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ABSTRACT

In this paper, we leverage randomized admissions lotteries to estimate the impact of attending a National Heritage Academy (NHA) charter school. NHA is the fourth largest forprofit charter operator in the country, enrolling more than 56,000 students in 86 schools across 9 states. Unlike several of the other large for-profit companies that operate virtual charters, NHA only has standard bricks-and-mortar schools. Our estimates indicate that attending a NHA charter school for one additional year is associated with a 0.04 standard deviation increase in math achievement. Effects on other outcomes are smaller and not statistically significant. In contrast to most prior charter school research which find the largest benefits for low-income, underrepresented minorities in urban areas, the benefits of attending an NHA charter network are concentrated among non-poor students attending charter schools outside urban areas. Using data from a survey of school administrators in traditional public and charter schools, we document several aspects of school organization, culture and instructional practice that might explain these positive effects.

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I. Introduction

Charter schools have expanded rapidly over the past decade, with charter enrollment growing from 2.4% to 6.2% of the public school population between 2008 and 2017 (National Alliance for Public Charter Schools, n.d.; Snyder and Dillow 2015). Growth in this sector has been particularly strong among schools that are part of large charter networks. The share of charter school students in non-profit Charter Management Organizations (CMOs) grew from 10.6% in 2008 to 26.5% in 2017. The market share enjoyed by for-profit charters in Education Management Organizations (EMOs) grew as well, and now EMOs educate roughly 20% of all charter school students in the country (National Alliance for Public Charter Schools, n.d.).

Yet, evidence of charter school effectiveness is almost exclusively based on non-profits, many (but not all) of which are independent or part of small management organizations.[1] In the postsecondary sector, evidence indicates that students at for-profit colleges do far worse than their peers at other schools, even controlling for underlying differences in student characteristics. Those who attend for-profit colleges are less likely to graduate, take on far more debt, are more likely to default on that debt, and do worse on the labor market than comparable college students (Cellini and Turner 2016). Across all levels of education, it is difficult to monitor effort, which may lead schools to skimp on inputs in order to maximize profits. In fact, this is sometimes offered as a justification the role of the public and non-profit sectors in education (Poterba 1996).

Given their very negative record at the postsecondary level, we might expect that for-profits would also harm students enrolled in elementary and secondary education. Indeed, the following eight states currently ban for-profit charters: Alaska, Connecticut, Mississippi, New Mexico, New York, Rhode Island, Tennessee, and Washington (National Alliance for Public Charter Schools, n.d.). Other states impose strict restrictions on for-profit charters, including

[1] A notable exception is the research done on KIPP Academies (see, for example, Angrist et al., 2010).

Massachusetts and Kentucky where evidence must be shown that the organization is a “proven provider,” and Maine which restricts for-profit management to virtual charters (National Alliance for Public Charter Schools, n.d.). Additional states such as California are mobilizing to restrict or ban for-profits (AB-406, 2017).

We provide the first evidence on the effect of for-profit charter schools on student outcomes. [2] Our analysis focuses on the universe of National Heritage Academy charter schools in the state of Michigan. NHA is the fourth largest for-profit charter operator in the country, enrolling more than 56,000 students in 86 schools across 9 states.[3] The two largest for-profit charter operators in the U.S. (K-12 Inc. and Connections Academy) exclusively provide on-line schooling. The only larger bricks-and-mortar for-profit charter school operator is Charter Schools USA.

We leverage randomized admissions lotteries from 44 NHA schools between 2003 and 2012 to estimate their impact on student achievement. Contrary to the experience in the postsecondary sector, we find that these for-profit schools improve some academic outcomes for their students. Our estimates indicate that attending a NHA charter school for one additional year is associated with a 0.04 standard deviation increase in math achievement. Effects on reading are smaller (0.026) and not statistically significant. We find no significant effects (either positive or negative) on attendance, grade progression, disciplinary incidents or special education placement. The effects, while not as large as the marquee results from the high-profile Harlem Children's Zone and Boston charters, are substantial.

[2] Throughout the paper, the term “for-profit charter school” refers to a charter school managed by a for-profit operator. For-profit companies are not allowed to hold a charter in Michigan.
[3] As a point of comparison, the well-known and frequently studied KIPP Charter School network serves roughly 90,000 students in 209 schools (http://www.kipp.org/).

This result is somewhat surprising, but makes sense once we consider how funding structures and incentives differ at for-profit charters and colleges. For-profit colleges are fully funded by tuition, which is largely covered by the financial aid dollars that students bring with them. The schools are unconstrained in the prices they can charge, and typically charge much higher tuition than public colleges. For this reason, compared with public colleges, for-profits have a much stronger incentive to enroll as many students as possible - even if those students are not expected to succeed. The marginal revenue produced by a for-profit college student is far higher than that generated by a student at a public college, which typically charges tuition prices considerably below the marginal cost of educating a student. They can charge these artificially low prices because of subsidies they receive from state and local governments.

In the elementary and secondary sectors, by contrast, for-profit charter schools cannot set prices. They are barred from charging students for attendance, and receive the same, per-pupil allocation from the state whether they are operated by a non-profit or for-profit company. Hence, non-profit and for-profit charters face the same financial incentives.

Our analysis reveals another interesting finding. In contrast to most of the prior lottery-based charter impact literature which has consistently found that low-income, underrepresented minorities in urban areas benefit most from attending the lottery schools, the largest impacts of attending the NHA charter network are concentrated among non-poor students attending charter schools outside urban areas. These findings further highlight the importance of exploring the full range of possibilities in the large, diverse charter school sector.

To explore potential mechanisms, we leverage data from a survey administered to all charter schools and a sample of traditional public schools in the same geographic areas as the charters. The survey asked school administrators about a variety of school policies and practices within five domains: instruction, school culture, organization and leadership, teacher compensation, and time use. We find that NHA schools share many of the same practices as the highly effective non-profit charters in Boston, New York and other urban areas, including a "No Excuses" culture and a focus on extra time and frequent assessment in core academic subjects. This is particularly interesting insofar as it suggests that the benefits of these practices and policies may generalize beyond the types of students and neighborhoods previously studied.

The remainder of the paper proceeds as follows. In Section II, we discuss the prior literature on charter schools, including research focusing on the performance of Michigan charter schools. In Section III, we provide background on the charter school market in Michigan. Sections IV and V describe our empirical strategy and data respectively. In Section VI, we present our findings and Section VII concludes.

II. Prior Literature on Charters

There is a large literature on charter schools, including many studies that seek to estimate the causal impact of attending charter schools on student academic achievement. A key challenge in this literature is addressing the selection bias that stems from the fact that students and families choose to attend charters.

A small but growing number of studies exploit admissions lotteries to account for such selection. In general, these studies have found that urban charter schools have large and statistically significant impacts on student performance (for example Abdulkadiroglu et al., 2011; Dobbie & Fryer, 2011, 2015; Gleason et al., 2010; Hoxby & Murarka 2009). This research includes several studies that focus on individual schools or schooling models. Angrist et al. (2010) study a KIPP middle school in Lawrence, MA and find positive effects. Gleason et al.

(2014) find similar results in a non-experimental study of 41 KIPP middle schools nationwide. Dobbie & Fryer (2011, 2015) study the Harlem Children's Zone charter school in NYC, and find very large positive effects.

The lottery-based studies find that non-urban charters do not systematically outperform traditional public schools (Angrist et al., 2013, Gleason et al., 2010). Some research suggests that the relative effectiveness of charters serving urban students is explained not only by the type of students they serve (and the fact that charters seem to be more effective for students with lower baseline achievement), but also by the fact that many urban charters employ a specific set of policies and practices which has come to be known as the "No Excuses" model (Angrist et al., 2013; Dobbie & Fryer, 2013).

While these lottery-based studies have strong internal validity, they have important limitations. First, even collectively, such lottery-based studies have examined few charter schools. Lottery studies to date have examined only 113 schools out of roughly 6,000 charters operating in the U.S (Chabrier, Cohodes, & Oreopoulos, 2016). The present study will increase that number by 44.

Second, most of these studies have focused on charter schools that enroll students at the start of middle school and high school, because these students have pre-lottery achievement scores that allow researchers to assess the randomness of the lotteries. As a result, an important subset of charter schools has not yet been examined using lottery methods. This study examines the performance of K-8 charter schools, thus helping to close this gap in the evidence.

Third, no studies to date have examined the effect of for-profit charter schools. Nationwide, non-profit charters are certainly the most common, but for-profit schools represent a substantial portion of the sector in many states. In Florida, 40 percent of charters are for-profit.

In Michigan, the state with the highest representation, for-profits enroll two-thirds of charter-school students.[4]

While lottery studies are the "gold standard" from the perspective of internal validity, there is also non-experimental evidence on charter schools. Booker et al. (2011) find that Florida students attending charter high schools are more likely to graduate high school. Sass et al. (2016) find that these students are also more likely to persist in college and have higher earnings in their mid-twenties.

The Center for Research on Education Outcomes (CREDO) has produced several high-profile studies. CREDO constructs matching estimators of charter school impacts by using student-level administrative data. CREDO matches charter students to traditional public school students from the same feeder schools (the traditional public school a student attended the year prior to entering a charter). Students are matched on demographic characteristics[5] and test scores from the previous grade.[6] Each charter student may be matched with multiple observations, and these comparison records are averaged into one "virtual control." Their outcome is student growth in achievement defined as the year over year difference in state standardized test scores (normalized into z-scores relative to the state and grade-level distribution that year).

A 2013 CREDO analysis of impacts for roughly 5,000 charter schools across 27 states finds that charter school students gain slightly more in reading and are no different in math relative to peers in traditional public schools (CREDO 2013a). They note that the average effect of charter schools in their sample has improved somewhat over time, due more to the closure of

[4] Information on for-profit status of charters compiled by authors using data from the Michigan Department of Education; the National Alliance for Public Charter Schools, Charter School Data Dashboard; Miron & Gulosino, 2013; and Mao & Landauer-Menchik, 2013.
[5] Students are matched exactly on gender, race/ethnicity, special education status, English language learner status, lunch status, and grade level.
[6] Matched student must have prior-year assessment scores within 0.1 standard deviations.

ineffective schools than to the improvement among existing schools. Another CREDO report that focuses on urban charter schools find that they are more effective than other charter schools, generating positive growth in test scores on average (0.05 standard deviations in math and 0.039 standard deviations in reading), and are especially effective for poor and minority students (CREDO 2013b). Finally, a 2017 CREDO report examined charters by organizational structure, comparing independent charters to CMOs, vendor operated schools, and hybrids.[7] This report generally finds that schools within a network perform better than individual charters or traditional public schools.

Several studies have looked at the evolution of charter schools over time. Studying charters in Texas using administrative data and a non-experimental design, Baude et al. (2016) find the effectiveness of charter schools in the state has increased over time due to exits from the sector by less effective charter schools, improvements of existing charter schools and expansions of effective charter management organizations. However, the authors also note that an increasingly positive selection of students might help explain the rising performance. A similar study in North Carolina by Ladd et al. (2015) finds that charter schools in that state have been shifting to serve a more affluent and white population of students over time. They conclude that the shift in the type of students enrolling in charters explains a corresponding rise in value added estimates for charter schools over the same period.

Prior Evidence on Michigan Charter Schools

The most recent evidence on charter schools in Michigan comes from CREDO (2009, 2013a, 2015, 2017). Most relevant for this study are the findings from the 2013 and 2017 studies,

[7] The organizational classifications differ from those used in this paper. CMOs are networks where the operator holds the charter and there are more than three schools. Vendor operated schools provide services to at least three schools, but do not hold the charter.

which produced estimates for the NHA network specifically (although these were not confined to campuses in Michigan and estimated the average impact over 71 schools in 2013). According to CREDO (2013a), NHA charters caused a statistically significant 0.03 standard deviation improvement in reading test scores relative to "virtual twins" at traditional public schools and a 0.04 standard deviation improvement in math test scores. These are assumed to be linear effects per year enrolled. When scaled linearly, this would translate to a 0.36 standard deviation improvement in math for a student who enrolled in kindergarten and stayed through eighth grade. The 2017 study paints an even rosier picture, and estimated that an additional year at an NHA school is associated with a 0.14 standard deviation increase in math scores and a 0.11 standard deviation increase in reading scores.

Several other studies have looked at charter school impacts on test scores in Michigan during the late 1990s. Eberts and Hollenbeck (2001) run student-level regressions controlling for observable characteristics and prior test scores estimate that charter schools cause students to score 2-4% lower on math in 4th grade. Using school-level data, Bettinger (2005) estimates a difference in differences and lagged dependent variable model comparing charter schools to traditional public schools within five miles and finds null or negative impacts of attending a charter school on average.

Bettinger (2005) also estimated the effect of charter school entry on traditional public school students' test scores. This is part of a rich literature on charter school spillovers in Michigan that has looked at the impact of charter schools on enrollment in traditional public schools, fiscal health at traditional public schools, and enrollment in private schools, usually finding a negative impact of charter school entry (Ni, 2009; Chakrabarti and Roy, 2010; Arsen and Ni, 2012; and Arsen et al., 2015).

Overall, the CREDO results support the conclusion that urban charters in Detroit, and the NHA in particular, are improving student test scores. Earlier Michigan-focused studies seem to argue the opposite. All of these studies are vulnerable to selection bias.

III. Background on Charters in Michigan

Charter schools first arrived in Michigan in 1994 following legislation passed in 1993. They serve a large and growing proportion of public school students in the state. As of the 2015-16 school year, there were 359 charter schools in Michigan, educating about ten percent of the state's students (Figure 1). The sector has expanded rapidly: in 2003, 204 schools enrolled less than four percent of the state's students. Across the state, charters enroll nearly 150,000 students. As of 2014, there is no cap on charters in Michigan (except for virtual, online charter schools).[8] Over two-thirds of states cap the total number of charter schools (including Massachusetts and New York).

The majority of charter funding comes from the state and is currently equal to the minimum per-pupil allowance for traditional public schools. In 2014-2015, the per-pupil allocation from the state was $7,391.[9] However, charters can raise additional funds through private donations (if non-profit), investment (if for-profit), and are eligible to apply for local and federal grants just as any public school would be. They have more leeway to charge fees for services such as transportation. In 2014, the average, per-pupil expenditure was $9200 for charter schools, and $9600 for traditional publics. In contrast to traditional public schools, however, charter schools must pay for capital costs (such as their building) out of these funds as well. According to the Center for Education Reform, charters are funded at about 64 percent of their

[8] Source: MDE charter FAQ ("Michigan Charter Schools - Questions and Answers," 2016). While there was initially a cap, a 2011 law raised the cap to 300 through 2012, 500 through 2014, and removed it thereafter.
[9] Source: Bulletin 1014 (MI, 2016).

district counterparts on average. Across the country, they average $7,131 in per-pupil expenditures compared to the average of $11,184 in the traditional public schools in 2009-2010. It is also common for charter schools to receive no additional funding for school facilities (Rebarber & Zgainer, 2014).

The charter landscape in Michigan differs from that in many other states in several important ways.[10] While the charter laws are not consistently more or less stringent than other states, they are notably looser when contrasted to Massachusetts and New York, two states where charter schools have been studied extensively. Anyone can apply for a charter, and multiple authorizers receive, review, and decide upon these applications. By contrast, about half of states with charter laws have only one authorizer. In Massachusetts, school districts can authorize charter schools, but the only other authorizer is the state itself. Michigan's authorizers also oversee the operation of charters, receiving a share of the state allocation to compensate them for their effort. Authorizers in Michigan include public universities, community colleges, traditional K-12 school districts, and intermediate school districts.

The guidelines for authorizing, evaluating and renewing a charter are not precisely specified by state law. Authorizers each create their own protocols for assessing charter applicants, selecting charter board members, and evaluating a charter's performance, including the timeline for renewal.[11] These provisions are placed in a charter contract. If a charter fails to meet the standards laid out in the contract, the consequences are at the discretion of the authorizer. Although the intent of the law is clear (an authorizer must hold a charter school accountable), there are no mandated guidelines for how to implement accountability protocols.

[10] Information on state charter laws and policies comes from the National Alliance for Public Charters Law Database, the Michigan Dept. of Education FAQ website "Michigan Charter Schools - Questions and Answers"; Rebarber & Zgainer, 2014; the Michigan legislature (MI, 2011); and the Education Commission of the States.
[11] Only urban charter high schools have a mandated timeline for renewal, which is 10 years (MI, 2011).

