The Impact of High School Choice on Mediators of Student Success*

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Abstract

While there is a large literature examining the effects of school choice on achievement, less attention has been given to the mechanisms by which students benefit (or not) from school choice. We use data from high school admissions in New York City to examine whether admission to a preferred school impacts measures of behavioral, cognitive, emotional, and academic engagement. In this preliminary draft, we focus on a subset of students who applied to oversubscribed, non-selective high schools in 2008 or 2009. In these cases school assignment was made at random, providing us plausible causal estimates of the effects of receiving a first choice school match. We find small but statistically significant effects of receiving a first choice on many dimensions of engagement. These effects, however, appear to be due to differences in school quality rather than an additional "match" or "fit" effect. When contrasting the engagement of students randomized into the same over-subscribed schools but admitted at different choice levels, the effects largely disappear.

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1 Introduction

The New York City Department of Education (NYCDOE) operates one of the largest public high school choice programs in the world. Since 2004, all rising 9th graders have been required to submit a list of up to 12 schools they would like to attend, ranked in order of preference. These student rankings—and in some cases, the schools' rankings of applicants—are then used in a centralized matching algorithm that assigns students to schools (Abdulkadiroğlu, Pathak, & Roth, 2005). As of 2012, approximately 80,000 students choose from among 680 high school programs each year (Bloom, Thompson, & Unterman, 2010; Corcoran & Levin, 2010).

Like other choice policies in the United States and abroad, New York City's aims to improve schooling outcomes through several channels. First, it enables families to choose from a large number of providers and seek out the highest quality school for their child. The ability to apply to any school citywide is expected to benefit most students who might otherwise be assigned to a lower-quality neighborhood school. Second, by offering a broad portfolio of options the system aims to facilitate matches between students' interests and needs on the one hand and school curricula, programs, and services on the other. A better fit may stimulate greater engagement with school, which may in turn promote better academic outcomes. Third, over the long run, choice may increase overall school quality if innovative and effective schools attract enrollment and ineffective ones close or are pressured to improve.

A growing body of evidence suggests school choice can have positive effects on student achievement, particularly when a chosen school is of higher quality than the alternative (e.g., Cullen, Jacob, & Levitt, 2006; Deming, Hastings, Kane, & Staiger, 2009; Hoxby & Murarka, 2009). Comparatively less attention, however, has been given to the mechanisms through which students benefit (or not) from school choice. We also have little evidence

on the relative importance of school effects (that accrue to any student enrolled in a given school) versus effects specific to the student-school match. Several papers have explored the relationship between school choice and "non-cognitive" outcomes such as attendance, behavior, and long-run attainment but these studies remain few in number (e.g., Cullen et al., 2005; Imberman, 2011; Booker, Sass, Gill, & Zimmer, 2011).

In this paper, we examine the impact of admission to a preferred school on mediators of student success, including measures from four broad categories of student engagement: behavioral, cognitive, psychological/emotional, and academic. On the behavioral dimension, for example, we ask whether students successfully matched to their first choice have better attendance or are more likely to participate in school activities than when matched to their second (or lower) choice. On the cognitive and psychological dimensions, we ask whether those assigned to their top choice perceive their school as furthering their academic goals, or as a place where they feel supported by their teachers and peers (all based on student responses to a citywide survey). Knowing whether such engagement effects exist is inherently of interest, but is also an important step toward understanding how school choice affects academic achievement.

Estimating the causal impact of attending a more preferred school is difficult in most settings. Typically one does not observe student preferences over schools, and the school a child attends is highly non-random. In New York City, however, we have rich information about each student's high school preferences. Moreover, many schools are oversubscribed, meaning more students are subject to being assigned there than there are seats available. When this occurs, the mechanism awards seats at random or in order of the school's own rankings, depending on the admissions method. This design generates hundreds of natural experiments that can be used to estimate causal effects by comparing otherwise equivalent

students who similarly ranked the school but who were or were not assigned to it.

The matching algorithm in New York City algorithm may also allow one to disentangle "school quality" effects from "preference" effects, by contrasting outcomes for students randomized into the same schools but at different preference points (assuming all less-preferred schools are otherwise of similar quality in expectation). If school impacts vary by students' preference for that school, it may indicate the presence of a student-school "match" or "fit" effect.

We analyze outcomes for two cohorts of 8th graders who applied to high schools in 2008 and 2009 and were observed in a public school the following year, approximately 133,000 students. (We exclude students accepted into one of the city's elite exam-based high schools). For the current version of this paper, we concentrate primarily on students who applied or were admitted to an oversubscribed, non-selective high school—about 20% of all participating students. In these cases we can be confident assignment to schools occurred at random, and thus are able to make more convincing causal claims about the effects of assignment to one's more preferred school. In future versions, we will incorporate a larger share of the sample, which requires taking into account other, non-random assignment mechanisms.[1]

Our analysis (which remains preliminary) finds small but statistically significant effects of receiving a first choice match on many dimensions of engagement. These models contrast students who made the same first choices, but who were or were not assigned there. Thus, these impact estimates should be interpreted as a combination of "school quality" effects, where admisssion to one's first choice school may mean a higher quality school than the alternative, and "preference" effects, where students have better outcomes in their more-

[1]For example, students who apply to oversubscribed "screened" schools are admitted in order of schools' ranked preferences. Using schools' rankings, we can exploit a regression discontinuity design to estimate the impact of admission to one's top choice school, comparing students ranked just below and above the effective cutoff for admission.

preferred school, holding quality constant. In an alternative set of models, we contrast students randomized into the same oversubscribed schools but at different preference points. Here we find almost no difference in the engagement of students admitted as their first choice and that of students admitted as lower choices. Though this approach has limitations, our results suggest that the effect of school choice on engagement operates primarily through school effects.

2 Related literature

2.1 The impact of choice on academic and non-academic outcomes

A growing literature examines the benefits of attending a more preferred school. Generally, this research pertains to "opt out" programs, in which families choose to apply to an alternative school, rather than universal choice programs. These include open enrollment (e.g., Cullen, Jacob, & Levitt, 2006; Deming, Hastings et al., 2009; Lai, 2007; Lauen, 2009), charter or magnet schools (e.g., Hoxby & Murarka, 2009; Abdulkadiroğlu et al., 2009; Zimmer, Gill, Booker, Lavertu, & Witte, 2012), vouchers (e.g., Hsieh & Urquiola, 2006; Rouse, 1998), and targeted transfer provisions, like that allowed under NCLB (e.g., Hastings & Weinstein, 2008).

