The Potential of Urban Boarding Schools for the Poor: Evidence from SEED
Author(s): Vilsa E. Curto and Roland G. Fryer Jr.
Source: *Journal of Labor Economics*, Vol. 32, No. 1 (January 2014), pp. 65-93
Published by: The University of Chicago Press on behalf of the Society of Labor Economists and the NORC at the University of Chicago
Stable URL: http://www.jstor.org/stable/10.1086/671798
Accessed: 09/12/2014 13:36

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

Vilsa E. Curto, *Stanford University*

Roland G. Fryer Jr., *Harvard University and the National Bureau of Economic Research*

The SEED schools, which combine a "No Excuses" charter model with a 5-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 a student's environment 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 deviation in reading and 0.229 standard deviation in math per year. However, subgroup analyses show that the effects may be driven by female students.

I. 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 Educa-

We are grateful to Eric Adler, Anjali Bhatt, Pyper Davis, and Rajiv Vinnakota for their cooperation in collecting data necessary for this project. Matt Davis, Will Dobbie, and Meghan Howard provided exceptional research assistance. Support from the Education Innovation Laboratory at Harvard University (EdLabs) is gratefully acknowledged. The usual caveat applies. Contact either author, Vilsa E. Curto at vcurto@stanford.edu or Roland G. Fryer Jr. at rfryer@fas.harvard.edu. Data are available as supplementary material online.

[*Journal of Labor Economics,* 2014, vol. 32, no. 1]

0734-306X/2014/3201-0003$10.00

tional Progress (NAEP), which measures students' levels of proficiency in reading and math, reported that 42% of white and 16% of black fourth-grade students are proficient in reading.[1] In math, 52% of white and 17% of black fourth-grade students are proficient. There is not one school district in NAEP in which more than 21% 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 et al. 2009; Gleason et al. 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 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, DC: 24.4% of blacks live in poverty, 23% of black children are reared in a two-parent household, 7.4% of black women will give birth while they are still teenagers, and nearly 50% of black men between the

[1] NAEP is a nationally representative set of assessments administered every 2 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 et al. (2011) for smaller classes; Bloom et al. (2010) for smaller schools; Jacob and Lefgren (2004) on summer school; Podgursky and Springer (2007) and Fryer (2013) on merit pay; Fryer (2011) on incentives; Jacob and Lefgren (2009) on grade retention; Decker et al. (2004) on Teach for America; and Kane et al. (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; Angrist et al. 2010; Gleason et al. 2010; Abdulkadiroglu et al. 2011; Dobbie and Fryer 2011). Raymond (2009) estimates that 17% of charter schools outperform traditional public schools.

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, 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 his or her home environment and the differences between them are correlated with academic achievement, then boarding schools will yield positive gains

[4] Figures on poverty and family structure were obtained from the US Census Bureau's 2006–8 American Community Survey 3-Year Estimates. In Washington, DC, 80% 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, DC, in 2008. Based on data from the US Census Bureau's 2006–8 American Community Survey 3-Year Estimates, there are about 12,332 black women aged 15–19 in Washington, DC. The corresponding birth rate among black women in Washington, DC, aged 15–19 is about 7.4%. 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% increase in annual earnings, reduced dependence on welfare and public assistance by about 2 weeks per year, and a fivefold 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, nonresidential setting—has had statistically insignificant results. Cave et al. (1993) found that effects on earnings 4 years after the program were not statistically significant, that 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 nonresidential programs.

in 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, DC, and Baltimore, MD, are America's only urban public boarding schools for the poor. These schools combine a "No Excuses" charter school model with a 5-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 6–12. Like other "No Excuses" charter schools (e.g., the Knowledge Is Power Program or the 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 to 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 composed 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 0.211σ (σ = standard deviation) in reading and 0.229σ in math. Taken at face value, these effects are enough to close the black-white achievement gap in both subjects

[6] Our analysis focuses on the results from the SEED School of Washington, DC, 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 et al. 2009).

in 4 years. Estimated treatment effects are substantially larger for girls than for 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 the SEED program 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 Rickford (1999) and Charity, Scarborough, and Griffin (2004) that shows that 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 nonstandard 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 1 year is associated with a 3.8% increase in earnings (Chetty, Friedman, and Rockoff 2012), a 1.0%–1.3% decrease in the probability of committing a property or violent crime (Levitt and Lochner 2001), and a 4.4%–6.6% 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 a graduate are more than $250,000.[7] Unfortunately, however, calculating the non-test-score benefits of attending a SEED school is difficult and, at this stage in the life cycle of their oldest cohorts, premature. Whether or not the total benefits of attending a SEED school outweigh the costs can be known with the passage of time.

