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BREAKING TIES:
REGRESSION DISCONTINUITY DESIGN MEETS MARKET DESIGN

By

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Breaking Ties:
Regression Discontinuity Design Meets Market Design*

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March 6, 2019

Abstract

Centralized school assignment algorithms must distinguish between applicants with the same preferences and priorities. This is done with randomly assigned lottery numbers, non-lottery tie-breakers like test scores, or both. The New York City public high school match illustrates the latter, using test scores, grades, and interviews to rank applicants to screened schools, combined with lottery tie-breaking at unscreened schools. We show how to identify causal effects of school attendance in such settings. Our approach generalizes regression discontinuity designs to allow for multiple treatments and multiple running variables, some of which are randomly assigned. Lotteries generate assignment risk at screened as well as unscreened schools. Centralized assignment also identifies screened school effects away from screened school cutoffs. These features of centralized assignment are used to assess the predictive value of New York City's school report cards. Grade A schools improve SAT math scores and increase the likelihood of graduating, though by less than OLS estimates suggest. Selection bias in OLS estimates is egregious for Grade A screened schools.

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1 Introduction

Large urban school districts increasingly use sophisticated matching mechanisms to assign their seats. In addition to producing fair and transparent admissions decisions, centralized assignment schemes offer a unique resource for research and accountability: the data they generate can be used to construct unbiased estimates of school value-added. This research dividend arises from the *tie-breaking* embedded in centralized matching. A commonly used school matching scheme, deferred acceptance (DA), takes as input information on applicant preferences and school priorities. In settings where slots are scarce, tie-breaking variables distinguish between applicants who have the same preferences and are subject to the same priorities. Holding preferences and priorities fixed, stochastic tie-breakers become a source of quasi-experimental variation in school assignment.

Many districts break ties with a single random variable, often described as a "lottery number". Abdulkadiroğlu, Angrist, Narita and Pathak (2017b) show that lottery tie-breaking assigns students to schools as in a stratified randomized trial. That is, conditional on preferences and priorities, admission offers generated by such systems are randomly assigned and therefore independent of potential outcomes. In practice, however, preferences and priorities, which we call applicant *type*, are too finely distributed for full non-parametric conditioning to be useful. The key to a feasible DA-based research design is the *DA propensity score*, defined as the probability of school assignment conditional on preferences and priorities. In a match with lottery tie-breaking, conditioning on the scalar DA propensity score is sufficient to make assignment ignorable, that is, independent of potential outcomes. Moreover, because the DA propensity score for a market with lottery tie-breaking depends on only a few school-level *cutoffs*, the score distribution is much coarser than the distribution of types.

We turn here to the problem of crafting research designs from a broad class of assignment mechanisms in which the tie-breaking variable is non-random and potentially correlated with unobserved potential outcomes. Non-random tie-breaking, used for school assignment in Boston, Chicago, and New York City, raises important challenges for causal inference in matching markets.[1] Most importantly, seat assignment under non-random tie-breaking is no longer ignorable conditional on type. Exam schools, for instance, select students with higher test scores, and these high-scoring students can be expected to do well no matter where they go to school. In regression discontinuity (RD) parlance, the *running variable* used to distinguish between applicants of the same type is a source of omitted variables bias (OVB).

Other barriers to causal inference in this setting are raised by the fact that the propensity score in a general tie-breaking scenario depends on the unknown distribution of tie-breakers for

[1]Non-lottery tie-breaking embedded in centralized assignment schemes has been used in econometric research on schools in Chile (Hastings, Neilson and Zimmerman, 2013; Zimmerman, 2019), Ghana (Ajayi, 2014), Italy (Fort, Ichino and Zanella, 2016), Kenya (Lucas and Mbiti, 2014), Norway (Kirkeboen, Leuven and Mogstad, 2016), Romania (Pop-Eleches and Urquiola, 2013), Trinidad and Tobago (Jackson, 2010, 2012; Beuermann, Jackson and Sierra, 2016), and the U.S. (Abdulkadiroğlu, Angrist and Pathak, 2014; Dobbie and Fryer, 2014; Barrow, Sartain and de la Torre, 2016). These studies treat different schools and tie-breakers in isolation, without exploiting centralized assignment. Other related work considers estimation methods in regression discontinuity designs with multiple assignment variables and multiple cutoffs (Papay, Willett and Murnane, 2011; Zajonc, 2012; Wong, Steiner and Cook, 2013; Cattaneo, Titiunik, Vazquez-Bare and Keele, 2016).

each applicant type. This means that the propensity score under general tie-breaking may be no coarser than the underlying type distribution. Moreover, with an unknown distribution of tie-breakers, we cannot easily estimate the propensity score by simulation. These problems are solved here by integrating the non-parametric RD framework introduced by Hahn, Todd and Van der Klaauw (2001) with the large-market matching model used to study random tie-breaking in Abdulkadiroğlu *et al.* (2017b).[2] Our results provide an easily-implemented framework for a wide variety of assignment schemes with multiple cutoffs and multiple running variables, some of which may be randomly assigned.[3]

The research value of a matching market with general tie-breaking is demonstrated through an investigation of the predictive value of New York City (NYC) high school report cards. Specifically, we exploit variation generated by the NYC high school match, which uses a DA mechanism that integrates distinct non-lottery "screened school" tie-breaking with a common lottery tie-breaker at "unscreened schools". The quasi-experimental assignment variation generated by this system is used here to answer questions about school quality in a two-stage least squares (2SLS) setup.

Our results show that attendance at one of NYC's "Grade A schools" boosts SAT math scores modestly and may have a small effect on high school graduation. These effects are smaller than the corresponding ordinary least squares (OLS) estimates of Grade A value-added. Grade A attendance also boosts measures of college and career readiness. The practical utility of our approach is seen in the markedly increased precision of estimates that exploit all sources of assignment risk. Motivated by the ongoing debate over screened admissions policies in public schools, we also compare 2SLS estimates of Grade A effects computed separately for screened and unscreened schools. These are similar, but OLS estimates showing a large Grade A screened school advantage are especially misleading. Finally, we address concerns that RD effects identified solely for applicants close to screened school cutoffs might be idiosyncratic. Specifically, we show that centralized assignment identifies screened school effects for applicants with tie-breakers away from screened school cutoffs.

2 School Choice Experiments

School assignment problems are defined by a set of applicants, schools, and school capacities. Applicants have preferences over schools while schools have priorities over applicants. For example, schools may prioritize applicants who live nearby or with currently enrolled siblings. Let $s = 0, 1, ..., S$ index schools, where $s = 0$ represents an outside option. The letter I denotes a set of applicants, indexed by i. I may be finite or, in our large-market model, a continuum, with applicants indexed by values in the unit interval. Seating is constrained by a capacity vector, $q = (q_0, q_1, q_2, ..., q_S)$, where q_s is defined as the proportion of I that can be seated at school s. We assume $q_0 = 1$.

[2] We also build upon the "local random assignment" interpretation of nonparametric RD, discussed by Frölich (2007); Cattaneo, Frandsen and Titiunik (2015); Cattaneo, Titiunik and Vazquez-Bare (2017); Frandsen (2017) and Sekhon and Titiunik (2017). See Lee and Lemieux (2010) for a survey of RD methods.

[3] Large-market results for the special case of serial dictatorship with a single non-random tie-breaker are sketched in Abdulkadiroğlu, Angrist, Narita, Pathak, and Zarate (2017a).

Applicant i's preferences over schools constitute a partial ordering, $\succ_i$, where $a \succ_i b$ means that i prefers school a to school b. Each applicant is also granted a priority at every school. Let $\rho_{is} \in \{1, ..., K, \infty\}$ denote applicant i's priority at school s, where $\rho_{is} < \rho_{js}$ means school s prioritizes i over j. For instance, $\rho_{is} = 1$ might encode the fact that applicant i has sibling priority at school s, while $\rho_{is} = 2$ encodes neighborhood priority, and $\rho_{is} = 3$ for everyone else. We use $\rho_{is} = \infty$ to indicate that i is ineligible for school s. Many applicants share priorities at a given school, in which case $\rho_{is} = \rho_{js}$ for some $i \neq j$. The vector $\rho_i = (\rho_{i1}, ..., \rho_{iS})$ records applicant i's priorities at each school.

Applicant type is defined as $\theta_i = (\succ_i, \rho_i)$, that is, the combination of an applicant's preferences and priorities at all schools. We say that an applicant of type θ has preferences $\succ_\theta$ and priorities ρ_θ. Θ denotes the set of possible types. A *mechanism* is a rule determining assignment as a function of type and a set of tie-breaking variables that schools use to discriminate between applicants of the same type.

In our framework, tie-breakers and priorities are distinct because the latter are fixed, while the former are modeled as random variables. Resampling tie-breakers makes the mechanisms of interest to us stochastic: the assignment distribution generated by any stochastic mechanism is induced by the distribution of tie-breakers. In particular, stochastic mechanisms generate a probability or "risk" of assignment for each applicant to each school. *Assignment risk* is created by repeatedly drawing tie-breakers from each applicant's tie-breaker distribution and re-running the match, fixing other market features.

Tie-breakers may be uniformly distributed lottery numbers, in which case they're distributed independently of type, or variables like entrance exam scores, that depend on type. With lottery tie-breaking, the relevant distribution is a permutation distribution under which all applicant orderings are equally likely. Tie-breakers overlap with the concept of a running variable in simple RD-style research designs. We prefer the term "tie-breaker" because this highlights the role such variables play in a centralized match. As is typical of RD, non-lottery tie-breakers in school choice are not uniformly distributed, and may depend on applicant characteristics like race or potential outcomes, as well as on type.

To describe assignment risk more formally, consider first a market with a single continuously distributed tie-breaker common to all schools, denoted R_i for applicant i. Although R_i is not necessarily uniform, we assume that it's scaled (preserving position or rank) to be distributed over $[0, 1]$, with continuously differentiable cumulative distribution function F_R^i (an assumption we maintain throughout). These common support and smoothness assumptions notwithstanding, tie-breakers may be correlated with type, so that R_i and R_j for applicants i and j are not necessarily identically or uniformly distributed, though they're assumed to be independent of one another.[4]

By the law of iterated expectations, the probability type θ applicants have a tie-breaker below any value r is $F_R(r|\theta) \equiv E[F_R^i(r)|\theta_i = \theta]$, where $F_R^i(r)$ is F_R^i evaluated at r and the expectation is assumed to exist. To be concrete, suppose that the tie-breaker is a test score.

[4]We assume that sets of applicants of the form $\{i|a < R_i \leq b\}$, where a and b are constants, are measurable. A sufficient condition for this is that the mapping from $i \in [0, 1]$ to $R_i \in [0, 1]$ be left continuous (Aliprantis and Border, 2006). This is satisfied by reordering applicants by their tie-breaker realizations.

Suppose also that type θ_0 applicants do exceptionally well on tests and therefore have tie-breaker values drawn from a distribution with higher mean than the score distribution for type θ_1. This implies $F_R(r|\theta_0) \neq F_R(r|\theta_1)$. By contrast, when R_i is a lottery number drawn independently from the same distribution for all applicants, $F_R(r|\theta) = F_R^i(r) = r$ for any $r \in [0,1]$ and for all i and θ. Although lottery tie-breaking is important, many real-world markets diverge from this.

2.1 OVB from Type and Tie-Breakers

Suppose we'd like to estimate the causal effect of attendance at school s on the likelihood of high school graduation. Under centralized assignment, offers of a seat at s are determined solely by type and tie-breakers. These variables are therefore the only confounding factors that might compromise causal inference. Provided we can eliminate OVB from these two sources, the offers generated by centralized assignment become powerful instrumental variables that identify causal effects of school attendance.

Our causal quest begins with strategies that eliminate OVB from type. Even in a market with lottery tie-breaking, students who list schools differently are likely to have different potential outcomes (many applicants prefer a neighborhood school, for example). On the other hand, since lottery tie-breakers are independent of potential outcomes, type is the only source of OVB in this case. Full-type conditioning therefore eliminates OVB in markets with lottery tie-breaking. In practice, however, matching markets typically have many types (almost as many as applicants in some cases), rendering full-type conditioning impractical. We therefore exploit the fact that the OVB induced by correlation between type and school offers is controlled by conditioning on a scalar function of type, the propensity score.[5]

To formalize the argument for propensity score conditioning in analyses of school choice, let $D_i(s)$ indicate whether applicant i is offered a seat at school s. The propensity score for school assignment is the conditional probability of assignment to s, which can be written

$$p_s(\theta) = E[D_i(s)|\theta_i = \theta].$$

The expectation here is computed using the distribution of tie-breakers. The probability $p_s(\theta)$ quantifies the "risk" of assignment to s faced by an applicant of type θ in repeated executions of a match, drawing tie-breakers anew each time; empirical models that control for $p_s(\theta)$ are likewise said to "control for risk."

Now, let W_i be any random variable independent of lottery numbers. This includes potential outcomes as well as applicant demographic characteristics. Lottery tie-breaking implies

$$P[D_i(s) = 1|\theta_i = \theta, W_i] = E[D_i(s) = 1|\theta_i = \theta] = p_s(\theta), \tag{1}$$

where $P[D_i(s) = 1|\cdot]$ is the conditional relative frequency of assignment to s determined by all possible lottery draws for subsets of applicants. Iterating expectations over type, (1) yields

$$P[D_i(s) = 1|p_s(\theta) = p, W_i] = p. \tag{2}$$

[5] Use of propensity score conditioning to control OVB originates with Rosenbaum and Rubin (1983).

In other words, control for risk makes assignment independent of W_i, eliminating OVB. This conditional independence (CI) relation means that in school choice markets with lottery tie-breaking, empirical strategies that control for risk identify causal effects.

Equation (2) provides a valuable foundation for causal inference. With lottery tie-breaking, $p_s(\theta)$ is typically a function of a few key cutoffs. This coarseness makes score-conditioning preferable to full type conditioning. With non-lottery tie-breaking, however, control for the propensity score fails to eliminate all sources of OVB: the tie-breaker itself is an omitted variable. Moreover, it no longer need be true that $p_s(\theta)$ has support coarser than θ. Finally, with unknown tie-breaker distributions, $p_s(\theta)$ is hard to estimate reliably. These problems are solved here by (a) using a theoretical propensity score to isolate the set of cutoffs that generate assignment risk; (b) focusing on applicants near these cutoffs. In a limit computed by shrinking bandwidths around relevant cutoffs, applicants have constant non-degenerate risk of clearing cutoffs even when tie-breakers are variables like test scores that are correlated with potential outcomes.

We illustrate this fundamental result in a simple scenario with three screened schools, A, B, and C, each of which uses a common non-lottery tie-breaker, a test score, say, to select applicants. Let R_i denote the tie-breaker. The assignment mechanism in this example is *serial dictatorship* (SD), with applicants ordered by the tie-breaker.

SD, a version of DA without priorities, works like this:

> Order applicants by tie-breaker. Proceeding in order, offer each applicant his or her most preferred school with seats remaining.

Like any mechanism in the DA class (defined below), SD generates a set of *randomization cutoffs*, denoted τ_s for school s. For any school s that ends up full, cutoff τ_s is given by the tie-breaker of the last student offered a seat at s. Otherwise, $\tau_s = 1$. Finite-market cutoffs are typically random, that is, they depend on the distribution of lottery draws. In large "continuum" markets, however, cutoffs are constant, a result that motivates our use of the continuum model.[6]

Suppose applicants differ in their preferences over B and C, but all list A first and that there are more applicants than seats at A (imagine A is a prestigious selective school). This market has two types of applicants, those who list B second and those who list C second. With everyone listing A first, SD assigns A to any applicant with R_i below the school-A randomization cutoff, τ_A. The propensity score for assignment to school A is therefore

$$p_A(\theta) = E[1(R_i \leq \tau_A)|\theta] = F_R(\tau_A|\theta).$$

This simple score nevertheless depends on the unknown distribution $F_R(\tau_A|\theta)$, itself a function of θ. Type is therefore a source of OVB; applicants preferring B to C might live in better neighborhoods and have higher test scores, for example. It's also clear that any applicant who does well on tests is more likely to be offered a seat at A. Nevertheless, Proposition 1 below shows that for applicants in a δ-neighborhood of τ_A, assignment risk converges to 0.5 as δ goes to zero, and equals 0 or 1 otherwise.

[6] Abdulkadiroğlu *et al.* (2017b) explores alternative justifications of the continuum model.

The "local risk" of qualification at A is formalized by partitioning the support of tie-breaker R_i into intervals around τ_A. Given bandwidth δ, these intervals are defined by

$$t_{iA}(\delta) = \begin{cases} n & \text{if } R_i > \tau_A + \delta \\ a & \text{if } R_i \leq \tau_A - \delta \\ c & \text{if } R_i \in (\tau_A - \delta, \tau_A + \delta]. \end{cases} \tag{3}$$

To establish the conditional independence properties of local risk, let W_i be any applicant characteristic, such as demographic characteristics and potential outcomes, that is unchanged by school assignment. This includes tie-breakers other than the one in use at school s.[7] Proposition 1 shows that for all applicant types and conditional on W_i, local risk is constant at 0.5 or degenerate:

Proposition 1. *Assume that τ_A is fixed. Let $F_R(\cdot|\theta, w) = E[F_R^i(\cdot)|\theta_i = \theta, W_i = w]$ and note that $F_R(\cdot|\theta, w)$ is differentiable at τ_A for every θ and w by virtue of continuous differentiability of $F_R^i(r)$. We also assume that $F_R'(\tau_A|\theta, w) \neq 0$. Then, for $t \in \{n, a, c\}$, all θ, and all w,*

$$\lim_{\delta \to 0} E[1(R_i \leq \tau_A)|\theta_i = \theta, t_{iA}(\delta) = t, W_i = w] = \psi_A(\theta, t),$$

where

$$\psi_A(\theta, t) = \begin{cases} 0 & \textit{if } t = n \\ 1 & \textit{if } t = a \\ 0.5 & \textit{if } t = c. \end{cases} \tag{4}$$

Proposition 1 is a restatement of results in Frölich (2007), which shows that limiting qualification risk at a single cutoff is constant at one-half, and in an unpublished draft of Frandsen (2017), which shows something similar for an asymmetric bandwidth. These earlier results omit conditioning variables and degenerate cases; for reference, our version is proved in the appendix.

The arguments of function $\psi_A(\theta, t)$ include applicant type because risk in more complicated matches (and for applicants who list A below first in this simple example) depends on type. Our formulation of Proposition 1 highlights the fact that risk is independent of confounding variables, potential outcomes, and other tie-breakers. The latter property helps us describe risk concisely in models with multiple tie-breakers. Proposition 1 can also be rewritten to show local conditional independence given the propensity score, a result stated below as a corollary:

Corollary 1 (Local Conditional Independence)**.** *Let $D_i(A) = 1(R_i \leq \tau_A)$. Then,*

$$\lim_{\delta \to 0} P[D_i(A) = 1|\theta_i = \theta, t_{iA}(\delta) = t, W_i = w, \psi_A(\theta, t) = p] = p$$

for $p \in \{0, 0.5, 1\}$.

This follows by observing that

$$P[D_i(A) = 1|\theta_i = \theta, t_{iA}(\delta) = t, W_i = w, \psi_A(\theta, t)] = P[D_i(A) = 1|\theta_i = \theta, t_{iA}(\delta) = t, W_i = w],$$

[7] Let $W_i = W_{0i}(1 - D_i(s)) + W_{1i}D_i(s)$, where W_{0i} is the potential value of W_i revealed when $D_i(s) = 0$, and W_{1i} is the potential value revealed when $D_i(s) = 1$. Then W_i is unchanged by school assignment when $W_{0i} = W_{1i}$ for all i. Covariates unchanged by school assignment are independent of lottery tie-breakers.

and then taking the limit of the right hand side. In this simple example, to know t is to know p, but the conditional independence described in the corollary carries over to more elaborate matches.

Corollary 1 formalizes the idea of "local random assignment" suggested by Cattaneo *et al.* (2015, 2017) and Sekhon and Titiunik (2017). As noted by Sekhon and Titiunik (2017), most theoretical work on nonparametric RD identification relies on continuity of conditional expectation functions for potential outcomes rather than restrictions on the assignment mechanism. Here, random assignment is a consequence of the fact that, given continuous differentiability of the tie-breaker distribution function, the tie-breaker density is approximately uniform in small enough neighborhoods around the cutoff.[8]

Proposition 1 is a key building block for more elaborate statements of risk. The limiting nature of this theoretical result raises the question of whether Proposition 1 and its corollary have an operational, empirical counterpart. We demonstrate the empirical conditional independence property stated in the corollary by evaluating qualification risk for a particular school in windows of various sizes around this school's cutoff (we say an applicant is empirically qualified at school s when he or she clears τ_s, without regard to school assignment).

Figure 1 describes qualification risk (rates) for applicants to one of NYC's most selective screened schools, Townsend Harris (TH). The top panel of the figure compares the probability of clearing the TH cutoff for two applicant types, those who list TH first and those who list it lower.[9] As can be seen in the left pair of bars in the top panel, applicants who list TH first tend to be high achievers and are therefore more likely than others to qualify for a seat at TH.

In a sample of applicants near the TH cutoff, qualification rates for the two types are closer. Specifically, for the sample of TH applicants with tie-breaker values inside an Imbens and Kalyanaraman (2012) (IK) bandwidth around the cutoff, qualification rates differ by only a few points. Moreover, cutting the window width to 75% of its original size and then in half leads to further convergence in qualification rates, with rates in both of these narrower groups remarkably close to 0.5. This is the convergence in assignment rates predicted by Proposition 1.