The most rigorous studies rely on random assignment in cases of oversubscription to estimate the causal impact of attending a preferred school. For example, Cullen et al. (2005, 2006) found that Chicago students admitted via lottery to their first choice high school were less likely to be involved in disciplinary incidents or arrests than those who lost these lotteries. They found little evidence of academic benefits, however. In contrast, Lauen (2009) found modest positive effects of high school choice in Chicago on the likelihood of graduation.

These benefits tended to be larger for residents of low-poverty neighborhoods than high-poverty neighborhoods. In a related study, Deming et al. (2009) found that students in Charlotte-Mecklenberg assigned to low-performing neighborhood high schools but admitted via lottery to a more preferred high school had significantly better academic outcomes than those who applied but lost the lottery. This study is noteworthy in that it examined the impact of choice on both high school graduation and post-secondary outcomes.

Outside of the U.S., Lai (2007) analyzed a large-scale school choice reform in Beijing and found, on average, no statistically significant test score gains for students admitted by lottery to their first choice middle school. Effects were heterogeneous, however, as students admitted to the most selective schools saw positive effects. Pop-Eleches and Urquiola (2011) used a regression discontinuity design to estimate the benefits of attending a selective high school in Romania. In that country, admission depends strictly on 8th grade test scores and GPA, with the cut-off varying by school. As in NYC, students submit ranked choices to a central authority, which allocates students to schools. They found students admitted to higher-quality schools performed better academically (see also Clark, 2010; Jackson, 2010). Perhaps even more interesting, this study is one of few to use survey data to compare the behaviors and perceptions of students (and their parents) who were and were not admitted to their school of choice. Here they find parents devote less time to schoolwork when their child is admitted to a more selective school, and are more likely to report their child does homework every day; students just above the cutoff for admission were more likely to perceive themselves as weak relative to their peers.[2]

Findings from the charter school literature largely parallel these, with several prominent studies finding beneficial effects of charter school attendance (Hoxby & Murarka, 2009;

[2]Several studies of private vouchers have examined the effects of winning a voucher on *parental* involvement and student satisfaction. For example, see Mayer et al. (2002).

Abdulkadiroğlu et al., 2009; Angrist, Dynarski, Kane, Pathak, & Walters, 2010; Dobbie & Fryer, 2009), and others finding neutral or negative effects (e.g., Bifulco & Ladd, 2006; Booker, Gilpatric, Gronberg, & Jansen, 2007; Center for Research on Education Outcomes [CREDO], 2009; Zimmer et al., 2012). An emerging finding from this work is that charter schools potentially benefit students in ways not fully captured by test scores, including high school completion and college attendance (Booker, Sass, Gill, & Zimmer, 2011).

To date the only study to use data from NYC high school choice is Bloom et al. (2010), which uses the matching mechanism to evaluate the impact of attending a small high school on achievement.[3] As we do, they relied on random assignment of students to schools in order to assess whether students choosing-and assigned to-small high schools had better outcomes than those who did not. They found students assigned to small schools were more likely to remain on track for graduation throughout high school, and were more likely to graduate within four years.

2.2 Student engagement and academic outcomes

Although causal estimates of the impact of student engagement on academic outcomes are difficult to obtain, a large literature suggests engagement may be an important factor in both academic and non-academic success in school. More engaged students are generally observed to have better grades and higher test scores (Akey, 2006; Lee & Smith, 1995; Roderick & Engle, 2001) and are less likely to drop out of school than their less engaged counterparts (Connell, Halpern-Felsher, Clifford, Crichlow, & Ursinger, 1995). Practitioners have seized on student engagement as a risk factor that is more amenable to intervention

[3] Abdulkadiroğlu, Angrist, and Pathak (2011) estimate the impact of admission to one of the city's elite exam-based schools. The admissions process for these schools, however, is separate from the broader choice program.

than other observable indicators associated with drop out behavior. For example, the recent move to replace large comprehensive high schools with small themed schools or "learning communities" was one such attempt to reduce dropout rates by promoting greater student engagement.

Engagement is a construct operationalized in the literature using four distinct but related dimensions (e.g., Appleton et al., 2006, 2008; Griffiths et al., 2009; Fredricks, Blumenfeld, & Paris, 2004). The first of these is *behavioral engagement*, actions that broadly reflect participation or involvement with school. Indicators of behavioral engagement include school attendance and extracurricular involvement, as well as disciplinary incidents. The second dimension is *cognitive engagement*, pertaining to students' perceived relevance of schoolwork, value of learning, personal goals, and willingness to exert effort. The third is *psychological engagement*—also referred to as *emotional* or *affective* engagement—which refers to affective connections with school, including students' sense of belonging, identification with school, and relationships with teachers and peers. Finally, *academic engagement* focuses on completion of required academic tasks, including course and exam completion. Comprehensive measurement of engagement requires a combination of directly observable indicators (e.g., attendance or credits earned) and less easily observed indicators (e.g., students' perceptions of relationships with teachers and peers). We describe our empirical analogs to these in a later section.

3 High school choice in New York City

In New York City, all incoming freshmen are required to submit a high school application listing up to 12 programs they would like to attend, ranked in order of preference.[4] Based on these rankings, a centralized algorithm—the High School Applications Processing System, or HSAPS—assigns students to schools in a manner like that used to match medical school graduates to residency programs (Abdulkadiroğlu, Pathak, & Roth, 2005; Bloom, Thompson, & Unterman, 2010). The application process is required: no student is permitted to attend a "default" school and avoid an active choice, and few are guaranteed admission to their first choice school.

In NYC, students choose from a large portfolio of high schools, including small themed schools, comprehensive schools, vocational schools, career academies, and performing arts schools.[5] Each school has well-defined admissions methods and admissions priorities, which may include geographic preferences. There are six categories of admissions methods, shown in Table 1: academically non-selective (limited unscreened, unscreened, and zoned), selective (test, screened, and audition), and partially selective schools (educational option). Within admissions methods, schools may give priority admission to students meeting certain criteria. For example, limited unscreened schools are academically non-selective but give priority to students who live in the same borough as the school and who attend a school information session or open house. In this version of the paper we focus primarily on students applying or admitted to the limited unscreened schools, where assignment to an oversubscribed school

[4]Students apply to *programs*, and not *schools* per se. Although the vast majority of schools have only one program, many offer multiple programs within the same building or school. At this point, this distinction is unimportant, and we refer primarily to "schools."

