and Katz (2007).

[18] See Appendix C for details of per pupil expenditure figures.

[19] See Appendix C for details of these calculations.

Pathak (2011), Angrist, Dynarski, Kane, Pathak, and Walters (2010), and Dobbie and Fryer (2011) demonstrate that the right combination of school inputs can be successful. The challenge going forward is to find ways to take these efforts to scale.

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Online Appendix A: SEED Program Details

The SEED (School for Educational Evolution and Development) Foundation was founded in 1997 and opened its first school in Washington, D.C., in the fall of 1998. The school enrolled 40 seventh graders in its inaugural year and differs from other charter schools in a significant way: the SEED School is an urban public boarding school. From Sunday night through Friday afternoon, students live on campus, returning home for 48 hours every weekend.

The impetus for the creation of SEED was the idea that the educational opportunities of many urban students are hindered both by failing public schools and by neighborhood risks and distractions that divert attention from educational pursuits. The founders of SEED believed that an urban boarding school could remove the dangerous distractions of the urban neighborhoods from which its students hailed, and provide its students with added support and activities during the after-school hours when traditional public schools send students home.

The school was originally housed in the attic of the Capital Children's Museum before moving in 2001 to its current location in Washington, D.C.'s impoverished Ward 7. The campus, which is on the site of a former public school, consists of an academic building, two dormitories (one male and one female), and a student center. The size of the student body has expanded from the original 40 seventh graders to now serve 320 students from sixth through twelfth grades.

The SEED Foundation opened its second school in the fall of 2008, located in Baltimore but open to students throughout Maryland. The Maryland school currently serves 160 students in sixth and seventh grades and will expand to serve 400 students in grades six through twelve. The school has a campus layout similar to that of the Washington, D.C., campus, with dormitory buildings, academic buildings, and recreational facilities. The major difference between the two campuses is size: the Washington, D.C., campus is four acres, while the Maryland campus is fifty-two acres and shares land with a natural preservation area.

Both schools admit students by a lottery if more students apply than there are spots available. To enter the lottery, students and their parents must complete a thorough application and prove eligibility for the lottery, including proof of residency and age eligibility. In order to promote geographic diversity, the SEED School of Maryland reserves a spot for one student from each county from which an eligible student applies.

Because of the boarding aspect of the program, the schools are much more expensive to operate than traditional public schools, or even more highly funded charter schools (such as the Harlem Children's Zone). In 2008, the District of Columbia Public Schools spent approximately $20,523 per pupil; SEED's expenditures were around $39,275 per pupil.

Academic Program

The academic component of the SEED model is broken into two basic pieces, the middle school program and the high school program. The middle school curriculum is focused on basic skills with the goal of allowing all students to enter the high school program performing at or above grade level. The benchmark standards that middle school students must master before promotion to high school are referred to as the "Gate." In order to help students meet the goals of the Gate, students are provided with tutoring outside of the classroom and extra periods of instruction. Students who need more time to master grade level skills can take a "growth year" during middle school. The middle school curriculum utilizes a readers and writers workshop model for language arts instruction and is designed such that all students will take algebra by eighth grade.

The high school curriculum is a college-preparatory program of studies for all students. To graduate, students must complete four years of English, four years of mathematics (through at least Algebra II), three years of social studies, three years of science, three years of a foreign language, one and a half years of physical education and health, one year of arts, one half year each of U.S. government and politics, Washington, D.C., history, and technology, as well as five and a half years' worth of elective courses. In addition to course requirements, students must also take the SAT or ACT college admissions test, apply to at least five colleges or universities, and complete sixty hours of community service in order to graduate. The school offers Advanced Placement courses in English Literature, English Language, U.S. History, Government, and Biology.

Both the Washington, D.C., and Maryland schools at the middle and high school levels have an extended school day, from 8 a.m. until 4 p.m., and provide students with extensive after-school tutoring as needed. Instruction within the schools relies heavily on data. The SEED schools use internal interim assessments and have data days every quarter for the staff (both academic and boarding) to review student data. There is a strong emphasis on preparation for college from the time the students enter the school that begins more informally in middle school and is a formal part of the curriculum in high school. While the SEED schools are only open for a traditional school year, SEED staff try to place students in educational programs during the summer months.