Only five other states do not specify an explicit timeline for renewal; the majority of states mandate a renewal term of 10 years or less. Surprisingly, in seven states, charter schools are responsible for evaluating themselves. Michigan is among states that require the authorizer to evaluate and renew the charter. In Massachusetts and New York, the state takes an active part in evaluating and renewing charters. In the former, the state is the sole authorizer (except for districts themselves), and in the latter, the state evaluates schools in addition to the authorizer.

The management and ownership structure of Michigan's charters is also distinctive. Nationwide, charters are typically non-profit organizations that are locally managed. That is, they are not part of a national network or chain. The next most common arrangement is a non-profit charter attached to a network, such as the KIPP schools. These non-profit charter networks are known as charter management organizations (CMOs).

In sharp contrast, Michigan is dominated by for-profit schools that are connected to a national network. Michigan has more for-profit charters – by far – than any other state. Two-thirds of Michigan's charter schools are run by for-profit operators, known as education management organizations (EMOs); see Figure 2. In 2015-2016, 74% of Michigan students enrolled in a charter school were attending a school managed by a for-profit. In all other states, only a minority of charter schools are operated by for-profit firms; in New York and many other states, they are prohibited outright. The only state that comes anywhere close to Michigan on this dimension is Florida, where 40 percent of charter schools are run by for-profit firms.

Characteristics of Michigan Charters

Charter schools are concentrated in Michigan's urban areas. Roughly half of charters are in cities, compared to 14.5 percent of traditional public schools (Table 1, Columns 1 and 2).

Charters therefore reflect the demographics of cities, with their students more likely than those at traditional public schools to be Black (54 percent vs. 16 percent), Hispanic (nine percent vs. seven percent) and economically disadvantaged (73 percent vs. 47 percent). Standardized scores are substantially lower in the charter sector than in traditional public schools: the gap is about 0.4 and 0.3 standard deviations in math and reading, respectively. Charters and traditional public schools also differ in the share of students who are categorized as English learners (nine percent and six percent, respectively), though they are relatively close in the share of students who qualify for special education (about 13 percent in both sectors).

Given that they dominate the state's charter sector, the profile of Michigan's for profit-schools (Table 1, Column 3) closely tracks that of charters overall (Column 2). A more instructive contrast is with the non-profit charters in the state (Column 5). For-profit charters, compared to non-profits, are less concentrated in urban areas (46 percent vs. 55 percent) and more concentrated in the suburbs (40 percent vs. 18 percent). For-profit schools enroll a greater share of White (36 percent vs. 28 percent), Hispanic (nine percent vs. six percent) and Asian (four percent vs. one percent) students than the non-profits. They enroll a smaller share of Blacks than non-profits (50 vs. 62 percent).

Charter schools also differ from traditional public schools in their grade structure. Over 35 percent of Michigan's charter schools combine elementary and middle grades, compared to about four percent of traditional public schools (Table 1, Columns 1 and 2). In the traditional public schools, the standard progression is from an elementary school to a middle school, with the transition at grade six or seven, depending on the district. A typical charter-school configuration in Michigan enrolls children from kindergarten through eighth grade, with some also providing pre-kindergarten.

National Heritage Academies (NHA) Charter Schools

The first NHA charter opened its doors in 1995. Since the inception of the first NHA school in Grand Rapids, the NHA network has expanded to 86 schools in nine states, with over half located in Michigan. Currently NHA is the largest charter operator in Michigan with 48 schools operating in 2016.

NHA schools served 5% of all third through eighth graders in Michigan in 2015-2016, or about 47% of all charter students in those grades. Figure 2 plots the share of Michigan students attending a school in the NHA network, as well as the share enrolled in any charter managed by a for-profit EMO. The figure shows most of the growth in charter school enrollment since 2003 has come from for-profit charter schools. In addition, enrollment at NHA has grown steadily, if not quite as quickly as charter enrollment overall.

Table 2 compares NHA schools with other charter schools in the state, as well as with traditional public schools. For the purpose of comparison, we only show schools that serve elementary or middle school students. The statistics are from the 2011-12 school year, when the data for this study was first collected, although the general patterns remain the same today.

Unlike the schools featured in many other lottery-based charter studies, NHA schools are not found predominantly in urban areas, as the network itself did not originate in Michigan's largest urban center, Detroit. Table 2, column (5) shows that almost half of NHA schools are located in suburban areas, with 40 percent in urban areas and 12 percent in towns or rural areas. Figure 3 shows the location of NHA schools across the state. The student demographics reflect the geographic distribution as well. Students served by the NHA are less likely to be Black and more likely to be Asian, Hispanic, or White than students enrolled in other charter schools in the state. Likewise, the share of NHA students who qualify for free or reduced price lunch is lower on average than other charter schools at 61% relative to 71% in columns (5) and (2) of Table 2, respectively.

Like other charter schools, NHA schools have a higher proportion of novice teachers relative to traditional public schools (7.7 percent vs. 1.1 percent). Similarly, the percent of teachers each year who are new to an NHA school is higher than in traditional public schools (19.5 percent vs. 11.7 percent).

The NHA Model

In contrast to some other charter organizations, NHA maintains full operational control over all of its schools, meaning they make all decisions including for hiring, training, and curriculum.[12] Generally organized as K-8 schools, the stated mission of the NHA is to develop a student's academic ability in tandem with her moral character.

The curriculum at NHA charters focus on math, social studies, science and literacy. Initially, curriculum was based on the Core Knowledge program, founded by E. D. Hirsch Jr. at the University of Virginia. Later, the NHA developed its own curriculum based on the ACT college-readiness standards and the subject areas covered by state tests. The curriculum now represents a combination of commercially-purchased curricular tools and internally-developed resources in order to better align to state standards and college readiness expectations.

However, the NHA network retains some unique features. NHA schools advertise a philosophy of partnering with parents to "reinforce the moral guidance a child receives at home,"

[12] In contrast, at KIPP, the largest nonprofit charter networks, individual schools license the KIPP name, which the national organization can revoke if a school is considered to be out of compliance with KIPP goals and standards, and no school staff are employed by the national KIPP organization (Angrist et al. 2011).

with a moral focus curriculum that is worked into the school day in many different ways. For example, each month a secular virtue, modeled on Plato's cardinal virtues, is integrated into lessons. Readings may focus on themes related to the virtues, which include wisdom, respect, gratitude, self-control, integrity, perseverance, courage, encouragement, and compassion.[13] And, in the past, the social studies and history curriculum has had a distinctly patriotic and America-centric perspective.

As discussed in more detail later, the instructional approach in NHA has several features that distinguish it from most traditional public schools. NHA schools somewhat more time to core academic subjects of math and ELA than other schools. In addition, they are much more likely than other schools to group students by ability level for instruction in core subjects, particularly math.

According to NHA staff, however, the most important feature of the NHA model is an emphasis on professional and leadership development.[14] In other schools, the principal or other top administrators are constrained by time and other responsibilities which make it difficult to respond to challenges within individual classrooms in a timely manner. Central to the school level leadership structure is what NHA refers to as the "Dean Model." At each school, three or four deans each govern a hall or wing (e.g., K-2, 3-5, 6-8) and manage about 15 direct reports. The day-to-day duties of a dean vary by school, but their primary goal is to coach the teachers. This relatively small administrator-to-teacher ratio allows deans to provide teachers with frequent and specific feedback. In addition, deans are responsible for duties such as overseeing student culture, managing parent relationships, and participating in teacher recruitment. At NHA,

[13] The distinctive focus on character and virtue have occasionally generated controversy, including a lawsuit by the ACLU which was dismissed in 2000 that accused one academy of promoting religious education.
[14] Much of the description of NHA in this section is based on personal communications with various NHA officials during 2017 and 2018.

the principals meet weekly with the deans to set goals and train them to effectively coach the teachers. This approach frees up the principal to manage the larger logistics of the school.

NHA schools place a high priority on developing talent within their schools. Beginning with teachers, NHA has developed a clear framework for developing goals and fostering growth. As teachers grow in their capacity and skills, they have opportunities to take on leadership roles within the school. In particular, high-performing teachers are identified by their principals to enter into the Dean Prep Academy. The Academy is a year-long online program which introduces teachers to the mindset and skills of an effective dean with the goal of investing in and preparing them to become a dean. All new deans enter into a two-year program that combines online sessions and in-person trainings that is specifically designed to meet the needs of new deans. After the first two years, all deans continue to participate in ongoing professional development at the school, regional, and national level.

NHA has developed additional leadership opportunities to maintain their highest performing deans. Deans who are not looking to become principals can serve as dean content leaders, which enables them to lead some of the professional development during regional meetings or participate in mentoring of new deans. Aspiring principals are selected to enter the Principal Prep Academy which prepares them to lead their own NHA school within one to two years.

The Principal Prep Academy is similar in structure to the track for deans, exposing candidates to all aspects of what is required of principals including foundational NHA procedures such as instructional leadership and data-driven instruction, developing school culture and a high performing staff, and school accounting and budgeting. New principals participate in a two-year new principal program to help ensure a smooth transition. Additionally, both principals and deans participate in an annual four-day leadership summit each summer. High performing principals are chosen to become executive principals, which requires them to visit other schools to provide feedback and support, and beyond these established tracks may move on to higher level positions at NHA.

IV. Empirical Strategy

The existence of admission lotteries simplifies the analysis dramatically. Assuming that the lotteries were indeed random, which we verify below, we can estimate the Intent-to-Treat (ITT) impact of charter schools simply by comparing outcomes of lottery winners and losers.

Lotteries occur by grade, school, and year. A student would apply, for example, to enroll in the first grade of School A for school year 2016-17. If that school-grade-year combination is over-subscribed, a lottery is held. Lotteries are therefore uniquely defined in our data by the interaction of school, year and grade dummies.

However, many students apply to more than one lottery. Since lotteries are independent, the probability a student wins will rise with the number of applications she makes. Further, admissions probabilities vary across schools. Following Abdulkadiroglu et al. (2011), we define a lottery risk set as the group of non-degenerate lotteries to which a student applied.[15] To estimate the ITT, we regress outcome y for student i on an offer indicator and additional covariates:

$$y_{ik\tau t} = \beta_0 + initialoffer_{ik\tau t}\beta_1 + X_i\Gamma + \gamma_{k\tau} + \varepsilon_{ik\tau t} \qquad (1.1)$$

[15] Suppose, for example, student 1 applied to lotteries A and B, student 2 applied to lotteries B and C, student 3 applied to lottery A only, and student 4 applied to lotteries A, B and C. In this case, the risk set for students 1 to 4 would be AB, BC, A, ABC. In our analysis, we identify the impact of charters by comparing students within lottery risk set. Hence, in the example above, student 1 would not serve as a comparison for student 2 even though both applied to lottery B.

where *y* is an outcome for student *i* in year *t* in risk set *k* in year τ . We define *initialoffer* to take a value of one if a student wins *at least one* lottery within her risk set. A set of lottery risk-set fixed effects (γ) limits the comparison to winners and losers within the same lottery. Student covariates, ***X***, can increase the precision of estimates. The coefficient β_1 provides a consistent estimate of the impact of winning at least one lottery to an NHA school. In most models, we present standard errors clustered by the risk-set, although we also show that clustering by student or the school attended in year *t* yields similar inferences.

Testing for Covariate Balance & Differential Attrition

Table 3 contains sample means for students in the analysis sample, pooled and separately by lottery status. Half of NHA applicants are female, while 27 percent are Black and seven percent are Hispanic. Applicants live in census block-groups where the median income is $64,632. The average age at application is 6.6 years, reflecting the fact that most apply by first grade for admission. For this reason, baseline test scores, reduced price lunch eligibility, special education eligibility, and limited English proficiency are only reported for students who were enrolled in Michigan public schools prior to the lottery, or about 40 percent of students. Means and adjusted differences in Table 3 for these outcomes are only for students with pre-lottery characteristics, and reported in columns (5) through (8).

To test for correlations between these characteristics and lottery status, we run regressions of the listed variables against the instrument and risk-set dummies. There are no significant differences by lottery status, except for age at application. Those who win the lottery are 0.015 years younger, which while statistically significant, is small in magnitude and plausibly due to chance. Joint tests of significance fail to reject the null.

Estimating the Impact of Attending an NHA School

To obtain the impact of the local average treatment effect of attending an NHA school, we estimate the following instrumental variable model:

$$y_{ik\tau t} = \beta_0 + cumyrs_{ik\tau t}\beta_1 + X_i\Gamma + \gamma_{k\tau} + \varepsilon_{ik\tau t} \qquad (1.2)$$

where *cumyrs* measures the number of years student *i* attended an NHA school in the risk set between the time of the lottery (τ) and the current year (*t*). The first stage model is:

$$cumyrs_{ik\tau t} = \alpha_0 + initialoffer_{ik\tau t}\alpha_1 + X_i\mathrm{A} + \gamma_{k\tau} + u_{ik\tau t} \qquad (1.3)$$

V. Data

This analysis focuses on the National Heritage Academies. We collected application data for lotteries that took place between 1999 and 2012 in 46 NHA schools (unless otherwise noted, we denote academic years by the Spring year, so that 1999 refers to the 1998-99 academic year).

Each school is responsible for managing its own application and admissions process, and is required to hold lotteries to allocate spaces for any year-grade that is oversubscribed. NHA stores the records centrally. For some schools, we obtained electronic records on all applicants that included information on which applicants were offered spots. Some schools also had physical "lottery boards" in which the names of applicants and their lottery results were recorded. Data from these boards were entered by hand and checked for accuracy. Our resulting records include school name, year of the lottery, grade to which the student is applying, and the student's student name, birthdate, gender, home address, and ethnicity. Appendix A provides more detail on the data collection process and the type of information we obtained.

Construction of Lottery Instrument

Students can apply to as many NHA schools as they like; the lotteries are independent, both statistically and administratively. When admitted students decline a seat, spots open for students farther down the lottery waitlist. Thus, some students are offered spots immediately, while others are offered admission later.

In theory, this allows for two binary charter instruments: initial offer and ever offer. *Initial offer* is an indicator variable set to one if a student is offered a seat at a school in the applicant's risk set at the time of the lottery. *Ever offer* is equal to one if initial offer is one, but also counts offers made after the lottery, as the schools move down their waiting lists.

In practice, initial-offer records are kept meticulously by NHA, but only about 75 percent of schools maintained consistent data on whether they ever offered admission to a student. We therefore focus on the initial offer instrument. Appendix B provides additional detail about the construction of these instruments.

Sample Construction

We begin with 79,459 applications to 2,907 lotteries at 46 NHA schools. We apply a series of restrictions to obtain the final analysis sample, as outlined in Appendix Table 1. Because state administrative data does not exist prior to 2001, we focus on lottery records collected for the years 2002-12. We exclude roughly eight thousand applications of children who had siblings enrolled in the school and thus did not participate in the lotteries. In addition, applications by individuals who are never observed in Michigan public schools in any years following the lottery are dropped. These students will have enrolled in private school or homeschool, or left the state. A key concern for the analysis is whether this type of sample attrition is correlated with lottery outcomes, as would be the case if lottery losers were more likely to enroll in private schools, for example. This type of sample selection could bias the estimates if lottery losers who chose to attend private schools were systematically different than lottery losers who chose to attend traditional public schools. As described in the next section, there is no difference in the match rate for lottery winners and losers.

We drop the small number of applicants who either previously attended the school or previously won a charter lottery (to the same or different school). As described in the prior section, the preferred estimation strategy utilizes one observation per student and controls for lottery risk sets.

A more important restriction, both conceptually and empirically, is that we limit the analysis to students applying to non-degenerate lotteries – that is, lotteries in which there is at least one winner and one loser. If all (or no) students in a lottery are offered admission to the school, there is no win-lose contrast to estimate and the lottery cannot contribute to the analysis. This restriction reduces the number of applications by nearly half, to about 31,000 (from about 57,000) and to 749 lotteries (from 2,809) for 44 schools (out of 46).

Students in degenerate risk sets, that is risk sets where all the students received an offer of admission or all failed to receive an offer, are also omitted. This removes an additional 4,157 applications and 14 lotteries. We are left with an analytical sample of 27,143 applications to 735 lotteries at 44 NHA schools, with 20,255 unique students.