The middle and bottom panels of Figure 1 document qualification rate equalization near cutoffs for groups of TH applicants defined by baseline scores rather than by type. The leftmost pair of bars compares all TH applicants in the upper and lower quartiles of the baseline math and ELA (reading) score distributions, without regard to cutoff proximity. Not surprisingly, applicants with high baseline math scores are far more likely to qualify for a seat at TH than are applicants with low baseline math scores. The qualification gap by baseline scores narrows for applicants with tie-breaker values in an IK bandwidth, however, and again approaches 0.5 for both groups as the window width is cut to 75% of its original size and then halved.

It's noteworthy that the IK bandwidth in this case is insufficiently narrow to equalize qualification rates across baseline score groups. In practice, most RD applications use a data-driven bandwidth combined with local linear regression to minimize bias. Our empirical strategy likewise uses an IK bandwidth to compute locally regression-adjusted comparisons that also condi-

[8]The empirical consequences of possible jumps and holes in screened school tie-breaker distributions are explored in the online appendix.

[9]Although TH runs only one program, it has a new cutoff each year. Qualified applicants in the figure clear the cutoff for the year they apply.

tion on the score. As in Robins (2000) and Okui, Small, Tan and Robins (2012), this strategy amounts to a doubly-robust estimator. We control for theoretical propensity scores, while also regression-adjusting for tie-breaker effects in case score control is imperfect. The covariate balance tests and robustness checks reported below suggest this approach works well.

2.2 Risk in Serial Dictatorship

Our TH example illustrates local risk. But real school matching problems involves many cutoffs and a rich variety of types. We explain real-world risk determination in two steps. First, as in Abdulkadiroğlu, Che and Yasuda (2015) and Azevedo and Leshno (2016), we employ a large-market model with a unit continuum of applicants to characterize global assignment risk. The continuum can be interpreted as the limit of a sequence that repeatedly doubles the number of applicants of each type while doubling each school's capacity. In the continuum, randomization cutoffs are fixed, that is, cutoffs are the same across repeated executions of the match with tie-breakers re-drawn each time. As in Abdulkadiroğlu *et al.* (2017b), the continuum model reveals which randomization cutoffs matter for each applicant facing risk at school s. Having identified which of these cutoffs are relevant for risk determination, we evaluate risk for applicants with tie-breakers close to them.

This strategy is outlined first for a realistic version of SD with many schools and types. In SD, applicants seated at school s qualify there and are (necessarily) disqualified at schools they like better. The building blocks for risk at school s are therefore (a) the cutoff at s and (b) cutoffs at schools preferred to s. The latter are characterized by a quantity we call *most informative disqualification* (MID), which tells us how the tie-breaker distribution among type θ applicants to s is truncated by offers at schools θ prefers to s. Formally, let Θ_s denote the set of applicant types who list s and let

$$B_{\theta s} = \{s' \in S \mid s' \succ_\theta s\} \text{ for } \theta \in \Theta_s \tag{5}$$

denote the set of schools type θ prefers to s. For each type and school, $MID_{\theta s}$ is a function of randomization cutoffs at schools in $B_{\theta s}$, specifically:

$$MID_{\theta s} \equiv \begin{cases} 0 & \text{if } B_{\theta s} = \emptyset \\ \max\{\tau_b \mid b \in B_{\theta s}\} & \text{otherwise.} \end{cases} \tag{6}$$

$MID_{\theta s}$ is zero when school s is listed first since all who list s first compete for a seat there. The second line reflects the fact that an applicant who lists s second is seated there only when disqualified at the school they've listed first, while applicants who list s third are seated there when disqualified at their first and second choices, and so on. Moreover, anyone who fails to clear cutoff τ_b is surely disqualified at schools with lower (less forgiving) cutoffs. For example, applicants who fail to qualify at a school with a cutoff of 0.5 are disqualified at schools with cutoffs below 0.5. We can therefore quantify the truncation induced by disqualification at schools preferred to s by recording the most forgiving cutoff among them.

Type θ cannot be seated at s when $MID_{\theta s} > \tau_s$ because those qualified at s can do better (they qualify at the school that determines $MID_{\theta s}$). This scenario is sketched in the top panel

of Figure 2. Assignment risk when $MID_{\theta s} \leq \tau_s$ is the probability that

$$MID_{\theta s} < R_i \leq \tau_s,$$

an event sketched in the middle panel of Figure 2. We summarize these facts in the following proposition, which is implied by a more general result for DA derived in the next section.

Proposition 2 (Global Score in Serial Dictatorship)**.** *Consider serial dictatorship in a continuum market. For all s and $\theta \in \Theta_s$, we have:*

$$p_s(\theta) = \max\left\{0, F_R(\tau_s|\theta) - F_R(MID_{\theta s}|\theta)\right\}.$$

SD assignment risk, which is positive only when when the randomization cutoff at s exceeds $MID_{\theta s}$, is given by the size of the group with R_i between $MID_{\theta s}$ and τ_s. This is

$$F_R(\tau_s|\theta) - F_R(MID_{\theta s}).$$

With lottery tie-breaking (and a uniformly distributed lottery number), the SD risk formula simplifies to $\tau_s - MID_{\theta s}$. With non-lottery tie-breaking, the SD propensity score depends on the conditional distribution function, $F_R(\cdot|\theta)$, evaluated at τ_s and $MID_{\theta s}$.

Proposition 2 leaves us with three empirical challenges not encountered in markets with lottery tie-breaking. First, with non-random tie-breakers like test scores, conditional tie-breaker distributions, $F_R(.|\theta)$, are likely to depend on θ, so the score in Proposition 2 need not have coarser support than does θ. This is in spite of the fact many applicants with different values of θ share the same $MID_{\theta s}$. Second, $F_R(.|\theta)$ is typically unknown. This precludes straightforward computation of the propensity score by repeatedly sampling from $F_R(.|\theta)$. Finally, while control for the propensity score eliminates confounding from type, assignments are a function of tie-breakers as well as type, and non-lottery tie-breakers are likely to be correlated with potential outcomes.

As in the simple example in the previous section, we address these challenges by evaluating risk for applicants close to cutoffs. Proposition 2 identifies the relevant cutoffs in markets with many schools and types. As before, intervals around each cutoff are encoded by relation (3), but now replacing $t_{iA}(\delta)$ with $t_{is}(\delta)$ for each school, s. We collect the set of these for all schools in the vector

$$T_i(\delta) = [t_{i1}(\delta), ..., t_{is}(\delta), ..., t_{iS}(\delta)]'.$$

The following is a consequence of Theorem 1 in the next section, which characterizes local risk for any DA match.

Proposition 3 (Local Score in Serial Dictatorship)**.** *Consider serial dictatorship in a continuum market. Assume that cutoffs τ_s are distinct. For each $s \in S$ and $\theta \in \Theta_s$ such that $MID_{\theta s} \neq 0$, suppose $MID_{\theta s} = \tau_{s'}$ for $s' \neq s$. For $T = [t_1, ..., t_s, ..., t_S]' \in \{n, a, c\}^S$, all $\delta > 0$, and all w,*

$$P[D_i(s) = 1|\theta_i = \theta, T_i(\delta) = T, W_i = w] = 0 \text{ if } \tau_{s'} > \tau_s.$$

Otherwise,

$$\lim_{\delta \to 0} P[D_i(s) = 1|\theta_i = \theta, T_i(\delta) = T, W_i = w] = \begin{cases} 0 & \text{if } t_s = n \text{ or } t_{s'} = a \\ 1 & \text{if } t_s = a \text{ and } t_{s'} = n \\ 0.5 & \text{if } t_s = c \text{ or } t_{s'} = c. \end{cases}$$

When $MID_{\theta s} = 0$, risk is determined by t_s alone as in Proposition 1.

Like Proposition 1 and its corollary, Proposition 3 establishes a key conditional independence result: limiting SD assignment risk depends only on tie-breaker proximity to the cutoff at s and to $MID_{\theta s}$; risk is otherwise unrelated to applicant characteristics.[10] Panel C in Figure 2 interprets this result. Type θ applicants with tie-breakers near either $MID_{\theta s}$ or the cutoff at s face risk of one-half. This fact is an extension of Proposition 1, applied here to the *pair* of cutoffs driving SD risk for each type. Applicants with $t_s = a$ and $t_{s'} = n$ have tie-breakers strictly between $MID_{\theta s}$ and τ_s, meaning they're disqualified at s' but qualified at s. Finally, applicants with $t_s = n$ or $t_{s'} = a$ cannot be seated at s, either because they're disqualified there or because they qualify at s'.

In the empirical (as opposed to theoretical) world, almost all applicants necessarily have tie-breaker values that are strictly above or below any particular randomization cutoff. We see applicants with tie-breakers close to either $MID_{\theta s}$ or the cutoff at s as special because it is these applicants for whom qualification is (almost) randomly assigned.

3 The DA Score with General Tie-Breaking

SD is a version of DA without priorities. *Student-proposing DA*, which nests all school choice mechanisms in wide use, works like this:

> Each applicant proposes to his or her most preferred school. Each school ranks these proposals, first by priority then by tie-breaker within priority groups, *provisionally* admitting the highest-ranked applicants in this order up to its capacity. Other applicants are rejected.
>
> Each rejected applicant proposes to his or her next most preferred school. Each school ranks these new proposals *together with applicants admitted provisionally in the previous round*, first by priority and then by tie-breaker. From this pool, the school again provisionally admits those ranked highest up to capacity, rejecting the rest.
>
> The algorithm terminates when there are no new proposals (some applicants may remain unassigned).

With multiple tie-breakers, different schools may order applicants differently, but the DA algorithm is otherwise unchanged. For example, NYC runs a centralized DA match for most of its high schools, a match that includes a diverse set of screened schools (Abdulkadiroğlu, Pathak and Roth, 2005, 2009). These schools order applicants using (mostly) school-specific tie-breakers derived from interviews, auditions, or GPA in earlier grades, as well as test scores. A few screened-school tie-breakers are shared by multiple programs. The NYC match also includes many "unscreened schools" that use a common lottery tie-breaker.

Formal analysis of markets with general tie-breaking requires notation to keep track of the tie-breakers. Let $v \in \{0, 1, ..., V\}$ index tie-breakers and let $\{S_v : v \in \{0, 1, ..., V\}\}$ be a partition

[10] Abdulkadiroğlu *et al.* (2017a) reference a version of Proposition 3 in a brief analysis of Chicago exam schools.

of schools such that S_v is the set of schools using tie-breaker v. Schools s and s' use the same tie-breaker if and only if $s, s' \in S_v$ for some v. The random variable R_{iv} denotes applicant i's tie-breaker at schools in S_v. For any v and students $i \neq j$, tie-breakers R_{iv} and R_{jv} are assumed to be independent when both exist, though not necessarily identically distributed.[11] Likewise, for $v \neq v'$, tie-breakers R_{iv} and $R_{iv'}$ are initially assumed to be independent, an assumption relaxed in Theorem 1 below.

Define the function $v(s)$ to be the index of the tie-breaker used at school s. By definition, $s \in S_{v(s)}$. We adopt the convention that $v = 0$ identifies the lottery tie-breaker, so S_0 denotes the set of unscreened (lottery) schools.

With a continuum of applicants, DA assignment risk depends on priorities as well as on tie-breakers and cutoffs. We therefore combine applicants' priority status and tie-breaking variables into a single number for each school, called *applicant position* at school s:

$$\pi_{is} = \rho_{is} + R_{iv(s)}.$$

Since the difference between any two priorities is at least 1 and tie-breaking variables are between 0 and 1, applicant position at s is a lexicographic ordering, first by priority then by tie-breaker. We also generalize cutoffs to incorporate priorities; these *DA cutoffs* are denoted ξ_s. For any s that ends up full, ξ_s is given by the position of the last student offered a seat at s. Otherwise, $\xi_s = K + 1$.

Our characterization of large-market DA with general tie-breakers follows from the large market model in Abdulkadiroğlu *et al.* (2017b), replacing position as function of a single tie-breaker ($\rho_{is} + R_i$) with the tie-breaker-specific π_{is} defined above.

In the large-market model, DA sets the cutoff to $K + 1$ at any school that remains unfilled and offers a seat at s to any applicant i listing s who has

$$\pi_{is} \leq \xi_s \text{ and } \pi_{ib} > \xi_b \text{ for all } b \succ_i s. \tag{7}$$

This is a consequence of the fact that the student-proposing DA mechanism is stable. In particular, if an applicant is seated at s but prefers b, she must be qualified at s and not have been offered a seat at b. Moreover, since DA-generated offers at b are made in order of position, the fact that she wasn't offered a seat at b means she is disqualified there.

Condition (7) nests our characterization of assignments under SD, since we can set $\rho_{is} = 0$ for all applicants and use a single tie-breaker to determine position. Statement (7) then amounts to saying that $R_i \leq \tau_s$ and $R_i > MID_{\theta s}$ for applicants with $\theta_i = \theta$. In finite markets, cutoffs ξ_s are stochastic, varying from tie-breaker draw to tie-breaker draw in repeated executions of the match. In large (continuum) markets, however, ξ_s is fixed. Equation (7) therefore yields a characterization of assignment risk determined by fixed cutoffs and priorities and by the distribution of stochastic tie-breakers.

Our characterization of DA assignment risk covers all mechanisms in the DA class. Assignments using mechanisms in this class can be *computed* by student-proposing DA, possibly with

[11] Real-world tie-breakers, including those in New York City, are often coded as ranks that may be correlated across applicants, even when the underlying orderings are independent. For example, in a sample of two, it matters that only one can be first. Such dependence vanishes as the number of applicants grows, as we show in Appendix B. Tie-breaker positions therefore satisfy our independence assumption in a continuum market.

applicant priorities replaced by $\boldsymbol{\phi}(\theta_i)$, where $\boldsymbol{\phi} : \Theta \to \mathbb{N}^{|S|}$ is a function of actual priorities. The DA class includes student- and school-proposing DA, serial dictatorship, and the immediate acceptance (Boston) mechanism. This class omits TTC, which need not satisfy equation (7).[12] After any transformation needed to facilitate DA computation, applicant position at school s is

$$\pi_{is} = \boldsymbol{\phi}_s(\theta_i) + R_{iv(s)}.$$

The propensity score can then be computed using this transformed position data. In what follows, we ignore any necessary transformations, continuing to denote priorities by ρ_{is}.

The propensity score for DA uses the notion of *marginal priority* at school s, denoted ρ_s and defined as $\text{int}(\xi_s)$, that is, the integer part of the DA cutoff. Applicants for whom seats are rationed by tie-breakers have priority ρ_s. Conditional on rejection by all more preferred schools, applicants to s are assigned s with certainty if $\rho_{is} < \rho_s$, that is, if they clear marginal priority. Applicants with $\rho_{is} > \rho_s$ have no chance of finding a seat at s. Applicants for whom $\rho_{is} = \rho_s$ are marginal: these applicants are seated at s when their tie-breaker values fall below randomization cutoff τ_s, which can now be written as the decimal part of the DA cutoff:

$$\tau_s = \xi_s - \rho_s.$$

When $\rho_{is} = \rho_s$,

$$\pi_{is} \leq \xi_s \Leftrightarrow R_{iv(s)} \leq \tau_s.$$

Again, this covers SD, since ρ_{is} can be fixed at zero for everyone.

These observations motivate a partition of the set of applicant types. Specifically, partition Θ_s, the set of applicant types who list s, according to:

i) $\Theta_s^n = \{\theta \in \Theta_s \mid \rho_{\theta s} > \rho_s\}$, (*never seated*)

ii) $\Theta_s^a = \{\theta \in \Theta_s \mid \rho_{\theta s} < \rho_s\}$, (*always seated*)

iii) $\Theta_s^c = \{\theta \in \Theta_s \mid \rho_{\theta s} = \rho_s\}$. (*conditionally seated*)

Never seated applicants have worse-than-marginal priority at s, so no one in this group is assigned to s. *Always seated* applicants clear marginal priority at s. Some of these applicants may end up seated at a school they prefer to s, but they're assigned s for sure if they fail to find a seat at any school they've listed more highly. Finally, *conditionally seated* applicants are marginal at s. These applicants are assigned s when not assigned a higher choice *and* when they draw a tie-breaker that clears the randomization cutoff at s. Under SD, all applicants are in Θ_s^c.

[12] Under TTC, equation (7) need not be satisfied for all matching problems. But the DA class includes China's parallel mechanisms (Chen and Kesten, 2017), England's first-preference-first mechanisms (Pathak and Sönmez, 2013), and the Taiwan mechanism (Dur, Pathak, Song and Sönmez, 2018). In large markets satisfying regularity conditions that imply a unique stable matching, the DA class includes school-proposing as well as applicant-proposing DA (these conditions are spelled out in Azevedo and Leshno (2016)). For serial dictatorship, $\boldsymbol{\phi}(\theta) = (0, ..., 0)$ for all $\theta \in \Theta$. For immediate acceptance, $\boldsymbol{\phi}_s(\theta_i) < \boldsymbol{\phi}_s(\theta_j)$ if i ranks s ahead of j, and $\boldsymbol{\phi}_s(\theta_i) < \boldsymbol{\phi}_s(\theta_j)$ if and only if i and j rank s the same and $\rho_{is} < \rho_{js}$ (Ergin and Sönmez, 2006).

3.1 Global DA Risk

Let $F_v^i(r)$ denote the cumulative distribution function (CDF) of R_{iv} evaluated at r and define

$$F_v(r|\theta) = E[F_v^i(r)|\theta_i = \theta]. \tag{8}$$

This is the fraction of type θ applicants with tie-breaker v below r (set to zero when type θ lists no schools using tie-breaker v). We again assume tie-breakers have support $[0, 1]$. As with a single tie-breaker, distributions of normalized R_{iv} depend on type.

With multiple tie-breakers, qualification at higher-listed choices may truncate the distribution of any or all R_{iv}. We therefore define tie-breaker-specific MIDs for each S_v. To this end, partition $B_{\theta s}$ into disjoint sets denoted by

$$B_{\theta s}^v = B_{\theta s} \cap S_v,$$

for each v. This partition is used to construct tie-breaker-specific MIDs:

$$MID_{\theta s}^v = \begin{cases} 0 & \text{if } \theta \in \Theta_b^n \text{ for all } b \in B_{\theta s}^v \text{ or if } B_{\theta s}^v = \emptyset \\ 1 & \text{if } \theta \in \Theta_b^a \text{ for some } b \in B_{\theta s}^v \\ \max\{\tau_b \mid b \in B_{\theta s}^v \text{ and } \theta \in \Theta_b^c\} & \text{otherwise} \end{cases}$$

This extends $MID_{\theta s}$ defined in (6) in two ways. In addition to capturing tie-breaker specificity, $MID_{\theta s}^v$ allows for *complete* truncation of R_{iv} when θ clears marginal priority at a school in $B_{\theta s}^v$.

$MID_{\theta s}^v$ and the partition of Θ_s by priority status determine global DA risk with general tie-breakers:

Proposition 4 (Global Score with General Tie-breaking)**.** *Consider continuum DA with multiple tie-breakers indexed by v, distributed independently of one another according to $F_v(r|\theta)$. For all s and θ in this match,*

$$p_s(\theta) = \begin{cases} 0 & \text{if } \theta \in \Theta_s^n \\ \prod_v (1 - F_v(MID_{\theta s}^v|\theta)) & \text{if } \theta \in \Theta_s^a \\ \prod_{v \neq v(s)} (1 - F_v(MID_{\theta s}^v|\theta)) \times \max\left\{0, F_{v(s)}(\tau_s|\theta) - F_{v(s)}(MID_{\theta s}^{v(s)}|\theta)\right\} & \text{if } \theta \in \Theta_s^c \end{cases}$$

where $F_{v(s)}(\tau_s|\theta) = \tau_s$ and $F_{v(s)}(MID_{\theta s}^{v(s)}|\theta) = MID_{\theta s}^0$ when $v(s) = 0$.

Proposition 4, which generalizes an earlier multiple lottery tie-breaker result in Abdulkadiroğlu *et al.* (2017b), covers three sorts of applicants, corresponding to the partition of Θ_s. First, applicants with less-than-marginal priority at s have no chance of being seated there. The second line of the theorem reflects the likelihood of qualification at schools preferred to s among applicants surely seated at s when they can't do better. Since tie-breakers are assumed independent, the probability of not doing better than s is described by a product over tie-breakers, $\prod_v (1 - F_v(MID_{\theta s}^v|\theta))$. If type θ is sure to do better than s, then $MID_{\theta s}^v = 1$ and risk at s is zero.

Finally, risk for applicants in Θ_s^c multiplies the term

$$\prod_{v \neq v(s)} \left(1 - F_v(MID_{\theta s}^v|\theta)\right)$$

by

$$\max\left\{0, F_{v(s)}(\tau_s|\theta) - F_{v(s)}(MID_{\theta s}^{v(s)}|\theta)\right\}.$$

The first of these is the probability of failing to improve on s by virtue of being seated at schools using a tie-breaker *other* than $v(s)$. The second parallels assignment risk in single-tie-breaker SD: to be seated at s, applicants in Θ_s^c must have $R_{iv(s)}$ between $MID_{\theta s}^{v(s)}$ and τ_s.