Residential Program

From Sunday evening through Friday afternoon, students live on campus, in double bedrooms in same-sex dorms. Students are organized into "houses" of 12-14 students within the dormitories. The houses are all named for a college or university and have study hall and meal times together, as well as other activities such as book clubs, field trips, and community service. The school offers athletic and other extracurricular activities to students after school hours, as well as a program known as HALLS (Habits for Achieving Life-Long Success) that teaches students study skills, time management, and interpersonal communication. Students can complete homework in their dorm rooms or in one of the common study spaces available throughout the dormitory. There is a computer in each dorm room as well as in the common areas and the residential staff are available during homework times to answer questions. The residential staff is separate from the school faculty, although the two groups interact often to discuss student progress.

Despite the fact that students are living away from their families for the majority of the week, SEED offers some opportunities for parental involvement. SEED holds community dinners and gives parents the opportunity to serve as tutors during study hall, assist during extracurricular activities, and participate in book clubs.

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Online Appendix B: Data Description

SEED

The data obtained from the SEED School in Washington, D.C., include lists of lottery applicants in 2007 and 2008 and whether or not they were admitted immediately, as well as call logs documenting the calls made to candidates on the waitlist. Data also include SEED enrollment lists from each year, as well as a number of other administrative files.

A typical student's data from SEED's administrative files contains the applicant's cohort, first and last names, date of birth, whether and how the applicant was offered admission (immediately, off the waitlist, or not at all), whether the applicant already had a sibling attending SEED (and was therefore guaranteed admission), whether the applicant applied late to SEED (and was therefore simply added to the end of the waitlist and not included in the lottery), and, if applicable, date of withdrawal from SEED. The files also include demographic data such as sex, race, free lunch eligibility, special education status, English Language Learner status, and family background variables such as the student's living situation, parents' marital status, and parents' highest level of education (though the data fields for the latter two variables are sparse). The files were used to compile a list of lottery applicants with their lottery outcomes and enrollment statuses. This list was examined by SEED officials, who used applicant records (such as copies of original SEED applications) to resolve discrepancies.

In addition, other administrative files that were provided by SEED contained lottery registration data, such as students' addresses, parents' names, previous school attended, and reasons for applying to SEED. The address data were used in conjunction with last names to determine siblings who registered for the same lottery.

District of Columbia Public Schools

District of Columbia Public Schools (DCPS) administrative data were collected for school years from 2005-06 through 2008-09. These files contain scores from the District of Columbia Comprehensive Assessment System (DC CAS) and enrollment files containing information on the school and grade level of each student in DCPS as well as demographic information such as race, sex, free lunch eligibility, special education status, and English Language Learner status. In addition, enrollment files contain last name, first name, and date of birth, which were used to match students to SEED data. Furthermore, all students in DCPS data are assigned a unique identifier called the "pupil number." This identifier was available for many students in the SEED data, as well.

In Washington, D.C., all public school students, including those attending charters, are required to take the reading and math tests unless they are medically excused or have a severe disability. Students with moderate disabilities or who are English Language Learners 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. The DC CAS is administered each April to students in grades 3 through 8 and 10. It measures knowledge and skills in reading and math. Students in grades 4, 7, and 10 also take a composition test; students in grades 5 and 8 also take a science test; and students in grades 9 through 12 who take biology also take a biology test.

Crime Rates

Data on criminal incidents was downloaded from the Washington D.C. Police Department (DCPD) Data Catolog, accessible at `http://data.octo.dc.gov/Main_DataCatalog.aspx`. We use tables indicating the geocoded location of all violent crimes and thefts recorded in 2007 (the year of the first SEED lottery we analyze). These tables were geographically merged with census tract shapefiles and population estimates obtained from the Census bureau. The crime rate variable that we use to separate lottery applicants into "high-crime" and "low-crime" tracts is the number of 2007 crimes in the DCPS database divided by the population estimate in the 2000 Census.

Online Appendix C: Cost-Benefit Analysis

Calculating Costs

According to an audited DCPS enrollment file, there were 329 students enrolled in the SEED School of Washington, D.C., as of October 6, 2008. According to the SEED School of Washington, D.C.'s financial report for the 2008-09 fiscal year, SEED's total expenses were $12,921,449. This amounts to $39,275 per student.

According to the National Center for Education Statistics (NCES), total expenditures per pupil in the District of Columbia Public Schools were $20,596 in 2007-08 dollars for the 2006-07 school year. This figure is obtained from Table 186 of the NCES's List of 2009 Digest Tables, which can be found at the following web site: `http://nces.ed.gov/programs/digest/2009menu_tables.asp`. Assuming similar expenditures for 2008-09, this amounts to $20,523 per student in 2009 dollars.