We are able to observe at least one post-lottery achievement score for 90% of students in the sample. We observe outcomes through grade 8 for roughly 51% of students in the sample, and through grade 12 for about 15%. For this reason, we limit the analysis to outcomes measured through grade 8. A little over 55% of lottery applications are for kindergarten entry, the most common entry grade for the NHA. The next most common application grade is first, at just under 10% of applications. Applications to grades pre-K and 2nd through 6th each make up about four or five percent of the sample. Finally, only one or two percent of the sample are applications for 8th or 7th grade, respectively.

Matching to State Outcome Data

We matched charter school applicants to administrative data from the Michigan Department of Education (MDE) which included all students enrolled in any Michigan public school (including charter schools) from 2002-03 through 2015-16.

Because neither lottery records nor state data contain a common unique identifier such as social security number, we relied on name, date of birth, gender and ethnicity to match records. We used probabilistic as well as exact matching techniques to account for minor misspellings, nicknames, date and month swaps in date of birth, and other common errors. Appendix C provides more detail on the matching.

We matched 93 percent of lottery application records to the state data. We find no statistically significant differences in the match rate of lottery winners and losers, as shown in Appendix Table 2. This table shows estimates from regressions in which the dependent variable is a dummy indicating a match to the state data. Paralleling our analysis of outcome effects, these regressions include risk-set dummies. The coefficient on the indicator for winning a lottery is small in magnitude (less than one percent) and statistically insignificant.

Outcome Measures

Administrative records from the MDE provide information on the school of enrollment and achievement scores. To facilitate comparisons over time and with previous research, we focus on scores in math and reading, which occur in grades three through eight. We standardize exams by grade, year and subject to a mean of zero and a standard deviation of one. We also examine grade retention, special education placement, and disciplinary incidents as well as for an indicator for scoring within the "proficient" range as defined by the state of Michigan.[16]

VI. Results

Table 4 presents first-stage estimates of the relationship between lottery offer and enrollment in an over-subscribed NHA charter school in the fall after the lottery. Recall that the instrument here is a dummy indicating that an applicant won admission at the time of the lottery; that is, she received an initial offer.

Lottery winners are 19 percentage points more likely than lottery losers to enroll in an NHA school the following fall. The F-statistic on this estimate is 97.75. The control mean indicates that a third of those who lose the lottery still manage to enroll in an oversubscribed NHA charter in the fall after the lottery. These are likely applicants from the randomly-ordered waitlists who received an offer after winners further up the list declined the offer of admission.

The other columns of Table 4 show that the relevant counterfactual for lottery winners is a traditional public school. Winning an NHA lottery decreases the likelihood of attending a traditional public school by 16 percentage points. By contrast, winning a lottery decreased the

[16] This definition changed over the span of the analytic sample, because the state raised the threshold for proficiency in 2011. We have coded the definition consistently, applying the more stringent threshold to all years of data.

likelihood of attending non-NHA charter by only half a percentage point, and of attending an NHA school outside the student's risk set by 1.6 percentage points.

It is possible that these initial differences in enrollment grow or shrink over time. To examine this, Figure 3 plots reduced form estimates of the difference between lottery winners and losers in the cumulative years enrolled in a risk set school, estimated separately each year after the lottery. Figure 3 also presents the treatment-control difference in the share of years a student enrolled out of all the years she could potentially have done so. The coefficients in Figure 3 are estimated controlling for student characteristics and risk-set fixed effects as in equation (1.1). While the difference in cumulative enrollment grows over time, the fraction of potential years remains constant or even falls slightly.

Main Outcome Estimates

Table 5 shows estimates of the relationship between attending an NHA charter school and student test scores. These estimates are based on a student-year panel that includes data on post-lottery test scores in grades three through eight. The 2SLS estimates reflect the impact of attending a charter school for one additional year. In Appendix Table 2, we confirm that receiving an initial offer is not associated with the likelihood of enrolling in Michigan public schools, on-time grade progression or having a standardized test score.

Consistent with prior lottery-based charter studies, we find that an additional year spent at an NHA charter is causally associated with positive test score gains in math, with smaller effects on reading scores that are not significant at conventional levels. The yearly gain in math is estimated at 0.04 standard deviations, and is significant at the ten percent level. In addition, there is a 1.4 percentage point increase in the likelihood of scoring above the proficiency cutoff. When scaled by the nine years a student could potentially spend at an NHA charter (given that it serves grades K-8), it suggests a maximum potential increase of 0.36 standard deviations in math scores.

We have also examined non-score outcomes, including the attendance rate, classification as a special-education student, and on-time grade progression. Effects for all of these outcomes were small and statistically insignificant. Results available upon request.

Heterogeneity in Effects

We explore heterogeneity of effects along several dimensions. To begin, we examine whether the effects vary by grade level. The impact of attending a charter school may differ across grade levels for various reasons, including (i) a nonlinear effect of years attended (e.g. one year attended is important but three years makes no additional difference), (ii) a change in the quality of the counterfactual (e.g., if traditional public middle schools are particularly weak), or (iii) differences in the nature of the material covered and school expertise (e.g., charter schools may be particularly good at teaching early, basic skills, in which case the impacts in early grades may be larger than impacts in later grades).

Figures 5a and 5b show the 2SLS estimates of math and reading scores separately by grade. The estimates are noisy, and exhibit an odd saw tooth pattern. Because the later lottery cohorts have not yet reached the older grades, it is possible that the patterns we see here are due in part to compositional changes and heterogeneity in effects by cohort. Figures 5a and 5b show the patterns for cohorts we can observe through grade eight. Here we see some evidence that the point estimates appear somewhat larger for students in the middle school grades, although the estimates are not precise enough that they are significantly different by grade.

We next examine how these effects differ by student demographics. Table 6 reports estimates of the impact of an additional year spent at an NHA school for different student characteristics. Several interesting patterns emerge. First, female students benefit more than male students (0.059 versus 0.010). Second, in contrast to prior research on charter schools, economically disadvantaged students do not appear to benefit as much as other students. For example, the reduced form estimate for non-poor students is 0.045 compared with -0.012 for poor students. Similarly, the 0.034 reduced form effect for White/Asian students is larger than the 0.015 estimate for Black/Hispanic students.[17]

Interestingly, these subgroup differences appear to be driven in part by differences in dosage. Economically disadvantaged students who won the lottery accumulated just 0.396 more years in an over-subscribed NHA school than those who lost the lottery (Column 4 first stage). For applicants who are not economically disadvantaged, the first stage is almost three times larger: 0.942 years. There is a similar, but less pronounced, pattern by race/ethnicity: the first stage for Whites/Asians is 0.851 additional years, while for Black/Hispanics it is 0.622. Indeed, our first-stage estimates are particularly low for applicants to NHA schools in urban areas.

If we focus on students who were applying to NHA schools in an urban area, we see that winning the admissions lottery is associated a mere 0.26 additional years of enrollment in an NHA school. While the urban sample size is too small to split further, we can split the non-urban sample into economically disadvantaged and less disadvantaged students within (columns (10) and (11) of Table 6). The same pattern holds, and is even more pronounced, with non-poor students benefitting more from enrollment in an NHA, and exhibiting a dosage almost three times the size of poor students.

[17] Patterns are similar, though less pronounced, for reading scores. Results available upon request.

We conducted several supplementary analyses to better understand the pattern of first-stage estimates. To determine whether the differential first stages shown in Table 6 result from differences in immediate enrollment versus subsequent mobility, we estimated the first-stage effect by year since lottery separately for each subgroup. We find that the differential effects are driven by the fact that disadvantaged students who win a lottery are substantially less likely to enroll in the NHA school next Fall when compared to more advantaged peers. For example, the first stage effect on enrollment next Fall is 22 percentage points for non-poor students, and only 13 percentage points for poor students. Similarly, the first stages for non-urban and urban students are 23 percentage points and 8.6 percentage points respectively (more detailed results are available upon request.)

One reason the first-stage could be lower for poor and urban students is that they face a relatively higher cost of commuting to an NHA school. To examine this, for each student we calculate the distance from his or her census block centroid to the nearest NHA school in the student's choice set, as well as the distance to the nearest other public school the student could have attended (based on district boundaries and grade level). We then calculate the difference between these distances – i.e., how much further would the student have to travel to attend the nearest NHA option in his or her risk set compared with the closest public school.

The average (s.d.) for poor children is -3.6 miles (7.74) compared with -3.2 (6.15) for non-poor children. For poor students, the nearest NHA school is on average 3.6 miles further than the nearest TPS, while it is 3.2 miles further than the nearest TPS for non-poor students. The urban and non-urban mean and standard deviations are -3.0 (6.3) and -3.5 (6.8), respectively. Theoretically, both the differential in relative distance to an NHA, and the fact that charter schools are not required to provide transportation for students, could create differential costs for poor students.

To examine this further, we include the interaction between winning an offer and student subgroup (e.g. poor or urban) in the first-stage regression. The regression also includes the differential distance measure, the two-way interactions between the distance measure and winning a lottery, and the distance measure and the student subgroup, and the triple interaction of the subgroup, offer, and distance measure, allowing the "cost" of distance to vary for poor vs. non-poor (or urban vs. non-urban) students separately. The regression shows no differential effect of the distance measure on poor or urban student take-up rates,[18] and does not explain the initial take-up gaps, though the difference in distance between NHA and TPS schools is strongly associated with enrollment in the NHA in general.

We conduct a similar exercise using the number of different schooling options available within a two, three and five-mile radius of a child's home census block. Once again, while the number of alternative options is negatively associated with NHA enrollment, it does not explain the differential take-up across demographic groups.

For the purpose of understanding the impact of attending the benefit of actually attending an NHA school, the differences in dosage do not change the patterns seen in the reduced form – namely, less disadvantaged students appear to benefit more from NHA. It does, however, provide information on the compliers who are identifying our IV estimate. Specifically, the pattern of results shown in Table 6 suggests that our average 2SLS estimate of .04 standard

[18] The coefficient is a precisely estimated zero for the triple interaction of subgroup, any initial offer, and the distance differential. The coefficient on the double interaction of any initial offer and the distance differential is a statistically significant 0.4 percentage points; for each mile closer that the closest NHA gets relative to the closest TPS, take-up increases by 0.4 points for all students. Results available upon request.

deviations is driven by students who are less economically disadvantaged, more likely to be white and/or Asian and more likely to be attending an NHA school in a non-urban area.

Effects on School Inputs

The results above suggest that attending a NHA school increases standardized math test scores, particularly in the middle school grades. In order to explore potential mechanisms, we estimate the models described above where the outcomes are characteristics of the schools actually *attended* by lottery applicants in years following the lottery (rather than the student's own standardized test scores). As above, Table 7 presents the reduced form estimates which capture the impact of receiving an initial offer as well as the 2SLS estimates which reflect the impact of *attending* an NHA school for an additional year. The results in this table tell us how NHA schools differ from the counterfactual school a student would have attended he she or he not received an offer from an NHA school.

A few differences stand out. First, attending an NHA school increases the likelihood of the student attending a K-8 school by 15 percentage points on average over all years in the panel (relative to baseline of 49 percent), and reduces the number of students per grade in the school by 8 (relative to baseline of 127).

Second, attending an NHA school changes the composition of peers a student faces. Students who are induced to attend an NHA school through winning an admissions lottery end up attending schools with two percentage points (five percent) fewer poor children, 0.8 percentage points (seven percent) fewer special education students, and peers who score 0.08 standard deviations higher on state math exams.

Third, attending an NHA school exposes students to a notably different set of teachers. Teachers in NHA schools have less experience and are more likely to turnover than teachers in schools that students would have attended otherwise. Attending an NHA school is associated with a 4.8 percentage point increase in the share of novice teachers, and reduces median teacher experience by a year relative to a baseline of 6.4 years. Further, receiving an offer to an NHA school exposes students to teachers who are slightly less likely to have attended competitive college. However, it also exposes students to teachers who, on average, score 0.05 standard deviations higher on the teacher certification exam.

In an effort to explore school policies and practices, we administered a survey to all general education charter schools in Michigan that were open during the 2012-13 and 2013-14 school years as well as the traditional public schools that each charter school's students would most likely have attended based on their neighborhood.[19] School leaders responded at very high rates, with 85% of charter school leaders and 76% of traditional public school leaders participating in the survey. A total of 435 schools, including 226 charter and 209 traditional public schools are represented in the study. We created 20 measures of school practices within 5 domains – instruction, school culture, organization and leadership, teacher compensation, and time – each of which is described in detail in Appendix Table 3.

Because we did not survey all nearby traditional public schools, students in our sample who won a lottery to an NHA school were substantially more likely to attend a school that had completed a survey. For this reason, we cannot replicate the analysis from Table 7 with our

[19] Because a given charter school can draw students from a number of different traditional public schools, we assign each charter school a counterfactual public school based on the modal school students would likely otherwise attend. In cases where charter schools span more than one school level (e.g., a K-8 charter school), we assign each grade span (e.g. K-5 and 6-8) its own traditional public school. See online appendix at http://edpolicy.umich.edu/publications/#policy-briefs for further explanation.

survey measures. However, we can compare school practices at the NHA schools to nearby traditional public schools that many, though not all, lottery losers attended. To do so, we regress each survey measure on binary indicators for NHA charter school and other charter schools as well as match group fixed effects, which serve to ensure that each charter school is being compared to the set of traditional public schools nearby. Results are reported in Table 8.

Several findings stand out. First, there are notable differences in school culture and organization. NHA schools score a full standard deviation higher on the "No Excuses" measure used in Dobbie & Fryer (2013). They also score 0.75 standard deviations higher on a measure of parent engagement.

There are also some potentially important differences in instructional practices – including the use of time – between NHA and other schools. While there is no difference in total instructional hours per year, NHA schools devote 0.23 more hours per week to math instruction than comparison traditional public schools. NHA schools are substantially more likely to use ability group in core academic subjects. Indeed, virtually all NHA schools report grouping students by ability for math instruction and 80 percent doing so for reading instruction. In contrast, only half of traditional public schools report doing so for either subject. NHA schools report administering diagnostic or benchmark assessments more frequently than other schools – a difference of 0.7 standard deviations on our measure of overall test frequency.[20] On the other hand, we found no significant differences in the nature or amount of tutoring provided in NHA schools, or the uniformity of instruction.

Third, we see some differences in the domain of school organization and leadership. NHA schools are 47 percentage points (150 percent) more likely to report that administrators act

[20] Because charters are required to take the same state exams as all public schools, the difference stems from the use of diagnostic or benchmark exams.

as mentors to teachers. NHA principals report observing teachers roughly 9 hours per year compared with principals in traditional public schools who report doing so only 2 hours per year. The high reported rates of administrative mentorship could be related to an organizational feature of the NHA. NHA schools employ three instructional deans in addition to a principal. The deans are primarily responsible for mentoring teachers in instructional practices and generally spend an hour a week observing them.

Administrators in NHA schools report having substantially more control over hiring decisions than administrators in other schools, and are substantially less likely to report that “bad teachers” are an important problem in their school.

The starting teacher salaries at NHA schools is roughly $1,500 less than the $37,400 reported by comparison traditional public schools, but about $2,400 higher than starting salaries in other charter schools. However, teachers at NHA charters are much more likely to be eligible for performance bonuses compared with teachers in traditional public schools or other charters.[21] Only 20 percent of comparison schools report having performance bonuses while all NHA schools report such bonuses. Conditional on the existence of bonuses, there is not a significant difference in the amount of the bonus, but the estimates are very noisy.

VII. Discussion and Conclusion

To the extent that there is a consensus in the charter literature to date, it seems to be that while charter school impacts vary considerably, they tend to be most effective in urban settings for poor and minority students. The most effective schools identified to date have been non-profits, and the most rigorous, non-experimental evidence suggests that, on average, for-profit

[21] The NHA reports that this practice is less prevalent in recent years relative to the survey year.

charters are less effective than the traditional public schools students would have attended in the absence of the charter.

The current study of NHA schools in Michigan presents a counterpoint to these consensus views, examining schools with a different management structure that tend to locate on the fringes of urban centers more often than within them, and which are effective among an entirely different population of students. Our estimates indicate that attending a NHA charter school for one additional year is associated with a 0.04 standard deviation increase in math achievement, not as large as the marquee results from the high-profile Harlem Children's Zone and Boston charters, but economically meaningful nonetheless.[22]

Our analysis reveals another interesting finding. In contrast to most of the prior lottery-based charter impact literature which has consistently found that low-income, underrepresented minorities in urban areas benefit most from attending the lottery schools, the largest impacts of attending the NHA charter network are concentrated among non-poor students attending charter schools outside urban areas. These findings further highlight the importance of exploring the full range of possibilities in the large, diverse charter school sector.