Proposition 4 allows for single tie-breaking, lottery tie-breaking, or a mix of non-lottery and lottery tie-breakers as in the NYC high school match. With a single tie-breaker, the risk formula simplifies, omitting product terms over v:

Corollary 2 (Abdulkadiroğlu *et al.* (2017b))**.** *Consider a continuum DA match using a single tie-breaker, R_i, distributed according to $F_R(r|\theta)$ for type θ. For all s and θ in this market, we have:*

$$p_s(\theta) = \begin{cases} 0 & \text{if } \theta \in \Theta_s^n, \\ 1 - F_R(MID_{\theta s}|\theta) & \text{if } \theta \in \Theta_s^a, \\ (1 - F_R(MID_{\theta s}|\theta)) \times \max\left\{0, \dfrac{F_R(\tau_s|\theta) - F_R(MID_{\theta s}|\theta)}{1 - F_R(MID_{\theta s}|\theta)}\right\} & \text{if } \theta \in \Theta_s^c, \end{cases}$$

where $p_s(\theta) = 0$ when $MID_{\theta s} = 1$ and $\theta \in \Theta_s^c$, and $MID_{\theta s}$ is as defined in Section 2.2, applied to a single tie-breaker.

Common lottery tie-breaking for all schools further simplifies the DA propensity score. When $v(s) = 0$ for all s, $F_R(MID_{\theta s}) = MID_{\theta s}$ and $F_R(\tau_s|\theta) = \tau_s$, as in the Denver match analyzed by Abdulkadiroğlu *et al.* (2017b). In this case, the DA propensity score is a function only of $MID_{\theta s}$ and the partition of Θ_s into applicants that are never, always, and conditionally seated. This contrasts with the scores in Proposition 2 and Proposition 4, which depend on the unknown and unrestricted conditional distributions of tie-breakers given type ($F_R(\tau_s|\theta)$ and $F_R(MID_{\theta s}|\theta)$ with a single tie-breaker; $F_v(\tau_s|\theta)$ and $F_v(MID_{\theta s}|\theta)$ with general tie-breakers). We therefore turn again to local risk to isolate risk that is independent of type and potential outcomes.

3.2 DA Goes Local

Under general DA, local risk is defined only in marginal priority groups. We therefore modify the set of t_{is} variables to be

$$t_{is}(\delta) = \begin{cases} n & \text{if } \theta \in \Theta_s^n \text{ or, if } v(s) \neq 0,\ \theta \in \Theta_s^c \text{ and } R_{iv(s)} > \tau_s + \delta \\ a & \text{if } \theta \in \Theta_s^a \text{ or, if } v(s) \neq 0,\ \theta \in \Theta_s^c \text{ and } R_{iv(s)} \leq \tau_s - \delta \\ c & \text{if } \theta \in \Theta_s^c \text{ and, if } v(s) \neq 0,\ R_{iv(s)} \in (\tau_s - \delta, \tau_s + \delta] \end{cases}$$

for each applicant and school. This expands the classification of applicants to school s into $t_{is}(\delta) = a$, n, or c by including those who fail to clear marginal priority at s in group n and by

including those who clear marginal priority at s in group a. These classifiers are again collected in the vector,

$$T_i(\delta) = [t_{i1}(\delta), ..., t_{is}(\delta), ..., t_{iS}(\delta)]'.$$

The *local DA propensity score* is defined as a function of type and cutoff proximity, as summarized by $T_i(\delta)$:

$$\psi_s(\theta, T) = \lim_{\delta \to 0} E[D_i(s)|\theta_i = \theta, T_i(\delta) = T],$$

for $T = [t_1, ..., t_s, ..., t_S]' \in \{n, a, c\}^S$. This describes assignment risk for applicants with tie-breaker values above, below, and near cutoffs for any and all schools in the match. We again require that all tie-breaker distributions be continuously differentiable at randomization cutoffs and that these cutoffs be distinct:

Assumption 1. *(a) For every v and for $r = \tau_1, ..., \tau_S$, $F_v^i(r|e)$ is continuously differentiable with $F_v^{i'}(r|e) > 0$ given any event e of the form that $\theta_i = \theta, R_{iu} > r_u$ for $u = 1, ..., v-1$, and $T_i(\delta) = T$. (b) $\tau_s \neq \tau_{s'}$ for any schools $s \neq s'$ with $\tau_s \neq 0$ and $\tau_{s'} \neq 0$.*

This set-up yields a compact and useful characterization of local assignment risk in continuum DA with general tie-breaking:

Theorem 1 (Local Score with General Tie-breaking)**.** *Consider continuum DA with multiple tie-breakers indexed by v, distributed according to $F_v(r|\theta)$, and suppose Assumption 1 holds. For all $s \in S$, $\theta \in \Theta_s$, $T = [t_1, ..., t_s, ..., t_S]' \in \{n, a, c\}^S$, and all w, we have*

$$\lim_{\delta \to 0} E[D_i(s)|\theta_i = \theta, T_i(\delta) = T, W_i = w] = \psi_s(\theta, T),$$

where $\psi_s(\theta, T) = 0$ if (a) $t_s = n$; or (b) $t_b = a$ for some $b \in B_{\theta s}$. Otherwise,

$$\psi_s(\theta, T) = \begin{cases} 0.5^{m_s(\theta,T)}(1 - MID_{\theta s}^0) & \text{if } t_s = a \\ 0.5^{m_s(\theta,T)} \max\left\{0, \tau_s - MID_{\theta s}^0\right\} & \text{if } t_s = c \text{ and } v(s) = 0 \\ 0.5^{1+m_s(\theta,T)}(1 - MID_{\theta s}^0) & \text{if } t_s = c \text{ and } v(s) > 0. \end{cases} \quad (9)$$

where $m_s(\theta, T) = |\{v > 0 : MID_{\theta s}^v = \tau_b \text{ and } t_b = c \text{ for some } b \in B_{\theta s}^v\}|$.

The local DA score for type θ applicants is determined in part by the screened schools θ prefers to s. Relevant screened schools are those at which applicants to s are in the marginal priority group with a tie-breaker close to randomization cutoffs. The variable $m_s(\theta, T)$ counts the number of tie-breakers involved in such close encounters. As expressed in equation (4) for the single-school case, applicants drawing screened school tie-breakers close to τ_b for some $b \in B_{\theta s}^v$ face qualification risk of 0.5.

Theorem 1 starts with a scenario where applicants to s are either sure to do better or are never seated at s and therefore face no risk there. In this case, we need not worry about whether s is a screened or lottery school. In other scenarios, where applicants fail to improve on s, risk at any lottery s is determined in part by truncation of the lottery tie-breaker at more preferred unscreened schools and by possible qualification at more preferred screened schools,

where qualification risk is 0.5. These sources of risk combine to produce the second line of (9). Similarly, risk at any screened s is determined by possible qualification at more preferred schools (lottery and screened) plus an additional 0.5 risk term for those marginal at s. This explains the addition of 1 to the exponent in the third line of equation (9).

This theorem also yields a general conditional independence relation, similar to Corollary 1:

$$\lim_{\delta \to 0} P[D_i(s) = 1 | \theta_i = \theta, T_i(\delta) = T, W_i = w, \psi_s(\theta, T) = p] = p, \tag{10}$$

for $p \in [0, 1]$. In other words, fixing $\psi_s(\theta, T)$, DA-generated offers are independent of type and any W_i that's unaffected by treatment. Local conditional independence allows us to eliminate OVB by conditioning on $\psi_s(\theta, T)$. Moreover, $\psi_s(\theta, T)$ is typically far coarser than the underlying type distribution.

3.3 Estimating the Local Score

A sample analog of the theoretical local DA score described by Theorem 1 is shown here to converge uniformly to the corresponding local score for a finite market, in an asymptotic sequence that increases market size with a shrinking bandwidth. Our empirical application establishes the relevance of this asymptotic result by showing that applicant characteristics are balanced by offer status conditional on estimates of the local propensity score.

The sequence used to study the estimated score increases the size of a random sample of N applicants. We refer to sampled applicants by the order in which they're sampled, that is, by $i \in \{1, 2, ..., N\}$. The applicant sample is augmented with information on applicant type and large-market school capacities, $\{q_s\}$, which give the proportion of the market that can be seated at s. Each applicant is associated with an individual tie-breaker distribution, $F_v^i(r)$, as described above. We observe a realized tie-breaker value for each applicant, but not the underlying distribution.

Fix the number of seats at school s in each sampled finite market to be the integer part of Nq_s and run DA with these applicants and schools. We consider the limiting behavior of an estimator that uses the resulting $MID_{\theta s}^v$, τ_s, and marginal priorities generated by this single realization. Also, given a bandwidth $\delta_N > 0$, we determine $t_{is}(\delta_N)$ for each i and s. This is used to compute

$$\hat{m}_{Ns}(\theta, T) = |\{v > 0 : MID_{\theta s}^v = \tau_b \text{ and } t_{ib}(\delta_N) = c \text{ for some } b \in B_{\theta s}^v\}|.$$

Empirical bandwidths in the application below are determined separately for each cutoff.

Our propensity score estimator is constructed by plugging these ingredients into the formula in Theorem 1. If $t_{is}(\delta_N) = n$ or $t_{ib}(\delta_N) = a$ for some $b \in B_{\theta s}$, then

$$\hat{\psi}_{Ns}(\theta, T; \delta_N) = 0.$$

Otherwise,

$$\hat{\psi}_{Ns}(\theta, T; \delta_N) = \begin{cases} 0.5^{\hat{m}_{Ns}(\theta,T)}(1 - MID_{\theta s}^0) & \text{if } t_{is}(\delta_N) = a \\ 0.5^{\hat{m}_{Ns}(\theta,T)} \max\left\{0, \tau_s - MID_{\theta s}^0\right\} & \text{if } t_{is}(\delta_N) = c,\ v(s) = 0 \\ 0.5^{1+\hat{m}_{Ns}(\theta,T)}(1 - MID_{\theta s}^0) & \text{if } t_{is}(\delta_N) = c,\ v(s) \neq 0. \end{cases}$$

Note that τ_s, $MID^0_{\theta s}$, and $\hat{m}_{Ns}(\theta, T)$ in this expression are sample quantities.

As a theoretical benchmark for the large-sample performance of $\hat{\psi}_{Ns}(\theta, T; \delta_N)$, we define the true local score for a finite market of size N. This is

$$\psi_{Ns}(\theta, T) = \lim_{\delta \to 0} E_N[D_i(s)|\theta_i = \theta, T_i(\delta) = T],$$

where E_N is the expectation induced by the set of tie-breaker distributions $\{F^i_v(r); i = 1, 2, ..., N\}$ for applicants in the finite market. This quantity fixes the distribution of types and the vector of proportional school capacities, as well as market size. $\psi_{Ns}(\theta, T)$ is the limit of the average of $D_i(s)$ across infinitely many tie-breaker draws in ever-narrowing windows near cutoffs in a match governed by these parameters. Because tie-breaker distributions are assumed to have continuous density in the neighborhood of any cutoff, the population average assignment rate is well-defined for any positive δ.

We're interested in the gap between the estimator $\hat{\psi}_{Ns}(\theta, T; \delta_N)$ and the true local score $\psi_{Ns}(\theta, T)$ as N grows and δ_N shrinks. We can show that $\hat{\psi}_{Ns}(\theta, T; \delta_N)$ described above converges uniformly to $\psi_{Ns}(\theta, T)$ in such a sequence. This result uses a regularity condition:

Assumption 2. *(Rich support) In the continuum market, for every school s and every priority ρ held by a positive mass of applicants at s, the proportion of applicants with $\rho_{is} = \rho$ who rank s first is also positive.*

This says that for each priority group at school s represented among applicants in the continuum, some applicants list s first.

Uniform convergence of $\hat{\psi}_{Ns}(\theta, T; \delta_N)$ is formalized below:

Theorem 2 (Consistency of the DA Local Score)**.** *In the asymptotic sequence described above and maintaining Assumptions 1 and 2, the estimated local propensity score $\hat{\psi}_{Ns}(\theta, T; \delta_N)$ is a consistent estimator of $\psi_{Ns}(\theta, T)$ in the following sense: For any δ_N such that $\delta_N \to 0$ and $N\delta_N \to \infty$ as $N \to \infty$,*

$$\sup_{\theta \in \Theta, s \in S, T \in \{n,c,a\}^S} |\hat{\psi}_{Ns}(\theta, T; \delta_N) - \psi_{Ns}(\theta, T)| \xrightarrow{p} 0,$$

as $N \to \infty$.

Proof. The proof uses lemmas established in the appendix. The first lemma shows that the vector of DA cutoffs computed for the sampled market, $\hat{\xi}_N$, converges to the vector of cutoffs in the continuum, that is,

$$\hat{\xi}_N \xrightarrow{a.s.} \xi,$$

where ξ denotes the vector of continuum cutoffs. This result implies that the estimated score converges to the large-market local score as market size grows and bandwidth shrinks. Specifically, for all $\theta \in \Theta$, $s \in S$, and $T \in \{n, c, a\}^S$, we have

$$\hat{\psi}_{Ns}(\theta, T; \delta_N) \xrightarrow{a.s.} \psi_s(\theta, T)$$

as $N \to \infty$ and $\delta_N \to 0$.

The second lemma shows that the true finite market score with a fixed bandwidth, defined as $\psi_{Ns}(\theta, T; \delta_N) \equiv E_N[D_i(s)|\theta_i = \theta, T_i(\delta_N) = T]$, also converges to $\psi_s(\theta, T)$ as market size grows and bandwidth shrinks. That is, for all $\theta \in \Theta, s \in S$, $T \in \{n, c, a\}^S$, and δ_N such that $\delta_N \to 0$ and $N\delta_N \to \infty$ as $N \to \infty$,

$$\psi_{Ns}(\theta, T; \delta_N) \xrightarrow{p} \psi_s(\theta, T)$$

as $N \to \infty$.

Finally, the definitions of $\psi_{Ns}(\theta, T; \delta_N)$ and $\psi_{Ns}(\theta, T)$ imply that $|\psi_{Ns}(\theta, T; \delta_N) - \psi_{Ns}(\theta, T)| \xrightarrow{a.s.} 0$ as $\delta_N \to 0$. Combining these results shows that for all $\theta \in \Theta, s \in S$, and T, as $N \to \infty$ and $\delta_N \to 0$ with $N\delta_N \to \infty$, we have

$$\begin{aligned}
&|\hat{\psi}_{Ns}(\theta, T; \delta_N) - \psi_{Ns}(\theta, T)| \\
=&|\hat{\psi}_{Ns}(\theta, T; \delta_N) - \psi_{Ns}(\theta, T; \delta_N) + \psi_{Ns}(\theta, T; \delta_N) - \psi_{Ns}(\theta, T)| \\
\leq&|\hat{\psi}_{Ns}(\theta, T; \delta_N) - \psi_{Ns}(\theta, T; \delta_N)| + |\psi_{Ns}(\theta, T; \delta_N) - \psi_{Ns}(\theta, T)| \\
\xrightarrow{p}&|\psi_s(\theta, T) - \psi_s(\theta, T)| + 0 \\
=&0.
\end{aligned}$$

This yields the theorem since Θ, S, and $\{n, c, a\}^S$ are finite. □

Theorem 2 justifies our use of the formula in Theorem 1 to eliminate OVB in empirical work estimating school attendance effects.

4 A Brief Report on NYC Report Cards

Since the 2003-04 school year, the NYC Department of Education (DOE) has used DA to assign rising ninth graders to high schools. Many high schools in the match host multiple programs, which act like schools, with their own admissions protocols. Each applicant for a ninth grade seat can list up to twelve programs. All traditional public high schools participate in the match, but charter schools and NYC's exam schools have separate admissions procedures.[13]

The NYC match is structured like the match described in Section 3: unscreened programs use a common randomly assigned tie-breaker, while screened programs use a variety of non-lottery tie-breaking variables. Screened tie-breakers are mostly distinct, with one for each school or program, though some screened programs share a tie-breaker. In any case, our theoretical framework accommodates all of NYC's many tie-breaking protocols.[14]

Our analysis uses Theorem 1 to compute propensity scores for programs rather than schools since programs are the unit of assignment. But since the match yields a single offer, we can sum program propensity scores to produce school-level scores and then sum again for groups of

[13] Some special needs students are also matched separately. The centralized NYC high school match is detailed in Abdulkadiroğlu *et al.* (2005, 2009). Abdulkadiroğlu *et al.* (2014) describe NYC exam school admissions.

[14] Screened tie-breakers are reported as an integer reflecting raw tie-breaker order in this group. We scale these so as to lie in $(0, 1]$ by transforming raw tie-breaking realizations R_{iv} into $[R_{iv} - \min_j R_{jv} + 1]/[\max_j R_{jv} - \min_j R_{jv} + 1]$ for each tie-breaker v. This transformation produces a positive cutoff at s when only one applicant is seated at s and a cutoff of 1 when all applicants who list s are seated there.

schools. The score for attendance at any screened Grade A school, for example, is the sum of the scores for all screened Grade A schools in the match. For our purposes, an "unscreened" school is a school hosting any lottery program; other schools are screened. Our analysis refers to all programs of these types as "screened" since all use some sort of non-lottery tie-breaker.[15]

In 2007, the NYC DOE launched a school accountability system that graded schools from A to F. This mirrors similar accountability systems in Florida and other states. NYC's school grades were determined by achievement levels and, especially, achievement growth, as well as by survey- and attendance-based features of the school environment. Growth looked at credit accumulation, Regents test completion and pass rates; performance measures were derived mostly from four- and six-year graduation rates. Some schools were ungraded. Figure 3 reproduces a sample letter-graded school progress report.[16]

The 2007 grading system was controversial. Proponents applauded the integration of multiple measures of school quality while opponents objected to high-stakes consequences of low school grades, such as school closure or consolidation. Rockoff and Turner (2011) provide a partial validation of the system by showing that low grades seem to have sparked school improvement. In 2014, the DOE replaced the 2007 scheme with school quality measures that place less weight on test scores and more on curriculum characteristics and subjective assessments of teaching quality. The relative merits of the old and new systems continue to be debated.

We showcase the use of centralized assignment with general tie-breaking for impact evaluation by estimating effects of being assigned to a Grade A school. This analysis uses application data from the 2011-12, 2012-13, and 2013-14 school years. Our sample includes first-time applicants seeking 9th grade seats, who submit preferences over programs in the main round of the NYC high school match. Data include school capacities and priorities, lottery numbers, and screened school tie-breakers, information that allows us to replicate the match. Detail related to our match replication effort appear in the online appendix.

Students at Grade A schools have higher average SAT scores and higher graduation rates than do students at other schools. Differences in graduation rates across schools feature in media accounts of socioeconomic differences in NYC high school match results (see, e.g., Harris and Fessenden (2017) and Disare (2017)). Grade A students are also more likely than students attending other schools to be deemed "college- and career-prepared" or "college-ready".[17] These and other school characteristics are documented in Table 1. Achievement gaps between screened and unscreened Grade A schools are especially large. This likely reflects selection bias induced by test-based screening.

Screened Grade A schools have a majority white or Asian student body, the only group of schools described in the table to do so. These schools are also over-represented in Manhattan,

[15]Some NYC high schools sort applicants on a coarse screening tie-breaker that allows ties, while breaking these ties using the common lottery number. Schools of this type are treated as unscreened schools, adding priority groups defined by values of the screened tie-breaker. Seats for Ed-Opt programs are split into halves, one of which screens applicants using a single non-lottery tie-breaker while the other uses the common lottery number. See the online appendix for an explanation of how Ed-Opt programs are integrated into our analysis.

[16]Walcott (January 2012) details the NYC grading methodology used in this period.

[17]These composite variables are determined as a function of Regents and AP scores, course grades, vocational or arts certification, and college admission tests.

a borough that includes most of New York's wealthiest neighborhoods (though average family income is higher on Staten Island). Teacher experience is similar across school types, while screened Grade A schools have somewhat more teachers with advanced degrees.

Table 2 describes the roughly 180,000 ninth graders enrolled in the 2012-13, 2013-14, and 2014-15 school years. Students enrolled in a Grade A school, including those enrolled in the Grade A schools assigned outside the match, are less likely to be black or Hispanic and have higher baseline scores than the general population of 9th graders. The 153,000 eighth graders who applied for ninth grade seats are described in column 3 of the table. Roughly 130,000 listed a Grade A school assigned in the match ("Match A") on their application form and a little over a third of these were offered a Grade A seat. The difference between total 9th grade enrollment (about 182,000) and the number of match participants is accounted for by groups of special education students outside the main match, direct-to-charter enrollment, and a few schools that straddle 9th grade. Applicants in the match have baseline (6th grade) scores above the overall district mean (baseline scores are standardized to the population of test-takers). As can be seen by comparing columns 3 and 4, in Table 2, however, the average characteristics of Grade A applicants are much like those of the entire applicant population.

The statistics in column 5 of Table 2 show that applicants *enrolled* in a Grade A school (among schools participating in the match) are somewhat less likely to be black and have higher baseline scores than the total applicant pool. Here too, these gaps likely reflect selection bias at screened Grade A schools. Most of those attending a Grade A school were offered a seat there, and most ranked a Grade A school first. Grade A students are about twice as likely to go to an unscreened school as to a screened school.