Calculating Benefits

Using evidence from the British National Child Development Study, Currie and Thomas (2001) find that students who score in the upper quartile of the reading exam earn 20 percent more than students who score in the lower quartile, while students who score in the upper quartile of the math exam earn 19 percent more than students who score in the lower quartile. Following Krueger (2003) and assuming a normal distribution of test scores, we can assume that the average score for the top quartile is about 2.5 standard deviations higher than the average score for the bottom quartile, so that a one-standard deviation increase in reading scores is associated with 8.0 percent higher earnings at age 33. A similar calculation reveals that a one-standard deviation increase in math scores is associated with 7.6 percent higher earnings at age 33.

Using the National Longitudinal Survey of Youth, Neal and Johnson (1996) find that a one-standard deviation increase in scores on the Armed Forces Qualification Test (AFQT) taken at age 15-18 (and adjusted for age at time of test) is associated with 20 percent higher earnings for both men and women. As Krueger (2003) points out, the differences in these two sets of estimates can be reconciled by the fact that Neal and Johnson (1996) estimate the effect of one achievement score, whereas Currie and Thomas (2001) include both reading and math achievement scores in their wage equation.[20]

Finally, Chetty, Friedman, and Rockoff (2012) estimate the long-run effects of being assigned to teachers of varying quality, as measured by value-added models. Pooling math and reading together, they find that teachers who raise test scores by one standard deviation on average also increase their students' income at age 28 by $1,815, or 8.7% of the sample mean – comfortably within the range of the estimates already discussed. Unlike the cross-sectional correlations, however, Chetty, Friedman, and Rockoff (2012)'s estimates directly relate treatment effects on educational achievement to subsequent earnings. Hence, for our preferred estimate, we assume that the increase in wages associated with a one-standard deviation increase in either reading or math is 8.7 percent. We also consider how the estimate changes if we use the lower and upper bounds of the effects summarized above.

Cost-Benefit Analysis

Let C_t denote the additional cost of SEED per pupil in year t, net of normal DCPS costs, and let r be the real discount rate. If $t = 1$ corresponds to the student's sixth grade year, the present value of the costs of SEED can therefore be written as

$$Present\ Value\ of\ Costs = \sum_{t=1}^{3} \frac{C_t}{(1+r)^t}. \tag{3}$$

For the 2007 and 2008 cohorts, admitted students were enrolled, on average, for about 77.5 percent of the time they could have potentially been enrolled in SEED. This means that one would expect them to spend about 2.33 years actually enrolled in SEED. From this, we can infer that $C_1 = C_2 =$ $\$18,752$ and $C_3 = .33 \cdot \$18,752 = \$6,188$.

Calculating the gain in lifetime earnings associated with increased achievement requires a few additional assumptions. Adopting the framework used in Krueger (2003), we let E_t represent an individual's average real earnings each year after entering the labor market at age 18. We let β represent the increase in earnings associated with a one-standard deviation increase in either

[20] Some of the difference can also be attributed to the fact that students studied by Neal and Johnson (1996) are older at the time of their exam, and that British and American labor markets are different.

reading or math achievement scores. As we argue above, the existing literature seems to suggest that that 8.7 percent is a reasonable value for β.

Let δ_R and δ_M be the increase in test scores in reading and math, respectively (in standard deviation units), as a result of attending SEED. Furthermore, assume that each individual works until age 65. The present value of the benefits from these increased earnings would be

$$Present\ Value\ of\ Benefits = \sum_{t=7}^{54} E_t \cdot \frac{\beta(\delta_R + \delta_M)}{(1+r)^t}. \tag{4}$$

Thus, SEED's internal rate of return can be calculated as the discount rate, r^*, that solves:

$$\sum_{t=1}^{3} \frac{C_t}{(1+r)^t} = \sum_{t=7}^{54} E_t \cdot \frac{\beta(\delta_R + \delta_M)}{(1+r)^t}.$$

We use values of average annual earnings by age group from the 2009 Current Population Survey for E_t. However, students who entered SEED in 2007 will enter the labor market in 2013, and real earnings will likely grow substantially between 2009 and 2013. If γ is the rate of productivity growth, then we can account for this growth by multiplying E_t by $(1+\gamma)^{t-4}$ in our above equations. As in Krueger (2003), we note that real earnings and productivity have grown by about 1 to 2 percent per year, so that these are plausible values for γ.