The next section of this article presents some theoretical explanations for why urban boarding schools may (or may not) increase achievement among the poor. Section III discusses our data and research design. Section IV presents the results of our analysis, and Section V concludes. In addition to an appendix (app. A) containing supplementary figure and table information, there are three online appendices: appendix B details programs and services provided by SEED schools in both their academic and residential boarding programs; appendix C is a data appendix that details our

[7] See online app. D for details of these calculations.

sample and variable construction; and appendix D provides further details and assumptions behind our cost-benefit calculations.

II. 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]

A. 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]

[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 (Levine 1980; Cookson and Persell 1985; Kahane 1988; 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, and 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 (Kleinfeld and Bloom 1977; Koss et al. 2003), this is generally focused on Native American boarding schools, which

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% 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 that these stress levels can be exacerbated by sending a youth to boarding school, particularly if the student lacks 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).

B. 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 female–headed household—69% of black family households with children under the age of 18 in Washington, DC, are single-mother households; for whites, this figure

students often attended unwillingly; its applicability to urban public boarding schools is doubtful.

is 14%.[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 et al. 1996; Ainsworth 2002; Rothstein 2004; Brooks-Gunn and Markman 2005; Duncan and Magnuson 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 noncognitive 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 "micromanage" 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.

C. 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 a problem than at traditional boarding schools, since students return to their homes and neighborhoods on the weekends. Moreover, 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)

[10] Data are obtained from the US Census Bureau's 2006–8 American Community Survey 3-Year Estimates.

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 that 22% of lottery applicants live in a dual-parent household, 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 noncognitive 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 a nonresidential school. If these noncognitive traits translate into education production, for example, by increasing study skills or grit or by highlighting the importance of education and college, then they might result in academic gains above and beyond those of a similar nonresidential 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 Washington, DC, 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 2002–3 school year (Whitman 2008). If a safer environment reduces stress or facilitates schoolwork in other ways, this could produce further achievement gains.

This article'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.

III. Data and Research Design

A. Data

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

[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 17 students are available from the authors upon request.

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 include the applicant's cohort, first and last names, and date of birth; whether and how the applicant was offered admission (immediately, off the waiting list, 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 waiting list and not included in the lottery); and, if applicable, the 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 2005–6 through 2008–9 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 vs. nonhyphenated);
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.

In our final sample, the proportion of students for whom at least one achievement test score was matched is 95% for SEED lottery winners (N = 129) and 92% 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 (Hoxby and Murarka 2009; Angrist et al. 2010; Abdulkadiroglu et al. 2011; Dobbie and Fryer 2011).

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,

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

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/nonrandomized 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 and 8th	132	.95	80	.95	52	.94
7th	2008	7th	89	.93	49	.96	40	.90
7th	Pooled	7th and 8th	221	.94	129	.95	92	.92

NOTE.—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, DC, 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.

and math and reading achievement scores in grades 3–8 and 10. The math and reading tests, extracted from the District of Columbia Comprehensive Assessment System (DC CAS), are administered each April to students in grades 3–8 and 10. The DC CAS exams measure knowledge and skills in reading and math.

In Washington, DC, 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 those who are English language learners must take both tests, but they may be granted special accommodations (additional time, translation services, etc.) at the discretion of school or state administrators.