Enthusiasm for Grade A schools is far from universal: just under half of all applicants in the match listed a Grade A school first. Around 31,000 Grade A applicants have non-degenerate risk of Grade A assignment, that is, an estimated $\hat{\psi}_{Ns}(\theta, T; \delta_N)$ strictly between 0 and 1, conditional on which there's variation in offer status. Throughout we use "assignment risk" to mean an estimated $\hat{\psi}_{Ns}(\theta, T; \delta_N)$ for the relevant set of treatment schools. Applicants at risk of Grade A assignment, described in column 6 of Table 2, have baseline scores and demographic characteristics much like those of the sample enrolled at a Grade A school. The ratio of screened to unscreened enrollment among those with Grade A risk is also similar to the corresponding ratio in the sample of enrolled students (compare 33.4/16.6 in the former group to 71.9/28.4 in the latter).

The balancing property of propensity score conditioning is documented in Table 3, which reports raw and score-controlled differences in covariate means for applicants who do and don't receive Grade A offers. Score-controlled differences are estimated in the following setup. Let D_{1i} be a dummy indicating match Grade A school offers and let $d_{1i}(x)$ be a dummy indicating $\hat{p}_{1i} = x$, where x indexes values the score might take. Likewise, let D_{0i} indicate offers at ungraded schools and let $d_{0i}(x)$ be a dummy indicating $\hat{p}_{0i} = x$. Estimated propensity scores for Grade A and ungraded schools offers, denoted $\hat{p}_{1i}$ and $\hat{p}_{0i}$, are computed by summing estimated scores for Grade A and ungraded schools, respectively. We control for ungraded school offers to ensure that estimated Grade A effects compare schools with high and low grades, omitting the

ungraded.[18]

Let W_i be any applicant covariate measured before assignment, including features of θ_i. Balance tests are estimates of parameter γ_1 in

$$W_i = \gamma_1 D_{1i} + \gamma_0 D_{0i} + \sum_x \alpha_1(x) d_{1i}(x) + \sum_x \alpha_0(x) d_{0i}(x) + h(\mathcal{R}_i) + \nu_i, \tag{11}$$

with local linear control for the screened tie-breaker parameterized as

$$h(\mathcal{R}_i) = \sum_{s \in S \setminus S_0} \omega_{1s} a_{is} + k_{is}[\omega_{2s} + \omega_{3s}(R_{iv(s)} - \tau_s) + \omega_{4s}(R_{iv(s)} - \tau_s)\mathbf{1}(R_{iv(s)} > \tau_s)], \tag{12}$$

where $\mathcal{R}_i \equiv [R_{i1}, ..., R_{iV}]'$ is the vector of screened tie-breakers, $S \setminus S_0$ is the set of screened programs, a_{is} indicates whether applicant i applied to program s, and $k_{is} = a_{is} \times \mathbf{1}(\tau_s - \delta_s < R_{iv(s)} < \tau_s + \delta_s)$ indicates applicants in a bandwidth of size δ_s around randomization cutoff τ_s. Parameters in (11) and (12) vary by application cohort. The sample is limited to applicants with non-degenerate Grade A offer risk. Bandwidths are estimated as suggested by Imbens and Kalyanaraman (2012), separately for each program, for the set of applicants in the relevant marginal priority group.[19]

As can be seen in column 2 of Table 3, applicants offered a Grade A seat are much more likely to have listed a Grade A school first, and listed more Grade A schools than did other applicants. Minority and free-lunch-eligible applicants are less likely to be offered a Grade A seat, while those offered a Grade A seat have much higher baselines scores, with gaps in the range of 0.3 and 0.4 standard deviations in favor of those offered. These raw differences notwithstanding, our theoretical results suggest that estimates of γ_1 in equation (11) should be close to zero. This is borne out by the estimates reported in column 4 of the table, which shows small, mostly insignificant differences in covariates by offer status when estimated using equation (11). The estimates establish the empirical relevance of both the large-market framework and the notion of limiting local risk underlying the theoretical results in Section 3.

The encouraging balance results in Table 3 are especially noteworthy in view of Figure 1, which shows that an IK bandwidth is insufficiently narrow to drive the propensity score for qualification at Townsend Harris to the theoretical limit of one-half. Screened tie-breaker control via local linear regression mitigates this approximation error. Our local linear regression estimation strategy, which combines saturated control for the propensity score with linear tie-breaker control can be seen as a "doubly robust" score-based estimator of the sort suggested by Robins (2000) and Okui *et al.* (2012), the latter in an IV context. Even if the local score is poorly approximated, screened tie-breaker controls minimize omitted variable bias from non-lottery tie-breakers. At the same time, the theoretical score tells us which tie-breakers are important and for whom.

Causal effects of school attendance on test scores are estimated by 2SLS, using offer dummies as instruments for years of exposure to schools of a particular type. Exposure variables are

[18] Ungraded schools are mostly new or had data insufficient to determine a grade.

[19] Bandwidths are also computed separately for each outcome variable; we use the smallest of these for each program. We set $\delta = 0$ for screened programs if either the number of applicants with $R_{iv} \in (\tau_s - \delta, \tau_s]$ or the number of applicants with $R_{iv(s)} \in (\tau_s, \tau_s + \delta]$ is less than five.

denoted C_{1i} and C_{0i} for Grade A and ungraded schools, respectively. Effects on graduation outcomes are estimated by replacing years of exposure with dummies for any Grade A exposure. The causal effects of interest are 2SLS estimates of parameter β_1 in

$$Y_i = \beta_1 C_{1i} + \beta_0 C_{0i} + \sum_x \alpha_{21}(x) d_{1i}(x) + \sum_x \alpha_{20}(x) d_{0i}(x) + g(\mathcal{R}_i) + \eta_i, \tag{13}$$

with associated first stage equations,

$$\begin{aligned} C_{1i} &= \gamma_{11} D_{1i} + \gamma_{10} D_{0i} + \sum_x \alpha_{11}(x) d_{1i}(x) + \sum_x \alpha_{10}(x) d_{0i}(x) + h_1(\mathcal{R}_i) + \nu_{1i} \\ C_{0i} &= \gamma_{01} D_{1i} + \gamma_{00} D_{0i} + \sum_x \alpha_{01}(x) d_{1i}(x) + \sum_x \alpha_{00}(x) d_{0i}(x) + h_0(\mathcal{R}_i) + \nu_{0i}. \end{aligned} \tag{14}$$

Screened tie-breaker control functions in these equations, denoted $h_1(\mathcal{R}_i), h_2(\mathcal{R}_i)$, and $g(\mathcal{R}_i)$, are analogous to (12). Risk set dummies $d_{1i}(x)$ and $d_{0i}(x)$ are included as in equation (11). Reported results are from specifications that also control for baseline math and English scores, free lunch, special education, English language learner indicators, and gender and race dummies (estimates without these controls are similar, though less precise). The three applicant cohorts in our sample are stacked, so all parameters except $\beta_1, \beta_0, \gamma_{11}, \gamma_{10}, \gamma_{01}$, and γ_{00} are interacted with cohort.

Theorems 1 and 2 imply that Grade A and ungraded school offers are locally and asymptotically independent of potential outcomes conditional on estimates of the relevant local score. Given an exclusion restriction, the conditional random assignment of school offers supports our interpretation of 2SLS estimates of β_1 and β_2 as capturing causal effects of attendance at different sorts of schools. The exclusion restriction in this case means that Grade A and ungraded school offers have no effect on outcomes other than by boosting time spent at Grade A and ungraded schools.

This exclusion restriction fails when Grade A and ungraded school offers change school quality by moving applicants between schools of different quality *within* the Grade A or another sector. In other words, exclusion fails if Grade A and ungraded school offers change the type of school attended through channels other than a school's grade. We therefore explore multi-sector models that distinguish causal effects of attendance at different sorts of Grade A schools, such as screened and unscreened. Estimates of these multi-sector models are discussed following the discussion of overall Grade A effects.

OLS estimates of Grade A effects, reported as a benchmark in the second column of Table 4, show Grade A attendance is associated with higher SAT scores and graduation rates, and increased college and career readiness. The OLS estimates in Table 4 are constructed by fitting equation (13), without propensity score controls or instrumenting, in a sample that includes all participants in the high school match without regard to offer risk. OLS estimates of SAT gains are around 6.5 points. Graduation gains are similarly modest at just under 4 points, but effects on college and career readiness are substantial, running 8-11 points on a base rate around 40.

First stage effects of Grade A offers on Grade A enrollment, computed by estimating equation (14) and reported in Panel A of Table 4, show that offers of a Grade A seat boost Grade A enrollment by 1.8 years between the time of application and SAT test-taking. Grade A offers

boost the likelihood of any Grade A enrollment by about 64 percentage points. This can be compared with Grade A enrollment rates around 17 percent among those not offered a Grade A seat in the match.

In contrast with the OLS estimates in column 2, the 2SLS estimates in column 4 of Table 4 suggest that most of the SAT gains associated with Grade A attendance reflect selection bias. The 2SLS estimate of an effect on SAT math is only around 2.2, though significantly different from zero with a standard error of about 0.7. The corresponding 2SLS estimate of reading gains is even smaller and not significantly different from zero, though estimated with similar precision. The 2SLS estimate for graduation shows a marginally significant gain of 3 percentage points. The estimated standard error of 0.013 associated with the graduation estimate seems especially noteworthy, as this means our research design has the power to uncover even modest improvements in high school completion.[20]

The strongest Grade A effects appear for indicators of college and career preparedness and college readiness. These two indicators, the construction of which is detailed in our online appendix, are determined by various sorts of achievement and certification milestones, and contribute to the determination of school grades. 2SLS estimates of effects on these outcomes are remarkably close to the corresponding OLS estimates and estimated with a level of precision similar to that associated with 2SLS estimates of graduation effects.

4.1 Screened versus Unscreened Grade A Schools

NYC education policy controversies often revolve around access to screened schools. Longstanding policy interest, along with concerns about within-sector changes in school quality that might violate our 2SLS exclusion restrictions, motivate an analysis that distinguishes screened from lottery Grade A effects.

The multi-sector estimates reported in Table 5 are from models that include separate endogenous variables for screened and unscreened Grade A schools, along with a third endogenous variable for the ungraded sector. Instruments for this just-identified set-up are two dummies indicating each sort of Grade A offer, as well as a dummy indicating ungraded school offers. 2SLS models include separate saturated propensity score controls for each Grade A school sector offer, as well as for the risk of an ungraded school offer. These multi-sector estimates are computed in a sample limited to applicants at risk of assignment to either a screened or unscreened Grade A school.

OLS estimates again provide an interesting benchmark. As can be seen in the first two columns of Table 5, screened Grade A students appear to reap a large SAT advantage even after controlling for baseline achievement and other covariates. In particular, OLS estimates of Grade A effects for schools in the screened sector are on the order of 16-19 points. At the same time, unscreened Grade A schools appear to generate achievement gains around only one point. By contrast, 2SLS estimates of multi-sector models, reported in columns 3 and 4 of Table 5, show equally modest SAT effects for Grade A schools in both sectors. These range from 2-3 points

[20]Empirical Appendix Table B1 shows little difference in follow-up rates between applicants who are and aren't offered a Grade A seat. The 2SLS estimates in Table 4 are therefore unlikely to be compromised by differential attrition associated with Grade A offers.

for math, with smaller estimates for reading that are not significantly different from zero. This suggests that OLS estimates of the screened school advantage are driven in part by selection bias.

2SLS estimates also suggest that screened and unscreened Grade A schools both boost graduation rates somewhat, with marginally significant gains ranging from 0.048 for screened to 0.024 for unscreened schools. Estimated Grade A effects on college and career preparedness and college readiness in column 3 and 4 are markedly larger than estimated effects on other outcomes. This may in part reflect the fact that Grade A schools are more likely than other schools to offer the sort of advanced courses that contribute to the college- and career-related composites. Across all outcomes, however, the 2SLS estimates for screened and unscreened schools in columns 3 and 4 are similar.

4.2 DA Assignment Dividends: RD Away from the Cutoff

The external validity of RD designs is sometimes said to be limited by the fact that causal effects identified for applicants local to cutoffs need not be representative of effects in the general population. NYC's centralized match provides two answers to this critique. The first arises from the fact that screened school effects can be identified by lottery risk alone. In particular, unscreened (lottery) schools create assignment risk at screened schools for applicants with tie-breaker values away from screened school cutoffs. To see this, define applicant classifier ℓ_{is}, as follows:

$$\ell_{is} = \begin{cases} n & \text{if } t_{is}(0) = n \\ a & \text{otherwise, if } t_{is}(0) = a \text{ or if } t_{is}(0) = c \text{ and } v(s) > 0 \\ c & \text{otherwise, if } t_{is}(0) = c \text{ and } v(s) = 0. \end{cases} \tag{15}$$

Classification variable ℓ_{is} is defined by setting $\delta = 0$, effectively turning screened-school tie-breakers into priorities.

Paralleling the collection of $t_{is(\delta)}$ in the vector $T_i(\delta)$, here we collect the group of ℓ_{is} in a vector,

$$L_i = [\ell_{i1}, ..., \ell_{is}, ..., \ell_{iS}]',$$

and define

$$\lambda_s(\theta, L) = E[D_i(s)|\theta_i = \theta, L_i = L],$$

for $L = [\ell_1, ..., \ell_s, ..., \ell_S]' \in \{n, a, c\}^S$. Note that, having fixed $\delta = 0$, we no longer need be concerned with limiting risk. Lottery risk can now be written,

$$\lambda_s(\theta, L) = \begin{cases} 0 & \text{if } \ell_s = n \text{ or if } \ell_b = a \text{ for some } b \in B_{\theta s} \\ (1 - MID^0_{\theta s}) & \text{if } \ell_s = a \\ \max\left\{0, \tau_s - MID^0_{\theta s}\right\} & \text{if } \ell_s = c. \end{cases} \tag{16}$$

The second line of (16) describes non-degenerate lottery risk at screened schools. Lotteries create risk at screened schools because students who list lottery schools ahead of screened schools need not qualify for lottery-based (unscreened) admission; this happens with probability $1 - MID^0_{\theta s}$. Note that $\lambda_s(\theta, L)$ is equal to zero or to one more often than $\psi_s(\theta, T)$, especially

for screened schools.[21] Still, lotteries may create risk enough to evaluate both screened and unscreened schools in a match where applicants list schools of both types. This is worth highlighting because evidence on screened school effects generated by lottery risk comes partly from applicants with tie-breakers far from cutoffs.

Each Grade A school in the NYC high school match has at least a few applicants exposed to non-degenerate assignment risk in at least one cohort. Many schools have applicants exposed to lottery risk, but many more applicants are exposed to general risk, that is, risk from lottery *or* non-lottery tie-breaking. The y-axis in Figure 4 shows the number of applicants to each Grade A school added to the at-risk sample by consideration of general risk instead of lottery risk. This is plotted against the number of applicants subject only to lottery risk on the x-axis. Applicants are said to have lottery risk when their estimated $\lambda_s(\theta, L)$ is strictly between 0 and 1. Orange and blue circles plot numbers of applicants at risk for each lottery and screened Grade A school, respectively, where circle sizes are scaled by school capacity.

General risk is especially valuable for screened schools. This is apparent from the many blue circles with positive general risk clustered near the y-axis, meaning they have few or no applicants subject to lottery risk. But the cloud of blue circles away from both axes show that many screened schools also have applicants subject to lottery risk. At the same time, screened schools clearly add applicants to the pool with assignment risk at many unscreened schools.

The estimates in columns 5 and 6 of Table 5, labeled "2SLS using lottery risk", rely on the assignment risk defined by $\lambda_s(\theta, L)$ to identify effects of Grade A attendance. Here, $\lambda_s(\theta, L)$ is computed separately for offers of seats at screened and unscreened Grade A schools. This allows us to distinguish causal effects of screened and unscreened Grade A attendance using distinct offer instruments. Importantly, the lottery-risk analysis generates estimates of screened school effects for screened school applicants with tie-breaker values away from screened-school cutoffs. The experiment implicit in this scenario arises from screened school applicants' disqualification at more preferred unscreened schools.

Perhaps surprisingly, lottery variation alone is sufficient to capture a reasonably precise screened school effect, with standard errors under 3 points for the estimated effects on SAT scores reported in column 5. Although the 2SLS estimates in this case are not significantly different from zero, they're close to the corresponding estimates computed using general risk. It's also worth noting that standard errors below 3 are small enough to provide statistical power for detection of SAT gains under one-tenth of a standard deviation (the standard deviation of an SAT score is around 100). It seems fair to say, therefore, that SAT effects identified using lottery risk alone are informative for schools in both the screened and unscreened sectors.

Graduation effects identified by lottery risk are small and not significantly different from zero for either type of school. But these estimates are not statistically distinguishable from the corresponding effects identified by general risk. Lottery-risk-based estimates of effects on college and career preparedness, however, are larger than the corresponding estimates identified by general risk (compare, for example, 0.17 in column 5 to 0.09 in column 3).

Screened school tie-breaking generates especially large precision gains for estimates of screened school effects. For example, lottery risk alone yields a standard error around 0.89 (shown in

[21] The third line of (16) describes lottery risk at lottery schools.

column 6) for the effects of unscreened school attendance on SAT math scores. This standard error falls to 0.76 (in column 4) when the unscreened school effect is estimated using general risk, a precision gain equivalent to increasing sample size by one-third. By contrast, the corresponding precision gain for estimates of screened school effects is dramatic: a standard error of 2.81 using lottery risk falls to 1.24 for estimates exploiting general risk, a gain that otherwise requires around five times as much data. Similar precision gains from the use of general risk to identify screened school effects are seen for other outcomes in the table. Lottery risk can indeed be used to generate useful estimates of screened school effects, but the most powerful estimators exploit all sources of risk.

A second response to concerns about external validity in this context begins with the observation that, even without lottery tie-breaking, DA may create risk at screened s for applicants away from τ_s. As with lottery risk, this sort of risk arises from risk at schools ranked ahead of s. In the scenario we're now contemplating, however, all risk is screened. Formally, this "inframarginal screened risk" is

$$\sigma_s(\theta, T) = \begin{cases} 0 & \text{if } t_s = n \text{ or if } t_b = a \text{ for some } b \in B_{\theta s} \\ 0.5^{m_s(\theta,T)} & \text{otherwise, if } t_s = a \\ 0.5^{1+m_s(\theta,T)} & \text{otherwise, if } t_s = c, \end{cases}$$

for any school with $v(s) > 0$. Among schools preferred to s, $m_s(\theta, \mathrm{T})$ counts screened school tie-breakers that generate risk. When $B_{\theta s}$ contains Grade B or lower schools, $\sigma_s(\theta, T)$ yields screened Grade A risk for inframarginal applicants to s, even in a match without lottery schools.

This sort of inframarginal screening risk is depicted in Figure 5, which describes the fate of four sorts of applicants. All four rank the school Bard first, while ranking Bard Queens, another school affiliated with Bard College, second. Bard is a Grade B school; Bard Queens is Grade A. Applicants 3 and 4, plotted in orange, fail to clear the Bard cutoff. This pair is marginal at Bard Queens, with tie-breaker values inside the Bard Queens bandwidth (applicant 3 is also marginal at Bard). Applicant 4 clears the Bard Queens cutoff, thereby obtaining a Grade A seat. Applicants 1 and 2 illustrate inframarginal Grade A risk: both are marginal at Bard, but below and outside the bandwidth at Bard Queens. One is seated at Bard, the other at Bard Queens. Applicants like this are exposed to Grade A screened-school risk at schools at which they're well-qualified.

The fruits of inframarginal risk are documented in Table 6, which distinguishes effects on applicants marginal at the screened Grade A school where they're offered a seat (that is, close to this school's cutoff) from effects on applicants offered a screened Grade A seat, though they're outside and below the offering school's bandwidth. To formalize this distinction, let s^* be a particular Grade A screened school, say Bard Queens. Marginal applicants at Bard Queens have $t_{is^*}(\delta) = c$. Other applicants offered a seat at s^* have $t_{is^*}(\delta) = a$.

As a first take on the importance of cutoff proximity, Columns 2 and 5 of Table 6 report 2SLS estimates identified using screened Grade A offers made to inframarginal applicants only. In particular, these estimates use a single instrumental variable constructed by interacting screened Grade A offers with an indicator for $t_{is^*}(\delta) = a$. The 2SLS setup in this case includes propensity score controls for this interacted instrument and uses a sample containing applicants subject to

general risk at either an unscreened Grade A school, a screened Grade A school where they're inframarginal, or both. This strategy produces estimates that differ little from those in columns 1 and 4, which repeat the SAT and high school graduation results reported in columns 3 and 4 of Table 5. Compare, for example, an estimated screened Grade A effect of 2.91 in column 1 and the corresponding estimate for inframarginal applicants of 2.59 in column 2. Because the inframarginal instrument discards information, however, the estimate in column 2 is less precise.

A further exploration of the importance of cutoff proximity adds an endogenous variable to the general risk 2SLS setup, generating a model with two screened Grade A effects. The first is for applicants who enroll in a Grade A screened school when assigned a screened Grade A seat at a school where they have $t_{is^*}(\delta) = c$; the second is for all others who attend a Grade A screened school. A screened Grade A offer dummy interacted with dummies for $t_{is^*}(\delta) = c$ and for $t_{is^*}(\delta) = a$ provides the two instruments needed to identify these two causal effects.