For simplicity, we think of SEED as a middle-school intervention serving grades six through eight and assume that annual productivity growth is 1 percent. This implies a return on investment of roughly 5.3 percent. If we assume instead that SEED is a six-year intervention (from grades seven through twelve), that SEED students therefore would attend for approximately 4.7 years, and that SEED students experience constant gains each year, then the estimated internal rate of return is roughly 5.7 percent. Letting the return to education vary between 7.6 and 10 percent (the lowest and highest values cited above) and annual productivity gains range from 1 to 2 percent, the estimated return fluctuates between 4.6 and 6.4 percent.

Caveats

There are two important caveats to our cost-benefit analysis. First, and most important, we restrict our attention to expected future income that may increase as a result of an increase in test scores and do not consider other important social outcomes. We do this because we have plausible estimates of the effect of increasing achievement on lifetime earnings from a variety of sources. There is evidence from several literatures that suggests higher achievement is correlated with other outcomes such as lower crime rates (Levitt and Lochner, 2001), lower incarceration rates (Neal and Johnson, 1996), better health outcomes and lower mortality (Lleras-Muney, 2005), and so on. We do not attempt to compute monetary benefits accrued from these effects for two reasons: (1) the private value of income is clear, but we did not want to make assumptions about the benefits of improving other social outcomes such as incarceration, teen pregnancy, and so on; and (2) we are more confident that more education causes higher income than we are that more education causes these other social outcomes (though the next section computes back-of-the-envelope estimates of the changes in these outcomes that SEED might induce)

Second, Chetty, Friedman, Hilger, Saez, Schanzenbach, and Yagan (2011) suggest that long-term benefits of a high-quality education may operate through non-test score outcomes not easily observed. In particular, Chetty, Friedman, Hilger, Saez, Schanzenbach, and Yagan (2011) argue that test scores may provide a short-run measure of the quality of an intervention. Therefore, even if test score gains do not persist, it is plausible that if an intervention demonstrates short-term test score gains, then it is likely to improve long-term outcomes. This argument is especially relevant for urban boarding schools, as they likely foster the development of non-cognitive skills that are not captured in our analysis. If urban boarding schools influence these types of skills – discount rates, for instance – then we are likely underestimating the long-term benefits. Given that we have no direct evidence in favor or against this hypothesis, it is an open question. However, considering our findings that SEED has large positive effects on test scores, it is reasonable to conjecture that urban boarding schools may have large positive effects on important long-term outcomes.

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Non-Monetary Benefits

Levitt and Lochner (2001) find that a one-quartile increase in AFQT scores is associated with a 3 to 4 percent decrease in self-reported property and violent crime participation. Assuming normality and using the average effect across both math and reading, this implies a $(0.220/0.67) = 1.0$ to 1.3 percent decrease in criminal participation for each year a student is enrolled. Auld and Sidhu (2005) find that a one standard deviation increase in AFQT scores is associated with a 20 to 30 percent decrease in the probability of reporting a health limitation, implying a $(0.220) * (0.2$ to $0.3) = 4.4$ to 6.6 percent decrease for each year a student is enrolled at SEED. Elias (2005) and Kaestner (2009) report similar findings using self-reported health status.

Table 1: Lottery and Match Summary

A. Lottery Records

	Lottery Cohort 2007	Lottery Cohort 2008
Total number of records	155	106
Excluding siblings	138	94
Excluding late/non-randomized applicants	133	91
Excluding applicants from wrong grade	132	89

B. Match Summary

Lottery Grade	Lottery Year	Grades Observed	Number of Applicants	Overall Match Rate	Number of Winners	Winner Match Rate	Number of Losers	Loser Match Rate
7th	2007	7th - 8th	132	0.95	80	0.95	52	0.94
7th	2008	7th	89	0.93	49	0.96	40	0.90
7th	Pooled	7th - 8th	221	0.94	129	0.95	92	0.92

NOTES: This table summarizes the lottery cohorts and match rates from SEED lottery files to SEED administrative data, District of Columbia Comprehensive Assessment System (DC CAS) data, and Maryland School Assessment (MSA) data. The sample consists of students in the SEED School of Washington, D.C., lotteries in 2007 and 2008. Panel A shows the breakdown of different types of records in the student lottery files. Panel B shows the breakdown of winners and losers in each lottery sample, as well as match rates. The match rate shown is the proportion of students for whom at least one DC CAS or MSA score in either math or reading was matched.