Summary statistics for the variables that we use in our core specifications, as well as student's living situation, are displayed in table 2. Column 1 includes all students who attended seventh grade in DCPS in 2007–8 and 2008–9, 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% (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 and 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, respectively. 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% (SE = 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

Table 2
Descriptive Statistics and Covariate Balance

	Means					Balance	
	DCPS Enrollees (1)	Lottery Applicant (2)	Lottery Winners (3)	Lottery Losers (4)	SEED Enrollees (5)	Applicants versus DCPS (6)	Winners versus DCPS (7)
Black	.837	1.000	1.000	1.000	1.000	.163	
						(.005)	
Nonblack	.163	.000	.000	.000	.000	−.163	
						(.005)	
Male	.506	.385	.574	.120	.596	−.123	
						(.033)	
Female	.494	.615	.426	.880	.404	.123	
						(.033)	
Baseline reading score	−.017	.041	.068	.003	.075	.061	.136
						(.055)	(.122)
Baseline math score	−.031	.033	.057	−.000	.064	.072	.184
						(.058)	(.128)
Free lunch	.678	.748	.778	.703	.753	.070	.041
						(.035)	(.079)
Special education	.215	.119	.094	.162	.098	−.096	−.095
						(.024)	(.060)
English language learner	.066	.083	.070	.107	.058	.020	−.097
						(.020)	(.045)
Lives with two parents		.223	.227	.217	.221		.017
							(.065)
Lives with mother		.623	.664	.565	.644		.120
							(.076)
Lives with grandparent(s)		.077	.070	.087	.087		−.043
							(.040)
Lives with other legal guardian		.077	.039	.130	.048		−.094
							(.049)
No. of students	5,045	221	129	92	104	5,266	221

NOTE.—Columns 1–5 report means of the variable indicated in each row. Column 1 reports means for students who were enrolled in seventh grade in District of Columbia Public Schools (DCPS) in 2007–8 and 2008–9. Column 2 reports means for all SEED lottery applicants. Column 3 reports means for SEED lottery winners, and col. 4 reports means for SEED lottery losers. Column 5 reports means for lottery applicants who enrolled in SEED for at least 1 day. Column 6 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 col. 1. 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, and results for col. 7 are not reported for sex indicator variables. Because every applicant in the lottery sample is black, results for col. 7 are not reported for race indicator variables. The pooled regression in col. 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.

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 A1 shows the geographic distribution of treatment and control students across Washington, DC, 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.

B. Research Design

Our research design exploits the fact that oversubscribed charter schools in Washington, DC, 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:

$$\text{achievement}_{igt} = \alpha_t + \beta_g + \delta' X_i + \rho \text{SEED}_{igt} + \varepsilon_{igt}, \tag{1}$$

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 (OLS) 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 waiting list. Given the size of the esti-

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

mated treatment effect, our results are robust to other definitions of "lottery winner."

Under two 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:

$$\begin{aligned}\text{SEED}_{igt} = \zeta_t + \eta_g + \sum_j \mu_j \,\text{lottery}_{ij} + \sum_j \left[\gamma_j \,\text{lottery}_{ij} \times 1(\text{female}_i)\right] \\ + \sum_j \left[\varphi_j \,\text{lottery}_{ij} \times 1(\text{sibling}_i)\right] + \iota X_i + \pi Z_i + \kappa_{igt},\end{aligned} \tag{2}$$

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

IV. 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 of Washington, DC. We report first-stage (col. 1), reduced-form (col. 22), and 2SLS estimates (col. 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 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 (SE = 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 et al. 2011). The 2SLS estimate, which captures the causal effect of attending the SEED school for 1 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.

Table 3
Lottery Results

Outcome/Controls	First Stage (1)	Reduced Form (2)	2SLS (3)
Reading (N = 30):			
Basic	.943	.264	.280
	(.074)	(.100)	(.106)
Baseline scores	.931	.193	.208
	(.075)	(.082)	(.087)
Baseline scores and demographics	.953	.201	.211
	(.078)	(.086)	(.092)
Math (N = 301):			
Basic	.942	.388	.412
	(.074)	(.117)	(.122)
Baseline scores	.930	.285	.307
	(.075)	(.085)	(.091)
Baseline scores and demographics	.952	.218	.229
	(.078)	(.082)	(.085)

NOTE.—This table reports estimates of the effect of attending SEED on achievement. The sample is students who applied to the SEED School of Washington, DC, 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 waiting list. 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.