Of course, the present analysis only examines a single charter school network, and thus cannot speak to the effectiveness of all for-profit charter schools in Michigan. In ongoing non-experimental work, we are examining the population of charter schools in Michigan, allowing us to explore factors associated with charter effectiveness, including for-profit status and network membership. However, by leveraging rich survey data, the present study sheds some light on potential mechanisms through which schools may influence student achievement.

[22] These estimates are consistent with the most recent non-experimental estimates of NHA reported by CREDO (2017).

Despite its different management structure and student population, NHA shares many of the policies and practices of previously studied successful charter schools. NHA provide extra time for core academic subjects, utilize ability grouping and frequent benchmark assessment, employ relatively young teachers, maintain a "No Excuses" culture, and have principals actively engaged in observing and mentoring teachers. These findings provide further support for the set of policies associated with successful schools in other work (Abdulkadiroğlu et al. 2011, Angrist et al. 2013, Dobbie and Fryer 2013, and Fryer 2014).

As powerful forces in domestic policy move to increase the prevalence of school choice mechanisms, including charter schools, it is more important than ever to understand the settings that are most conducive to their success.

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Table 1: Summary Statistics on Michigan Charter Schools in 2014-15

	Traditional Public Schools	Charter Schools				
		All	All For-Profits	Independent For-Profits	All Non-Profits	Independent Non-profits
	(1)	(2)	(3)	(4)	(5)	(6)
Number of Schools	2,687	367	237	31	105	46
Number of Students	1,331,666	147,525	108,743	10,766	32,658	12,827
% Urban Schools	14.5	49.4	46.4	48.4	55.2	34.8
% Suburban Schools	41.1	33.7	40.1	29	18.1	26.1
% Town/Rural Schools	44.4	16.9	13.5	22.6	26.7	39.1
% Elementary Schools	54	20.2	20.3	19.4	21	19.6
% Middle Schools	17.5	5.7	5.5	6.5	6.7	6.5
% High Schools	18.2	20.7	15.6	12.9	29.5	34.8
% Elementary + Middle	4.4	35.4	43	41.9	20	23.9
% Elementary + High	1.5	10.9	10.5	16.1	12.4	8.7
% Middle + High	4.4	7.1	5.1	3.2	10.5	6.5
% Black	16.4	53.6	49.9	55.7	62.4	50.7
% Hispanic	7.0	8.6	9.3	8.1	6.4	4.7
% Asian	3.7	3.5	4.3	1.1	1.0	1.1
% White	71.7	33.4	35.9	34.4	27.9	39.2
% Female	48.8	49.9	50.0	50.0	49.8	50.6
% Poor	46.8	73.2	72.7	70.2	73.3	68.6
% Special Education	13.1	12.5	12.1	11.8	13.0	12.6
% LEP	5.9	9.0	10.9	8.1	3.3	2.3
Std. Math Score	0.04	-0.37	-0.32	-0.47	-0.47	-0.39
Std. Reading Score	0.03	-0.3	-0.25	-0.38	-0.38	-0.29
Neighborhood Poverty Rate	16.46	29.13	29.67	26.80	26.90	24.43
Neighborhood % BA+	24.49	18.11	17.97	20.54	19.29	20.02

Notes: Sample is taken from all schools in the MSDS student/year-level file for the 2014-15 academic year, merged to charter designations from the EEM/CCD school-level file and to state standardized assessment data. Elementary schools begin in kindergarten or 1st grade and end in grade 5 or 6; middle schools begin in grade 6 or 7 and end in grade 8 or 9. Statistics are weighted by the number of students in the school.

Table 2: Summary Statistics for NHA Schools

	K-8 Schools in 2011-2012				NHA Schools		
	Traditional Public Schools	Charter Schools	Non-Profit Charter Schools	For-Profit Charters, Excluding NHA	2011-2012	2003-2015	Weighted by Prevalence in Lottery Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Number of Schools	2,452	246	62	138	43	49	44
Number of Students	1,009,940	106,617	21,320	55,593	28,654	312,057	28,255
Number of Applications							20,986
School Characteristics							
Median Enrollment Per Grade	140.29	75.83	75.39	75.02	78.17	76.89	80.28
Pct. Urban Schools	17.01	50.71	64.13	51.74	39.48	36.66	23.26
Pct. Suburban Schools	47.08	36.71	17.08	37.40	48.75	53.07	45.52
Pct. Town/Rural Schools	35.92	12.58	18.79	10.86	11.77	10.26	31.23
Pct. Elementary Schools	60.69	18.73	26.21	21.72	7.04	6.11	7.68
Pct. Middle Schools	28.00	5.20	10.09	5.53	0.00	0.03	0.00
Pct. Elementary + Middle Schools	5.45	51.92	34.68	37.54	92.96	93.86	92.32
Student Characteristics							
Standardized Math Score	0.01	-0.30	-0.33	-0.42	-0.04	-0.04	0.32
Standardized Reading Score	0.00	-0.27	-0.28	-0.38	-0.05	-0.05	0.20
Pct. Black	17.08	54.83	62.93	56.34	45.55	41.83	29.07
Pct. Hispanic	6.88	7.24	4.91	7.58	8.50	9.17	8.36
Pct. Asian	3.29	2.94	0.98	1.70	6.87	5.77	15.86
Pct. White	71.40	34.06	28.48	33.90	38.53	42.58	46.09
Pct. Female	48.38	50.20	49.39	50.67	49.83	49.63	49.82
Pct. Economically Disadvantaged	51.04	71.10	70.11	76.64	60.89	54.96	41.23
Pct. Special Education	14.44	11.67	12.02	11.73	11.19	11.81	9.24
Pct. Limited English Proficient	5.17	6.80	1.97	9.80	4.82	5.07	5.24
Neighborhood Poverty Rate	15.98	29.24	29.34	31.88	24.65	24.47	15.80
Neighborhood Pct. with BA+	26.16	18.26	19.65	15.68	21.92	22.28	32.09

Table 2 (continued)

	K-8 Schools in 2011-2012				NHA Schools		
	Traditional Public Schools	Charter Schools	Non-Profit Charter Schools	For-Profit Charters, Excluding NHA	2011-2012	2003-2015	Weighted by Prevalence in Lottery Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Teacher Characteristics							
Pupil-Administrator Ratio	78.25	82.93	129.79	81.67	55.78	56.93	75.63
Pupil-Aide Ratio	360.13	200.48	259.92	196.44	169.02	204.16	167.55
Pupil-Teacher Ratio	18.52	17.38	16.86	16.99	18.50	17.66	19.51
Pct. Novice Teachers	1.07	9.28	8.73	10.44	7.65	9.12	7.15
Median Teacher Experience	10.65	4.29	4.45	4.10	4.50	3.79	4.41
Pct. Teachers New to School	11.71	23.17	22.51	25.50	19.47	25.50	20.25
Pct. Teachers from Competitive Colleges	4.86	4.26	5.09	4.24	3.59	3.80	5.08
Pct. Teachers with BSE Scores	57.92	88.50	87.12	88.72	89.24	85.99	88.76
Average BSE Score	-0.06	-0.22	-0.19	-0.29	-0.10	-0.08	0.00

Notes: Sample in columns (1) through (5) is taken from all schools in the MSDS student/year-level file for the 2011-12 academic year, merged to charter designations from the EEM/CCD school-level file and to state standardized assessment data. Sample in column (7) is all applicants to NHA lotteries between 2003 and 2012, observed in the 2011-2012 school year, with all sample restrictions detailed in Appendix Table 1. Elementary schools begin in kindergarten or 1st grade and end in grade 5 or 6; middle schools begin in grade 6 or 7 and end in grade 8 or 9. Statistics in columns (1) through (6) are weighted by the number of students enrolled in each school (except for the total number of schools, the total number of students, and the median enrollment per grade). Column (7) is weighted by the number of applications in the lottery analysis sample.

Table 3: Covariate Balance

	All Students				Students with Pre-Lottery Characteristics			
	Overall Mean (1)	Treat. (2)	Control (3)	Adj. Diff. (4)	Overall Mean (5)	Treat. (6)	Control (7)	Adj. Diff. (8)
Female	0.50	0.49	0.50	-0.01	0.49	0.47	0.49	-0.02
				(.008)				(.013)
Black	0.27	0.29	0.24	0.00	0.35	0.37	0.34	0.01
				(.006)				(.011)
Hispanic	0.07	0.07	0.08	0.00	0.08	0.07	0.09	-0.01
				(.004)				(.007)
Age	6.62	6.47	6.74	0.02	7.99	7.84	8.07	0.02
				(.007)				(.013)
Median Household Income, Thousands	64.63	61.90	66.70	0.48	63.65	62.71	64.24	-0.30
				(.415)				(.681)
Enrolled MI Public Schools Pre-Lottery	0.41	0.37	0.44	0.00	1.00	1.00	1.00	0.01
				(.006)				(.008)
Prior Math Score					-0.02	0.00	-0.04	-0.03
								(.051)
Prior Reading Score					-0.05	-0.04	-0.05	0.01
								(.047)
Subsidized Lunch Eligible					0.39	0.40	0.39	0.01
								(.011)
Special Education					0.15	0.16	0.14	0.02
								(.009)
Limited English Proficiency					0.10	0.09	0.11	0.00
								(.007)
Number of Students	21,661	9,173	12,488	21,661	8,836	3,388	5,448	8,836
P-value for Joint Test				0.20				0.41

Notes: This table reports means and treatment-control differences in pre-lottery characteristics for students who applied to a NHA lottery between 2003 and 2012, subject to all restrictions in Appendix Table 1. Columns (1) through (4) report on pre-lottery characteristics available for all students, while the remaining columns report on characteristics for students who were enrolled in MI public schools prior to the lottery. The sample includes one observation per student, lottery grade, and lottery year grouping. Columns (4) and (8) report the coefficients on receiving an initial offer in a regression controlling for risk set fixed effects, with standard errors clustered by the risk set reported in parentheses. "Median Household Income, Thousands" is in 2015 U.S. dollars, defined at the block-group level, and taken from the 2010 Census.

Table 4: First Stage Estimates of the Impact of Winning a Charter School Lottery

	Dependent variable = Enrolled in Fall after Lottery in			
	NHA School, In Risk Set	NHA School, Not In Risk Set	Non-NHA Charter School	Traditional Public School
	(1)	(2)	(3)	(4)
Initial Offer	0.19***	-0.016***	-0.005***	-0.16***
	(0.019)	(0.004)	(0.001)	(0.018)
Control Mean	0.339	0.074	0.009	0.546
F-Statistic	97.75	14.29	11.66	78.71
Number of students	21,593	21,593	21,593	21,593

Notes: This table reports estimates of the effect of receiving an initial offer on the day of the lottery on whether the student enrolled in various types of schools during the following fall. The sample includes all students in NHA risk sets between 2003 and 2012, subject to the restrictions in Appendix Table 1. There is one observation per student, risk-set, and lottery year grouping. Standard errors are reported in parentheses, clustered by the risk set. All models control for demographic variables including the full interaction of female, ethnicity, and poverty status, as well as census block-group characteristics, missing indicators for covariates, and pre-lottery test scores, special education status, limited English proficiency status, and poverty status if available.

Table 5: Lottery Results

	First Stage (1)	Control Mean (2)	Reduced Form (3)	2SLS (4)	CCM (5)
Math Standardized Score	0.745***	[0.407]	0.029*	0.040*	{0.324}
	(0.102)		(0.016)	(0.023)	
Obs.	80,828		80,832	80,828	
Proficient in Math	0.745***	[0.458]	0.010*	0.014*	{0.468}
	(0.102)		(0.005)	(0.008)	
Obs.	80,828		80,832	80,828	
ELA Standardized Score	0.744***	[0.335]	0.019	0.026	{0.282}
	(0.102)		(0.015)	(0.020)	
Obs.	80,805		80,809	80,805	
Proficient in ELA	0.744***	[0.561]	0.006	0.008	{0.570}
	(0.102)		(0.005)	(0.007)	
Obs.	80,805		80,809	80,805	

Notes: This table reports reduced form and 2SLS estimates of the effect of each additional year spent in an NHA charter school. The instrument is an indicator for receiving an initial offer on the day of the lottery for at least one school in a student's risk-set. The sample includes all students in NHA risk sets between 2003 and 2012, subject to the restrictions in Appendix Table 1. There is one observation per student, risk-set, year grouping. The full panel includes all observations through a student's expected 8th-grade year. Column (1) reports the effect of an initial offer on the cumulative years spent in any risk-set school (the first stage). All standard errors are reported in parentheses, clustered by the risk set. Column (5) reports control complier means in curly braces. All models control for demographic variables including the full interaction of female, ethnicity, and poverty status, as well as census block-group characteristics, tested grade fixed effects, missing indicators for covariates, and pre-lottery test scores, special education status, limited English proficiency status, and poverty status if available.

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 6: Differences in Lottery Effect on Last Observed Math Score, by Subgroup

	All (1)	Male (2)	Female (3)	Poor (4)	Non-Poor (5)	Black-Hispanic (6)	White-Asian (7)	Urban (8)	Non-Urban (9)	Non-Urban/ Poor (10)	Non-Urban/ Non-Poor (11)
Reduced Form	0.029*	0.008	0.042*	-0.012	0.045**	0.015	0.034*	0.036	0.028	-0.031	0.046**
	(0.016)	(0.025)	(0.022)	(0.025)	(0.020)	(0.025)	(0.020)	(0.031)	(0.019)	(0.032)	(0.022)
First Stage	0.745***	0.820***	0.722***	0.396***	0.942***	0.622***	0.851***	0.259**	0.894***	0.421***	1.093***
	(0.102)	(0.125)	(0.108)	(0.088)	(0.132)	(0.092)	(0.133)	(0.116)	(0.123)	(0.110)	(0.148)
2SLS	0.040*	0.010	0.059*	-0.030	0.048**	0.025	0.041	0.140	0.031	-0.073	0.042*
	(0.023)	(0.030)	(0.031)	(0.063)	(0.024)	(0.040)	(0.025)	(0.129)	(0.022)	(0.080)	(0.022)
CCM	{0.324}	{0.381}	{0.278}	{-0.085}	{0.621}	{-0.186}	{0.616}	{-0.113}	{0.459}	{0.042}	{0.687}
Obs.	80,828	40,301	40,501	28,950	51,814	27,245	52,947	20,169	60,659	17,576	43,045

Notes: This table reports reduced form and 2SLS estimates of the effect of each additional year spent in an NHA charter school, estimated separately by student subgroups. The instrument is an indicator for receiving an initial offer on the day of the lottery for at least one school in a student's risk-set. The sample includes all students in NHA risk sets between 2003 and 2012, subject to the restrictions in Appendix Table 1. There is one observation per student, risk-set, tested year grouping. The full panel includes all observations through a student's expected 8th-grade year observed through 2017. Column (1) reports the effect of an initial offer on the cumulative years spent in any risk-set school (the first stage). All standard errors are reported in parentheses, clustered by the risk set. Column (5) reports control complier means in curly braces. All models control for demographic variables including the full interaction of female, ethnicity, and poverty status, as well as census block-group characteristics, tested grade fixed effects, missing indicators for covariates, and pre-lottery test scores, special education status, limited English proficiency status, and poverty status if available.