Table 2: Descriptive Statistics And Covariate Balance

	Means				Balance		
	All DCPS Enrollees	SEED Zip Codes	Lottery Winners	Lottery Losers	Applicants v. DCPS	Applicants v. SEED Zips	Winners v. Losers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Black	0.837	0.958	1.000	1.000	0.163	0.042	–
					(0.005)	(0.004)	
Non-black	0.163	0.042	0.000	0.000	-0.163	-0.042	–
					(0.005)	(0.004)	
Male	0.506	0.511	0.574	0.120	-0.123	-0.127	–
					(0.033)	(0.034)	
Female	0.494	0.489	0.426	0.880	0.123	0.127	–
					(0.033)	(0.034)	
Baseline reading score	-0.017	-0.174	0.068	0.003	0.061	0.216	0.136
					(0.055)	(0.057)	(0.122)
Baseline math score	-0.031	-0.207	0.057	-0.000	0.072	0.243	0.184
					(0.058)	(0.060)	(0.128)
Free lunch	0.678	0.729	0.778	0.703	0.070	0.019	0.041
					(0.035)	(0.035)	(0.079)
Special education	0.215	0.237	0.094	0.162	-0.096	-0.115	-0.095
					(0.024)	(0.024)	(0.060)
English Language Learner	0.066	0.019	0.070	0.107	0.020	0.066	-0.097
					(0.020)	(0.020)	(0.045)
Lives with two parents	–	—	0.227	0.217	–	–	0.017
							(0.065)
Lives with mother	–	—	0.664	0.565	–	–	0.120
							(0.076)
Lives with grandparent(s)	–	—	0.070	0.087	–	–	-0.043
							(0.040)
Lives with other legal guardian	–	—	0.039	0.130	–	–	-0.094
							(0.049)
Number of students	5045	2570	129	92	5266	2791	221

NOTES: Column (1) reports means for students who were enrolled in seventh grade in District of Columbia Public Schools (DCPS) in 2007-08 and 2008-09. Column (2) restricts the sample to those students in a zip code in which at least 5.8% (the median value in the DCPS sample) of eligible students enter a SEED lottery. Columns (3) and (4) report means for SEED lottery winners and losers, respectively. Column (5) reports coefficients from regressions of the variable indicated in each row on an indicator variable equal to one if the student was a SEED lottery applicant and zero if the student is from the DCPS seventh grade sample from Column (1). Column (6) reports similar coefficients comparing the SEED lottery sample to the zip-code-restricted sample in Column (2). Column (7) reports coefficients from regressions of the variable indicated in each row on an indicator variable equal to one if the student won the lottery. Because SEED holds separate lotteries for male and female applicants, these regressions include an indicator variable equal to one if the student is male interacted with a cohort indicator, and results for Column (7) are not reported for sex indicator variables. Because every applicant in the lottery sample is black, results for Column (7) are not reported for race. The pooled regression in Column (7) combines the 2007 and 2008 cohorts and includes dummies for applicant year as well as a contemporaneous sibling dummy and the interaction of the contemporaneous sibling dummy with applicant year. Robust standard errors are reported in parentheses.

Table 3: Lottery Results

Outcome	Controls	First Stage (1)	Reduced Form (2)	2SLS (3)
Reading	Basic	0.943	0.264	0.280
		(0.074)	(0.100)	(0.106)
	Baseline Scores	0.931	0.193	0.208
		(0.075)	(0.082)	(0.087)
	Baseline Scores and Demographics	0.953	0.201	0.211
		(0.078)	(0.086)	(0.092)
			303	
Math	Basic	0.942	0.388	0.412
		(0.074)	(0.117)	(0.122)
	Baseline Scores	0.930	0.285	0.307
		(0.075)	(0.085)	(0.091)
	Baseline Scores and Demographics	0.952	0.218	0.229
		(0.078)	(0.082)	(0.085)
			301	