As can be seen in columns 3 and 6 of Table 6, estimates of effects on marginal and other applicants are similar, though the estimates for marginal applicants are considerably more precise. Test statistics (not reported in the table) show the two sorts of screened A effects to be statistically indistinguishable. On balance, the estimates in Table 6 suggest the causal effects of screened Grade A attendance are unlikely to be compromised by applicants' marginal qualification for selective programs.[22]

5 Summary and Next Steps

Centralized student assignment opens new horizons for the measurement of school quality. The research potential of matching markets is extended here by marrying the conditional random assignment generated by lottery tie-breaking with RD-style variation at screened schools. The propensity score for DA with mixed multiple tie-breakers allows us to exploit all sources of quasi-experimental variation arising from any stochastic match in the DA class. Our analysis also shows how markets with general tie-breakers can be used to study treatment effects at screened schools for applicants with tie-breakers away from screened-school cutoffs. This addresses concerns, often raised in an RD context, that causal effects identified for applicants local to cutoffs need not be relevant for the general population.

Our analysis of NYC school report cards suggests Grade A schools generate some gains for their students, boosting Math SAT scores and graduation rates by a few points. OLS estimates, by contrast, show considerably larger effects of Grade A attendance. Grade A screened schools enroll some of the city's highest achievers, but large OLS estimates of achievement gains from attendance at Grade A screened schools appear to be an artifact of selection bias. Concerns about access to such schools (expressed, for example, in Harris and Fessenden (2017)) may therefore be overblown. On the other hand, Grade A attendance convincingly increases composite indicators of college and career preparedness. These results may reflect the greater availability of the advanced courses that contribute to the composites in Grade A schools.

On the methodological side, the NYC analysis demonstrates the precision gains from ex-

[22] Angrist and Rokkanen (2015) reach similar conclusions regarding the external validity of RD estimates of the effects of attending a Boston exam school.

ploitation of general risk in markets with mixed multiple tie-breakers. But because different risk sources affect screened and unscreened school attendance rates differently, the IV exclusion restriction in this context turns in part on a common effects assumption. It's therefore worth asking whether screened and unscreened schools should indeed be treated as having the same effect. Our analysis supports the idea that screened and unscreened Grade A schools can be pooled and treated as a homogeneous sector with a common average causal effect.

In an ongoing work, Angrist, Pathak and Zarate (2017) deploy the methods developed here in a study of Chicago's exam schools. Chicago combines centralizing non-lottery tie-breaking with school-specific lotteries. Further afield, our theoretical framework may be applicable to an analysis of causal effects of centrally-assigned entry-level jobs in medicine and law. For example, the US National Residency Matching Program assigns medical school graduates to hospitals using a version of DA with non-lottery tie-breakers (Roth and Peranson, 1999). This match can be leveraged to answer questions about the effects of alternative medical internships, such as the value of experience in high-volume hospitals. Our framework may also be useful to study the effects of resources allocated by some auction mechanisms.

Our provisional agenda for further research also includes an investigation of econometric implementation strategies, such as bandwidth selection. Zajonc (2012) and Papay *et al.* (2011) propose procedures for joint bandwidth selection in RD designs with multiple tie-breakers. Multivariate procedures may have better properties than our one-size-fits-all approach. The relative statistical performance of 2SLS and semiparametric estimators likewise warrants investigation, as does the development of propensity score estimators that compute the score by simulation. Finally, inference on treatment effects in the application discussed here relies on conventional large sample reasoning of the sort widely applied in empirical RD applications. It seems natural to consider permutation or randomization inference along the lines suggested by Canay and Kamat (2017) and Cattaneo *et al.* (2015, 2017), along with optimal inference and estimation strategies such as those recently introduced by Armstrong and Kolesár (2018) and Imbens and Wager (2018).

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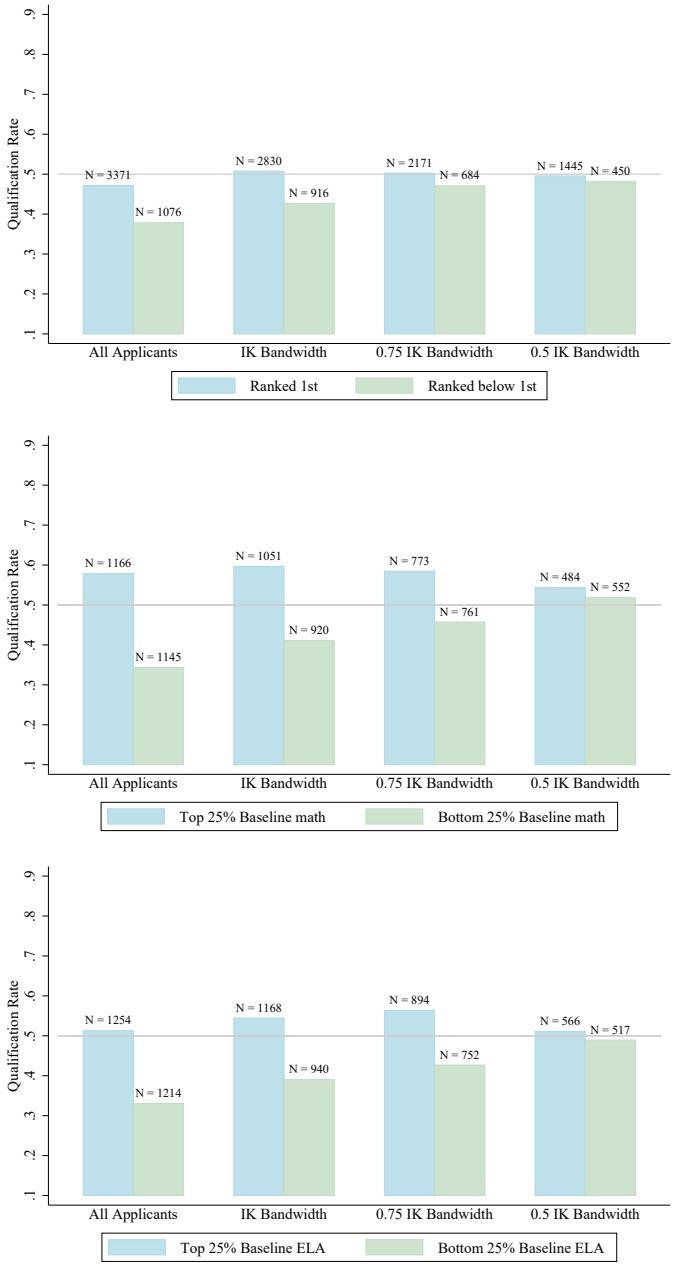
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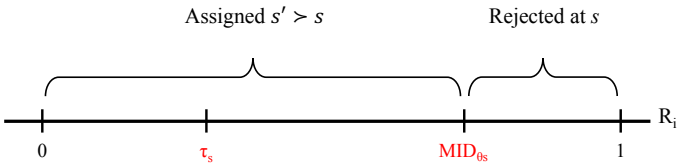
Figure 1: Qualification Rates Near the TH Cutoff



Notes: Bars show pooled qualification rates for applicants to Townsend Harris. The first panel shows qualification rates separately for applicants ranking the program as first choice and for applicants ranking it second or lower. The second and third panel show qualification rates separately for groups of applicants with baseline math and ELA scores in the upper and lower quartiles of the applicant score distribution. Qualification is defined as clearing the relevant program cutoff. The figure aggregates data for the three cohorts in the analysis sample. Bandwidths are 0.007 in 2011/12, 0.073 in 2012/13, and 0.437 in 2013/14, for a running variable between 0-1. Baseline scores are from 6th grade.

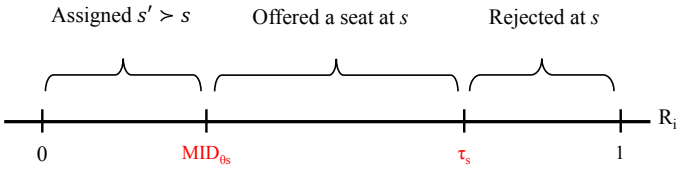
Figure 2: Visualizing Risk under Serial Dictatorship

A. No Risk at s



Probability of assignment to s is zero when $\tau_s < MID_{\theta s}$

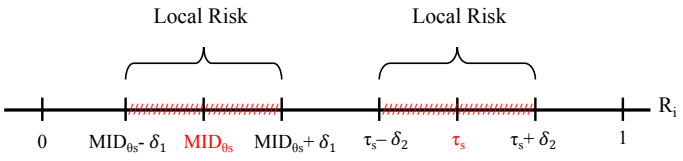
B. Global Risk at s



Probability of assignment to s when $MID_{\theta s} < \tau_s$

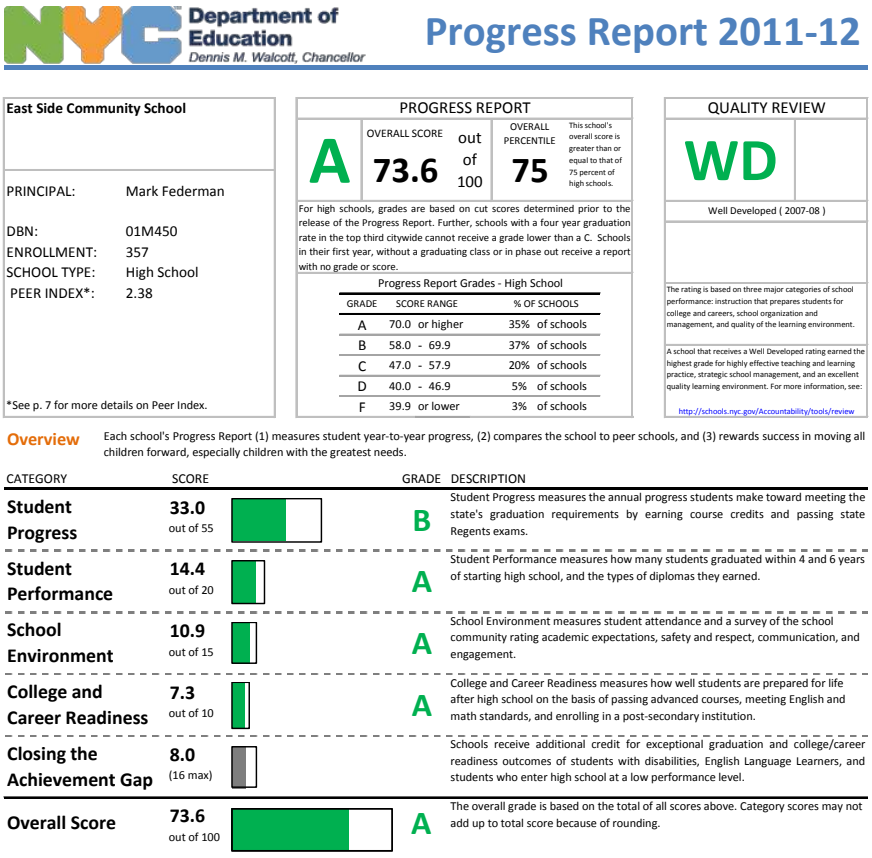
$\tau_s - MID_{\theta s}$	when R_i is uniform
$F_R(\tau_s \mid \theta) - F_R(MID_{\theta s} \mid \theta)$	when R_i has distribution F

C. Local Risk at s



Notes: This figure illustrates risk under serial dictatorship. R_i is the tie-breaker. $MID_{\theta s}$ is the most forgiving cutoff at schools preferred to s and τ_s is the cutoff at s.

Figure 3: 2011/12 Progress Report for East Side Community School



NYC Department of Education
Dennis M. Walcott, Chancellor

Progress Report 2011-12

East Side Community School

PRINCIPAL: Mark Federman

DBN: 01M450
ENROLLMENT: 357
SCHOOL TYPE: High School
PEER INDEX*: 2.38

*See p. 7 for more details on Peer Index.

PROGRESS REPORT

A

OVERALL SCORE 73.6 out of 100

OVERALL PERCENTILE 75 — This school's overall score is greater than or equal to that of 75 percent of high schools.

For high schools, grades are based on cut scores determined prior to the release of the Progress Report. Further, schools with a four year graduation rate in the top third citywide cannot receive a grade lower than a C. Schools in their first year, without a graduating class or in phase out receive a report with no grade or score.

Progress Report Grades - High School

GRADE	SCORE RANGE	% OF SCHOOLS
A	70.0 or higher	35% of schools
B	58.0 - 69.9	37% of schools
C	47.0 - 57.9	20% of schools
D	40.0 - 46.9	5% of schools
F	39.9 or lower	3% of schools

QUALITY REVIEW

WD

Well Developed (2007-08)

The rating is based on three major categories of school performance: instruction that prepares students for college and careers, school organization and management, and quality of the learning environment.

A school that receives a Well Developed rating earned the highest grade for highly effective teaching and learning practice, strategic school management, and an excellent quality learning environment. For more information, see:

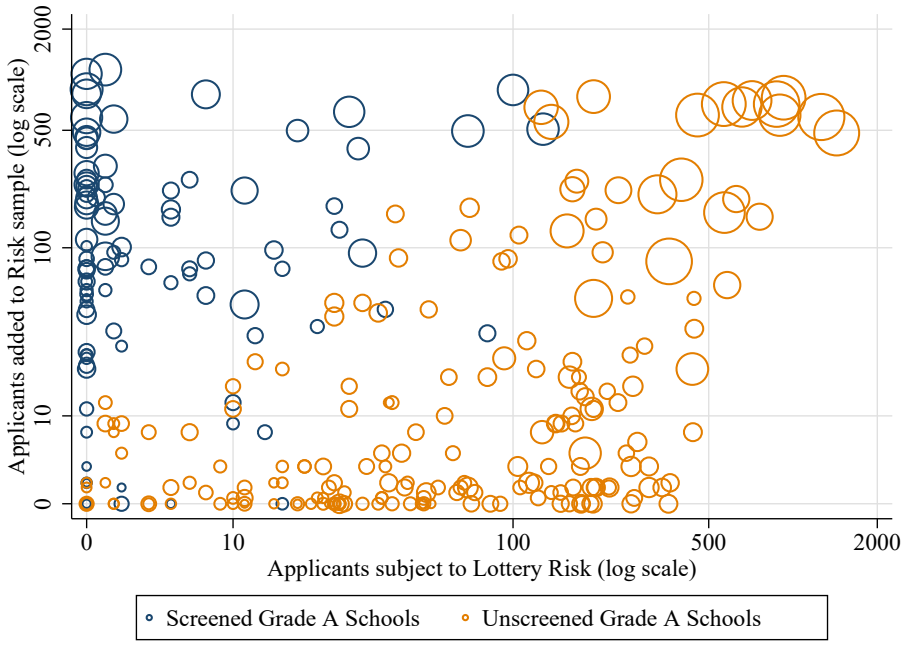
http://schools.nyc.gov/Accountability/tools/review

Overview Each school's Progress Report (1) measures student year-to-year progress, (2) compares the school to peer schools, and (3) rewards success in moving all children forward, especially children with the greatest needs.

CATEGORY	SCORE	GRADE	DESCRIPTION
Student Progress	**33.0** out of 55	B	Student Progress measures the annual progress students make toward meeting the state's graduation requirements by earning course credits and passing state Regents exams.
Student Performance	**14.4** out of 20	A	Student Performance measures how many students graduated within 4 and 6 years of starting high school, and the types of diplomas they earned.
School Environment	**10.9** out of 15	A	School Environment measures student attendance and a survey of the school community rating academic expectations, safety and respect, communication, and engagement.
College and Career Readiness	**7.3** out of 10	A	College and Career Readiness measures how well students are prepared for life after high school on the basis of passing advanced courses, meeting English and math standards, and enrolling in a post-secondary institution.
Closing the Achievement Gap	**8.0** (16 max)		Schools receive additional credit for exceptional graduation and college/career readiness outcomes of students with disabilities, English Language Learners, and students who enter high school at a low performance level.
Overall Score	**73.6** out of 100	A	The overall grade is based on the total of all scores above. Category scores may not add up to total score because of rounding.

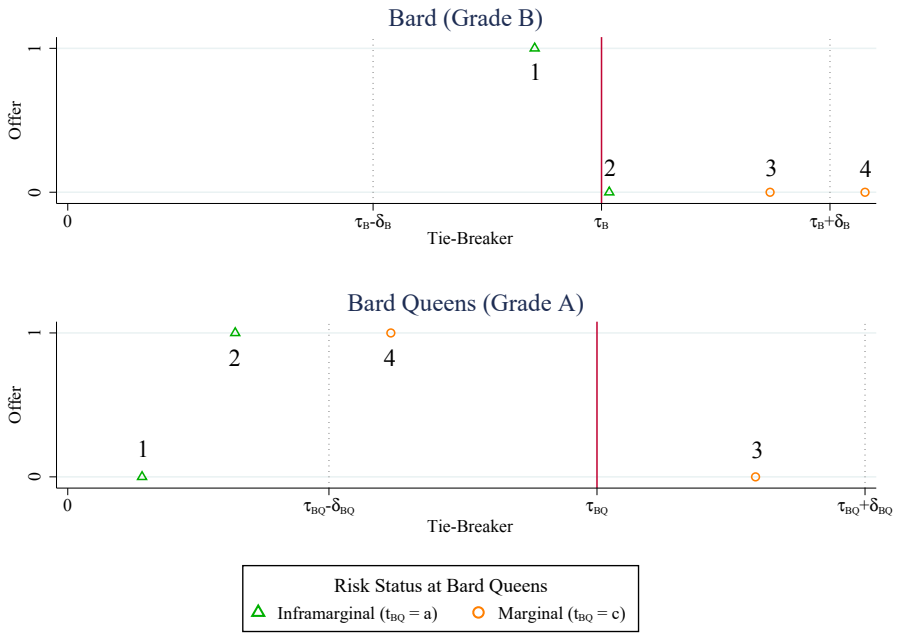
Notes: This figure shows the 2011/12 progress report for East Side Community School. Source: *www.crpe.org*

Figure 4: Sample Size Gains Relative to Lottery Risk



Notes: This figure plots increases in the number of applicants with non-degenerate risk of assignment at individual schools, ordered by the number of applicants who have risk when screened school admission is treated as determined solely by priorities. The number of applicants added measures the number of additional students at risk when risk is determined by running variable variation in a bandwidth around screened school cutoffs as well as by lottery risk. Circle sizes plot school capacity. Declines in risk are not shown.

Figure 5: The Fate of Four Applicants



Notes: This figure describes risk for four applicants who rank Grade B Bard first and Grade A Bard Queens second, both screened schools. The x-axes show tie-breakers for each school. Applicants plotted in orange fail to clear the Bard cutoff, though only one is marginal there. Both are marginal at Bard Queens; one gets an offer. Applicants plotted in green fall outside and below the Bard Queens bandwidth, though both are marginal at Bard. For these two, risk at Bard Queens (a Grade A school) is generated by proximity to the cutoff at higher-ranked Bard.

Table 1. New York High School Characteristics

	Grade A schools			Grade B-F Schools	Ungraded Schools
	All	Screened	Unscreened		
	(1)	(2)	(3)	(4)	(5)
Panel A. Average Performance Levels					
SAT Math (200-800)	531	606	481	464	440
SAT Reading (200-800)	522	587	479	465	449
Graduation	0.75	0.89	0.68	0.59	0.38
College- and career-prepared	0.65	0.84	0.54	0.39	0.27
College-ready	0.59	0.82	0.45	0.34	0.24
Panel B. School Characteristics					
Black	0.20	0.12	0.25	0.32	0.39
Hispanic	0.35	0.26	0.41	0.40	0.43
Special Education	0.12	0.06	0.16	0.17	0.27
Free or Reduced Price Lunch	0.68	0.55	0.76	0.77	0.75
In Manhattan	0.27	0.49	0.12	0.16	0.28
Number of grade 9 students	420	430	414	413	86
Number of grade 12 students	374	413	348	351	53
High school size	1596	1700	1527	1509	426
Inexperienced teachers	0.11	0.10	0.12	0.11	0.28
Advanced degree teachers	0.53	0.59	0.49	0.50	0.30
New school	0.00	0.00	0.01	0.00	0.21
School-year observations	355	119	236	694	715

Notes. This table reports weighted average characteristics of school-year observations. Specialized and charter high schools admit applicants in a separate match and are considered screened and unscreened schools, respectively. Panel A reports outcomes for cohorts enrolled in ninth grade in 2012-13, 2013-14 and 2014-15, and Panel B school characteristics in 2012-13, 2013-14 and 2014-15 by type of school. A screened school is any school without unscreened programs. Graduation outcomes condition on ninth grade enrollment in the year following the match and are available for the first and second cohort only. Inexperienced teachers have 3 or fewer years of experience and advanced degree teachers a Masters or higher degree.