* p<0.10, ** p<0.05, *** p<0.01

Table 7: Lottery Effects on School Inputs

	Control Mean (1)	Reduced Form (2)	Obs. (3)	2SLS (4)	CCM (5)
Grade Size	146.7	-4.62***	150,791	-7.79***	134.1
		(1.17)		(1.67)	
Number of Students in School	708.4	12.17***	150,791	20.52***	712.2
		(3.82)		(6.08)	
Fraction of Black Students	0.241	-.006**	150,791	-0.009***	0.292
		(0.002)		(0.004)	
Fraction of Hispanic Students	0.082	0.001	150,791	0.001	0.086
		(0.001)		(0.002)	
Fraction of Special Education Students	0.118	-0.004***	150,791	-0.007***	0.118
		(0.001)		(0.001)	
Fraction of Economically Disadvantaged Students	0.397	-0.012***	150,791	-0.021***	0.455
		(0.003)		(0.004)	
School Is a K-8 School	0.366	0.079***	150,791	0.133***	0.482
		(0.012)		(0.009)	
School Is in a City	0.242	-0.001	150,243	-0.002	0.276
		(0.005)		(0.009)	
School Is in a Suburb	0.571	-0.048***	150,243	-0.080***	0.591
		(0.012)		(0.017)	
Students' Average Standardized Math Score	0.205	0.042***	131,547	0.078***	0.101
		(0.008)		(0.010)	
Fraction of Students New to School	0.123	0.001	150,791	0.002	0.132
		(0.002)		(0.003)	
Pupil/Teacher Ratio	19.492	0.178	150,403	0.299	19.277
		(0.148)		(0.245)	
Pupil/Administrator Ratio	82.193	2.089**	146,973	3.51**	76.645
		(0.991)		(1.59)	
Pupil/Aide Ratio	301.280	-22.21***	147,507	-37.39***	280.1
		(3.41)		(3.20)	
Fraction of Novice Teachers	0.195	0.028***	150,403	0.046***	0.238
		(0.004)		(0.005)	
Median Teacher Experience	7.365	-0.589***	150,403	-0.989***	6.417
		(0.084)		(0.083)	
Fraction of Teachers New to School	0.168	0.009***	150,403	0.015***	0.184
		(0.002)		(0.004)	
Fraction of Teachers from Competitive Colleges	0.017	0.001	150,403	0.002**	0.017
		(0.001)		(0.001)	

Table 7 (continued)

	Control Mean	Reduced Form	Obs.	2SLS	CCM
	(1)	(2)	(3)	(4)	(5)
Fraction of Teachers with BSE Scores	0.719	0.028***	150,320	0.048***	0.764
		(0.004)		(0.003)	
Teachers' Average BSE Score	0.014	0.006**	150,235	0.009*	-0.018
		(0.003)		(0.005)	

Notes: The sample includes all students who entered kindergarten-8th grade lotteries, subject to the restrictions in Appendix Table 1. There is one observation per student, risk-set, tested year grouping. The full panel includes all observations through a student's expected 8th-grade year observed through 2016. All models control for demographic variables including the full interaction of female, ethnicity, and poverty status, as well as census block-group characteristics, tested grade fixed effects, missing indicators for covariates, and pre-lottery test scores, special education status, limited English proficiency status, and poverty status if available. Coefficients on any initial offer (Reduced Form), followed by standard errors clustered by risk set in parentheses. Control Mean is mean of outcome variable among students with no initial offers; CCM stands for Control Complier Mean. The endogenous variable in the 2SLS regressions is the cumulative years spent at a school in the student's risk-set. The instrument is whether a student received any initial offer to a school in her risk-set.

Table 8: Differences in Survey Responses by School Type

Domain	Measure	Control Mean (1)	Other Charter (2)	NHA Charter (3)
School Culture	No Excuses culture (std)	-0.206	0.168	1.09***
		(0.941)	(0.112)	(0.176)
	School discipline (std)	-0.285	0.596***	1.28***
		(0.953)	(0.122)	(0.166)
	Parent Engagement (std)	-0.255	0.447***	0.745***
		(0.804)	(0.117)	(0.186)
Time	Instructional hours per year	1,133	71.7***	-3.67
		(122)	(21.3)	(31.6)
	Instructional hours per week (reading)	1.42	0.100	0.136
		(0.437)	(0.280)	(0.129)
	Instructional hours per week (math)	1.19	0.023	0.231***
		(0.347)	(0.067)	(0.066)
Instruction	Test frequency (std)	-0.183	0.358**	0.685**
		(0.864)	(0.112)	(0.230)
	Instructional uniformity	0.621	-0.017	-0.137
		(0.456)	(0.078)	(0.122)
	Hours/year principal observes teachers	2.09	0.565	7.10***
		(1.45)	(0.332)	(1.59)
	Ability grouping in math	0.488	-0.056	0.475***
		(0.449)	(0.068)	(0.114)
	Ability grouping in reading	0.474	-0.135*	0.315*
		(0.458)	(0.067)	(0.123)
	Offers tutoring	0.819	-0.040	0.137
		(0.349)	(0.050)	(0.090)
	Mandatory tutoring	0.326	0.115	0.160
		(0.445)	(0.103)	(0.126)
	Hours/month tutoring	9.18	-3.47***	-1.49
		(8.40)	(1.06)	(1.83)

Table 8 (continued)

Domain	Measure	Control Mean (1)	Other Charter (2)	NHA Charter (3)
School Organization and Leadership	Principal tenure	9.38	-1.33*	-2.10
		(4.42)	(0.521)	(1.29)
	Administrators are mentors	0.294	0.092	0.474***
		(0.428)	(0.071)	(0.093)
	Bad teachers are a problem (std)	0.424	-1.12***	-0.546**
		(0.968)	(0.125)	(0.186)
	Hiring control difference (std)	-0.390	0.974***	0.633**
		(0.880)	(0.122)	(0.206)
	Curriculum control difference (std)	-0.361	0.992***	-0.364
		(0.764)	(0.123)	(0.190)
	School autonomy (std)	-0.366	0.970***	0.167
		(0.650)	(0.095)	(0.134)
Compensation	Average Salary of New Teacher	37,363	-3,938***	-1,469
		(5,899)	(756.2)	(1,026)
	Teachers are eligible for performance-based bonuses	0.203	0.342***	0.868***
		(0.376)	(0.061)	(0.062)
	Average amount of performance bonus	971.8	537	269.94
		(2,125)	(2,381)	(562.4)

Notes: This table reports coefficients from separate, school-level regressions of each survey measure on an indicator for whether the school is an NHA charter, an indicator for whether the school is a non-NHA charter, comparison group fixed effects, and a constant. Comparison groups are determined by geographic proximity and detailed in an online appendix at http://edpolicy.umich.edu/publications/#policy-briefs. Standard errors are robust to heteroskedasticity.

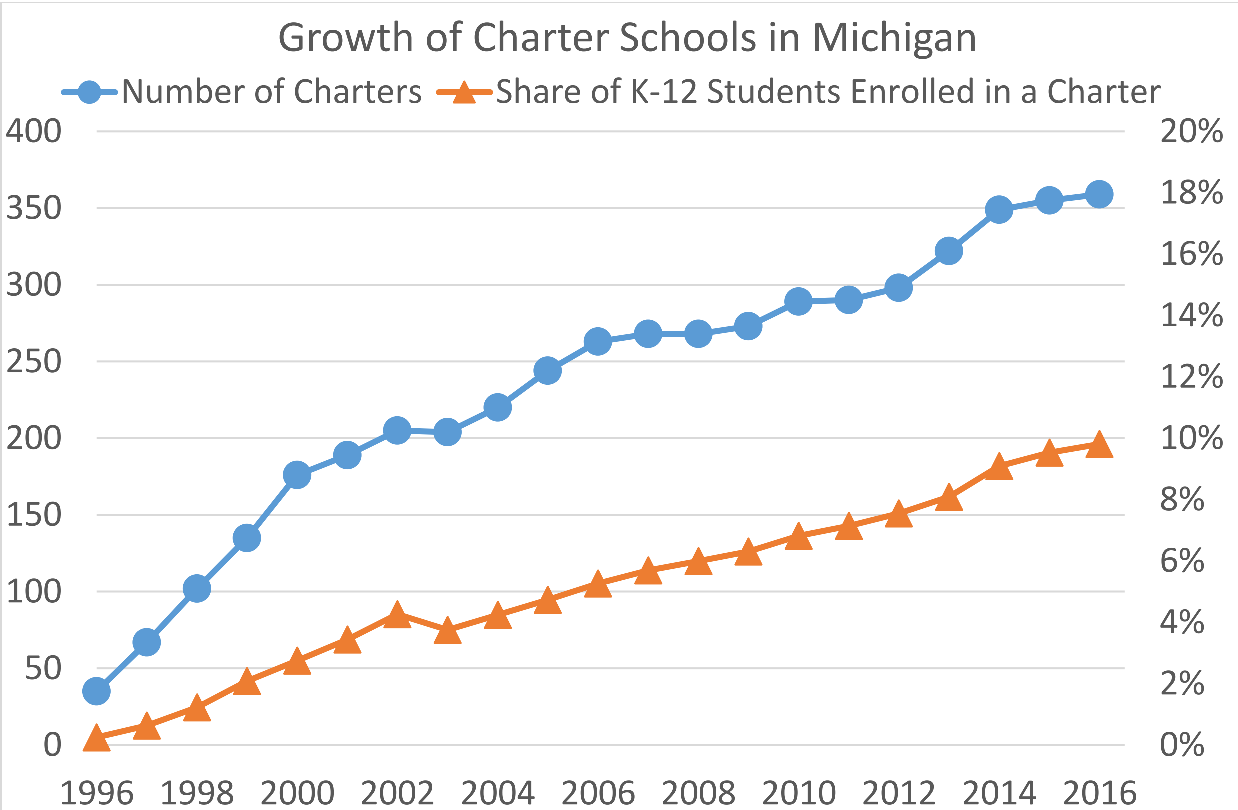


Figure 1: The number of charters and the share of K-12 students enrolled in a charter in Michigan has increased by a factor of ten between 1996 and 2016. The figure has a series break between 2002 and 2003. Data on enrollment from 1996 to 2002 comes from the National Center for Education Statistics Common Core of Data (CCD), and data after 2002 comes from the Michigan Department of Education, student-level administrative records.

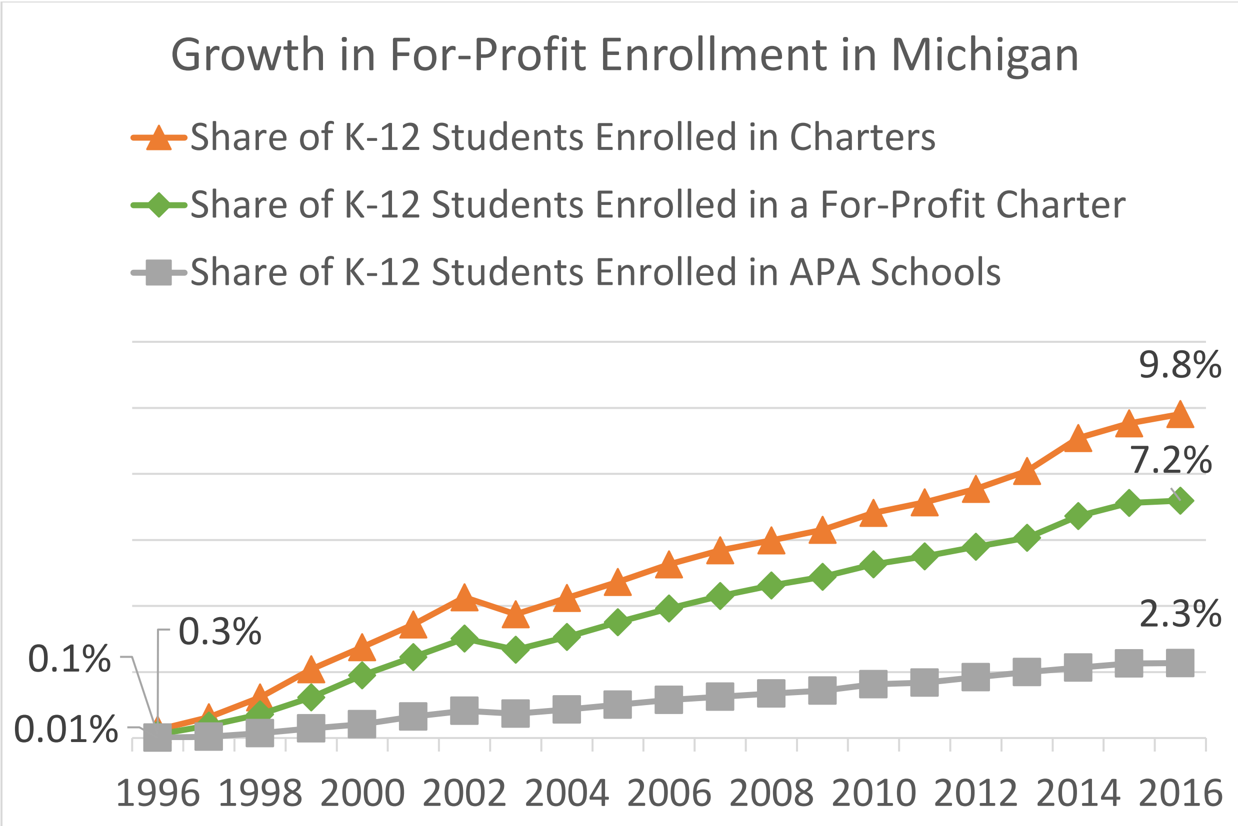


Figure 2: Almost three-quarters of the growth in charter school enrollment between 1996 and 2016 in Michigan has come from enrollment in for-profit charter schools. NHA enrollment has increased as well, and represents about a fifth of charter enrollment in 2016. The figure has a series break between 2002 and 2003. Data on enrollment from 1996 to 2002 comes from the National Center for Education Statistics Common Core of Data (CCD), and data after 2002 comes from the Michigan Department of Education, student-level administrative records.

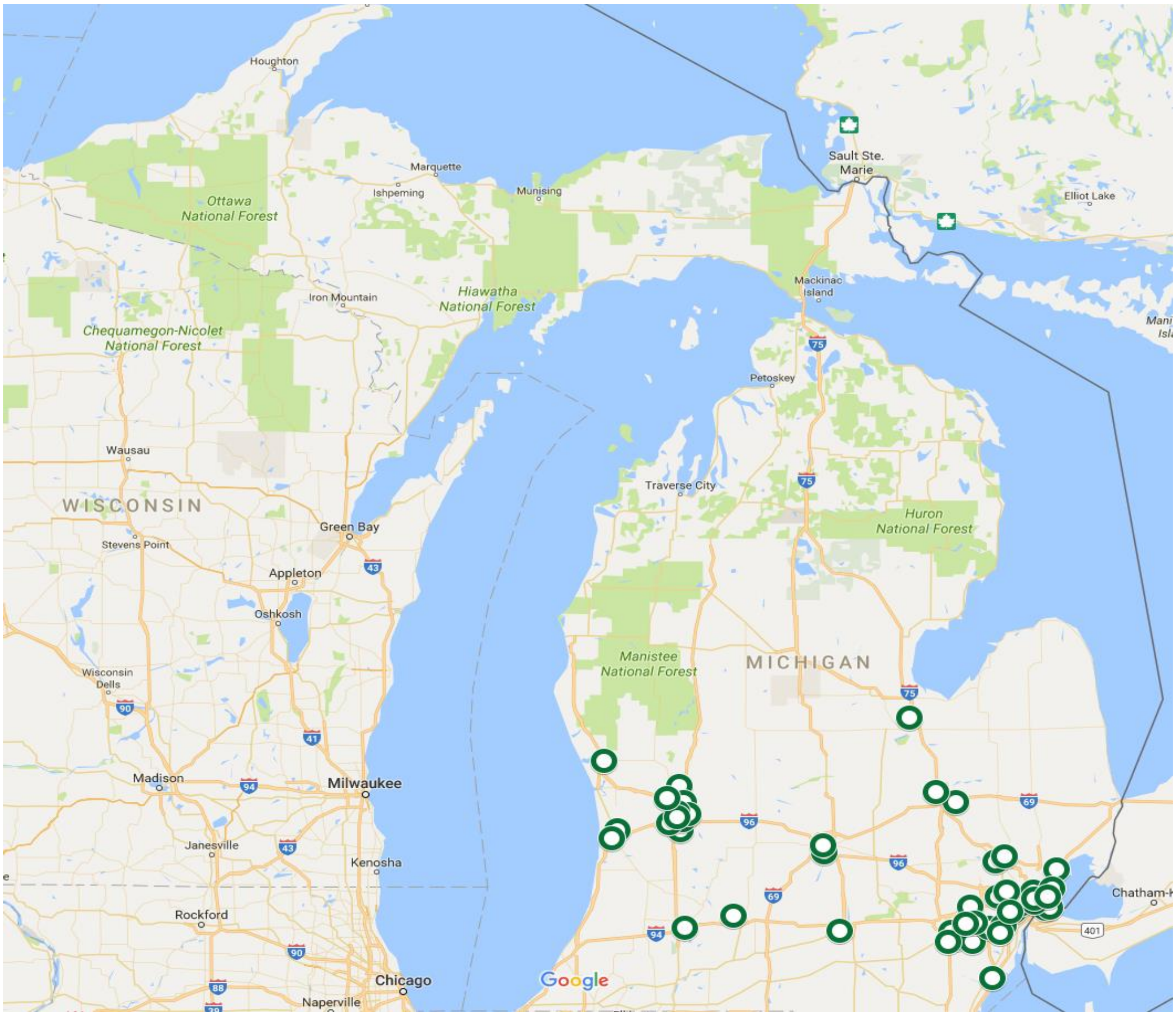


Figure 3: Location of NHA charters in Michigan as of 2017.