NOTES: This table reports estimates of the effect of attending SEED on achievement. The sample is students who applied to the SEED School of Washington, D.C., in 2007 and 2008. Columns (1)-(3) report the first stage, reduced form, and 2SLS coefficients from instrumenting years in SEED using an indicator for having won the SEED lottery. This indicator is equal to one if the applicant was offered admission either immediately or off the waitlist. Applicants with sibling priority or who applied late and were not included in the original lottery are excluded. All regressions include an indicator variable for sex, interacted with the cohort indicator, since separate lotteries were conducted for males and females. All regressions combine the 2007 and 2008 cohorts and include dummies for grade of test and applicant year as well as a contemporaneous sibling dummy and the interaction of the contemporaneous sibling dummy with applicant year. Estimates are also reported for regressions including controls for baseline test scores in reading and math as well as demographic controls for free lunch eligibility, special education status, and English Language Learner status. Because every applicant in the lottery sample is black, race controls are not included. Robust standard errors (clustered at the student level) are reported in parentheses. Numbers of observations are reported directly below each set of estimates.

Table 4: Lottery Results by Subsample

Outcome	Full Sample (1)	Male (2)	Female (3)	p-value (4)	Free Lunch (5)	Non-Free Lunch (6)	p-value (7)	Special Education (8)	Non-Special Education (9)	p-value (10)
Reading	0.211	-0.138	0.382	0.014	0.267	0.037	0.107	0.120	0.232	0.663
	(0.092)	(0.145)	(0.155)		(0.122)	(0.074)		(0.237)	(0.099)	
	303	94	126		184	58		38	250	
Math	0.229	0.037	0.265	0.280	0.196	0.115	0.594	0.104	0.283	0.574
	(0.085)	(0.156)	(0.142)		(0.106)	(0.111)		(0.304)	(0.090)	
	301	94	125		183	58		38	249	

NOTES: This table reports estimates of the effect of attending SEED on achievement for subsets of the lottery sample. Columns (1)-(3), (5)-(6), and (8)-(9) report 2SLS coefficients from instrumenting years in SEED using an indicator for having won the SEED lottery. This indicator is equal to one if the applicant was offered admission either immediately or off the waitlist. Columns (4), (7), and (10) report p-values for the F-test for the hypothesis that the coefficients in the preceiing two columns are equal. Applicants with sibling priority or who applied late and were not included in the original lottery are excluded. All regressions include an indicator variable for sex interacted with a cohort indicator, since separate lotteries were conducted for males and females. Because every single male student in the 2008 lottery was offered admission to SEED, the regressions for males and females in Columns (2) and (3) only include the 2007 cohort and include grade of test dummies and a contemporaneous sibling dummy. All other regressions combine the 2007 and 2008 cohorts and include dummies for grade of test and applicant year as well as a contemporaneous sibling dummy and the interaction of the contemporaneous sibling dummy with applicant year. All regressions include controls for baseline test scores in reading and math as well as demographic controls for race, free lunch eligibility, special education status, and English Language Learner status. Because every applicant in the lottery sample is black, race controls are not included. Robust standard errors (clustered at the student level) are reported in parentheses. Numbers of observations are reported directly below standard errors.

Table 5: Distribution Effects

		Effects by Baseline Score Quantile			Baseline Score Interaction	
	Non-Missing Baseline Score	Below Median	Above Median	p-value	Main Effect	Interaction Term
Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Reading	0.165	0.347	-0.044	0.015	0.179	-0.126
	(0.087)	(0.137)	(0.082)		(0.093)	(0.131)
	272	139	133		272	272
Mean Score by Quantile		-0.491	0.653			
Math	0.248	0.358	0.162	0.281	0.263	-0.084
	(0.087)	(0.139)	(0.117)		(0.092)	(0.120)
	270	141	129		270	270
Mean Score by Quantile		-0.532	0.663			