[5]Charter schools have played a major role in the city's reform efforts; however, there were virtually no charter high schools in NYC during the period of this study. Almost all were K-8 schools (Hoxby & Murarka, 2009).

is random conditional on priority group (Bloom et al., 2010).

As Table 2 shows, roughly half of all students were assigned to their first choice high school in 2008 and 2009, and 70% received one of their top three choices. About 8% of students received none of their top 12 choices; these students were required to submit new choices in a "supplemental round" for remaining seats. 2.8% of students remained unmatched after this round. Those who arrive late to the district or otherwise fail to participate in HSAPS visit an enrollment office to be assigned to a school with available seats (Quint et al., 2010).

In assigning schools, the algorithm processes applicants in random order and matches them to schools based on their choices and the admissions methods and priorities used by their ranked schools. The system is a "student-proposing deferred acceptance" mechanism, meaning that students are assigned their most preferred school subject to seat availability and the constraint that school priorities take precedence over student rankings (Pathak, 2011; Gale & Shapley, 1962). In other words, a conditionally assigned student A can be "bumped" by student B from an oversubscribed school even if A ranked the school 1st and B ranked the school 3rd, as long as B has higher priority at the school and has not already been matched to his 1st or 2nd choice. If they have the same priority, each has an equal chance of assignment, by virtue of the random ordering. The implication for oversubscribed non-selective schools is that students not already assigned elsewhere are assigned at random, conditional on priority group.[6] Moreover, students can be assigned to a school at difference preference points. That is, oversubscribed schools do not necessarily fill up with students who ranked that school first, as would be the case if the mechanism prioritized student rankings over priority groups (the so-called Boston mechanism).

[6]As shown in Pathak (2011), this mechanism is strategy-proof, meaning that students can do no better than reporting their true preferences, regardless of what other students do (i.e., there are no incentives to behave strategically). The mechanism is not necessarily ex-post Pareto optimal. In some cases, it may be possible to re-allocate students and make some better off without making anyone worse off.

4 Data

We rely on several large administrative databases provided by the NYCDOE. First and foremost are the original HSAPS databases for two cohorts of 8th graders who applied to high schools in 2008 and 2009—approximately 80,000 students each year. In addition to details on every student's ranked high school choices, this data includes variables such as students' age, feeder school, borough and zipcode of residence, academic history, math and ELA test scores, special education and limited English proficiency status, schools' rankings of students (for screened schools), and final match. Using unique student identifiers, we matched the HSAPS data to additional administrative data from earlier (e.g., 7th) and later (e.g., 9th - 12th) grades.[7] This data includes demographics (e.g., race/ethnicity and gender), eligibility for subsidized meals (a proxy for low income), immigrant status, schools attended, attendance rates, courses attempted and passed, GPA, Regents examinations taken and passed, services received (e.g., special education), and graduation outcomes. We also matched students to their 9th grade responses on the citywide School Survey, described in greater detail below.[8]

To construct our base sample, we began by selecting all students who completed the HSAPS process in 8th grade in either cohort (i.e., did not opt out at any point) and were ultimately assigned to a high school program.[9] Only students who enrolled in a public high school in the year following HSAPS were included, and the 5-6% of students who were offered

[7]Applicants from private school had HSAPS identifiers that differed from the identifier used in NYCDOE administrative data (available for those who later enrolled in a public school). We obtained a crosswalk of IDs allowing us to match these students across data sources.

[8]See http://schools.nyc.gov/Accountability/tools/survey/default.htm for details on the School Survey. Individual student responses are kept confidential, and not shared with the school. The NYCDOE contracts with a third party to collect the data, encrypt identifiable IDs, and produce school-level reports. Our access to the data is through this third party.

[9]Of the 154,127 students shown in Table 2, 153,491 were assigned to a program (more than 99%).

and accepted a seat at one of the city's elite specialized high schools were dropped. Together these criteria restrict our sample to 133,215 students (66,073 in 2008 and 67,142 in 2009). This represents about 74% of the enrolled 9th grade classes in 2008 and 2009.[10]

Table 3 provides mean characteristics for students in our base sample, and two subsamples of students who applied or were admitted to an oversubscribed non-selective limited unscreened high school. Roughly 42% of students in the base sample are Hispanic, 34% are black, 12% are Asian, and 12% are white. More than 75% come from low-income families, based on their history of participation in the free or reduced-price lunch program. Close to 10% are English language learners, and 19% are foreign born. Given that many of the highest achieving students in the city are excluded (those accepting offers at exam schools), mean standardized ELA and math scores are roughly 0.09 standard deviations below the 8th grade average. The mean student ranked about seven schools on their choice form, and 52% were assigned to their first choice high school. As shown in the last line of Table 3, more than 77% of the students in our base sample responded to the School Survey.[11]

The rightmost columns of Table 3 show that the population of students applying or admitted to an oversubscribed limited unscreened school is generally more disadvantaged than the base sample. For example, their average ELA and math scores were 0.25 to 0.34 standard deviations below that in the base sample. 81% were eligible for free or reduced price lunch (versus 75% of the base sample), and 7.5% were receiving special education (versus 6.6% of the base sample). 3.7% to 6.0% of this sample was Asian or white (respectively), as

[10]Students matched by HSAPS or admitted to a specialized high school make up roughly 78.7% of the enrolled 9th grade classes in 2008 and 2009 (141,645 of 180,041 students). The remaining 21% either (1) entered the New York City public schools after the choice process was complete, (2) were continuing in the same school, if it offered both 8th and 9th grade, or (3) were repeating the 9th grade.

[11]This is comparable to the overall response rate for students in grades 6-12 (80-82%). We examined characteristics of survey respondents and non-respondents in our base sample and found some predictable differences. Not surprisingly, respondents had higher average attendance than non-respondents (93% vs. 77%). Later we address implications of differential response to the survey for our findings.

compared with 12.1% and 11.6% of the base sample.