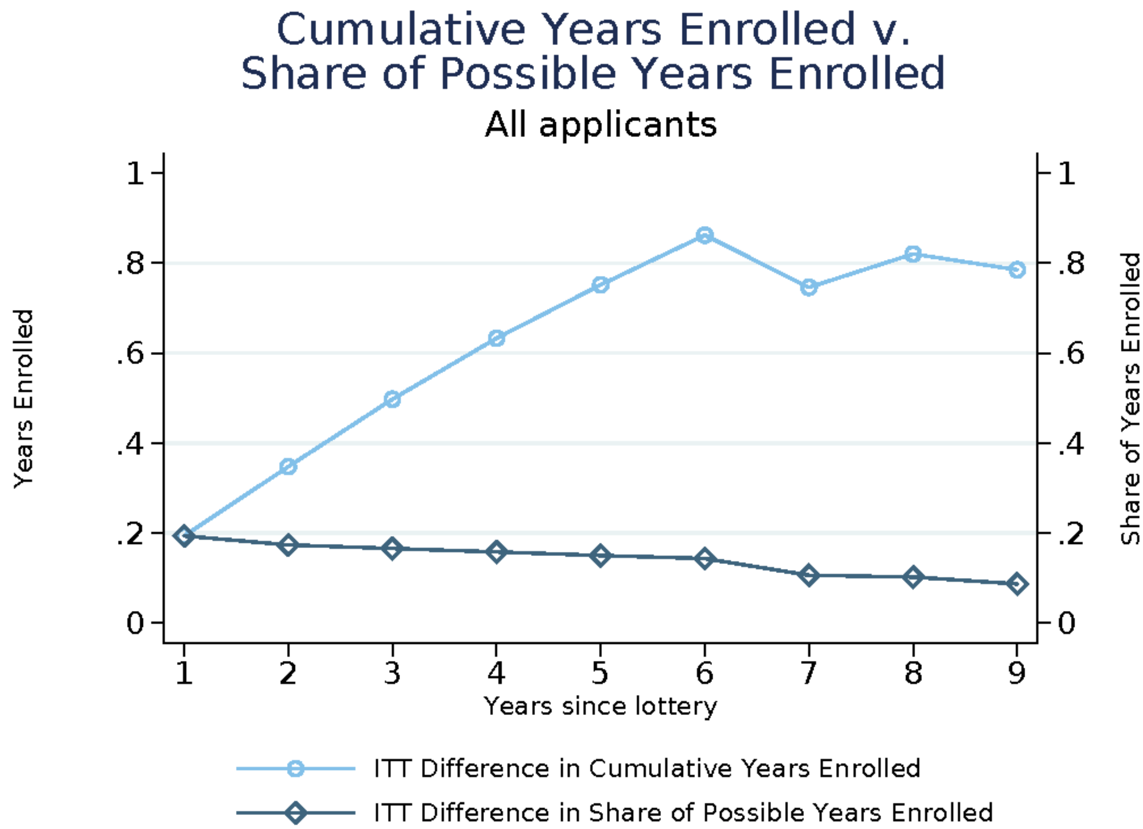


Figure 4a: This figure plots the OLS coefficient on receiving an initial lottery offer for two regressions. The top line is the reduced form effect of winning a lottery on the cumulative years a student spent in any school within her risk-set, estimated separately by years since the lottery. The bottom line is the reduced form effect of winning a lottery on the share of years that a student enrolls in any school in her risk-set, out of all possible years a student could have been enrolled. Again, this is estimated separately by years since the lottery for all lottery applicants, subject to the sample restrictions in Appendix Table 1. See Table 5 notes for details of the regression model.

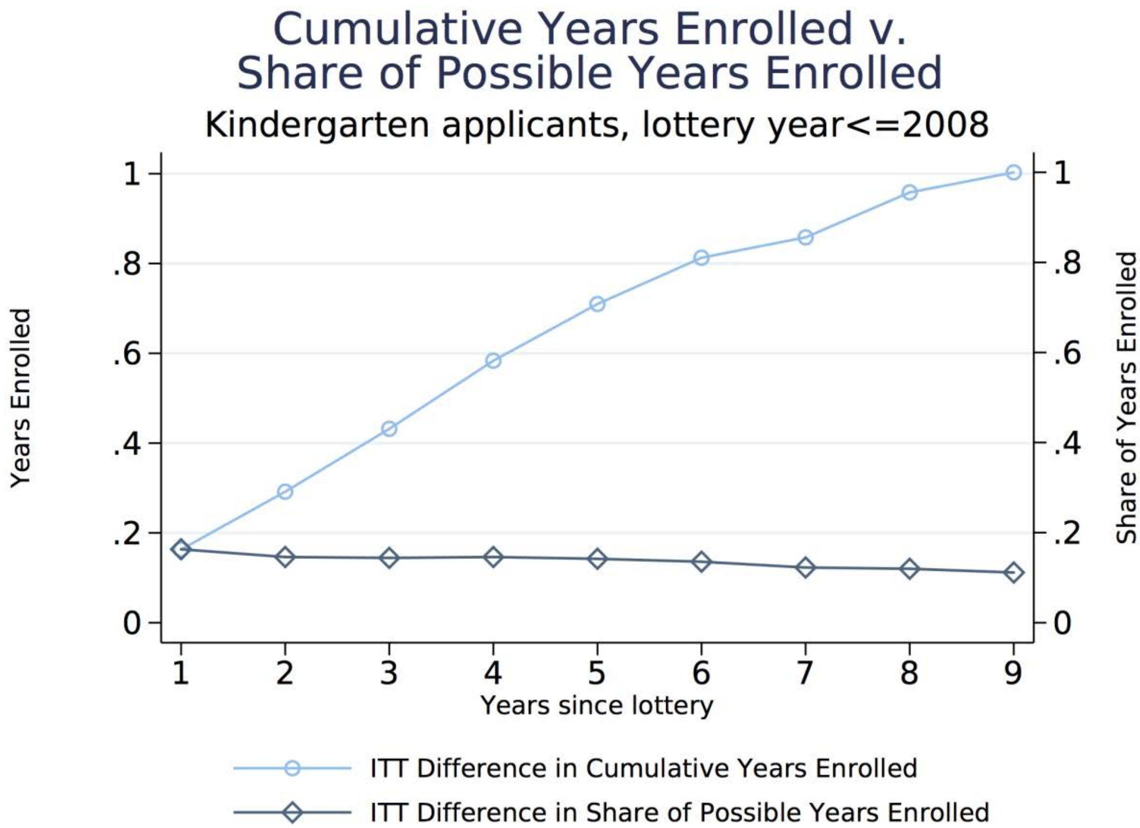
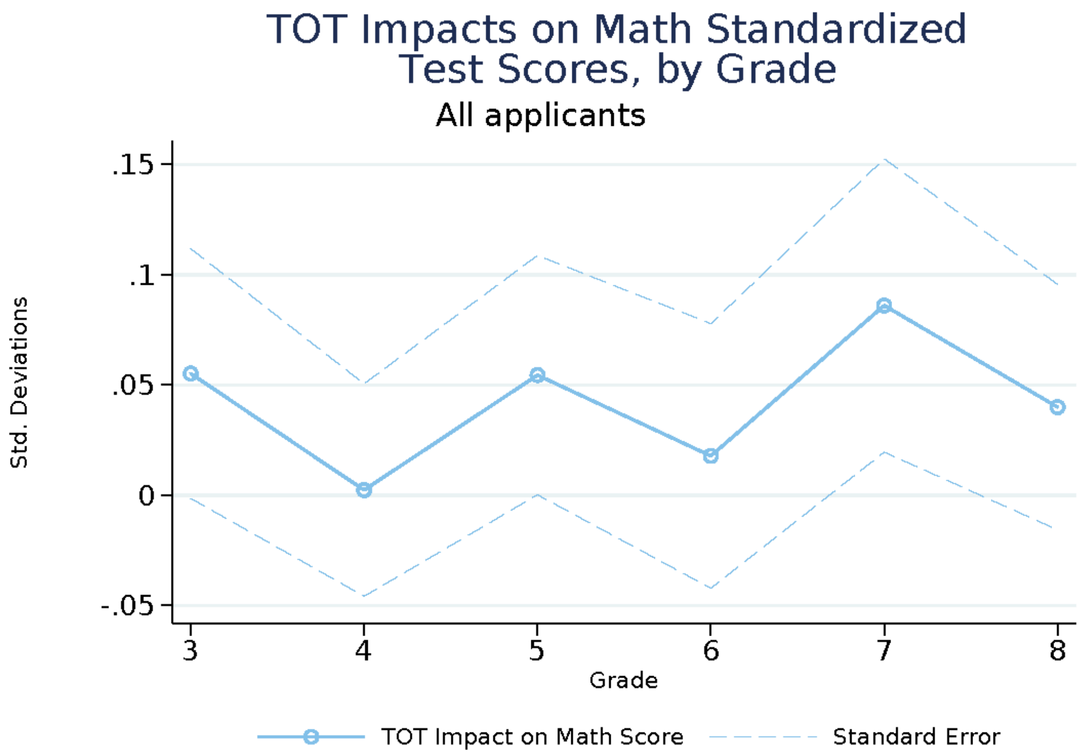
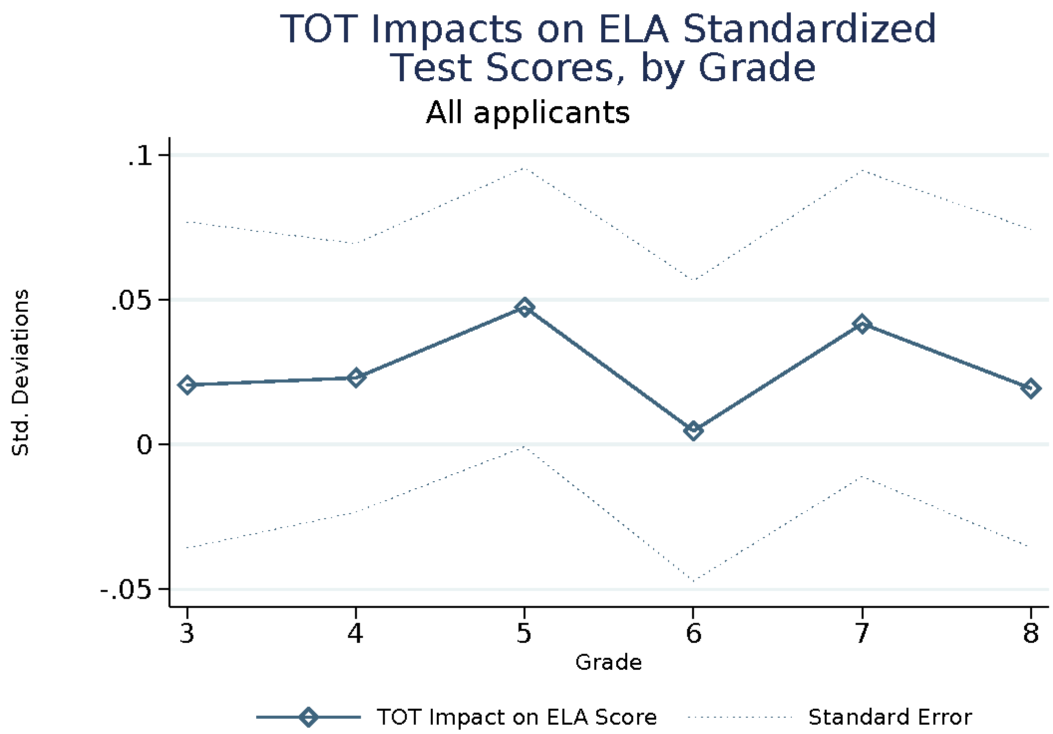


Figure 4b: This figure plots the OLS coefficient on receiving an initial lottery offer for two regressions. The top line is the reduced form effect of winning a lottery on the cumulative years a student spent in any school within her risk-set, estimated separately by years since the lottery. The bottom line is the reduced form effect of winning a lottery on the share of years that a student enrolls in any school in her risk-set, out of all possible years a student could have been enrolled. Again, this is estimated separately by years since the lottery, for all lottery applicants between 2003 and 2008, subject to the restrictions in Appendix Table 1. See Table 5 notes for details of the regression model.

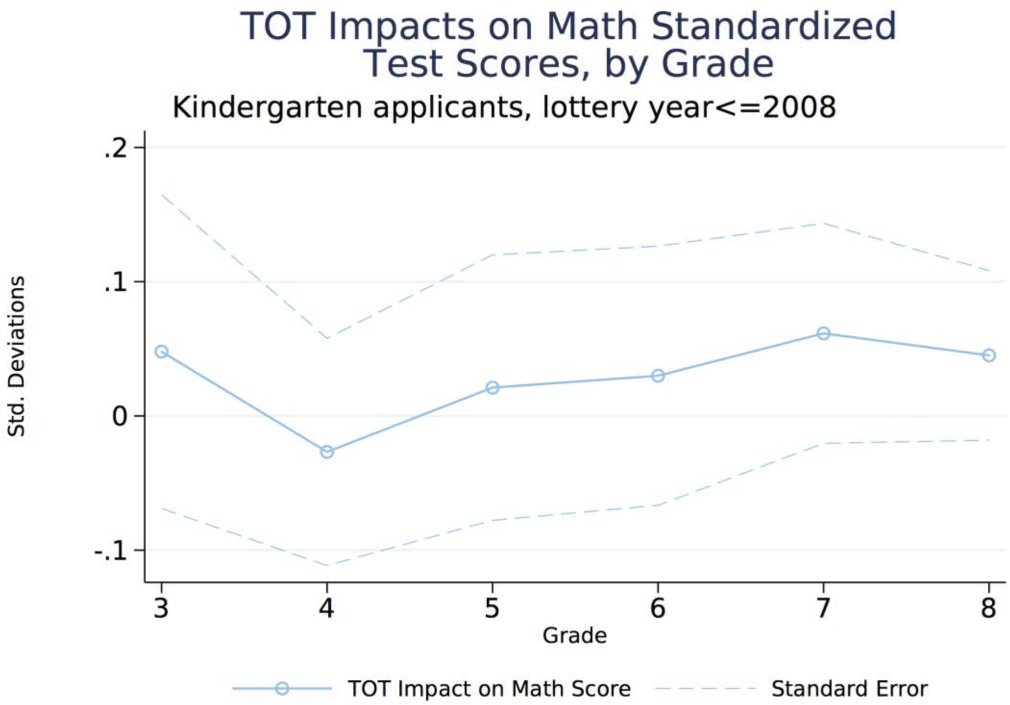

TOT Impacts on Math Standardized
Test Scores, by Grade
All applicants
Std. Deviations
.15
.1
.05
0
-.05
3
4
5
6
7
8
Grade
TOT Impact on Math Score
Standard Error

(a)



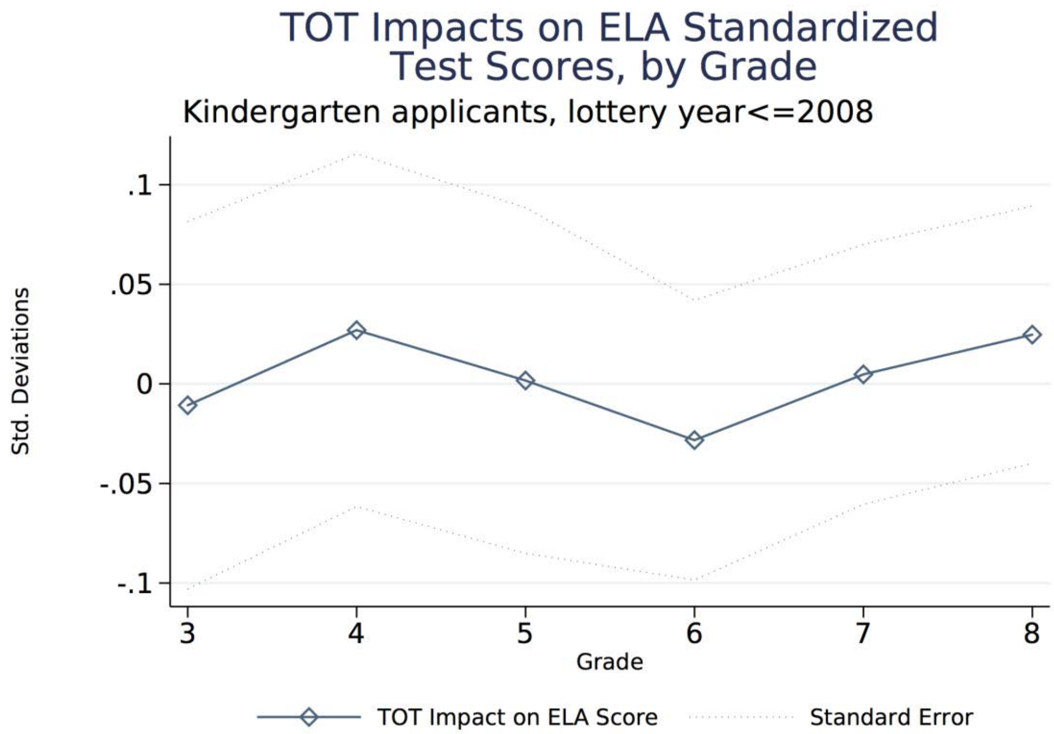
(b)

Figure 5: This figure plots the 2SLS coefficient from a regression of test scores on the cumulative years spent at an NHA charter, estimated separately by grade for all lottery applicants, subject to the sample restrictions in Appendix Table 1. See Table 5 notes for details of the regression model.