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 (Angrist et al. 2010; Abdulkadiroglu et al. 2011; 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; Hoxby and Murarka 2009; Angrist et al. 2010; Abdulkadiroglu et al. 2011; Dobbie and Fryer 2011; 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 (Rickford 1999; Charity et al. 2004). Charity et al. (2004) argue that if students speak nonstandard 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

Table 4
Lottery Results by Subsample

		Gender			Lunch Status			Education Status		
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	.211	−.138	.382	.014	.267	.037	.107	.120	.232	.663
	(.092)	(.145)	(.155)		(.122)	(.074)		(.237)	(.099)	
	[303]	[94]	[126]		[184]	[58]		[38]	[250]	
Math	.229	.037	.265	.280	.196	.115	.594	.104	.283	.574
	(.085)	(.156)	(.142)		(.106)	(.111)		(.304)	(.090)	
	[301]	[94]	[125]		[183]	[58]		[38]	[249]	

NOTE.—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 waiting list. Columns 4, 7, and 10 report p-values for the F-test for the hypothesis that the coefficients in the preceding 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 cols. 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 in brackets.

Table 5
Distribution Effects

		Effects by Baseline Score Quantile			Baseline Score Interaction	
Outcome	Nonmissing Baseline Score (1)	Below Median (2)	Above Median (3)	p-Value (4)	Main Effect (5)	Interaction Term (6)
Reading	.165	.347	−.044	.015	.179	−.126
	(.087)	(.137)	(.082)		(.093)	(.131)
	[272]	[139]	[133]		[272]	[272]
Mean score by quantile		−.491	.653			
Math	.248	.358	.162	.281	.263	−.084
	(.087)	(.139)	(.117)		(.092)	(.120)
	[270]	[141]	[129]		[270]	[270]
Mean score by quantile		−.532	.663			

Note.—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 waiting list. Column 1 reports estimates for the sample of students with nonmissing 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 in brackets.

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 underpowered 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 has a particularly bad effect for boys. We urge caution in overinterpreting a single subgroup finding, however, as our preferred interpretation is that this result is suggestive at best.[14]

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 pretreatment 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 et al. (2010) for the effects of attending a "No Excuses" charter school in Lynn, MA, 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 postlottery scores. If lottery winners and losers have different rates of selection into this sample, our results may be biased.

[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 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 or her primary caregiver. The results (shown in app. table A1) show no evidence of significant heterogeneity along these dimensions.

Table 6 compares the rates of attrition of lottery winners and lottery losers. In the pooled sample, 86.1% of winners and 87.9% of losers have reading scores. 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 3–5 of table 6—the difference is statistically zero. Similarly, 86.1% of winners and 86.4% 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]

V. 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 4 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 1σ 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 et al. 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 A2 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% more likely to attrite; dropping the lowest-performing 2% of the control sample also does not affect our results.

Table 6
Attrition

Outcome	Share of Lottery Winners with Scores (1)	Share of Lottery Losers with Scores (2)	Differential Follow-up (Winner − Loser): Basic (3)	Differential Follow-up (Winner − Loser): Baseline Scores (4)	Differential Follow-up (Winner − Loser): Baseline Scores and Demographics (5)
Reading	.861	.879	.008	.006	−.002
			(.024)	(.024)	(.024)
Math	.861	.864	.020	.017	.006
			(.026)	(.026)	(.026)
No. of observations	209	140	332	332	332

NOTE.—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 nonmissing 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 nonmissing 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.

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] Angrist et al. (2010) and Abdulkadiroglu et al. (2011) 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 United States 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 of Washington, DC, 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 policy makers: 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 1 year is associated with a 3.8% increase in earnings (Chetty et al. 2012), a 1.0%–1.3% decrease in the probability of committing a property or violent crime (Levitt and Lochner 2001), and a

[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 et al. (2007).

[18] See online app. D for details of per pupil expenditure figures.

4.4%–6.6% 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; Oreopoulos 2007). The public benefits alone from converting a high school dropout to a graduate are more than $250,000.[19] Moreover, results from Chetty et al. (2011) suggest that long-term benefits of a high-quality education may operate through non–test score channels we do not observe in this article.

We hope that our analysis provides a sense of optimism for work on the achievement gap. Evidence from SEED, along with recent results in Angrist et al. (2010), Abdulkadiroglu et al. (2011), 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.

[19] See online app. D for details of these calculations.