Table 4 provides descriptive statistics for indicators of student engagement. The first group of measures reflects behavioral engagement, indicators of students' participation and involvement in school. Among these is the attendance rate, defined as the percent of enrolled days that the student was in attendance. In 8th grade, students had a mean attendance rate of 90.8% (SD=9.8); this slipped to 89.1% in 9th grade (SD=15.7) and 86.2% in 10th (SD=19.3; available only for the 2008 cohort). Conditional on having data in both years, the average student's attendance dropped 1.8 percentage points from 8th to 9th grade (SD=10.9), and 4.8 points from 8th to 10th (SD=14.5).[12] Additional indicators of behavioral engagement are derived from self-reported participation in activities before and after school or during free periods. Students selected from a list of activities on the School Survey that included art, music, dance, theater, foreign language, computer skills/technology, school sports teams or clubs, and tutoring/enrichment activities. About 65% of students participated in at least one activity, and 40% participated in two or more. The mean number of activities was 1.5 (SD=1.6).

The second group of measures in Table 4 reflects academic engagement, or completion of formal academic requirements of school. Here we include both credits attempted and earned in 9th grade, attempts to pass the New York State Regents exams, and (credit weighted) GPA. The Regents are offered in several subjects and can be taken at any time during high school. A passing score of 65 is required on five of these exams to graduate. In 9th grade the mean number of course credits attempted was 14.5 (SD=4.4) and the mean credits earned was 11.6 (SD=5.4). Students attempted an average of 1.6 Regents exams in their 9th grade

[12]Based on a 180-day school year, a one percentage point drop in attendance translates to 1.8 school days. The 25th percentile of the 8th to 9th grade change in attendance was 3.1 points, or about 5.6 school days. The 25th percentile of the 8th to 10th grade change in attendance was 6.2 points, or about 11.2 days.

year (SD=1.2) and passed an average of 1.0 Regents exam (SD=1.0).

The remaining indicators in Table 4 represent measures of cognitive or psychological engagement derived from the School Survey. We conducted an exploratory factor analysis based on individual survey items in 2009 and 2010 and responses from all high school students. We identified three common factors, one related to cognitive engagement and two related to psychological engagement. The individual items are shown in Table 4, along with composite measures created using the simple average of items within factors. The composite measures are moderately correlated with one another (ranging from 0.31 to 0.55), and coefficient alphas for the factor scores—indicating acceptable internal consistency—are shown in Table 5.

Most of the individual survey items are on a 1-4 Likert scale with higher values corresponding to positive responses (1 = strongly disagree, 4 = strongly agree).[13] Collectively, the cognitive engagement items address students' perceptions of the value of school (e.g., "my school helps me develop challenging academic goals"), their own investment in learning (e.g. "I need to work hard to get good grades at my school"), and the relevance of school work (e.g. "my teachers connect what I am learning to life outside the classroom"). The psychological engagement items address feelings of belonging and relationships with teachers and peers (e.g. "how comfortable are you talking to teachers and other adults at your school about a problem you are having in class?" and "most students in my school help and care about each other").

The rightmost columns of Table 4 show few significant differences in engagement between the subsample of students applying and admitted to an oversubscribed limited unscreened

[13]Six of the survey items listed in Table 4 provided a fifth option of "Don't know"; these were treated as missing values. The survey items included in "psychological engagement measures (1)" are also on a 1-4 scale, but response anchors ranged from 1=uncomfortable to 4=comfortable, or 1=unavailable to 4=available.

school and the base sample, however most of the scores on the School Survey tend to be higher (more positive) on average for this group of students.

5 Empirical model

We are chiefly interested in the causal impact of assignment to one's first choice high school on engagement, relative to assignment to one's second, third, or lower choice. Estimating causal impacts of choice received is challenging if one's nth choice—or the propensity to receive one's nth choice—is related to student or school characteristics associated with engagement. As an illustration, consider a regression model in which an engagement measure for student i (Y_i) is modeled as a function of an indicator Z_{1i} equal to one if student i is assigned her first choice (and zero otherwise) and student-level covariates $\mathbf{X}_i$ related to engagement:

$$Y_i = \beta_0 + \gamma_1 Z_{1i} + \beta \mathbf{X}_i + u_i \tag{1}$$

If students were randomly assigned to schools with no consideration of their ranked preferences, Z_{1i} will be uncorrelated with any student factor (observable or unobservable) that might be associated with engagement. Furthermore, Z_{1i} will be uncorrelated with any school factors associated with engagement (e.g., school size or practices that promote engagement). Because students are assigned at random, some will receive their first choice while others will not; some will be assigned to schools that are generally more conducive to engagement, while others will not. Randomization should ensure treated and untreated students are otherwise equivalent in expectation so that the OLS estimator of γ_1 will be consistent for the average treatment effect of receiving one's first choice school. Students are not assigned entirely at random, however. Rather, Z_{1i} is likely to correated with student

or school characteristics associated with outcomes. (Consider applicants to high-quality oversubscribed schools versus those applying to low-quality undersubscribed schools).

To account for systematic variation in students choosing schools of a particular type, one could include fixed effects α_{1s} to contrast students who ranked school s first:

$$Y_i = \beta_0 + \gamma_1 Z_{1i} + \beta \mathbf{X}_i + \alpha_{1s} + u_i \tag{2}$$

In the case of students applying to oversubscribed non-selective schools, Z_{1i} will be random conditional on priority group.[14] Importantly, γ_1 represents the *total* effect of being assigned to school s as one's first choice, versus any less-preferred school in the choice set. As noted, this effect may be comprised of both a "quality" and a "preference" effect. One approach to disentangling these would be to include fixed effects for students ranking school s 1st, 2nd, 3rd, along with indicators Z of assignment to these schools as one's nth choice:

$$Y_i = \beta_0 + \gamma_1 Z_{1i} + \gamma_2 Z_{2i} + \gamma_3 Z_{3i} + \beta \mathbf{X}_i + \alpha_{1s} + \alpha_{2s} + \alpha_{3s} + u_i \tag{3}$$

Then, under random assignment and the assumption that the average quality of schools ranked lower than the one assigned is the same in expectation, the difference in the γ will represent a "preference" effect: the additional effect of being assigned to a school s as one's first choice, say, relative to being assigned to s as a lower choice (e.g., $\gamma_1 > \gamma_2 > \gamma_3$). An alternative approach is to estimate (3) with school fixed effects, rather than school-by-preference group effects. While less preferred, the interpretation is similar: differential effects by preference group are estimated using within-school variation in assignment. As with model (3), this approach requires sufficient mixing of students with differing preferences

[14] Thus in practice, we operationalize the first choice school fixed effect as a first choice x priority group combination.

attending the same schools, a requirement that holds for most schools in NYC.