TOT Impacts on Math Standardized Test Scores, by Grade
Kindergarten applicants, lottery year<=2008
Std. Deviations
.2
.1
0
-.1
3
4
5
6
7
8
Grade
TOT Impact on Math Score
Standard Error

(a)



(b)

Figure 6: This figure plots the 2SLS coefficient from a regression of test scores on the cumulative years spent at an NHA charter, estimated separately by grade. The sample is all lottery applicants between 2003 and 2008. All regressions use the same model as reported in Table 5.

Appendix Tables and Figures

Appendix Table 1: Sample Restrictions

Sample	Restriction	Number of Applications	Number of Schools	Number of Lotteries	Number of Students
1	All NHA applications	79,459	46	2,907	50,854
2	Remove applications that predate the SRSD	78,892	46	2,881	50,316
3	Remove applicants with enrolled-sibling preference	69,373	46	2,869	42,175
4	Remove applications from students who have previously won a charter lottery	63,300	46	2,855	42,175
5	Remove applications from students who never match to SRSD	58,807	46	2,844	39,113
6	Remove applications without later SRSD matches	57,820	46	2,837	38,518
7	Remove applications from students who have previously attended their target school	56,827	46	2,809	37,671
8	Remove applications from degenerate lotteries on InitialOffer	31,300	44	749	21,576
9	Remove applications in risk sets that are degenerate on InitialOffer	27,143	44	735	20,255

Notes: This table lists all sample restrictions for the main analysis sample, as well as the number of applications, schools, lotteries, and students that remain after the restriction.

Appendix Table 2: Effect of Lottery Offer on Enrollment in Michigan Public Schools After Lottery

	Lottery Fixed Effects Specification		
	Treatment Mean	Control Mean	Adjusted Difference
All applicants	0.932	0.905	-0.001
	[20,238]	[43,062]	(0.004)
Applicants to Pre-K and K Lotteries	0.935	0.911	0.004
	[12,140]	[15,525]	(0.005)
Applicants to Grade 1-8 Lotteries	0.929	0.901	-0.011
	[8,098]	[27,537]	(0.008)

Notes: Regressions contain one observation per lottery application. Adjusted differences are coefficients from a regression of an indicator for whether the applicant enrolls in a Michigan public school regressed on an indicator for receiving an initial offer at any point after the lottery, controlling for lottery fixed effects. Standard errors in parentheses, clustered by lottery; treatment and control sample sizes in brackets in their respective columns. Sample subject to all restrictions detailed in Appendix Table 1.

Appendix Table 3 - Description of Survey Composite Measures

Domain	Measure	Description	Variable
Assessment	Test frequency	This is a scale ranging from 0 to 4 that measures the frequency of both internal and external type assessments. It was created by taking the mean value of Q41a (internally developed assessment), Q41b (externally purchased assessment), Q41c (norm-referenced assessment), Q41d (criterion reference assessment), with the values of 0 = Never; 1 = per semester; 2 = quarterly; 3 = monthly; and 4 = weekly.	testfreq
Compensation	Average Salary of New Teacher	The average salary at each school for a first year teacher without a master's degree.	salary
Compensation	Teachers are eligible for performance-based bonuses	Binary indicator for whether teachers are eligible for a performance-based bonus	bonus
Compensation	Average amount of performance bonus	Performance-based bonus pay. This is set to 0 (in regressions only) if respondent's answered no that teachers are not eligible for a performance-based bonus.	bonus_amount
Instruction	Instructional uniformity	if classroom instruction is standardized or customized by teacher. This was created from questions 11 (are teachers expected to adhere to scripted lessons) and 12 (does your school provide common templates). If either one was selected as yes, then the composite is set to 1. If both are not selected, then the composite is set to 0.	inst_unif
Instruction	Hours/year principal observes teachers	The average of questions 64a (how many minutes per year do principals observe new teachers?) and 64b (how many minutes per year do principals observe experienced teachers?).	principal_observe
Instruction	Ability grouping in math	Students are assigned to math classes based on ability.	trackm
Instruction	Ability grouping in reading	Students are assigned to reading classes based on ability.	trackr
Instruction	Offers tutoring	The school offers tutoring outside classroom hours.	hastutor
Instruction	Mandatory tutoring	Students are assigned to mandatory tutoring based on low performance.	mantutor
Instruction	Hours/month tutoring	The variable indicates hours of tutoring per month, which is the product of Q36 (how often tutoring sessions are) and Q37 (how long tutoring sessions are). The coding for question 36 was converted from daily, weekly, or monthly to numeric values for days/week (assuming a 5 day week): daily = 20; weekly = 4; monthly =1; and "other" that indicated 2-4 times/week = 12. Question 37 was coded with the midpoint for each range so that 1-30 minutes = 15; 31-60 = 45; 61-90 = 75; and over 90 minutes = 90.	tutorfreq

School Culture	No Excuses culture	A composite that ranges from 0-10. It is the sum of the 10 binary variables for the no excuses questions: 1) school-wide policies enforced the same way; 2) new student orientation to learn school policies; 3) teacher dismisses class, not bell; 4) teacher addresses student problems immediately; 5) student does other work if task completed early; 6) students sit up & track teacher with eyes; 7) only necessary items on student desk; 8) silence in hallways during transition time; 9) students silently working on activity at start of class; and 10) all backpacks consistently stored in one place. Standardized into z-score form for regressions in columns M and N.	noexcuse
School Culture	School discipline	A scale ranging from 0 to 3. This is the sum of three binary variables for school behavioral questions: 1) students required to wear school uniforms; 2) students required to sign behavior contract; and 3) parents required to sign contract. Standardized into z-score form for regressions in columns M and N.	schdiscip
School Culture	Parent Engagement	The average fraction of parents involved across the following 5 school activities: 1) open house; 2) parent-teacher conference; 3) volunteer at school; 4) PTA; and 5) homework notification service. Each of the five questions were coded so that the answer choices corresponded to few = .1; less than half = .25; half = .5; more than half = .75; and almost all = .9. Standardized into z-score form for regressions in columns M and N.	pareng
Organization & Leadership	Principal tenure	The sum of years the principal has served as an administrator, both at the current school and prior.	prinyears
Organization & Leadership	Administrators are mentors	An indicator for whether school administrators are teachers' typical mentors.	mentors
School Organization & Leadership	Bad teachers are a problem	On a 0-3 scale, how much does firing bad teachers prevent the school from improving? Standardized into z-score form for regressions in columns M and N.	badtch

School Organization & Leadership	Curriculum control difference	This variable is created from two variables, int_cur and ext_cur, which are each composed of questions asking about curriculum decisions based on the balance of power between internal actors (principals, teachers, and non-instructional staff) and external actors (management company, authorizer, district/central office, superintendent, school board, and consultants). Int_cur int_cur is based on questions 13a, 13b, and 13g (how much influence do principals, teachers, and staff have on curriculum development?); ext_cur is based on questions 13c, 13d, 13e, and 13g (how much influence do management companies, authorizers, school district, school board, and other outside groups have on curriculum development?). 13g is a free response question in which respondents can list the groups that are relevant; staff, school board, and other outside groups are common responses. For each of these two variables, a school is coded as 0 for no influence, 1 for minor influence, 2 for moderate influence, or 3 for high influence for the respective group. The difference is then taken between int_curr and ext_curr and the variable is standardized.	curr_ctrl
School Organization & Leadership	Hiring control difference	This variable is created from two variables, int_hire and ext_hire, which are each composed of questions asking about hiring decisions based on the balance of power between internal actors (principals, teachers, and non-instructional staff) and external actors (management company, authorizer, district/central office, superintendent, school board, and consultants). int_hire and ext_hire are based on questions 52a and 52b (who performs initial screening of teachers, and who makes final hiring decisions?); int_hire includes principals only, while ext_hire includes district offices, superintendents, and school boards for traditional public schools and management companies and school boards for charters. For each of these two variables, a school is coded as 9 for having no part of either initial or final teacher hiring, 1 for part of either initial or final teacher hiring, or 2 for having part of both initial and final teacher hiring for the respective group. The difference is then taken between int_hire and ext_hire and the variable is standardized.	hire_ctrl
School Organization & Leadership	School autonomy	The school autonomy variable is the mean of the curriculum and hiring control difference variables.	sch_aut
Time	Instructional hours per year	The number of instructional hours per year; the product of question 26 (number of instructional days) and 28-30 (number of instructional hours per day for grade 3 or grade 8).	hrsyr

Time	Instructional hours per week (reading)	From question 28-30 for ELA: the number of minutes per week spent on English language arts. Multiplied by 5 if less than 120; set to missing if greater than 600, and divided by 60 to convert to hours.	ela_hrs
Time	Instructional hours per week (math)	From question 28-30 for math: the number of minutes per week spent on math. Multiplied by 5 if less than 120; set to missing if greater than 600, and divided by 60 to convert to hours.	math_hrs

Notes: This table describes the construction of all the survey measures tested in Table 8. Each measure is created from a survey of Michigan public schools. See online appendix at http://edpolicy.umich.edu/publications/#policy-briefs for further description of the survey, and Appendix Table 4 for descriptive statistics.

Appendix Table 4 - Summary Statistics for School Practice Measures

		All schools					TPS Comparison Schools	All Charters	All non-profit	All for-profit	NHA
	Measure	Obs.	Min.	Max.	Mean	SD	Mean				
Compensation	Average Salary of New Teacher	362	17,500	53,000	35,728	4,501	36,420	34,785	33,947	34,998	35,299
Compensation	Teachers are eligible for performance-based bonuses	379	0	1	0.387	0.488	0.15	0.743	0.441	0.817	1
Compensation	Average amount of performance bonus	143	50	10,000	1,483	1,416	893	1,634	2,251	1,561	1,537
Instruction	Test frequency	397	0	4	1.62	0.694	1.51	1.78	1.76	1.79	1.92
Instruction	Instructional uniformity	372	0	1	0.64	0.481	0.627	0.657	0.651	0.655	0.602
Instruction	Hours/year principal observes teachers	363	0.15	45	2.97	3.94	2.05	4.39	3.24	4.69	7.34
Instruction	Ability grouping in math	400	0	1	0.488	0.50	0.452	0.544	0.372	0.588	0.799
Instruction	Ability grouping in reading	375	0	1	0.440	0.497	0.452	0.42	0.301	0.445	0.564
Instruction	Offers tutoring	392	0	1	0.825	0.381	0.787	0.881	0.91	0.873	0.893
Instruction	Mandatory tutoring	306	0	1	0.329	0.329	0.269	0.411	0.541	0.379	0.293
Instruction	Hours/month tutoring	370	0.5	30	7.90	7.92	8.26	7.38	8.10	7.27	6.59
School Culture	No Excuses culture	411	0	10	4.64	1.96	4.3	5.17	4.32	5.39	6.17
School Culture	School discipline	391	0	3	2	2.00	1.61	2.56	2.32	2.62	2.79
School Culture	Parent Engagement	388	0.1	0.9	0.486	0.142	0.465	0.517	0.537	0.513	0.574
Organization & Leadership	Principal tenure	410	1	20	9.2	4.66	10	7.89	8.11	7.81	7.75
Organization & Leadership	Administrators are mentors	384	0	1	0.332	0.472	0.256	0.443	0.239	0.498	0.506
Organization & Leadership	Bad teachers are a problem	386	0	3	0.995	1.05	1.37	0.439	0.172	0.512	0.664

Organization & Leadership	Curriculum control difference	397	-3	3	0.031	1.40	-0.338	0.601	1.62	0.330	0.452
Organization & Leadership	Hiring control difference	391	-2	2	0.241	1.25	-0.218	0.898	1.29	0.792	0.567
Organization & Leadership	School autonomy	404	0	1	0.333	0.383	0.206	0.527	0.745	0.470	0.299
Time	Instructional hours per year	284	850	1,680	1,151	123	1,141	1,165	1170	1165	1119
Time	Instructional hours per week (reading)	269	0.5	2.25	1.39	0.444	1.33	1.49	1.45	1.49	1.59
Time	Instructional hours per week (math)	295	0.5	2	1.20	0.343	1.14	1.28	1.24	1.28	1.40

Notes: Descriptive statistics for all measures tested in Table 8. Each measure is created from a survey of Michigan public schools. See online appendix at http://edpolicy.umich.edu/publications/#policy-briefs for further description of the survey, and Appendix Table 3 for detailed description of the measures.

Appendix A – Collection of Lottery Data

We solicited lottery data from all existing and eligible charter schools in Michigan from May 2011 to June 2012 and attended a subset of school lotteries in spring 2012 and 2013. Out of the 252 eligible[23] charter schools we contacted, we obtained usable lottery records from 67 schools (27%) (See Table A1). We collected lottery data from as many years as available in each school.

Table A1: Lottery Data Collection Status by School Level

	Eligible schools*	Participating schools	Schools with lottery**	Schools with usable data***
K-5	18	16	5	3
K-8	99	86	51	46
K-12	29	23	7	6
9th-12th	24	18	7	4
Other	82	69	20	8
Total	252	212	90	67

* Schools open in 2011-12, remained open in 2012-13, and general education focused (not alternative schools).

** Schools where the principal or school staff recalled conducting a lottery

*** Schools with usable lottery records as determined by project team.

Since no central body collects lottery data from all schools, we had to reach out to each school to learn its lottery history and gain access to its records. Schools fell into one of three categories: 1) they had no memory of a prior lottery; 2) they could recall a prior lottery but did not save sufficient records; 3) they had a prior lottery and saved sufficient records.

Rather than cold-calling charter schools, we reached out to the charter school authorizers (through the Michigan Council of Charter School Authorizers) and larger management companies to get their buy-

[23] We did not include alternative schools, i.e., strict discipline academies, vocational academies, and second-chance schools.

in and assistance with contacting their schools. We then contacted school principals, explained the study, asked about their lottery history, and in most cases, worked with the staff to locate, sift through, and understand their lottery records. This entailed many in-person visits, emails, and phone calls to ensure we had complete records with proper documentation. When we could, we attended the actual lotteries of schools in our sample often as a way to fill in any gaps unintentionally left by school staff.

Over the course of our year-long fieldwork, we took note of the significant role the open enrollment period appeared to play in whether a school had a lottery or not. The open enrollment period is the only time a lottery can be legally triggered. Each school sets its own period with the only requirement that it be at least two weeks. During this period, if more students apply than open seats for a grade, then legally a lottery must occur. If there are enough open seats for all the students that apply, then all the students are accepted without needing a lottery. The timing of a school's open enrollment period could directly impact the likelihood of a lottery. For example, schools that had an earlier open enrollment period for only two weeks could usually accept all the eager and savvy applicants that applied and keep a first-come first-serve waitlist after the open enrollment period. Schools that had a longer and later open enrollment period (when parents were more likely to be thinking about the next school year) were more likely to need a lottery, since more students could apply during this time. And with a lottery came the inability to just accept all the students, presumably with the savviest parents, who applied first. And since each school set its own open enrollment period, parents had to be exceptionally organized if applying to multiple schools with multiple deadlines. We also noticed schools getting into open enrollment bidding wars, setting their open enrollment periods to be earlier than competing schools so as to not miss out on prime students.