Appendix A

Table A1
Lottery Results by Home Environment Subsamples

	High Crime (1)	Low Crime (2)	p-Value (3)	Single Mother (4)	Other Caregiver(s) (5)	p-Value (6)
Reading	.176	.230	.769	.184	.188	.983
	(.148)	(.110)		(.121)	(.134)	
	[151]	[150]		[192]	[85]	
Math	.218	.186	.856	.210	.287	.640
	(.120)	(.128)		(.107)	(.123)	
	[150]	[149]		[192]	[84]	

NOTE.—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 waiting list. Columns 2 and 3 report estimates for students who live in Census tracts with crime rates that are above the median and below the median rate in the lottery sample. Column 4 reports estimates for students living with a single mother at the time of the lottery; col. 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 subgroups 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 in brackets.

Table A2
Lower Bound of Attrition-Adjusted Results

Outcome/Controls	First Stage (1)	Reduced Form (2)	2SLS (3)
Reading (N = 332):			
Basic	.946	.209	.220
	(.073)	(.093)	(.096)
Baseline scores	.935	.127	.136
	(.073)	(.077)	(.081)
Baseline scores and demographics	.958	.136	.142
	(.075)	(.081)	(.084)
Math (N = 332)			
Basic	.946	.321	.339
	(.073)	(.110)	(.114)
Baseline scores	.935	.211	.226
	(.073)	(.082)	(.086)
Baseline scores and demographics	.958	.152	.159
	(.075)	(.078)	(.080)

NOTE.—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 note 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.

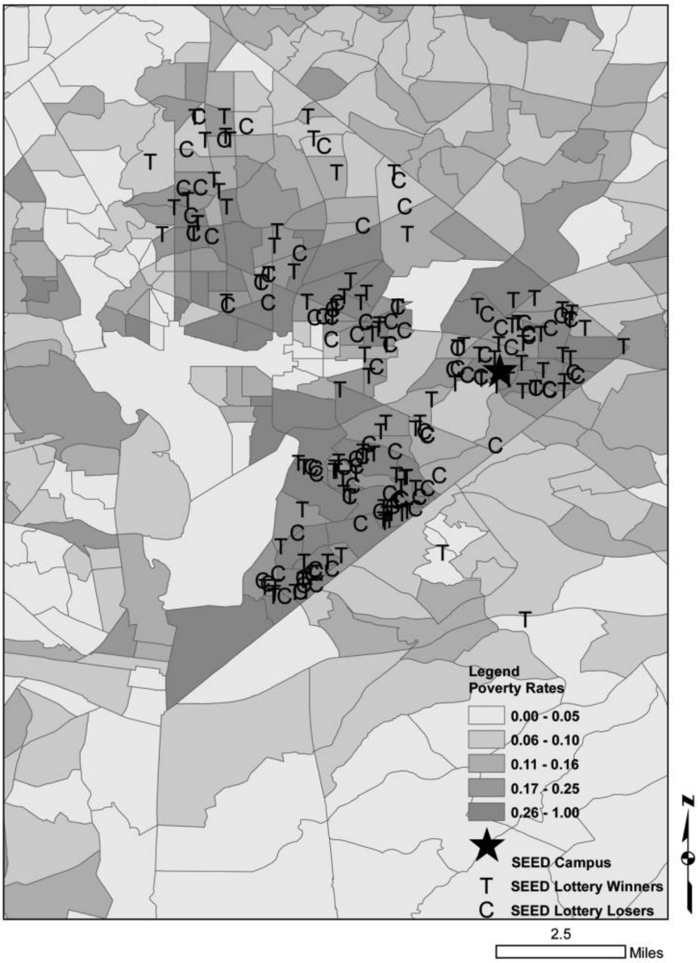

FIG. A1.—SEED treatment and control households. A color version of the map appears in the online version of this article.

References

Aaronson, Daniel, Lisa Barrow, and William Sander. 2007. Teachers and student achievement in the Chicago public high schools. *Journal of Labor Economics* 25, no. 1:95–135.

Abdulkadiroglu, Atilla, Joshua D. Angrist, Susan M. Dynarski, Thomas J. Kane, and Parag A. Pathak. 2011. Accountability and flexibility in public schools: Evidence from Boston's charters and pilots. *Quarterly Journal of Economics* 126, no. 2:699–748.

Adams, David Wallace. 1988. Fundamental considerations: The deep meaning of Native American schooling, 1880–1900. *Harvard Educational Review* 58, no. 1:1–28.