Taken together, in this preliminary version of our paper we estimate model (2) within and without school effects for three separate samples. First, we estimate "naïve" models using the full set of students in our base sample. Because students assigned to screened or partially-screened schools are included, we cannot interpret estimates of γ from this sample as causal. Next, we estimate models for the subsample of students who ranked an oversubscribed non-selective (limited unscreened) school first, about 15% of our sample. In this case, admission to one's first choice school would have occurred via random assignment, and thus our estimates of γ can plausibly be interpreted as the causal impact of receiving one's first choice on engagement (at least for this population). In an attempt to differentiate "preference" effects, we finally estimate a version of model (3) with school fixed effects for the sample of students who were matched to an oversubscribed non-selective school, at any choice level, which represents about 20% of our sample (and encompassing many of the students in the second sample). In this case, identification of γ comes from within-school variation in school assignment (i.e., assigned as one's 1st choice, 2nd, choice, 3rd choice, or 4th-12th choice). Because students entering at a lower choice may differ systematically from those entering at higher choice (in ways not captured in the covariates), we estimate a second version of this model that controls for the type of school ranked as one's first choice (e.g., screened, unscreened, zoned).

Finally, we note that we are agnostic at this point about the motivation behind students' rankings of schools, only hypothesizing that students' desire to attend their assigned school is potentially related to engagement. As others have shown (e.g. Hastings, Kane, & Staiger, 2009), students rank schools high for a variety of reasons. For example, the school might be viewed as a particularly good fit for the students' interests or needs, or considered the

highest quality school among the choice set. The school may be geographically accessible to home, work, or public transportation, or the student may simply wish to attend a school with friends.

6 Results

In Figure 1 we show how mean engagement varies with the school placement received by incoming freshmen in our base sample. For ease of presentation, we collapsed the many possible entry points into six categories: 1st, 2nd, or 3rd choice in the main HSAPS round, 4th-12th choice in the main round, any choice in the supplemental round, and unmatched by HSAPS. As noted, more than 75% of students are in the first three categories. This figure shows some differences in attendance and activity participation by choice received. For example, the mean number of activities participated in falls slightly for those not receiving their first choice. Similarly, attendance is much lower for those matched in the supplemental round or left unmatched. These comparisons are misleading, however, as they do not account for systematic differences among students in the propensity to receive their nth choice (as discussed in Section 5). For example, the significantly lower attendance observed among those matched in the supplemental round or later is also seen in their 8th grade attendance (i.e. prior to high school). This highlights the need to adjust for observed pre-existing differences between students and to account for unobserved differences by exploiting HSAPS' random assignment mechanism.

Our main regression results are presented in Tables 6-8. In Tables 6-7, each cell represents the estimated difference in the outcome/engagement measure listed in the leftmost column between those assigned their first choice and those that were not, conditional on other included controls. In Table 8, each row reports the results from a separate regression, with

estimated differences in engagement for those assigned as their 2nd, 3rd, and 4th-12th choices shown. All regression models condition on student-level covariates, including gender, age, race/ethnicity (Asian, Hispanic, black, white), immigrant status, private school applicant, 8th grade special education and English language learner status, and 8th grade reading and math standardized scores. The attendance rate models also include lagged (8th grade) attendance as a control.

The models shown in Table 6 are our "naïve" models, estimated using the entire base sample. This sample includes students assigned to selective as well as non-selective high schools, and thus it is probable the propensity to receive one's first choice school is related to student or school-level factors omitted from the model. The direction of bias is uncertain, however. On one hand, highly engaged students might possess qualities that make it more likely to receive their first choice; for example, these qualities may result in their being ranked highly by a screened school. On the other hand, engaged students might be more likely to receive a lower choice if their top choice is highly competitive. As a partial solution, our models in the first column ("w/o school effects") include fixed effects for each first choice program preference and priority group combination, as well as controls for the type of program the student is matched to (e.g., screened, limited unscreened, zoned). In these models, students are effectively contrasted with others with the same first choice preference and priority group are compared. The models in the second column ("with school effects") add fixed effects for the school the students are ultimately assigned to.

In the naïve models of Table 6, students assigned to their first choice high school appear more engaged by most measures, although the implied effect sizes are mostly modest. For example, we find attendance rates are 0.95 points higher among students receiving their first choice, about 0.061 standard deviations (s.d.) in the overall distribution of 9th grade

attendance. Rates of participation in activities are 3-4 points higher, and the mean number of extracurricular activities reported was 0.13 higher for those receiving their first choice (0.080 s.d.). Our composite measures of cognitive and psychological engagement are all higher for students receiving their first choice: 0.05 points for the cognitive scale (0.080 s.d.), 0.04 for psychological engagement as measured by relationships with teachers (0.052 s.d.), and 0.11 higher for the psychological scale related to perceptions of peer culture (0.164 s.d.). Two of the three indicators of academic engagement are higher for those receiving their first choice, including credits attempted (a difference of 0.11 or 0.025 s.d.) and credits earned (0.22 or 0.041 s.d.).

When we include controls for both first choice program preference and final school assignment (the second column of Table 6), most of the differences observed above become statistically significant and/or smaller in size. For example, the difference in attendance is about 0.48 percentage points, about half the size found when school assignment effects are excluded. Only the credits attempted and earned effects remain similar in size. While only suggestive, these results indicate that students admitted to their first choice school are more engaged, which may be due to a combination of "school quality" effect, "preference" effect, and unobserved factors related to engagement and the likelihood of receiving one's first choice. When controlling for school assignment—effectively making within-school comparisons—these differences are smaller, suggesting that the quality effect (or selection) is dominant.