<u>Cleaning Lottery Data</u>

Since each school managed their own lottery, there was little uniformity in lottery records across schools and sometimes even within schools due to personnel changes. Due to this variation, we developed a custom intake process by lottery record to standardize the format and information.

Fortunately, there was one large management company, National Heritage Academies (NHA), which maintained physical and electronic records of lotteries from all of their schools. We applied the same intake rules to these records.

Lottery Records from the NHA Network

We received lottery records from the NHA in two formats: (1) an excel spreadsheet with information from its centralized enrollment database; and (2) physical lottery boards collected from its network of schools.

The excel spreadsheet (or “electronic files”), contained first name, last name, date of birth, full address, gender, school name, academic year, and grade for all applicants that participated in a lottery from 2004-2012. They also contained the date admission was offered and the date a student was added to the waitlist, which identified which students were admitted from the lottery, which were placed on the waitlist and which, if any, of these waitlist students were later admitted. We did not know the waitlist order, since the randomly drawn lottery number was not saved. We received a separate sibling file where, for the most part, we could identify if an applicant had an applying or currently enrolled sibling.

The physical lottery board files showed the order the applicant was drawn and whether they had “won” an open seat or were placed on the waitlist. Since these were records from the day of the lottery, we could not tell which waitlist students were later admitted. The lottery board also contained student and sibling information but was missing student date of birth.

The ideal combination was both the electronic and lottery board file for each lottery, since the electronic file contained admission data and student date of birth, while the lottery board file contained the waitlist order. We combined the information from both sources when available, but in some cases, we only had one. Prior to 2004, only physical lottery boards were available, since electronic records did not go back that far. And for some schools, we only had electronic lottery files, since it was up to the school whether to save the physical lottery boards (not required by the NHA).

Appendix B – Construction of Lottery Instruments

In any lottery study, there are several options for constructing a lottery instrument. The initial offer instrument is an indicator variable equal to one if a student is offered a seat in at least one school in her risk set at the time of the lottery. The ever offer instrument is equal to one if initial offer is one, but also counts offers made later, that is, offers extended to students on the waiting list after a student with an initial offer decides to turn it down. If offers are given in the ordering established on the day of the lottery, both initial offer and ever offer should be unrelated to most other characteristics of the student and predict enrollment.

As explained in Appendix A, lottery information comes from two sources: pictures of lottery boards (LBs) and electronic files (EFs). LB records contain lottery numbers and offers made on the day of the lottery. EFs contain all offers (both initial and ever), but no lottery numbers. Thus, although ever offer and initial offer can both be constructed from the EFs, verifying that a school complied with the lottery ordering can only be achieved by cross- referencing the EF with the LB. In the full sample, 66% of lottery applications with EFs have LB information. After sample restrictions, 58% of the EFs have corresponding LB information.

Initial Offer

When both EFs and LBs are available, initial offers correspond about 98% of the time, according to Table B1 below. LB records supersede EF records when both are available. Skipped lottery numbers and repeat lottery numbers within the same lottery are also rare at 6.2% and 1.8% of all lottery applicants with LBs, respectively. Thus, initial offer plausibly captures the random ordering established by the lottery.

Table B1: Cross-tab of Initial Offer According to Electronic File Records Versus Lottery Board Records

	Lottery Board			
Electronic File	0	1	Missing	Total
0	5,502	51	3,295	8,848
1	312	2,590	3,171	6,073
Missing	312	313	0	625
Total	6,126	2,954	6,466	15,546

Notes: Comparison of recorded offers awarded on the day of the lottery according to two sources of information: lottery boards and electronic files. Comparison data only available for a subsample of schools that saved lottery boards.

However, initial offer is less predictive of enrollment than ever offer. Some students do not accept offers of admission, particularly in later grades where students must switch schools to take up an offer.

Ever Offer

We check whether there exists any student who never received an offer and has a smaller lottery number than somebody who did receive an offer in the same lottery. That is, we check for offers given out of order. About 13% of all offers where both EF and LB records are available are out of order. Within a lottery, the average fraction of offers that is out of order is 0.16, and the median fraction of out-of-order offers is 0.11. Only 40 lotteries give more than 20% of their offers out of order, and 30 of these lotteries extend fewer than 30 offers (in this case even one out-of-order offer is a large share of the total offers). In addition, while some lotteries seem to be worse than others, averaged across all observed lotteries, only two schools with more than 30 applications give 20% or more offers out of order.

This suggests that many lotteries and schools are doing a good job of following the lottery number ordering, but for half of the sample, which schools and lotteries are doing a good job is unobservable. It's even possible that the sample for which we observe both LB and EF records has a lower than average rate of out-of-order offers. Because some lotteries and schools are likely to be giving non-random offers of admission, and it is impossible to observe this, the ever offer instrument is not preferred.

Alternate Ever Offer Instruments

For the subset of lottery applications where both LB and EF records, two alternative instruments can be constructed to attempt to "fix" any out of order offers. The adjustment depends on the type of error assumed. If schools are failing to record offers (as opposed to failing to give offers in order), the adjusted instrument should fill in offers for applicants who were skipped. We call this a backfilled instrument. If schools are failing to give offers in order, but never fail to record offers, then we can count the number of offers recorded and assign them according to the lottery number. We call this the lottery number instrument.

However, both of these alternative instruments exist for a strange subset of students since schools idiosyncratically saved LB information when it was not necessary to do so. Table B2 describes observable characteristics for the different samples available for each instrument. Students in the alternate samples apply to more lotteries (see Table B2). The alternate samples are observably different. For example, students are more likely to come from non-urban areas, more likely to apply in 2009 or later, and are slightly less likely to apply to middle school grades. Finally, the alternate samples are less likely to be made up of underrepresented minorities (particularly Black students) and more likely to contain Asian students. Because of the limited sample size and the irregularity of the sample, neither of the alternate ever offer instruments are preferred, though results for these are reported in Table B3.

Table B2: Characteristics of samples used with each instrument				
	Initial Offer Sample (1)	Ever Offer Sample (2)	Lottery Number Sample (3)	Backfilled Ever Offer Sample (4)
Number of Students	21661	14597	10159	4104
Number of Applications	27264	18971	14460	5969
Number of Applications per Student				
1	0.84	0.81	0.74	0.71
2	0.10	0.12	0.17	0.20
3 or more	0.06	0.07	0.09	0.09
Number of Offers per Student				
0	0.58	0.66	0.68	0.70
1	0.41	0.33	0.30	0.28
2	0.01	0.01	0.02	0.02
3 or more	0.00	0.00	0.00	0.00
Applied to Mostly Urban Schools	0.25	0.20	0.20	0.21
Applied to Mostly Non-Urban Schools	0.75	0.80	0.80	0.79
Applied 2003	0.03	0.01	0.01	0.00
Applied 2004	0.06	0.05	0.00	0.00
Applied 2005	0.09	0.09	0.07	0.04
Applied 2006	0.08	0.05	0.06	0.03
Applied 2007	0.09	0.08	0.07	0.09
Applied 2008	0.10	0.11	0.09	0.06
Applied 2009	0.12	0.10	0.13	0.16
Applied 2010	0.08	0.07	0.11	0.16
Applied 2011	0.17	0.17	0.10	0.14
Applied 2012	0.18	0.25	0.37	0.33
Applied in Pre-K	0.07	0.04	0.05	0.09
Applied in Kindergarten	0.55	0.53	0.59	0.52
Applied in Grades 1-5	0.32	0.36	0.32	0.36
Applied in Grades 6-8	0.07	0.07	0.04	0.04
Female	0.50	0.51	0.51	0.51
Black	0.27	0.24	0.20	0.18
Hispanic	0.07	0.08	0.07	0.06
Asian	0.16	0.18	0.20	0.22

Notes: Column (1) uses the full sample subject to the restrictions in Appendix Table 1. Samples differ between columns due to the exclusion of degenerate lotteries and risk sets according to each instrument. Column (2) reports on the ever offer sample, column (3) on the lottery number instrument, which counts the number of ever offers and assigns them according to the lottery number, and column (4) reports on the backfilled instrument, which assigns an offer to anyone with a lottery number smaller than the maximum which received an "ever offer."

DREO Estimator

Table B3 reports estimates using the initial offer instrument, the ever offer instrument, and the ever offer instrument weighted to account for the fact that the number of ever offers depends on the number of takers, following Behaghel and Chaisemartin (2017). Behaghel and Chaisemartin (2017) show that without this weighting, the ever offer instrument is inconsistent. The reweighted estimator is called the doubly-reweighted ever offer (DREO) estimator. The initial offer and DREO estimators should both be consistent, though the initial offer instrument yields less precise estimates. The two-stage least squares regressions for the DREO estimator use weights w_{ik}^{DR}, which are constructed as follows:

$$w_{ik}^{R} = Z_{ik}\left[1 - \frac{D_{ik}}{S_k}\right] + (1 - Z_{ik})$$

$$w_{ik}^{DR} = w_{ik}^{R}\left[Z_{ik} \times \frac{L-K}{N-K} \times \frac{N_k - 1}{L_k - 1} + (1 - Z_{ik}) \times \frac{N-L}{N-K} \times \frac{N_k - 1}{N_k - L_k}\right]\frac{N_k}{N_k - 1}.$$

The first set of weights, w_{ik}^{R}, down-weights each student with an offer in a way that is equivalent to removing one taker from each risk set. The second set of weights, w_{ik}^{DR}, acts like a propensity score weighting. Here Z_{ik} and D_{ik} denote whether applicant *i* in risk set *k* received an offer and whether she enrolled in a charter, respectively. In addition, *K* denotes the total number of risk sets, N_k is the number of applicants in risk set *k, N* is the total number of applicants, L_k is the number of offers in risk set *k,* and *L* is the total number of offers across all risk sets. Finally, S_k represents the number of seats in each risk-set, and is imputed from the number of offers that are taken up in the data.

Table B3, column (1) replicates the analysis in the paper. Columns (2) and (3) report results for the sample where ever offer records are available, and columns (4) through (9) report results for the alternate instruments. These show that the alternate instruments do not have much more predictive power than the initial offer instrument, possibly suggesting that they are not more accurate than the recorded offers. For example, the first stage in column (6) for the backfilled instrument is weaker than the first stage in column (4) for the initial offer instrument. If one takes the recorded offers at face value, then results for the ever offer and DREO estimators in column (3) line up fairly well with the initial offer results in column (2); they are not statistically distinguishable, even though the ever offer estimates are possibly biased. The treatment on the treated estimates using the DREO estimator are more precise than those estimated with the initial offer, and larger than the results using ever offer.

Table B3: Robustness to alternative instruments and samples.

	Full Sample	Ever Offer Sample		Backfilled Ever Offer Sample			Lottery Number Sample		
	Initial Offer	Initial Offer	Ever Offer	Initial Offer	Ever Offer	Ever Offer Alternate 1	Initial Offer	Ever Offer	Ever Offer Alternate 2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ITT	0.030*	0.022	0.045**	0.068*	0.013	-0.013	0.041**	0.038*	0.01
	(0.016)	(0.019)	(0.019)	(0.035)	(0.031)	(0.037)	(0.021)	(0.021)	(0.021)
TOT	0.040*	0.024	0.018**	0.053*	0.006	-0.012	0.043*	0.016*	0.008
	(0.023)	(0.021)	(0.008)	(0.030)	(0.013)	(0.035)	(0.023)	(0.009)	(0.018)
Cumulative Yrs.	0.745***	0.950***	2.45***	1.27***	2.35***	1.07***	0.950***	2.44***	1.16***
	(0.102)	(0.129)	(0.092)	(0.176)	(0.127)	(0.190)	(0.154)	(0.101)	(0.131)
Obs.	80,832	53,381	53,381	14,593	14,593	14,593	35,801	35,801	35,801
DREO - ITT			0.046*		-0.019	-0.039		0.028	0.024
			(0.024)		(0.045)	(0.055)		(0.025)	(0.021)
DREO - TOT			0.020*		-0.009	-0.033		0.013	0.019
			(0.011)		(0.020)	(0.047)		(0.011)	(0.017)
Cumulative Yrs.			2.28***		2.23***	1.20***		2.27***	1.25***
			(0.091)		(0.146)	(0.168)		(0.109)	(0.132)
Obs.			53,244		14,558	14,566		35,717	35,801

* = 10%, **=5%, ***=1%

Notes: Outcome is math standardized test score, endogenous regressor is cumulative years spent at a charter school in the student's risk set, and four instruments are used: initial offer, ever offer, and two alternate ever offer instruments. Regressions cluster standard errors at the risk-set level, control for risk-set fixed effects, and the standard vector of covariates. Column (1) uses the full analysis sample. Columns (2)-(3) drop lotteries and risk-sets that are degenerate on the ever offer instrument. Columns (4)-(6) drop lotteries and risk-sets that are degenerate on the first alternative ever offer instrument, which assigns an offer to anyone with a lottery number smaller than the maximum lottery number to have received an "ever offer." Columns (7)-(9) drop lotteries and risk sets that are degenerate on the second alternative ever offer instrument, which counts the number of ever offers, and assigns them according to the lottery number. DREO estimates reweight the estimates according to Behaghel and Chaisemartin (2017).

Appendix C – Matching Lottery Records to Michigan Department of Education Administrative Data

The analysis uses lottery records matched to administrative records maintained by the Michigan Department of Education. Matching occurred using a combination of exact and probabilistic matching techniques. Probabilistic matching can account for minor misspellings, nicknames, date and month swaps in date of birth, and other common errors. We could also place a higher weight on certain variables like date of birth compared to others, like ethnicity. In cases where it was difficult to read an applicant's name or date of birth, we would create multiple entries with the possible variations. We reconciled these duplicates after the matching process. Overall, we matched 96% of the 98,499 lottery application student records. We did not match 4,150 applicants and had to drop these applicants since we could not obtain their state administrative outcome data.

Probabilistic Matching

We used Link King software for the probabilistic matching. Note that this was only possible for records with non-missing date of birth.[24] We then divided our date of birth records by twins and non-twins, since Link King was less precise with twins. We ran the records lacking date of birth and the records marked as twins through exact matching in STATA.

Of the 87% of lottery applicants who had date of birth and were not a twin, we matched 97% of these in Link King. At a minimum, we matched on the binding variables first name, last name and date of birth, and whenever available, we included the non-binding variables zip code, gender, and ethnicity. Link King outputted a match level of 1, 2, 3, 4, 6, and 7 (no 5) to indicate match strength, with match levels 1, 2, and 3 reflecting stronger matches; 4, 6, and 7 reflecting weaker matches; and no match level for records with no match. We accepted all level 1, 2, and 3 matches (95%), manually reviewed level 4, 6, and 7 matches (2%), and accepted all no match level as unmatched (3%) If a lottery applicant matched

[24] Some schools did not retain date of birth with their lottery records.

to multiple students at the same match level, we considered these students unmatched to avoid false positives (less than 1%).

Of the 11% of lottery applicants that were missing date of birth, we matched 86% of these applicants in STATA. We ran multiple rounds, matching on all available information for an applicant. For example, if an applicant had first name, last name, gender, zip code, lottery year and grade, we ran a one-to-many merge using all these variables. If a student uniquely matched, we considered this a proper match. If a student had multiple matches, we checked street address if available to identify the correct match. If we still could not identify a unique match, we considered this student unmatched to avoid false positives. We then did the same for students with one less variable, and so on, establishing five sequential matching rounds:

1) Lottery year, grade, first name, last name, zip code, gender;
2) Lottery year, grade, first name, last name, zip code;
3) Lottery year, grade, first name, last name;
4) First name, last name, zip code;
5) First name, last name.

To prevent false positives in the later rounds, we used the lottery year and grade from the applicant lottery record to estimate the applicant's age. We then checked the date of birth from the matching state administrative record to see if the date of birth lined up with the estimated age, within a plus or minus two year range. For those that fell outside of this age range, we considered them false positives and classified them as unmatched.

Of the 3% of records we identified as twins (same last name, same date of birth, different first name), we matched 99% of these applicants in STATA. We followed the same process as above except with more matching rounds:

1) Lottery year, grade, first name, last name, zip code, gender, date of birth;
2) Lottery year, grade, first name, last name, zip code; date of birth;

3) Lottery year, grade, first name, last name; date of birth;

4) First name, last name, zip code; date of birth;

5) First name, date of birth;

6) Last name, date of birth;

7) Manual match.