Table 2. Student Characteristics

	Ninth Grade Students		Eighth Grade Applicants in Match			
	All	Enrolled in Grade A	All	Listed Match A	Enrolled in Match A	At Risk at Match A
	(1)	(2)	(3)	(4)	(5)	(6)
Demographics						
Black	30.7	20.1	29.1	29.3	22.9	22.5
Hispanic	40.2	33.7	38.9	39.3	38.2	40.1
Female	49.2	53.1	51.5	52.5	54.0	51.3
Special education	19.0	5.6	7.6	7.3	6.4	6.0
English language learners	7.5	4.4	6.0	5.7	5.2	5.0
Free lunch	78.6	70	77.3	77.2	73.6	75.6
Baseline scores						
Math (standardized)	0.056	0.528	0.207	0.233	0.334	0.333
English (standardized)	0.022	0.466	0.168	0.196	0.288	0.274
Offer rates						
Grade A school		81.8	29.4	34.6	87.2	47.2
Grade A screened school		28.5	9.9	11.7	26.5	12.9
Grade A unscreened school		53.3	19.5	22.9	60.6	34.3
Listed Grade A first		82.6	47.3	55.6	84.4	78.1
9th grade enrollment						
Grade A school	30.9	100	32.7	37.6	100	49.7
Grade A screened school	11.6	39.8	13.2	15.0	28.4	16.6
Grade A unscreened school	19.4	60.5	19.7	22.8	71.9	33.4
Students	182,249	48,985	153,107	130,160	40,301	30,760
Schools	603	174	569	567	159	532
School-year observations	1672	355	1584	1562	319	1420

Notes. This table describes the population of NYC students. Column 1 and 2 show statistics for students enrolled in ninth grade in the 2012-13, 2013-14 and 2014-15 school years with non-missing demographics and baseline test scores. Columns 3 to 6 show statistics for ninth grade applicants, who participated in the NYC high school match one year earlier. A match A school is a Grade A school that participates in the main NYC high school match. Students are said to have risk when they have a propensity score strictly between zero and one and they're in a score cell with variation in Grade A school offers. Baseline scores are from sixth grade and demographics from eighth grade.

Table 3. Statistical Tests for Balance

	All Applicants		Grade A Applicants with General Risk	
	Non-offered mean	Offer gap	Non-offered mean	Offer gap
	(1)	(2)	(3)	(4)
	Panel A. Application Covariates			
Grade A listed first	0.393	0.483***	0.761	0.010*
		(0.002)		(0.005)
# of screened Grade A schools listed	1.11	0.533***	1.19	0.005
		(0.011)		(0.006)
# of unscreened Grade A schools listed	1.69	0.228***	2.10	-0.012
		(0.008)		(0.017)
	Panel B. Baseline Covariates			
Black	0.339	-0.130***	0.230	-0.003
		(0.003)		(0.006)
Hispanic	0.405	-0.055***	0.402	0.005
		(0.003)		(0.007)
Female	0.527	0.003	0.517	0.002
		(0.003)		(0.008)
Special education	0.078	-0.019***	0.061	-0.004
		(0.001)		(0.004)
English language learners	0.075	-0.014***	0.050	0.001
		(0.001)		(0.004)
Free lunch	0.846	-0.100***	0.775	-0.009
		(0.003)		(0.007)
Baseline scores				
Math (standardized)	0.110	0.379***	0.284	0.006
		(0.005)		(0.010)
English (standardized)	0.081	0.349***	0.219	0.007
		(0.006)		(0.012)
N		130,160		30,760

Notes. This table reports balance statistics, computed by regressing covariates on dummies indicating a Grade A offer and an ungraded school offer, controlling for saturated Grade A and ungraded school propensity scores and running variable controls (column 4). The sample is limited to applicants with non-missing demographics and baseline test scores. Robust standard errors are in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 4. Grade A School 2SLS Estimates

	All Applicants		Applicants with General Risk	
	Non-enrolled mean	OLS	Non-offered mean	2SLS
	(1)	(2)	(3)	(4)
Panel A. First Stage Estimates				
SAT outcomes (years of exposure)			0.500	1.79***
				(0.023)
Binary outcomes (ever enrolled)			0.173	0.636***
				(0.008)
Panel B. Second Stage Estimates				
SAT Math (200-800)	474	6.48***	515	2.23***
	(106)	(0.152)	(109)	(0.716)
SAT Reading (200-800)	473	5.39***	510	0.594
	(93)	(0.138)	(93)	(0.657)
N		124,902		22,944
Graduated	0.697	0.038***	0.793	0.029**
		(0.003)		(0.013)
College- and career-prepared	0.422	0.105***	0.587	0.095***
		(0.003)		(0.015)
College-ready	0.367	0.075***	0.541	0.057***
		(0.003)		(0.014)
N		120,716		19,202

Notes. This table reports estimates of the effects of Grade A high school enrollment. OLS estimates are from models that omit propensity score controls and include all students in the three match cohorts. 2SLS estimates are from models with dummies for Grade A and ungraded schools treated as endogenous, limiting the sample to students with Grade A assignment risk. All models include controls for baseline math and English scores, free lunch status, SPED and ELL status, gender, and race/ethnicity indicators. Estimates in column 4 are from models that include running variable controls. Robust standard errors are in parenthesis for estimates and standard deviations for non-offered means. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5. Multi-Sector Grade A 2SLS Estimates

		OLS		2SLS Using General Risk		2SLS Using Lottery Risk	
		Screened Grade A	Unscreened Grade A	Screened Grade A	Unscreened Grade A	Screened Grade A	Unscreened Grade A
		(1)	(2)	(3)	(4)	(5)	(6)
SAT Math		18.8***	1.32***	2.91**	2.39***	1.86	2.05**
(200-800)		(0.277)	(0.167)	(1.24)	(0.757)	(2.81)	(0.885)
	p-value			0.690		0.943	
SAT Reading		15.9***	0.987***	1.42	0.525	-0.900	0.614
(200-800)		(0.252)	(0.153)	(1.14)	(0.697)	(2.64)	(0.806)
	p-value			0.451		0.561	
	N	124,902		24,943		13,097	
Graduated		0.018***	0.035***	0.048***	0.024	-0.017	0.024
		(0.004)	(0.003)	(0.018)	(0.015)	(0.056)	(0.019)
	p-value			0.223		0.467	
College- and career-prepared		0.112***	0.087***	0.086***	0.101***	0.167**	0.136***
		(0.004)	(0.003)	(0.021)	(0.016)	(0.070)	(0.020)
	p-value			0.503		0.662	
College-ready		0.103***	0.044***	0.107***	0.042***	0.018	0.051***
		(0.004)	(0.003)	(0.021)	(0.015)	(0.067)	(0.019)
	p-value			0.004		0.623	
	N	120,716		20,596		11,493	

Notes. This table reports OLS and 2SLS estimates for models that separately identify screened and unscreened Grade A effects. OLS models omit propensity score controls and include all students in the three match cohorts. 2SLS models treat both sectors as well as ungraded as endogenous, and limit the sample to students with either screened or unscreened Grade A assignment risk. All models include baseline covariate controls, described in the notes to Table 4. Columns 3 and 4 include running variable controls. P-values are from tests that screened and unscreened Grade A effects are equal in columns 3 and 4, and in columns 5 and 6, respectively. Robust standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6. Marginal and Non-marginal Screened Grade A Effects

	SAT Math			Graduation		
	(1)	(2)	(3)	(4)	(5)	(6)
Unscreened Grade A	2.39***	2.11**	2.42***	0.024	0.027	0.023
	(0.757)	(0.842)	(0.760)	(0.015)	(0.016)	(0.015)
Screened Grade A	2.91**	2.59		0.048***	0.066	
	(1.24)	(2.68)		(0.018)	(0.047)	
Screened Grade A - Marginal (t_c)			2.58**			0.047***
			(1.30)			(0.018)
Screened Grade A - Not marginal			1.91			0.071*
			(2.44)			(0.040)
Instruments for Screened Grade A	Screened Grade A	Screened Grade A × t_a	Screened Grade A × t_a Screened Grade A × t_c	Screened Grade A	Screened Grade A × t_a	Screened Grade A × t_a Screened Grade A × t_c
N	24,943	19,685	24,887	20,596	16,147	20,554

Notes. Columns 1 and 4 repeat estimates from columns 3 and 4 in Table 5. Estimates in columns 2 and 5 are from models that instrument screened attendance with a dummy indicating receipt of screened Grade A offer for applicants inframarginal (t_a) at the school where the offer was received. Columns 3 and 6 report 2SLS estimates that allow screened Grade A effects to differ for marginal (t_c) applicants at the school where the offer was received and non-marginal applicants. Non-marginal applicants include inframarginal applicants (t_a) and students who enrolled in a Screened Grade A school for which they did not receive an offer. The sample here includes applicants with either Grade A unscreened school or Grade A screened school assignment risk, and with assignment risk for any of the instruments used in each model. All models include propensity score controls, running variable controls, and the covariates used as controls for Table 5. Robust standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%.

A Proofs

A.1 Proposition 1

When $t = n$, $R_i > \tau_A + \delta$ for sufficiently small $\delta > 0$, and hence $1(R_i \leq \tau_A) = 0$. This implies

$$\lim_{\delta \to 0} E[1(R_i \leq \tau_A) | \theta_i = \theta, t_{iA}(\delta) = n, W_i = w] = 0.$$

When $t = a$, $R_i \leq \tau_A - \delta$ for sufficiently small $\delta > 0$, and hence $1(R_i \leq \tau_A) = 1$. This implies

$$\lim_{\delta \to 0} E[1(R_i \leq \tau_A) | \theta_i = \theta, t_{iA}(\delta) = a, W_i = w] = 1.$$

Finally, when $t = c$, we have:

$$\begin{aligned}
&\lim_{\delta \to 0} E[1(R_i \leq \tau_A) | \theta_i = \theta, t_{iA}(\delta) = c, W_i = w] \\
&\quad = \lim_{\delta \to 0} \frac{P(\tau_A - \delta < R_i \leq \tau_A | \theta_i = \theta, W_i = w)}{P(\tau_A - \delta < R_i \leq \tau_A + \delta | \theta_i = \theta, W_i = w)} \\
&\quad = \lim_{\delta \to 0} \frac{F_R(\tau_A | \theta, w) - F_R(\tau_A - \delta | \theta, w)}{F_R(\tau_A + \delta | \theta, w) - F_R(\tau_A - \delta | \theta, w)} \\
&\quad = \lim_{\delta \to 0} \frac{\{F_R(\tau_A | \theta, w) - F_R(\tau_A - \delta | \theta, w)\}/\delta}{\{F_R(\tau_A + \delta | \theta, w) - F_R(\tau_A | \theta, w)\}/\delta + \{F_R(\tau_A | \theta, w) - F_R(\tau_A - \delta | \theta, w)\}/\delta} \\
&\quad = \frac{F'_R(\tau_A | \theta, w)}{2F'_R(\tau_A | \theta, w)} = 0.5,
\end{aligned}$$

where the last equality uses Assumption 1(a), which states $F'_R(\tau_A | \theta, w) \neq 0$. The second-to-last equality is a consequence of the limit laws; e.g., $\lim_{x \to a} \frac{f(x)}{g(x)} = \frac{\lim_{x \to a} f(x)}{\lim_{x \to a} g(x)}$ provided that $\lim_{x \to a} g(x) \neq 0$.

A.2 Proposition 4

We prove Proposition 4 using a similar strategy as the proof of Theorem 1 in Abdulkadiroğlu *et al.* (2017b). Note first that admissions cutoffs ξ in a continuum market do not depend on the realized tie-breakers R_is and bandwidth δ: DA in the continuum depends on the R_is only through $G(I_0)$, the fraction of applicants in set $I_0 = \{i \in I \mid \theta_i \in \Theta_0, r_{iv} \leq r_v \text{ for all } v\}$ with various choices of Θ_0 and r. In particular, $G(I_0)$ doesn't depend on tie-breaker realizations in the continuum market. For the empirical CDF of each tie-breaker conditional on each type, $\hat{F}_v(\cdot|\theta)$, the Glivenko-Cantelli theorem for independent but non-identically distributed random variables implies $\hat{F}_v(\cdot|\theta) = F_v(\cdot|\theta)$ for any v and θ (Wellner, 1981). $G(I_0)$ also doesn't depend on reference tie-breaker r and bandwidth δ since they affect only the distribution of a single student i's tie-breaker R_i, which has no effect on $G(I_0)$ or cutoffs. Since cutoffs ξ are constant, marginal priority ρ_s is also constant for every school s.

Now, consider the propensity score for school s. First, applicants who don't rank s have $p_s(\theta) = 0$. If $\theta \in \Theta_s^n$, then $\rho_{\theta s} > \rho_s$. Therefore,

$$p_s(\theta) = 0 \text{ if } \theta \in \Theta_s^n \cup (\Theta \backslash \Theta_s).$$

Second, if $\theta \in \Theta_s^a \cup \Theta_s^c$, then the type θ applicant may be assigned a more preferred school $\tilde{s} \in B_{\theta s}$, where $\rho_{\theta\tilde{s}} = \rho_{\tilde{s}}$. For each tie-breaker v, the proportion of type θ applicants assigned some $\tilde{s} \in B_{\theta s}^v$ where $\rho_{\theta\tilde{s}} = \rho_{\tilde{s}}$ is $F_v(MID_{\theta s}^v|\theta)$. This means for each v, the probability of not being assigned any $\tilde{s} \in B_{\theta s}^v$ where $\rho_{\theta\tilde{s}} = \rho_{\tilde{s}}$ is $1-F_v(MID_{\theta s}^v|\theta)$. Since tie-breakers are assumed to be distributed independently of one another, the probability of not being assigned any $\tilde{s} \in B_{\theta s}$ where $\rho_{\theta\tilde{s}} = \rho_{\tilde{s}}$ for a type θ applicant is $\Pi_v(1 - F_v(MID_{\theta s}^v|\theta))$. Every applicant of type $\theta \in \Theta_s^a$ who is not assigned a more preferred choice is assigned s because $\rho_{\theta s} < \rho_s$, and so

$$p_s(\theta) = \Pi_v(1 - F_v(MID_{\theta s}^v|\theta)) \text{ if } \theta \in \Theta_s^a.$$

Finally, consider applicants of type $\theta \in \Theta_s^c$ who are not assigned a more preferred choice than s. The fraction of applicants $\theta \in \Theta_s^c$ who are not assigned a more preferred choice is $\Pi_v(1 - F_v(MID_{\theta s}^v|\theta))$. Also, the values of the tie-breaking variable $v(s)$ of these applicants are larger than $MID_{\theta s}^{v(s)}$. If $\tau_s < MID_{\theta s}^{v(s)}$, then no such applicant is assigned s. If $\tau_s \geq MID_{\theta s}^{v(s)}$, then the fraction of applicants who are assigned s within this set is given by $\frac{F_{v(s)}(\tau_s|\theta) - F_{v(s)}(MID_{\theta s}^{v(s)}|\theta)}{1-F_{v(s)}(MID_{\theta s}^{v(s)}|\theta)}$. Hence, conditional on $\theta \in \Theta_s^c$ and not being assigned a choice higher than s, the probability of being assigned s is given by $\max\{0, \frac{F_{v(s)}(\tau_s|\theta) - F_{v(s)}(MID_{\theta s}^{v(s)}|\theta)}{1-F_{v(s)}(MID_{\theta s}^{v(s)}|\theta)}\}$. Therefore,

$$p_s(\theta) = \prod_{v \neq v(s)} (1 - F_v(MID_{\theta s}^v|\theta)) \times \max\left\{0, F_{v(s)}(\tau_s|\theta) - F_{v(s)}(MID_{\theta s}^{v(s)}|\theta)\right\} \text{ if } \theta \in \Theta_s^c.$$

Note that when $v(s) = 0$, we have the lottery tie-breaker. In that case, $F_{v(s)}(\tau_s|\theta) = \tau_s$ and $F_{v(s)}(MID_{\theta s}^{v(s)}|\theta) = MID_{\theta s}^0$.

A.3 Theorem 1

Take any continuum market with the general tie-breaking structure in Section 3. For each $\delta > 0$ and each tie-breaker $v = 2, ..., V+1$, let $e(v)$ be short-hand notation for "$\theta_i = \theta, R_{iu} > MID_{\theta s}^u$ for $u = 1, ..., v-1, T_i(\delta) = T$, and $W_i = w$." Similarly, $e(1)$ is short-hand notation for "$\theta_i = \theta, T_i(\delta) = T$, and $W_i = w$." Let $\psi_s(\theta, T, \delta, w) \equiv E[D_i(s)|e(1)]$ be assignment risk for an applicant with $\theta_i = \theta, T_i(\delta) = T$, and characteristics $W_i = w$. Our proofs use a lemma that describes this assignment risk. To state the lemma, for $v > 0$, let

$$\Phi_\delta(v) \equiv \begin{cases} \dfrac{F_v(MID_{\theta s}^v|e(v)) - F_v(MID_{\theta s}^v - \delta|e(v))}{F_v(MID_{\theta s}^v + \delta|e(v)) - F_v(MID_{\theta s}^v - \delta|e(v))} & \text{if } t_b(\delta) = c \text{ for some } b \in B_{\theta s}^v \\ 1 & \text{otherwise.} \end{cases}$$

We use this object to define $\Phi_\delta \equiv (1 - MID_{\theta s}^0)\Pi_{v=1}^V \Phi_\delta(v)$. Finally, let

$$\Phi'_\delta \equiv \begin{cases} \max\left\{0, \dfrac{F_{v(s)}(\tau_s|e(V+1)) - F_{v(s)}(\tau_s - \delta|e(V+1))}{F_{v(s)}(\tau_s + \delta|e(V+1)) - F_{v(s)}(\tau_s - \delta|e(V+1))}\right\} & \text{if } v(s) > 0 \\ \max\left\{0, \dfrac{\tau_s - MID_{\theta s}^0}{1 - MID_{\theta s}^0}\right\} & \text{if } v(s) = 0. \end{cases}$$

Lemma 1. *In the general tie-breaking setting of Section 3, for any fixed $\delta > 0$ such that $\delta < \min_{\theta,s,v} |\tau_s - MID^v_{\theta,s}|$, we have:*

$$\psi_s(\theta, T, \delta, w) = \begin{cases} 0 & \text{if } t_s(\delta) = n \text{ or } t_b(\delta) = a \text{ for some } b \in B_{\theta s}, \\ \Phi_\delta & \text{otherwise and } t_s(\delta) = a, \\ \Phi_\delta \times \Phi'_\delta & \text{otherwise and } t_s(\delta) = c. \end{cases}$$

Proof of Lemma 1. We start verifying the first line in $\psi_s(\theta, T, \delta, w)$. Applicants who don't list s have $\psi_s(\theta, T, \delta, w) = 0$. Among those who list s, those of $t_s(\delta) = n$ have $\theta \in \Theta^n_s$ or, if $v(s) \neq 0$, $\theta \in \Theta^c_s$ and $R_{iv(s)} > \tau_s + \delta$. If $\theta \in \Theta^n_s$, then $\rho_{\theta s} > \rho_s$ so that $\psi_s(\theta, T, \delta, w) = 0$. Even if $\theta \notin \Theta^n_s$, as long as $\theta \in \Theta^c_s$ and $R_{iv(s)} > \tau_s + \delta$, student i never clears the cutoff at school s so $\psi_s(\theta, T, \delta, w) = 0$.

To show the remaining cases, take as given that it is not the case that $t_s(\delta) = n$ or $t_b(\delta) = a$ for some $b \in B_{\theta s}$. Applicants with $t_b(\delta) \neq a$ for all $b \in B_{\theta s}$ and $t_s(\delta) = a$ or c may be assigned $b \in B_{\theta s}$, where $\rho_{\theta b} = \rho_b$. Since the (aggregate) distribution of tie-breaking variables for type θ students is $\hat{F}_v(\cdot|\theta) = F_v(\cdot|\theta)$, conditional on $T_i(\delta) = T$, the proportion of type θ applicants not assigned any $b \in B_{\theta s}$ where $\rho_{\theta b} = \rho_b$ is $\Phi_\delta = (1 - MID^0_{\theta s})\Pi_v \Phi_\delta(v)$ since each $\Phi_\delta(v)$ is the probability of not being assigned to any $b \in B^v_{\theta s}$. To see why $\Phi_\delta(v)$ is the probability of not being assigned to any $b \in B^v_{\theta s}$, note that if $t_b(\delta) \neq c$ for all $b \in B^v_{\theta s}$, then $t_b(\delta) = n$ for all $b \in B^v_{\theta s}$ so that applicants are never assigned to any $b \in B^v_{\theta s}$. Otherwise, i.e., if $t_b(\delta) = c$ for some $b \in B^v_{\theta s}$, then applicants are assigned to s if and only if their values of tie-breaker v clear the cutoff of the school that produces $MID^v_{\theta s}$, where applicants have $t_s(\delta) = c$. This event happens with probability

$$\frac{F_v(MID^v_{\theta s}|e(v)) - F_v(MID^v_{\theta s} - \delta|e(v))}{F_v(MID^v_{\theta s} + \delta|e(v)) - F_v(MID^v_{\theta s} - \delta|e(v))},$$

implying that $\Phi_\delta(v)$ is the probability of not being assigned to any $b \in B^v_{\theta s}$.