Table 7 reports results from the same models estimated in Table 6, but for the subsample of students applying to a non-selective, oversubscribed limited unscreened school as their first choice. Recall that in cases of oversubscription for these school types, students are effectively assigned to (or not) their first choice at random. In this case it is unlikely that

the differences we observe between those receiving their first choice and those not are due to unobserved heterogeneity in students. Here we find a similar pattern of results to that shown in Table 6: mostly positive effects that largely diminish or become statistically insignificant when controlling for school assignment effects. Many of the differences found in the models without school effects are larger than those found for the baseline sample. For example, we find a 1.16 percentage point higher rate of attendance for those receiving their first choice, about 0.073 s.d. in the distribution for this subsample. The effects on credits earned and attempted are 0.392 and 0.951 respectively, or about 0.085 and 0.176 s.d. On the other hand, the effects on cognitive and psychological engagement are smaller, from 0.061 to 0.129 s.d. These findings are broadly consistent with those in the naïve model: an apparently significant combined quality and preference effect that appears dominated by the quality effect. They are also consistent with the findings of Bloom et al. (2010) who find positive academic effects for the population of students applying to oversubscribed small non-selective high schools, which has considerable overlap with this sample.

Finally, Table 8 restricts the sample to students admitted to an oversubscribed non-selective limited unscreened high school. In this case, however, students may have been admitted as their 1st, 2nd, 3rd, or lower choice. All models include admitted school fixed effects, such that comparisons of engagement are within schools. Among our three main model specifications, this one comes the closest to isolating the "preference" or "match"—i.e. the return to being admitted as one's nth choice above and beyond the school effect on engagement. Here we mostly find no significant differences in the engagement between students admitted as their 1st choice (the omitted group) and those admitted as their 2nd, 3rd, or lower choice. The pattern of coefficients is such that the point estimate tends to be more negative for students admitted at lower choices, but the differences are small and

mostly statistically insignificant. Notable exceptions include attendance rates, which are about 0.548 points lower for those admitted to their school as their 4th or lower choice (or 0.028 s.d.), cognitive engagement, which is about 0.030 lower for the same group (0.059 s.d.), and credits attempted and earned, 0.347 and 0.424 lower for the same group, respectively (0.075 and 0.078 s.d.). Credits attempted and earned are also lower for those admitted as their 3rd choice, relative to those admitted as their 1st choice, about 0.197 to 0.235 fewer.

In future revisions of this paper, we will extend our model to include students applying and admitted to other school types, as well as explore possible reasons for the small preference effects uncovered here. One possibility for the latter that we have identified is that student rank very similar schools among their top choices (and in a small number of cases, different programs located within the same school). Such preferences would be consistent with finding minimal differences in the engagement of students matched to their 1st, 2nd, or 3rd choices. The modest positive effects found in the limited unscreened case are consistent with strong school effects on engagement, particularly when the alternative is a lower quality school (as may be the case for the population of students applying to limited unscreened schools; see Bloom et al., 2010).

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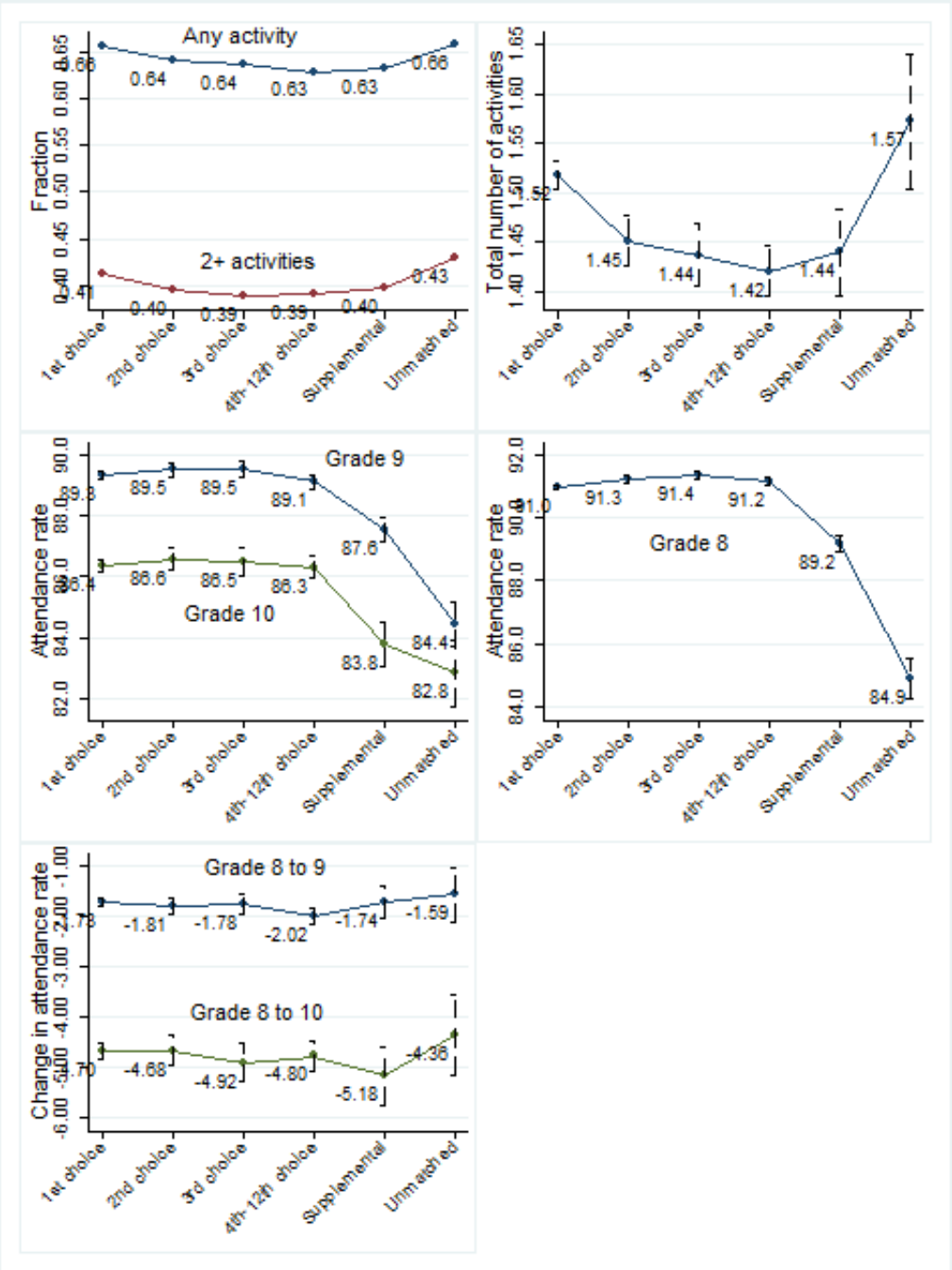