———. 1995. *Education for extinction: American Indians and the boarding school experience*. Lawrence: University Press of Kansas.

Ainsworth, James W. 2002. Why does it take a village? The mediation of neighborhood effects on educational achievement. *Social Forces* 81, no. 1:117–52.

Almond, Douglas, and Janet Currie. 2011. Human capital development before age five. In *Handbook of labor economics*, vol. 4B, ed. Orley Ashenfelter and David Card. Amsterdam: North-Holland/Elsevier.

Angrist, Joshua D., Susan M. Dynarski, Thomas J. Kane, Parag A. Pathak, and Christopher R. Walters. 2010. Who benefits from KIPP? NBER Working Paper no. 15740, National Bureau of Economic Research, Cambridge, MA.

Arieli, Mordecai, Jerome Beker, and Yitzhak Kashti. 2001. Residential group care as a socializing environment: Toward a broader perspective. *Child and Youth Care Forum* 30:403–14.

Auld, Christopher, and Nirmal Sidhu. 2005. Schooling and cognitive ability. *Health Economics* 14, no. 10:1013–34.

Barnes, Grace M., and Michael P. Farrell. 1992. Parental support and control as predictors of adolescent drinking, delinquency, and related problem behaviors. *Journal of Marriage and the Family* 54, no. 4: 763–76.

Bloom, Howard S., Saskia Levy Thompson, Rebecca Unterman, Corinne Herlihy, and Collin F. Payne. 2010. Transforming the high school experience: How New York City's new small schools are boosting student achievement and graduation rates. Report, Manpower Demonstration Research Corporation, New York.

Brooks-Gunn, Jeanne, Greg J. Duncan, and Nancy Maritato. 1999. Poor families, poor outcomes: The well-being of children and youth. In *Consequences of growing up poor*, ed. Greg J. Duncan and Jeanne Brooks-Gunn, 1–17. New York: Russell Sage Foundation.

Brooks-Gunn, Jeanne, and Lisa B. Markman. 2005. The contribution of parenting to ethnic and racial gaps in school readiness. *The future of children* 15, no. 1:139–68.

Card, David. 1999. The causal effect of education on earnings. In *Handbook of Labor Economics*, vol. 3, ed., 1801–63. Amsterdam: North-Holland /Elsevier.

Cave, George, Hans Bos, Fred Doolittle, and Cyril Toussaint. 1993. JOBSTART: Final report on a program for school dropouts. Manpower Demonstration Research Corporation, New York.

Charity, Anne H., Hollis S. Scarborough, and Darion M. Griffin. 2004. Familiarity with school English in African American children and its relation to early reading achievement. *Child Development* 75, no. 5: 1340–56.

Chetty, Raj, John N. Friedman, Nathaniel Hilger, Emmanuel Saez, Diane Whitmore Schanzenbach, and Danny Yagan. 2011. How does your kindergarten classroom affect your earnings? Evidence from Project STAR. *Quarterly Journal of Economics* 126, no. 4:1593–1660.

Chetty, Raj, John N. Friedman, and Jonah E. Rockoff. 2012. The long-term impacts of teachers: Teacher value-added and student outcomes in adulthood. NBER Working Paper no. 17699, National Bureau of Economic Research, Cambridge, MA.

Chilcoat, Howard D., and James C. Anthony. 1996. Impact of parent monitoring on initiation of drug use through late childhood. *Journal of the American Academy of Child and Adolescent Psychiatry* 35, no. 1:91–100.

Cookson, Peter W., and Caroline H. Persell. 1985. English and American residential secondary schools: A comparative study of the reproduction of social elites. *Comparative Education Review* 29, no. 3:283–98.

Currie, Janet, and Duncan Thomas. 1995. Does Head Start make a difference? *American Economic Review* 85, no. 3:341–64.

Decker, Paul, Daniel Mayer, and Steven Glaserman. 2004. The effects of Teach for America on students: Findings from a national evaluation. Report no. 8792-750, Mathematica Policy Research, Princeton, NJ.

Dick, Rhonda W., Spero M. Manson, and Janette Beals. 1993. Alcohol use among male and female Native American adolescents: Patterns and correlates of student drinking in a boarding school. *Journal of Studies on Alcohol* 54, no. 2:172–77.