Given this fact, to see the second line, note that every applicant of type $t_s(\delta) = a$ who is not assigned a higher choice is assigned s for sure because $\rho_{\theta s} < \rho_s$ or $\rho_{\theta s} + R_{iv(s)} < \xi_s$. Therefore, we have

$$\psi_s(\theta, T, \delta, w) = \Phi_\delta.$$

Finally, consider applicants with $t_s(\delta) = c$. The fraction of those who are not assigned a higher choice is Φ_δ, as explained above. Also, for tie-breaker $v(s)$, the tie-breaker values of these applicants are larger (worse) than $MID^{v(s)}_{\theta s}$. If $\tau_s < MID^{v(s)}_{\theta s}$, then no such applicant is assigned s. If $\tau_s \geq MID^{v(s)}_{\theta s}$, then the fraction of applicants who are assigned s conditional on $\tau_s \geq MID^{v(s)}_{\theta s}$ is given by

$$\max\left\{0, \frac{F_{v(s)}(\tau_s|e(V+1)) - \max\{F_{v(s)}(MID^{v(s)}_{\theta s}|e(V+1)), F_{v(s)}(\tau_s - \delta|e(V+1))\}}{F_{v(s)}(\tau_s + \delta|e(V+1)) - \max\{F_{v(s)}(MID^{v(s)}_{\theta s}|e(V+1)), F_{v(s)}(\tau_s - \delta|e(V+1))\}}\right\} \text{ if } v(s) \neq 0$$

and

$$\max\left\{0, \frac{\tau_s - MID^0_{\theta s}}{1 - MID^0_{\theta s}}\right\} \text{ if } v(s) = 0.$$

If $MID^{v(s)}_{\theta s} < \tau_s$, then $\delta < \min_{\theta,s,v} |\tau_s - MID^v_{\theta,s}|$ implies $MID^{v(s)}_{\theta s} < \tau_s - \delta$. This in turn implies

$$\max\{F_{v(s)}(MID^{v(s)}_{\theta s}|e(V+1)), F_{v(s)}(\tau_s - \delta|e(V+1))\} = F_{v(s)}(\tau_s - \delta|e(V+1)).$$

If $MID^{v(s)}_{\theta s} > \tau_s$, then $\delta < \min_{\theta,s,v} |\tau_s - MID^v_{\theta,s}|$ implies $MID^{v(s)}_{\theta s} > \tau_s + \delta$. By the definition of $e(V+1)$, $R_{iu} > MID^u_{\theta s}$ for $u = 1, ..., V$. Therefore, there is no applicant with $R_{iv(s)} > MID^{v(s)}_{\theta s}$ and $R_{iv(s)} \in [\tau_s - \delta, \tau_s + \delta]$.

Hence, conditional on $t_s(\delta) = c$ and not being assigned a choice more preferred than s, the probability of being assigned s is given by Φ'_δ. Therefore, for students with $t_s(\delta) = c$, we have $\psi_s(\theta, T, \delta, w) = \Phi_\delta \times \Phi'_\delta$. □

Lemma 2. *In the general tie-breaking setting of Section 3, for all s, θ, and sufficiently small $\delta > 0$, we have:*

$$\psi_s(\theta, T, \delta, w) = \begin{cases} 0 & \text{if } t_s(0) = n \text{ or } t_b(0) = a \text{ for some } b \in B_{\theta s}, \\ \Phi^*_\delta & \text{otherwise and } t_s(0) = a, \\ \Phi^*_\delta \times & \dfrac{F_{v(s)}(\tau_s|e(V+1)) - F_{v(s)}(\tau_s - \delta|e(V+1))}{F_{v(s)}(\tau_s + \delta|e(V+1)) - F_{v(s)}(\tau_s - \delta|e(V+1))} \\ & \text{otherwise and } t_s(0) = c \text{ and } v(s) \neq 0. \\ \Phi^*_\delta \times & \max\{0, \dfrac{\tau_s - MID^0_{\theta s}}{1 - MID^0_{\theta s}}\} \\ & \text{otherwise and } t_s(0) = c \text{ and } v(s) = 0. \end{cases} \tag{17}$$

where

$$\Phi^*_\delta(v) \equiv \begin{cases} \dfrac{F_v(MID^v_{\theta s} + \delta|e(v)) - F_v(MID^v_{\theta s}|e(v))}{F_v(MID^v_{\theta s} + \delta|e(v)) - F_v(MID^v_{\theta s} - \delta|e(v))} & \text{if } MID^v_{\theta s} = \tau_b \text{ and } t_b = c \text{ for some } b \in B^v_{\theta s}, \\ 1 & \text{otherwise} \end{cases}$$

and

$$\Phi^*_\delta \equiv (1 - MID^0_{\theta s}) \Pi_{v=1}^V \Phi^*_\delta(v).$$

Proof of Lemma 2. The first line follows from Lemma 1 and the fact that $t_s(0) = n$ or $t_b(0) = a$ for some $b \in B_{\theta s}$ imply $t_s(\delta) = n$ or $t_b(\delta) = a$ for some $b \in B_{\theta s}$ for sufficiently small $\delta > 0$.

For the remaining lines, first note that conditional on $t_s(0) \neq n$ and $t_b(0) \neq a$ for all $b \in B_{\theta s}$, we have $\Phi^*_\delta(v) = \Phi_\delta(v)$ and so $\Phi^*_\delta = \Phi_\delta$ holds for small enough δ. Φ^*_δ therefore is the probability of not being assigned to a school preferred to s in the last three cases.

The second line is then by the fact that $t_s(0) = a$ implies $t_s(\delta) = a$ for small enough $\delta > 0$. The third line is by the fact that for small enough $\delta > 0$,

$$\begin{aligned} \Phi'_\delta &= \max\left\{0, \frac{F_{v(s)}(\tau_s|e(V+1)) - F_{v(s)}(\tau_s - \delta|e(V+1))}{F_{v(s)}(\tau_s + \delta|e(V+1)) - F_{v(s)}(\tau_s - \delta|e(V+1))}\right\} \\ &= \frac{F_{v(s)}(\tau_s|e(V+1)) - F_{v(s)}(\tau_s - \delta|e(V+1))}{F_{v(s)}(\tau_s + \delta|e(V+1)) - F_{v(s)}(\tau_s - \delta|e(V+1))}, \end{aligned}$$

where we invoke Assumption 1(b), which implies $MID^v_{\theta s} \neq \tau_s$. The last line directly follows from Lemma 1. □

We use Lemma 2 to derive Theorem 1. We characterize $\lim_{\delta\to 0}\psi_s(\theta,T,\delta,w)$ and show that it coincides with $\psi_s(\theta,T)$ in the main text. In the first case in Lemma 2, $\psi_s(\theta,T,\delta,w)$ is constant (0) for any small enough δ. The constant value is also $\lim_{\delta\to 0}\psi_s(\theta,T,\delta,w)$ in this case.

To characterize $\lim_{\delta\to 0}\psi_s(\theta,T,\delta,w)$ in the remaining cases, note that by the differentiability of $F_v(\cdot|e(v))$ (recall Assumption 1), L'Hopital's rule implies:

$$\lim_{\delta\to 0}\frac{F_{v(s)}(\tau_s|e(V+1))-F_{v(s)}(\tau_s-\delta|e(V+1))}{F_{v(s)}(\tau_s+\delta|e(V+1))-F_{v(s)}(\tau_s-\delta|e(V+1))}=\frac{F'_{v(s)}(\tau_s|e(V+1))}{2F'_{v(s)}(\tau_s|e(V+1))}=0.5$$

and

$$\lim_{\delta\to 0}\frac{F_{v}(MID^v_{\theta s}+\delta|e(v))-F_{v}(MID^v_{\theta s}|e(v))}{F_{v}(MID^v_{\theta s}+\delta|e(v))-F_{v}(MID^v_{\theta s}-\delta|e(v))}=\frac{F'_{v}(MID^v_{\theta s}|e(v))}{2F'_{v}(MID^v_{\theta s}|e(v))}=0.5.$$

This implies $\lim_{\delta\to 0}\Phi^*_\delta(v)=0.5^{1\{MID^v_{\theta s}=\tau_b \text{ and } t_b=c \text{ for some } b\in B^v_{\theta s}\}}$ since $1\{MID^v_{\theta s}=\tau_b \text{ and } t_b=c \text{ for some } b\in B^v_{\theta s}\}$ does not depend on δ. Therefore

$$\lim_{\delta\to 0}\Phi^*_\delta=(1-MID^0_{\theta s})0.5^{m_s(\theta,T)}$$

where $m_s(\theta,T)=|\{v>0: MID^v_{\theta s}=\tau_b \text{ and } t_b=c \text{ for some } b\in B^v_{\theta s}\}|$.

Combining these limiting facts with the fact that the limit of a product of functions equals the product of the limits of the functions, we obtain the following: $\lim_{\delta\to 0}\psi_s(\theta,T,\delta,w)=0$ if (a) $t_s=n$ or (b) $t_b=a$ for some $b\in B_{\theta s}$. Otherwise,

$$\lim_{\delta\to 0}\psi_s(\theta,T,\delta,w)=\begin{cases}0.5^{m_s(\theta,T)}(1-MID^0_{\theta s}) & \text{if } t_s(0)=a\\ 0.5^{m_s(\theta,T)}\max\left\{0,\tau_s-MID^0_{\theta s}\right\} & \text{if } t_s(0)=c \text{ and } v(s)=0\\ 0.5^{1+m_s(\theta,T)}(1-MID^0_{\theta s}) & \text{if } t_s(0)=c \text{ and } v(s)>0.\end{cases}$$

This expression coincides with $\psi_s(\theta,T)$, completing the proof of Theorem 1.

A.4 Theorem 2

Here we prove the following lemmas used in the proof of Theorem 2 in the main text.

Lemma 3. *(Cutoff almost sure convergence)* $\hat{\xi}_N \xrightarrow{a.s.} \xi$ *where* ξ *denotes the vector of continuum market cutoffs.*

Lemma 4. *(Estimated local propensity score almost sure convergence) For all* $\theta\in\Theta, s\in S$, *and* $T\in\{a,c,n\}^S$, *we have* $\hat{\psi}_{Ns}(\theta,T;\delta_N)\xrightarrow{a.s.}\psi_s(\theta,T)$ *as* $N\to\infty$ *and* $\delta_N\to 0$.

Lemma 5. *(Bandwidth-specific propensity score almost sure convergence) For all* $\theta\in\Theta, s\in S$, $T\in\{a,c,n\}^S$, *and* δ_N *such that* $\delta_N\to 0$ *and* $N\delta_N\to\infty$ *as* $N\to\infty$, *we have* $\psi_{Ns}(\theta,T;\delta_N)\xrightarrow{p}\psi_s(\theta,T)$ *as* $N\to\infty$.

Proof of Lemma 3

The proof of Lemma 3 is analogous to the proof of Lemma 3 in Abdulkadiroğlu *et al.* (2017b) and available upon request. The main difference is that to deal with multiple non-lottery tie-breakers, the proof of Lemma 3 needs to invoke Assumption 1 and the Glivenko-Cantelli theorem for independent but non-identically distributed random variables (Wellner, 1981).

Proof of Lemma 4

$\hat{\psi}_{Ns}(\theta, T; \delta_N)$ is almost everywhere continuous in finite sample cutoffs $\hat{\xi}_N$, finite sample MIDs ($MID^v_{\theta s}$), and bandwidth δ_N. Since every $MID^v_{\theta s}$ is almost everywhere continuous in finite sample cutoffs $\hat{\xi}_N$, $\hat{\psi}_{Ns}(\theta, T; \delta_N)$ is almost everywhere continuous in finite sample cutoffs $\hat{\xi}_N$ and bandwidth δ_N. Recall $\delta_N \to 0$ by assumption while $\hat{\xi}_N \xrightarrow{a.s.} \xi$ by Lemma 3. Therefore, by the continuous mapping theorem, as $N \to \infty$, $\hat{\psi}_{Ns}(\theta, T; \delta_N)$ almost surely converges to $\hat{\psi}_{Ns}(\theta, T; \delta_N)$ with ξ replacing $\hat{\xi}_N$, which is $\psi_s(\theta, T)$.

Proof of Lemma 5

We use the following fact, which is implied by Example 19.29 in van der Vaart (2000).

Lemma 6. *Let X be a random variable distributed according to some CDF F over $[0,1]$. Let $F(\cdot|X \in [x-\delta, x+\delta])$ be the conditional version of F conditional on X being in a small window $[x-\delta, x+\delta]$ where $x \in [0,1]$ and $\delta \in (0,1]$. Let $X_1, ..., X_N$ be iid draws from F. Let $\hat{F}_N$ be the empirical CDF of $X_1, ..., X_N$. Let $\hat{F}_N(\cdot|X \in [x-\delta, x+\delta])$ be the conditional version of $\hat{F}_N$ conditional on a subset of draws falling in $[x-\delta, x+\delta]$, i.e., $\{X_i | i = 1, ..., n, X_i \in [x-\delta, x+\delta]\}$. Suppose (δ_N) is a sequence with $\delta_N \downarrow 0$ and $\delta_N \times N \to \infty$. Then $\hat{F}_N(\cdot|X \in [x-\delta_N, x+\delta_N])$ uniformly converges to $F(\cdot|X \in [x-\delta_N, x+\delta_N])$, i.e.,*

$$\sup_{x' \in [0,1]} |\hat{F}_N(x'|X \in [x-\delta_N, x+\delta_N]) - F(x'|X \in [x-\delta_N, x+\delta_N])| \to_p 0 \text{ as } N \to \infty \text{ and } \delta_N \to 0.$$

Proof of Lemma 6. We first prove the statement for $x \in (0,1)$. Let P be the probability measure of X and $\hat{P}_N$ be the empirical measure of $X_1, ..., X_N$. Note that

$$\begin{aligned}
&\sup_{x' \in [0,1]} |\hat{F}_N(x'|X \in [x-\delta_N, x+\delta_N]) - F(x'|X \in [x-\delta_N, x+\delta_N])| \\
=&\sup_{t \in [-1,1]} |\hat{F}_N(x+t\delta_N|X \in [x-\delta_N, x+\delta_N]) - F(x+t\delta_N|X \in [x-\delta_N, x+\delta_N])| \\
=&\sup_{t \in [-1,1]} |\frac{\hat{P}_N[x-\delta_N, x+t\delta_N]}{\hat{P}_N[x-\delta_N, x+\delta_N]} - \frac{P_X[x-\delta_N, x+t\delta_N]}{P_X[x-\delta_N, x+\delta_N]}| \\
=&\frac{1}{\hat{P}_N[x-\delta_N, x+\delta_N] P_X[x-\delta_N, x+\delta_N]} \\
&\times \sup_{t \in [-1,1]} |\hat{P}_N[x-\delta_N, x+t\delta_N] P_X[x-\delta_N, x+\delta_N] - \hat{P}_N[x-\delta_N, x+\delta_N] P_X[x-\delta_N, x+t\delta_N]|
\end{aligned}$$

$$
\begin{aligned}
=&\frac{1}{\hat{P}_N[x-\delta_N, x+\delta_N]P_X[x-\delta_N, x+\delta_N]} \\
&\times \sup_{t\in[-1,1]} |\hat{P}_N[x-\delta_N, x+t\delta_N](P_X[x-\delta_N, x+\delta_N] - \hat{P}_N[x-\delta_N, x+\delta_N]) \\
&\quad + \hat{P}_N[x-\delta_N, x+\delta_N](\hat{P}_N[x-\delta_N, x+t\delta_N] - P_X[x-\delta_N, x+t\delta_N])| \\
\leq&\frac{1}{\hat{P}_N[x-\delta_N, x+\delta_N]P_X[x-\delta_N, x+\delta_N]} \\
&\times \{ \sup_{t\in[-1,1]} \hat{P}_N[x-\delta_N, x+t\delta_N]|\hat{P}_N[x-\delta_N, x+\delta_N] - P_X[x-\delta_N, x+\delta_N]| \\
&\quad + \sup_{t\in[-1,1]} \hat{P}_N[x-\delta_N, x+\delta_N]|\hat{P}_N[x-\delta_N, x+t\delta_N] - P_X[x-\delta_N, x+t\delta_N]|\} \\
=&\frac{1}{P_X[x-\delta_N, x+\delta_N]} \\
&\times \{|\hat{P}_N[x-\delta_N, x+\delta_N] - P_X[x-\delta_N, x+\delta_N]| + \sup_{t\in[-1,1]} |\hat{P}_N[x-\delta_N, x+t\delta_N] - P_X[x-\delta_N, x+t\delta_N]|\} \\
=&\frac{A_N}{P_X[x-\delta_N, x+\delta_N]},
\end{aligned}
$$

where

$$
A_N = |\hat{P}_N[x-\delta_N, x+\delta_N] - P_X[x-\delta_N, x+\delta_N]| + \sup_{t\in[-1,1]} |\hat{P}_N[x-\delta_N, x+t\delta_N] - P_X[x-\delta_N, x+t\delta_N]|.
$$

The above inequality holds by the triangle inequality and the second last equality holds because $\sup_{t\in[-1,1]} \hat{P}_N[x-\delta_N, x+t\delta_N] = \hat{P}_N[x-\delta_N, x+\delta_N]$.

We show that $A_N / P_X[x-\delta_N, x+\delta_N] \xrightarrow{p} 0$. Example 19.29 in van der Vaart (2000) implies that the sequence of processes $\{\sqrt{n/\delta_N}(\hat{P}_N[x-\delta_N, x+t\delta_N] - P_X[x-\delta_N, x+t\delta_N]) : t \in [-1,1]\}$ converges in distribution to a Gaussian process in the space of bounded functions on $[-1,1]$ as $N \to \infty$. We denote this Gaussian process by $\{\mathbb{G}_t : t \in [-1,1]\}$. We then use the continuous mapping theorem to obtain

$$
\sqrt{n/\delta_N} A_N \xrightarrow{d} |\mathbb{G}_1| + \sup_{t\in[-1,1]} |\mathbb{G}_t|
$$

as $N \to \infty$. Since $\{\mathbb{G}_t : t \in [-1,1]\}$ has bounded sample paths, it follows that $|\mathbb{G}_1| < \infty$ and $\sup_{t\in[-1,1]} |\mathbb{G}_t| < \infty$ for sure. By the continuous mapping theorem, under the condition that $N\delta_N \to \infty$,

$$
\begin{aligned}
(1/\delta_N) A_N &= (1/\sqrt{N\delta_N}) \times \sqrt{n/\delta_N} A_N \\
&\xrightarrow{d} 0 \times (|\mathbb{G}_1| + \sup_{t\in[-1,1]} |\mathbb{G}_t|) \\
&= 0.
\end{aligned}
$$

This implies that $(1/\delta_N)A_N \xrightarrow{p} 0$, because for any $\epsilon > 0$,

$$\begin{aligned}\Pr(|(1/\delta_N)A_N| > \epsilon) &= \Pr((1/\delta_N)A_N < -\epsilon) + \Pr((1/\delta_N)A_N > \epsilon) \\ &\leq \Pr((1/\delta_N)A_N \leq -\epsilon) + 1 - \Pr((1/\delta_N)A_N \leq \epsilon) \\ &\to \Pr(0 \leq -\epsilon) + 1 - \Pr(0 \leq \epsilon) \\ &= 0,\end{aligned}$$

where the convergence holds since $(1/\delta_N)A_N \xrightarrow{d} 0$. To show that $A_N/P_X[x-\delta_N, x+\delta_N] \xrightarrow{p} 0$, it is therefore enough to show that $\lim_{N\to\infty}(1/\delta_N)P_X[x-\delta_N, x+\delta_N] > 0$. We have

$$\begin{aligned}(1/\delta_N)P_X[x-\delta_N, x+\delta_N] &= (1/\delta_N)(F_X(x+\delta_N) - F_X(x-\delta_N)) \\ &= (1/\delta_N)(2f(x)\delta_N + o(\delta_N)) \\ &= 2f(x) + o(1) \\ &\to 2f(x) \\ &> 0,\end{aligned}$$

where we use Taylor's theorem for the second equality and the assumption of $f(x) > 0$ for the last inequality.

We next prove the statement for $x = 0$. Note that

$$\begin{aligned}&\sup_{x'\in[0,1]} |\hat{F}_N(x'|X \in [-\delta_N, \delta_N]) - F(x'|X \in [-\delta_N, \delta_N])| \\ =& \sup_{t\in[0,1]} |\hat{F}_N(t\delta_N|X \in [0, \delta_N]) - F(t\delta_N|X \in [0, \delta_N])| \\ =& \sup_{t\in[0,1]} |\frac{\hat{F}_N(t\delta_N)}{\hat{F}_N(\delta_N)} - \frac{F_X(t\delta_N)}{F_X(\delta_N)}| \\ =& \frac{1}{\hat{F}_N(\delta_N)F_X(\delta_N)} \sup_{t\in[0,1]} |\hat{F}_N(t\delta_N)F_X(\delta_N) - \hat{F}_N(\delta_N)F_X(t\delta_N)| \\ =& \frac{1}{\hat{F}_N(\delta_N)F_X(\delta_N)} \sup_{t\in[0,1]} |\hat{F}_N(t\delta_N)(F_X(\delta_N) - \hat{F}_N(\delta_N)) + \hat{F}_N(\delta_N)(\hat{F}_N(t\delta_N) - F_X(t\delta_N))| \\ \leq& \frac{1}{\hat{F}_N(\delta_N)F_X(\delta_N)} \{ \sup_{t\in[0,1]} \hat{F}_N(t\delta_N)|\hat{F}_N(\delta_N) - F_X(\delta_N)| + \sup_{t\in[0,1]} \hat{F}_N(\delta_N)|\hat{F}_N(t\delta_N) - F_X(t\delta_N)| \} \\ =& \frac{1}{F_X(\delta_N)} \{ |\hat{F}_N(\delta_N) - F_X(\delta_N)| + \sup_{t\in[0,1]} |\hat{F}_N(t\delta_N) - F_X(t\delta_N)| \} = \frac{A_N^0}{F_X(\delta_N)},\end{aligned}$$

where $A_N^0 = |\hat{F}_N(\delta_N) - F_X(\delta_N)| + \sup_{t\in[0,1]} |\hat{F}_N(t\delta_N) - F_X(t\delta_N)|$. By the argument used in the above proof for $x \in (0,1)$, we have $(1/\delta_N)A_N^0 \xrightarrow{p} 0$. It also follows that

$$\begin{aligned}(1/\delta_N)F_X(\delta_N) &= (1/\delta_N)(f(0)\delta_N + o(\delta_N)) \\ &= f(0) + o(1) \\ &\to f(0) \\ &> 0.\end{aligned}$$

Thus, $\frac{A_N^0}{F_X(\delta_N)} \xrightarrow{p} 0$, and hence $\sup_{x' \in [0,1]} |\hat{F}_N(x'|X \in [-\delta_N, \delta_N]) - F(x'|X \in [-\delta_N, \delta_N])| \xrightarrow{p} 0$. The proof for $x = 1$ follows from the same argument. □

Consider any deterministic sequence of economies $\{g_N\}$ such that $g_N \in \mathcal{G}$ for all N and $g_N \to G$ in the $(\mathcal{G}, d)$ metric space. Let (δ_N) be an associated sequence of positive numbers (bandwidths) such that $\delta_N \to 0$ and $N\delta_N \to \infty$ as $N \to \infty$. Let $\psi_{Ns}(\theta, T; \delta_N) \equiv E_N[D_i(s)|\theta_i = \theta, T_i(\delta_N) = T]$ be the (finite-market, deterministic) bandwidth-specific propensity score for particular g_N and δ_N.