Figure 1: Mean engagement measures, by choice received

Table 1: Public high school program admissions methods in New York City

Admissions method	Description
Screened	Students are ranked by the programs based on their prior years academic record, standardized test scores, and/or attendance.
Educational option	Structured to yield a particular mix of students based on their seventh grade reading achievement. Students are matched and distributed amongst three categories: high (top 16% in reading), middle (mid 68%), and low (bottom 16%). Fifty percent of matches represent students ranked by the school, while the other 50% are matched at random. Students in the top 2% citywide are guaranteed a match to an educational option program if it is their first choice.
Limited unscreened	Students are selected at random by computer. Priority is given to students who attended a school information session or open house.
Audition	Students are ranked based on an audition (e.g. art, design, and performing arts programs) and a review of their academic record.
Zoned	Students living within a specific geographic area are given priority.
Unscreened	Students are matched randomly.
Test	Schools requiring the Specialized High School Admissions Test (e.g. Stuyvesant High School, Brooklyn Tech).

Source: New York City Department of Education

Table 2: High school match outcomes, 2008 and 2009 cohorts

	N	(%)
Offered/accepted		
specialized H.S. or LGA	8,454	5.5
1st choice	74,684	48.5
2nd choice	22,933	14.9
3rd choice	13,551	8.8
4th choice	8,358	5.4
5th choice	5,402	3.5
6th 12th choice	8,173	5.3
Supplemental 1st choice	4,415	2.9
Supplemental 2nd choice	1,587	1.0
Supplemental 3rd choice	912	0.6
Supplemental 4th 12th choice	1,188	0.8
Unmatched	4,470	2.9
Total	154,127	100
Total in base sample	133,387	86.5

Notes: authors' calculations using HSAPS data for the 2008 and 2009 cohorts (year refers to fall of these students' 9th grade year). Students who declined a specialized high school offer and were matched to one of their other choices are included among the other choice counts. The base sample count refers to students who completed the HSAPS process (e.g., did not opt out), were observed in a high school in 9th grade, and were not in a specialized high school.

Table 3: Sample means - students

	Sample 1: Base sample	Sample 2: Oversubscribed Limited Unscreened 1st choice	Sample 3: Matched to Oversubscribed Limited Unscreened
Student characteristics, 8th grade:			
Applicant from private school (%)	4.1	4.1	4.3
Female (%)	49.6	49.8	50.8
Asian (%)	12.1	3.7	3.7
Black (%)	34.1	41.4	44
Hispanic (%)	41.5	48.2	46.8
White (%)	11.6	6	4.7
ELA z-score	-0.084	-0.256	-0.27
Math z-score	-0.087	-0.316	-0.335
Special education (%)	6.6	7.5	7.5
English language learner (%)	10.2	8.6	9
Foreign born (%)	19	12.6	13.3
Eligible for free/reduced lunch (%)	75.9	80.7	81.2
Attendance rate (% of days on roll)	90.8	89.9	89.8
Age (March)	14.1	14.1	14.2
HSAPS choices:			
Number of choices in main round*	7.2	7.9	8.2
Received first choice (%)	52	72.8	58.4
Received choice 1-3 (%)	77	90.3	85.3
HSAPS match:			
Audition or portfolio admissions (%)	4.3	0.7	-
Screened admissions (%)	24	4.2	-
Educational option admissions (%)	33.3	9.4	-
Limited unscreened admissions (%)	22.6	82.7	100
Unscreened admissions (%)	2	0.2	-
Zoned school admissions (%)	13.6	2.8	-
Have school survey, grade 9	77.5	77.4	77.5
Enrolled in matched school	92.7	90.2	88.9
N	133,387	20,543	25,483

Notes: authors' calculations using HSAPS data for the 2008 and 2009 cohorts (year refers to fall of the 9th grade year). See Table 2 for a definition of the base sample. ELA and math score only available for applicants from public schools. "Number of choices" is conditional on having any choices in the main round (a small minority of students did not begin participating until the supplemental round).

Table 4: Descriptive statistics: engagement measures

	Sample 1: Mean	Sample 1: SD	Sample 2: Mean	Sample 2: SD
Behavioral Engagement: Attendance:				
Percent of days in attendance, 8th grade	90.8	9.8	89.9	9.9
Percent of days in attendance, 9th grade	89.1	15.7	87.8	15.9
Percent of days in attendance, 10th grade (2008 cohort only)	86.2	19.3	84.7	19.7
Change in attendance, 8th-9th grade	-1.7	10.9	-2.1	11.5
Change in attendance, 8th-10th grade (2008 cohort only)	-4.5	14.5	-5.1	15.1
Behavioral Engagement: Extracurricular activities:				
Number of extracurricular activities	1.48	1.63	1.57	1.62
Participated in any extracurricular activity	0.65	0.48	0.68	0.47
Participated in two or more extracurricular activities	0.40	0.49	0.44	0.50
Academic Engagement:				
Total credits attempted	14.5	4.4	14.7	4.6
Total credits earned	11.6	5.4	11.8	5.4
Total Regents exams attempted	1.5	1.2	1.5	1.3
Total Regents exams passed (65)	1.0	1.0	0.9	1.0
GPA (0-100)	71.4	17.2	70.3	17.6
Cognitive engagement measures:				
Mean score	3.06	0.51	3.1	0.51
Adults ... help me understand what I need to do to succeed	3.17	0.70	3.2	0.70
Teachers encourage me to succeed	3.17	0.71	3.21	0.72
I need to work hard to get good grades at my school	3.45	0.70	3.47	0.70
My school helps me to develop challenging academic goals	2.88	0.74	2.91	0.74
Someone helps me understand what courses I need	3.11	0.76	3.14	0.75
My teachers expect me to continue my education after HS	3.27	0.74	3.31	0.74
My HS provides helpful career/college counseling	2.97	0.83	2.99	0.84
Teachers in my school treat students with respect	2.90	0.82	2.95	0.81
My teachers enjoy the subjects they teach	3.12	0.75	3.17	0.75
My teachers inspire me to learn	2.86	0.82	2.9	0.83
My teachers give me extra help when I need it	3.06	0.79	3.15	0.77
My teachers connect what I am learning to life outside the classroom	2.80	0.87	2.83	0.87
Psychological engagement measures (1): teachers:				
Mean score	2.64	0.77	2.66	0.77
Comfortable talking to teachers about class problem	2.63	0.98	2.64	0.99
Comfortable talking to teachers about something bothering you	2.44	1.02	2.45	1.03
Teachers available to talk about class problem	2.79	0.93	2.81	0.93
Teachers available to talk about something bothering you	2.71	0.95	2.73	0.95
Psychological engagement measures (2): peer culture:				
Mean score	2.46	0.67	2.44	0.66
Students who get good grades respected by other students	2.58	0.83	2.56	0.83
Students in my school treat teachers with respect	2.36	0.89	2.35	0.88
Students help and care about each other	2.46	0.87	2.44	0.87
Students respect other students	2.43	0.85	2.41	0.86