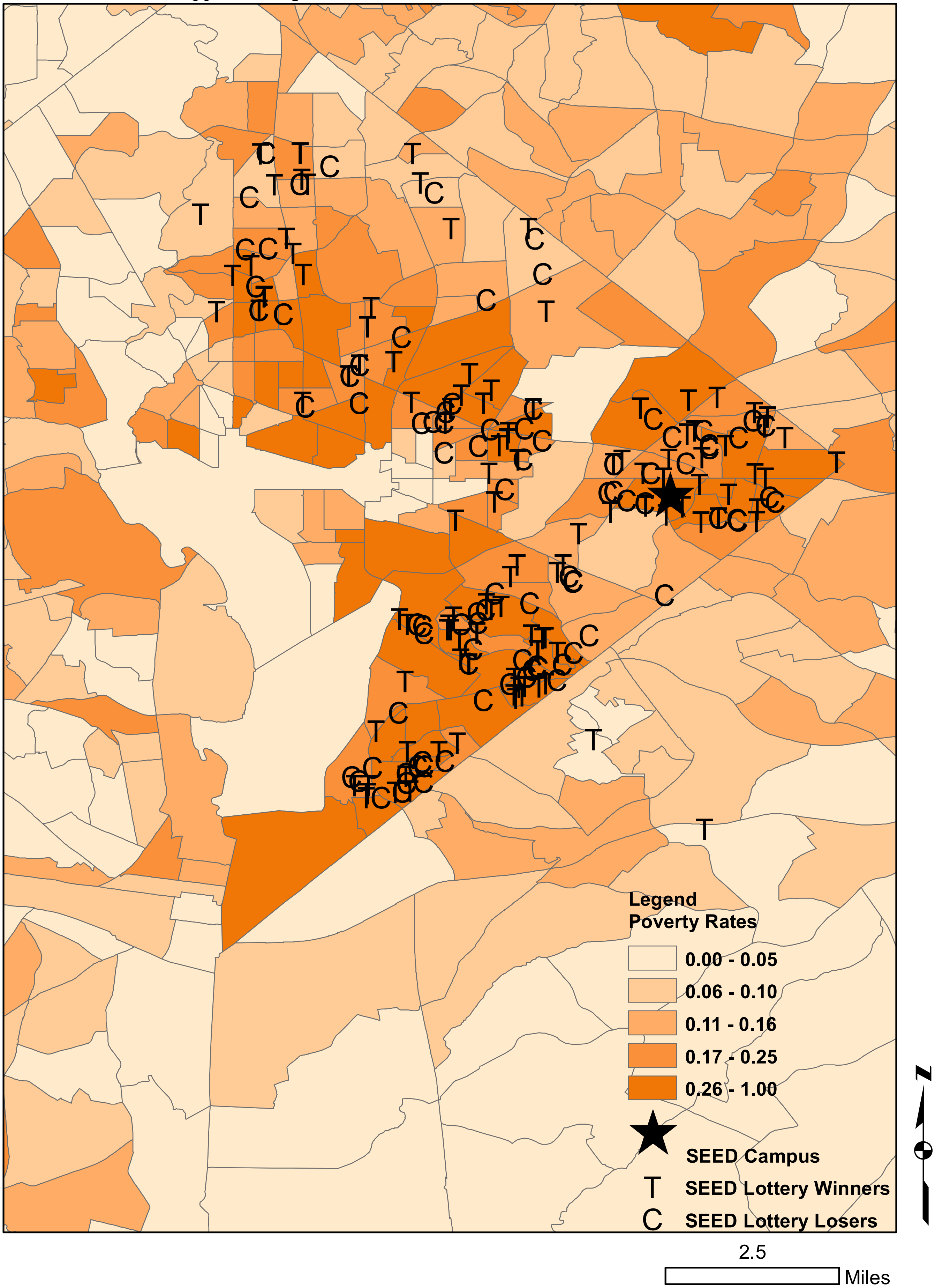
NOTES: This table reports estimates of the effect of attending SEED on achievement for students from different parts of the baseline test score distribution. Columns (1)-(3) report 2SLS coefficients from instrumenting years in SEED using an indicator for having won the SEED lottery. This indicator is equal to one if the applicant was offered admission either immediately or off the waitlist. Column (1) reports estimates for the sample of students with non-missing baseline scores in the same subject as the outcome. Columns (2)-(3) report estimates for the groups that are below the median and above the median in terms of baseline score in the same subject as the outcome. Column (4) reports the p-value for the F-test for the hypothesis that the coefficients for the Below Median and Above Median groups are the same. Columns (5) and (6) report results from models interacting baseline test score with years in SEED. Main effects are at the mean. The interaction models are estimated by including an indicator for having won the SEED lottery interacted with baseline score as a second instrument. Applicants with sibling priority or who applied late and were not included in the original lottery are excluded. All regressions include an indicator variable for sex interacted with a cohort indicator, since separate lotteries were conducted for males and females. All regressions combine the 2007 and 2008 cohorts and include dummies for grade of test and applicant year as well as a contemporaneous sibling dummy and the interaction of the contemporaneous sibling dummy with applicant year. All regressions include controls for baseline test scores in reading and math as well as demographic controls for free lunch eligibility, special education status, and English Language Learner status. Because every applicant in the lottery sample is black, race controls are not included. Robust standard errors (clustered at the student level) are reported in parentheses. Numbers of observations are reported directly below standard errors. Mean baseline score by quantile is also reported below each set of estimates.