Dishion, Thomas J., and Robert J. McMahon. 1998. Parental monitoring and the prevention of child and adolescent problem behavior: A conceptual and empirical formulation. *Clinical Child and Family Psychology Review* 1, no. 1:61–75.

Dobbie, Will, and Roland Fryer. 2011. Are high-quality schools enough to increase achievement among the poor? Evidence from the Harlem Children's Zone. *American Economic Journal: Applied Economics* 3, no. 3:158–87.

Duncan, Greg J., and Katherine A. Magnuson. 2005. Can family socioeconomic resources account for racial and ethnic test score gaps? *Future of Children* 15, no. 1:35–54.

Elias, Julio. 2005. The effects of ability and family background on non-monetary returns to education. PhD diss., Department of Economics, University of Chicago.

Ellis, Clyde. 1996. *To change them forever: Indian education at the Rainy Mountain Boarding School, 1893–1920*. Norman: University of Oklahoma Press.

Fisher, Shirley, Norman A. Frazer, and Keith Murray. 1986. Homesickness and health in boarding school children. *Journal of Environmental Psychology* 6, no. 1:35–47.

Fisher, Shirley, Keith Murray, and Norman A. Frazer. 1985. Homesickness, health and efficiency in first year students. *Journal of Environmental Psychology* 5, no. 2:181–95.

Fordham, Signithia, and John U. Ogbu. 1986. Black students' school success: Coping with the burden of "acting white." *Urban Review* 18, no. 3:176–206.

Fryer, Roland G. 2011. Financial incentives and student achievement: Evidence from randomized trials. *Quarterly Journal of Economics* 126, no. 4:1755–98.

———. 2012. Injecting successful charter school strategies into traditional public schools: Early results from an experiment in Houston. NBER Working Paper no. 17494, National Bureau of Economic Research, Cambridge, MA.

———. 2013. Teacher incentives and student achievement: Evidence from New York City public schools. *Journal of Labor Economics* 31, no. 2: 373–427.

Fryer, Roland G., and Paul Torelli. 2010. An empirical analysis of "acting white." *Journal of Public Economics* 94, nos. 5–6:380–96.

Gleason, Philip, Melissa Clark, Christina Clark Tuttle, Emily Dwoyer, and Marsha Silverberg. 2010. *The evaluation of charter school impacts.* Washington, DC: National Center for Education Evaluation and Regional Assistance, Institute of Education Sciences, US Department of Education.

Gonzales, Nancy A., Ana Mari Cauce, Ruth J. Friedman, and Craig A. Mason. 1996. Family, peer, and neighborhood influences on academic achievement among African-American adolescents: One-year prospective effects. *American Journal of Community Psychology* 24, no. 3: 365–87.

Hanushek, Eric, and Steven Rivkin. 2005. Teachers, schools and academic achievement. *Econometrica* 73, no. 2:417–458.

Hoxby, Caroline M., and Sonali Murarka. 2009. Charter schools in New York City: Who enrolls and how they affect their students' achievement. NBER Working Paper no. 14852, National Bureau of Economic Research, Cambridge, MA.

Jacob, Brian, and Lars Lefgren. 2004. Remedial education and student achievement: A regression-discontinuity analysis. *Review of Economics and Statistics* 86, no. 1:226–44.

———. 2009. The effect of grade retention on high school completion. Working Paper no. 12, Center for Local, State, and Urban Policy, Ford School, University of Michigan, Ann Arbor.

Jones, Maggie. 2009. The inner-city prep school experience. *New York Times*, September 25.

Juhn, Chinhui. 2003. Labor market dropouts and trends in the wages of black and white men. *Industrial and Labor Relations Review* 56, no. 4: 643–62.

Kaestner, Robert. 2009. Adolescent cognitive and non-cognitive correlates of adult health. NBER Working Paper no. 14924, National Bureau of Economic Research, Cambridge, MA.

Kahane, Reuven. 1988. Multicode organizations: A conceptual framework for the analysis of boarding schools. *Sociology of Education* 61, no. 4:211–26.

Kane, Thomas J., Jonah Rockoff, and Douglas Staiger. 2008. What does certification tell us about teacher effectiveness? Evidence from New York City. *Economics of Education Review* 27, no. 6:615–31.