Notes: authors' calculations using NYCDOE School Survey and HSAPS data for the 2008 and 2009 cohorts (year refers to fall of the 9th grade year). See Table 2 for a definition of the base sample. Sample size varies depending on response rates to individual items, but approximately 20,400 students are included in the 9th grade attendance rate statistics and 15,700 students are included in the 9th grade survey means.

Table 5: Engagement factors

Survey year	Group	Factor	Reliability Coefficient (Alpha)	Mean	SD
2009	All H.S. students	Cognitive	0.89	3.04	0.52
		Psychological (1)	0.81	2.66	0.78
		Psychological (2)	0.78	2.5	0.67
	2008 cohort	Cognitive	0.88	3.04	0.51
		Psychological (1)	0.79	2.61	0.76
		Psychological (2)	0.77	2.42	0.67
2010	All H.S. students	Cognitive	0.89	3.09	0.52
		Psychological (1)	0.82	2.71	0.78
		Psychological (2)	0.79	2.58	0.67
	2008 cohort	Cognitive	0.89	3.08	0.5
		Psychological (1)	0.8	2.66	0.76
		Psychological (2)	0.78	2.49	0.66

Notes: Alphas are based on standardized items: 12 for the cognitive factor, 4 for the psychological (1) factor (relationships with teachers) and 4 for the psychological (2) factor (perception of peer culture).

Table 6: Regression results—first choice effects, base sample (all school types)

Outcome:	Models w/o school effects	Models with school effects	N
Attendance in grade 9	0.95***	0.48***	120,200
	(0.08)	(0.097)	
Any activities	0.03***	0.01	92,397
	(0.004)	(0.005)	
Two or more activities	0.04***	0.01	92,397
	(0.004)	(0.005)	
Number of activities	0.13***	0.02	92,397
	(0.014)	(0.02)	
Cognitive engagement	0.05***	0.02***	93,371
	(0.004)	(0.005)	
Psychological engagement (1)	0.04***	0.02***	93,772
	(0.006)	(0.008)	
Psychological engagement (2)	0.11***	0.004	93,888
	(0.005)	(0.006)	
Credits attempted	0.11***	0.11***	120,615
	(0.029)	(0.029)	
Credits earned	0.22***	0.24***	120,615
	(0.035)	(0.038)	
Regents attempted	-0.06***	0.007	120,615
	(0.008)	(0.008)	

Note: each cell represents a coefficient estimate from a separate regression, where the indicated outcome variable is used. Models include student-level covariates, cohort dummies, and program type effects. Standard errors are in parentheses.

Table 7: Regression results—first choice effects, applicants to oversubscribed limited unscreened schools

Outcome:	Models w/o school effects	Models with school effects	N
Attendance in grade 9	1.16***	0.57***	19,065
	(0.29)	(0.39)	
Any activities	0.034***	0.023	14,881
	(0.013)	(0.018)	
Two or more activities	0.017	0.008	14,881
	(0.014)	(0.019)	
Number of activities	0.075	0.077	14,881
	(0.045)	(0.062)	
Cognitive engagement	0.047***	-0.004	14,691
	(0.014)	(0.020)	
Psychological engagement (1)	0.047***	-0.007	14,764
	(0.021)	(0.030)	
Psychological engagement (2)	0.085***	-0.01	14,791
	(0.018)	(0.025)	
Credits attempted	0.392***	0.017	19,143
	(0.100)	(0.112)	
Credits earned	0.951***	0.437**	19,143
	(0.118)	(0.150)	
Regents attempted	0.144***	0.044	19,143
	(0.027)	(0.032)	

Note: each cell represents a coefficient estimate from a separate regression, where the indicated outcome variable is used. Models include student-level covariates, and cohort dummies. Standard errors in parentheses.

Table 8: Regression results—effects relative to first choice, students matched to an oversubscribed limited unscreened school (models with school effects)

Outcome:	2nd choice	3rd choice	4th-12th choice
Attendance in grade 9	0.105	-0.129	-0.548*
	(0.210)	(0.272)	(0.234)
Any activities	-0.012	-0.030*	-0.022*
	(0.010)	(0.013)	(0.011)
Two or more activities	-0.003	-0.019	-0.016
	(0.010)	(0.013)	(0.011)
Number of activities	0.028	-0.034	-0.023
	(0.033)	(0.044)	(0.037)
Cognitive engagement	-0.005	-0.017	-0.030*
	(0.010)	(0.014)	(0.012)
Psychological engagement (1)	-0.005	-0.027	-0.001
	(0.016)	(0.021)	(0.018)
Psychological engagement (2)	-0.002	0.012	0.012
	(0.013)	(0.017)	(0.015)
Credits attempted	-0.031	-0.197*	-0.347***
	(0.070)	(0.091)	(0.078)
Credits earned	-0.092	-0.235*	-0.424***
	(0.086)	(0.111)	(0.096)
Regents attempted	0.001	-0.020	-0.013
	(0.018)	(0.023)	(0.020)

Note: each cell represents a coefficient estimate from a separate regression, where the indicated outcome variable is used. Models include student-level covariates, and cohort dummies. Standard errors in parentheses.