Table 6: Attrition

Outcome	Share of Lottery Winners with Scores	Share of Lottery Losers with Scores	Differential Follow-up (Winner - Loser)		
			Basic	Baseline Scores	Baseline Scores and Demographics
	(1)	(2)	(3)	(4)	(5)
Reading	0.861	0.879	0.008	0.006	-0.002
			(0.024)	(0.024)	(0.024)
Math	0.861	0.864	0.020	0.017	0.006
			(0.026)	(0.026)	(0.026)
Number of observations	209	140	332	332	332

NOTES: This table reports differential rates of attrition for SEED lottery winners and losers. Columns (1) and (2) report shares of lottery winners and losers with non-missing values for the outcomes indicated in each row. Columns (3)-(5) report coefficients from regressions of an indicator variable equal to one if the outcome indicated in the same row is non-missing on an indicator for having won the SEED lottery. Samples and specifications are otherwise identical to those reported in Table 3. Robust standard errors (clustered at the student level) are reported in parentheses. Numbers of observations are reported directly below estimates.

Appendix Figure 1. SEED Treatment and Control Households



Appendix Table 1: Lottery Results By Home Environment Subsamples

	High Crime	Low Crime	p-value	Single Mother	Other Caregiver(s)	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Reading	0.176	0.230		0.184	0.188	
	(0.148)	(0.110)	0.769	(0.121)	(0.134)	0.983
	151	150		192	85	
Math	0.218	0.186		0.210	0.287	
	(0.120)	(0.128)	0.856	(0.107)	(0.123)	0.640
	150	149		192	84	

NOTES: This table reports estimates of the effect of attending SEED on achievement for students from different home environments. Columns (1)-(3) report 2SLS coefficients from instrumenting years in SEED using an indicator for having won the SEED lottery. This indicator is equal to one if the applicant was offered admission either immediately or off the waitlist. Columns (2)-(3) report estimates for students who live in census tracts with crime rates that are above the median and belowthe median rate in the lottery sample. Column (4) reports estimates for students living with a single mother at the time of the lottery; column (5) restricts the sample to students living with both parents or their grandparents. Columns (3) and (6) report p-values of the F-test for the hypothesis that the SEED coefficients for within the crime and caregiver subroups are identical. Applicants with sibling priority or who applied late and were not included in the original lottery are excluded. All regressions include an indicator variable for sex interacted with the cohort indicator, since separate lotteries were conducted for males and females. All regressions combine the 2007 and 2008 cohorts and include dummies for grade of test and applicant year as well as a contemporaneous sibling dummy and the interaction of the contemporaneous sibling dummy with applicant year. All regressions include controls for baseline test scores in reading and math as well as demographic controls for free lunch eligibility, special education status, and English Language Learner status. Because every applicant in the lottery sample is black, race controls are not included. Robust standard errors (clustered at the student level) are reported in parentheses. Numbers of observations are reported directly below standard errors. Mean baseline score by quantile is also reported below each set of estimates.

Appendix Table 2: Lower Bound of Attrition-Adjusted Results

Outcome	Controls	First Stage (1)	Reduced Form (2)	2SLS (3)
Reading	Basic	0.946	0.209	0.220
		(0.073)	(0.093)	(0.096)
	Baseline Scores	0.935	0.127	0.136
		(0.073)	(0.077)	(0.081)
	Baseline Scores and Demographics	0.958	0.136	0.142
		(0.075)	(0.081)	(0.084)
			332	
Math	Basic	0.946	0.321	0.339
		(0.073)	(0.110)	(0.114)
	Baseline Scores	0.935	0.211	0.226
		(0.073)	(0.082)	(0.086)
	Baseline Scores and Demographics	0.958	0.152	0.159
		(0.075)	(0.078)	(0.080)
			332	

NOTES: This table reports estimates of the effect of attending SEED on achievement, after imputing scores to students who attrite from our sample. All specifications are identical to those described in the notes of Table 3. We impute scores for attriters by the following procedure: first, we sort students into bins based on their demographic covariates and their baseline test decile. Where demographic data are missing, we use as much information as is available when constructing bins. Then, we impute the 75th percentile of the score distribution within each bin for lottery losers who attrite and the 25th percentile for lottery winners.