Kleinfeld, Judith, and Joseph Bloom. 1977. Boarding schools: Effects on the mental health of Eskimo adolescents. *American Journal of Psychiatry* 134, no. 4:411–17.

Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz. 2007. Experimental analysis of neighborhood effects. *Econometrica* 75, no. 1:83–119.

Koss, Mary P., Nicole P. Yuan, Douglas Dightman, Ronald J. Prince, Mona Polacca, Byron Sanderson, and David Goldman. 2003. Adverse childhood exposures and alcohol dependence among seven Native American tribes. *American Journal of Preventive Medicine* 25, no. 3: 238–44.

Krueger, Alan B. 1999. Experimental estimates of education production functions. *Quarterly Journal of Economics* 114, no. 2:497–532.

Krueger, Alan B., and Diane M. Whitmore. 2001. The effect of attending a small class in the early grades on college–test taking and middle school test results: Evidence from Project STAR. *Economic Journal* 111, no. 468: 1–28.

Lee, David. 2009. Training, wages, and sample selection: Estimating sharp bounds on treatment effects. *Review of Economic Studies* 76, no. 3: 1071–1102.

Levine, Steven B. 1980. The rise of American boarding schools and the development of a national upper class. *Social Problems* 28, no. 1:63–94.

Levitt, Steve, and Lance Lochner. 2001. The determinants of juvenile crime. In *Risky behavior among youths: An economic analysis*, ed. Jonathan Gruber, 327–73. Chicago: University of Chicago Press.

Lotke, Eric. 1998. Hobbling a generation: Young African American men in Washington, DC's criminal justice system—five years later. *Crime and Delinquency* 44, no. 3:355–66.

Ludwig, Jens, and Deborah Phillips. 2007. The benefits and costs of Head Start. NBER Working Paper no. 12973, National Bureau of Economic Research, Cambridge, MA.

Mallar, Charles. 1982. *Evaluation of the economic impact of the Job Corps Program: Third Follow-up report.* Washington, DC: Employment and Training Administration, Office of Policy, Evaluation, and Research.

Manski, Charles. 1995. *Identification problems in the social sciences*, Cambridge, MA: Harvard University Press.

Mickelson, Roslyn A. 1990. The attitude-achievement paradox among black adolescents. *Sociology of Education* 63, no. 1:44–61.

National Center for Education Statistics. 2011. *The Nation's Report Card: Trial Urban District Assessment, 2011*. Washington, DC: National Center for Education Statistics.

Oreopoulos, Philip. 2007. Do dropouts drop out too soon? Wealth, health, and happiness from compulsory schooling. *Journal of Public Economics* 91, nos. 11–12:2213–29.

Podgursky, Michael J., and Matthew G. Springer. 2007. Teacher performance pay: A review. *Journal of Policy Analysis and Management* 26, no. 4:909–50.

Raymond, Margaret. 2009. Multiple choice: Charter school performance in 16 states. Report, Center for Research on Education Outcomes, Stanford University, Stanford, CA.

Rickford, John R. 1999. *African American Vernacular English: Features, evolution, educational implications*. Oxford: Wiley-Blackwell.

Rockoff, Jonah E. 2004. The impact of individual teachers on student achievement: Evidence from panel data. *American Economic Review* 94, no. 2:247–52.

Rosen, Bernard C. 1956. The achievement syndrome: A psychocultural dimension of social stratification. *American Sociological Review* 21, no. 2:203–11.

Rothstein, Richard. 2004. *Class and schools: Using social, economic, and educational reform to close the achievement gap*. Washington, DC: Economic Policy Institute.

Whitman, David. 2008. *Sweating the small stuff: Inner-city schools and the new paternalism*. Washington, DC: Thomas B. Fordham Foundation and Institute.

Zimmer, Ron, Brian Gill, Kevin Booker, Stephane Lavertu, Tim R. Sass, and John Witte. 2009. *Charter schools in eight states: Effects on achievement, attainment, integration, and competition*. Santa Monica, CA: RAND Corporation.

Zweigenhaft, Richard L. 1992. The application of cultural and social capital: A study of the 25th year reunion entries of prep school and public school graduates of Yale College. *Higher Education* 23, no. 3:311–20.