For Lemma 5, it is enough to show deterministic convergence of this finite-market score, that is, $\psi_{Ns}(\theta, T; \delta_N) \to \psi_s(\theta, T)$ as $g_N \to G$ and $\delta_N \to 0$. To see this, let G_N be the distribution over $I(\Theta_0, r_0, r_1)$'s induced by randomly drawing N applicants from G. Note that G_N is random and that $G_N \xrightarrow{a.s.} G$ by Wellner (1981)'s Glivenko-Cantelli theorem for independent but non-identically distributed random variables. $G_N \xrightarrow{p} G$ and $\psi_{Ns}(\theta, T; \delta_N) \to \psi_s(\theta, T)$ allow us to apply the Extended Continuous Mapping Theorem (Theorem 18.11 in van der Vaart (2000)) to obtain $\tilde{\psi}_{Ns}(\theta, T; \delta_N) \xrightarrow{p} \psi_s(\theta, T)$ where $\tilde{\psi}_{Ns}(\theta, T; \delta_N)$ is the random version of $\psi_{Ns}(\theta, T; \delta_N)$ defined for G_N.

For notational simplicity, consider the single-school RD case analyzed in Proposition 1, where there is only one school s making offers based on a single non-lottery tie-breaker $v(s)$ (without using any priority). A similar argument with additional notation shows the result for DA with general tie-breaking.

For any $\delta_N > 0$, whenever $T_i(\delta_N) = a$, it is the case that $D_i(s) = 1$. As a result,

$$\psi_{Ns}(\theta, a; \delta_N) \equiv E_N[D_i(s)|\theta_i = \theta, T_i(\delta_N) = a] = 1 \equiv \psi_s(\theta, a).$$

Therefore, $\psi_{Ns}(\theta, a; \delta_N) \to \psi_s(\theta, a)$ as $N \to \infty$. Similarly, for any $\delta_N > 0$, whenever $T_i(\delta_N) = n$, it is the case that $D_i(s) = 0$. As a result,

$$\psi_{Ns}(\theta, n; \delta_N) \equiv E_N[D_i(s)|\theta_i = \theta, T_i(\delta_N) = n] = 0 \equiv \psi_s(\theta, n).$$

Therefore, $\psi_{Ns}(\theta, n; \delta_N) \to \psi_s(\theta, n)$ as $N \to \infty$. Finally, when $T_i(\delta_N) = c$, let

$$F_{N,v(s)}(r|\theta) \equiv \frac{\sum_{i=1}^N 1\{\theta_i = \theta\} F_{v(s)}^i(r)}{\sum_{i=1}^N 1\{\theta_i = \theta\}}$$

be the aggregate tie-breaker distribution conditional on each applicant type θ in the finite market. As in the proof of Lemma 3, $\tilde{\xi}_{Ns}$ denotes the random cutoff at school s in a realized economy g_N. For any ϵ, there exists N_0 such that for any $N > N_0$, we have

$$\begin{aligned}
\psi_{Ns}(\theta, c; \delta_N) &\equiv E_N[D_i(s)|\theta_i = \theta, T_i(\delta_N) = c] \\
&= P_N[R_{iv(s)} \leq \tilde{\xi}_{Ns}|\theta_i = \theta, R_{iv(s)} \in (\tilde{\xi}_{Ns} - \delta_N, \tilde{\xi}_{Ns} + \delta_N]] \\
&\in (P[R_{iv(s)} \leq \xi_s|\theta_i = \theta, R_{iv(s)} \in (\xi_s - \delta_N, \xi_s + \delta_N]] - \epsilon/2, \\
&\quad P[R_{iv(s)} \leq \xi_s|\theta_i = \theta, R_{iv(s)} \in (\xi_s - \delta_N, \xi_s + \delta_N]] + \epsilon/2),
\end{aligned}$$

where ξ_s is school s's continuum cutoff, P is the probability induced by the tie-breaker distributions in the continuum economy, and the inclusion is by Assumption 1 and Lemmata 3 and 6. Again for any ϵ, there exists N_0 such that for any $N > N_0$, we have

$$
\begin{aligned}
&(P[R_{iv(s)} \leq \xi_s | \theta_i = \theta, R_{iv(s)} \in (\xi_s - \delta_N, \xi_s + \delta_N]] - \epsilon/2, \\
&\ P[R_{iv(s)} \leq \xi_s | \theta_i = \theta, R_{iv(s)} \in (\xi_s - \delta_N, \xi_s + \delta_N]] + \epsilon/2) \\
&= (\frac{F_{v(s)}(\xi_s|\theta) - F_{v(s)}(\xi_s - \delta_N|\theta)}{F_{v(s)}(\xi_s + \delta_N|\theta) - F_{v(s)}(\xi_s - \delta_N|\theta)} - \epsilon/2, \\
&\quad \frac{F_{v(s)}(\xi_s|\theta) - F_{v(s)}(\xi_s - \delta_N|\theta)}{F_{v(s)}(\xi_s + \delta_N|\theta) - F_{v(s)}(\xi_s - \delta_N|\theta)} + \epsilon/2) \\
&= (\frac{\{F_{v(s)}(\xi_s|\theta) - F_{v(s)}(\xi_s - \delta_N|\theta)\}/\delta_N}{\{F_{v(s)}(\xi_s + \delta_N|\theta) - F_{v(s)}(\xi_s|\theta)\}/\delta_N + \{F_{v(s)}(\xi_s|\theta) - F_{v(s)}(\xi_s - \delta_N|\theta)\}/\delta_N} - \epsilon/2, \\
&\quad \frac{\{F_{v(s)}(\xi_s|\theta) - F_{v(s)}(\xi_s - \delta_N|\theta)\}/\delta_N}{\{F_{v(s)}(\xi_s + \delta_N|\theta) - F_{v(s)}(\xi_s|\theta)\}/\delta_N + \{F_{v(s)}(\xi_s|\theta) - F_{v(s)}(\xi_s - \delta_N|\theta)\}/\delta_N} + \epsilon/2) \\
&\in (0.5 - \epsilon, 0.5 + \epsilon) \\
&= (\psi_s(\theta, c) - \epsilon, \psi_s(\theta, c) + \epsilon),
\end{aligned}
$$

completing the proof.

B Running Variables Coded as Ranks

Rank transformation of independent tie-breakers can induce dependence. This section shows that tie-breakers transformed into ranks become independent as the number of students grows to infinity. The assumption of independent tie-breakers therefore holds for a continuum market as long as the underlying tie-breakers are independent. Let $(X_1, X_2, \ldots)$ be a sequence of independent random variables. Define the rank function as follows.

$$\text{rank}_N(t) = \frac{1}{N}\sum_{i=1}^{N} \mathbb{1}\{X_i < t\}.$$

Let

$$F_N(t) = \frac{1}{N}\sum_{i=1}^{N} P(X_i < t).$$

Proposition 5. *For all k, we have*

$$\left|\text{rank}_N(X_k) - F_N(X_k)\right| \to 0 \quad \text{a.s.}$$

Thus the process $(\text{rank}_N(X_k) : k \in \mathbb{N})$ converges to the independent sequence $(F_N(X_k) : k \in \mathbb{N})$ uniformly in k on a set of measure 1.

Proof of Proposition 5. We prove Proposition 5 using a few lemmas below.

Lemma 7 (Hoeffding's maximal inequality; Lemma 5.1 in van Handel (2016))**.** *Let A be a finite subset of $\mathbb{R}^N$ and write $\|A\|_2 = \sup_{a\in A}\|a\|_2$, where $||\cdot||_2$ is the square root of the sum of squares ($||x||_2 \equiv \sqrt{x_1^2 + ... + x_m^2}$ for any vector $x \equiv (x_1, ..., x_m)$). Let $X_1, \ldots, X_N$ be independent, centered (mean zero) random variables supported on $[-1, 1]$. Then we have*

$$E\sup_{a\in A}\left\{\sum_{i=1}^{N} a_i X_i\right\} \leq \|A\|_2\sqrt{2\log|A|},$$

where $|A|$ is the cardinality of set A.

Lemma 8. *The expected supremum of the rank process satisfies*

$$E\sup_{t\in\mathbb{R}}\left\{\left|\frac{1}{N}\sum_{i=1}^{N}(\mathbb{1}\{X_i < t\} - P(X_i < t))\right|\right\} \leq \sqrt{\frac{8\log(n+1)}{n}}.$$

Proof of Lemma 8. Let $f_t(s) = \mathbb{1}\{s < t\}$. For each k, construct an independent random variable Y_k with the same distribution as X_k. Note that (i) $E[f_t(Y_k)] = P(X_k < t)$ and (ii) the law of $f_t(X_k) - f_t(Y_k)$ is symmetric around 0. By Jensen's inequality, we have

$$E\sup_{t\in\mathbb{R}}\left\{\left|\frac{1}{N}\sum_{i=1}^{N}(f_t(X_i) - P(X_i < t))\right|\right\} \leq E\sup_{t\in\mathbb{R}}\left\{\left|\frac{1}{N}\sum_{i=1}^{N}(f_t(X_i) - f_t(Y_i))\right|\right\}.$$

By symmetry and independence of the summands, their joint distribution does not change if we multiply each by an independent random variable ε_k that is uniformly distributed on $\{\pm 1\}$. This gives us

$$\begin{aligned} E \sup_{t\in\mathbb{R}} \left\{ \left| \frac{1}{N}\sum_{i=1}^{N} (f_t(X_i) - f_t(Y_i)) \right| \right\} &= E \sup_{t\in\mathbb{R}} \left\{ \left| \frac{1}{N}\sum_{i=1}^{N} \varepsilon_i \big(f_t(X_i) - f_t(Y_i)\big) \right| \right\} \\ &\leq E\left[\sup_{t\in\mathbb{R}} \left\{ \left| \frac{1}{N}\sum_{i=1}^{N} \varepsilon_i f_t(X_i) \right| \right\} + \sup_{t\in\mathbb{R}} \left\{ \left| \frac{1}{N}\sum_{i=1}^{N} -\varepsilon_i f_t(Y_i) \right| \right\} \right] \\ &= 2 \cdot E \sup_{t\in\mathbb{R}} \left\{ \left| \frac{1}{N}\sum_{i=1}^{N} \varepsilon_i f_t(X_i) \right| \right\} \end{aligned}$$

We then have

$$\begin{aligned} 2 \cdot E \sup_{t\in\mathbb{R}} \left\{ \left| \frac{1}{N}\sum_{i=1}^{N} \varepsilon_i f_t(X_i) \right| \right\} &= 2 \cdot E\left[E\left[\sup_{t\in\mathbb{R}} \left\{ \left| \frac{1}{N}\sum_{i=1}^{N} \varepsilon_i f_t(X_i) \right| \right\} \middle| X_1, \ldots, X_N \right] \right] \\ &= 2 \cdot E\left[E\left[\sup_{t\in\mathbb{R}} \left\{ \left| \frac{1}{N}\sum_{i=1}^{N} \varepsilon_i f_t(X_{(i)}) \right| \right\} \middle| X_1, \ldots, X_N \right] \right]. \end{aligned}$$

Here $X_{(i)}$ refers to the i^{th} smallest element of $\{X_1, \ldots, X_N\}$; we use the fact that the inner sum is invariant to re-ordering. As $t \in \mathbb{R}$ varies, the vector

$$u_t(X_1, \ldots, X_N) = \big(\tfrac{1}{N} f_t(X_{(1)}), \ldots, \tfrac{1}{N} f_t(X_{(N)})\big)$$

takes on at most $n+1$ values, and we always have $||u_t||_2 \leq 1/\sqrt{N}$. This follows from observing that nu_t takes values in the set of increasing binary sequences of length N. Applying Lemma 7 to the inner expectation then gives

$$2 \cdot E\left[E\left[\sup_{t\in\mathbb{R}} \left\{ \left| \frac{1}{N}\sum_{i=1}^{N} \varepsilon_i f_t(X_{(i)}) \right| \right\} \middle| X_1, \ldots, X_N \right] \right] \leq \sqrt{\frac{8\log(N+1)}{N}}.$$

□

Lemma 9. *Write*

$$h(X_1, \ldots, X_N) = \sup_{t\in\mathbb{R}} \left\{ \left| \frac{1}{N}\sum_{i=1}^{N} \left(\mathbb{1}\{X_i < t\} - P(X_i < t)\right) \right| \right\}.$$

Then $P\big(|h(X_1, \ldots, X_N) - E[h(X_1, \ldots, X_N)]| > \delta\big) \leq e^{-2N\delta^2}$.

Proof of Lemma 9. Note that varying $X_i(\omega)$ can change $h(\omega)$ by at most $1/n$, for all ω in the sample space. Lemma 9 then follows from McDiarmid's inequality as stated in van Handel (2016) (Theorem 3.11). □

Combining Lemmas 8 and 9, we obtain

$$P\left(\sup_{t\in\mathbb{R}}\left\{\left|\frac{1}{N}\sum_{i=1}^{N}(\mathbb{1}\{X_i<t\}-P(X_i<t))\right|\right\}\geq\sqrt{\frac{8\log(N+1)}{N}}+\delta\right)$$

$$\leq P\left(\sup_{t\in\mathbb{R}}\left\{\left|\frac{1}{N}\sum_{i=1}^{N}(\mathbb{1}\{X_i<t\}-P(X_i<t))\right|\right\}\geq E\big[\sup_{t\in\mathbb{R}}\left\{\left|\frac{1}{N}\sum_{i=1}^{N}(\mathbb{1}\{X_i<t\}-P(X_i<t))\right|\right\}\big]+\delta\right)$$

$$\leq e^{-2N\delta^2}$$

Since the sequence on the right-hand side is summable for any fixed δ, we apply the Borel-Cantelli lemma to obtain

$$P\left(\limsup_{N\to\infty}\sup_{t\in\mathbb{R}}\left\{\left|\frac{1}{N}\sum_{i=1}^{N}(\mathbb{1}\{X_i<t\}-P(X_i<t))\right|\right\}\geq\delta\right)=0.$$

Taking $\delta\to 0$ gives

$$\limsup_{N\to\infty}\sup_{t\in\mathbb{R}}\left\{\left|\frac{1}{N}\sum_{i=1}^{N}(\mathbb{1}\{X_i<t\}-P(X_i<t))\right|\right\}=0\quad\text{a.s.}$$

This implies that

$$\big|\mathrm{rank}_N(X_k)-F_N(X_k)\big|\leq\sup_{t\in\mathbb{R}}\Big\{\big|\mathrm{rank}_N(t)-F_N(t)\big|\Big\}\downarrow 0\quad\text{a.s.},$$

completing the proof of Proposition 5. □

C Empirical Appendix

C.1 Additional Results

Table A1 reports estimates of the effect of Grade A offers on attrition, computed by estimating models like those used to gauge balance. Under general risk, applicants who receive Grade A school offers have a slightly higher likelihood of taking the SAT. Decomposing Grade A schools into screened and unscreened schools, applicants who receive unscreened Grade A school offers are 1.7 percent more likely to have SAT scores, while offers to Grade A screened schools do not correspond to a statistically significant difference in the likelihood of having follow-up SAT scores. This modest difference seems unlikely to bias the 2SLS Grade A estimates reported in Tables 4 and 5.

Table A2 reports estimates of the effect of enrollment in an ungraded high school. These use models like those used to compute the estimates presented in Table 4. OLS estimates show a small positive effect of ungraded school attendance on SAT scores and a strong negative effect on graduation outcomes. 2SLS estimates, by contrast, suggest ungraded school attendance is unrelated to these outcomes.

C.2 Bandwidth Computation and Robustness Checks

Bandwidths are estimated as suggested by Imbens and Kalyanaraman (2012), separately for each screened program that has applicants on both sides of the cutoff. Bandwidths are estimated for the set of applicants who are in the relevant marginal priority group. Bandwidths are also computed separately for each outcome variable. Estimates reported in the text use the smallest of these.

The robustness of this procedure is evaluated by comparing the results generated under alternative restrictions on the distribution of tie-breakers and bandwidth size. Estimates in column 2 of Table A3 are from a model that ignores general risk stemming from screened programs with four or more duplicate tie-breaker values in the estimated bandwidth, resulting in modest changes in sample and effect size. Estimates in column 3 are from a model that ignores general risk stemming from screened programs with any gap of four or more in tie-breaker positions within the estimated bandwidth. Columns 4 and 5 show results that combine these restrictions, reducing the sample of applicants with general risk by 17 and 31 percent, respectively. The restrictions leave effect size and precision largely unchanged.

Table A4 reports 2SLS estimates computed using alternative bandwidths. Halving the bandwidth size at screened programs reduces the sample of applicants with general risk by about 20 percent, while doubling of bandwidth size increases sample size by about 20 percent. The estimates in column 4 are from regressions that use sixth grade baseline scores instead of SAT and graduation outcomes when computing the bandwidth. These variations leave the magnitude and precision of Grade A school effects mostly unchanged.

Table A1. Differential Attrition

		General Risk				Lottery Risk
		Non-offered mean	Grade A School			Any Grade A
			Any	Screened	Unscreened	
		(1)	(2)	(3)	(4)	(5)
Took SAT exam		0.761	0.018***	-0.002	0.017**	0.021**
			(0.007)	(0.012)	(0.007)	(0.008)
	N		30,760	10,497	25,918	18,743
Has binary outcomes (Enrolled in ninth grade)		0.635	0.003*	0.003	0.002	0.003
			(0.002)	(0.002)	(0.002)	(0.002)
	N		30,760	10,497	25,918	18,743

Notes. This table reports differential attrition estimates, computed by regressing covariates on dummies indicating a Grade A offer and an ungraded school offer, controlling for saturated Grade A and ungraded school propensity scores (columns 2-5), and running variable controls (columns 2-4). Robust standard errors are in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%

Table A2. Ungraded School 2SLS Estimates

		All applicants		Applicants with General risk	
		Non-enrolled mean	OLS	Non-offered mean	2SLS
		(1)	(2)	(3)	(4)
SAT Math (200-800)		470	0.640***	515	2.70
		(102)	(0.191)	(109)	(1.87)
SAT Reading (200-800)		470	0.803***	510	1.70
		(91)	(0.178)	(93)	(1.79)
	N		124,902		22,944
Graduated		0.611	-0.227***	0.793	0.064*
			(0.003)		(0.034)
College- and Career-prepared		0.365	-0.113***	0.587	0.053
			(0.003)		(0.037)
College-ready		0.321	-0.073***	0.541	0.019
			(0.003)		(0.036)
	N		120,716		19,202

Notes. This table reports OLS and 2SLS estimates of ungraded school effects produced by the same models reported in and described in the notes to Table 4. Robust standard errors are in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%

Table A3. Grade A Effects with Running Variable Restrictions

	No RV Restriction	4+ Duplicates in BW	4+ Gap in BW	4+ Gap or 4+ Duplicates in BW	3+ Gap or 3+ Duplicates in BW
	(1)	(2)	(3)	(4)	(5)
SAT Math	2.23***	1.97**	2.91***	2.68***	1.37*
(200-800)	(0.716)	(0.728)	(0.750)	(0.755)	(0.814)
SAT Reading	0.594	0.566	0.919	0.690	0.464
(200-800)	(0.657)	(0.669)	(0.687)	(0.695)	(0.741)
N	22,944	21,740	20,291	19,273	16,147
Graduated	0.029**	0.028**	0.026*	0.025*	0.024
	(0.013)	(0.014)	(0.014)	(0.015)	(0.017)
N	19,202	18,056	16,926	15,999	13,160

Notes. This table reports 2SLS estimates of the effects of Grade A school enrollment, computed as described in the notes to Table 4. Estimates in column 1 correspond to the estimates in column 4 in Table 4 and impose no restriction on the distribution of running variables. Estimates in column 2 are from a model that excludes general risk that is created at screened programs with four or more duplicate running variable values in the bandwidth. Estimates in column 3 are from a model that excludes general risk that is created at screened programs with a gap in running variable ranks in the bandwidth of four ranks or larger. Estimates in column 4 are from a model that combines the two restrictions from columns 2 and 3, excluding general risk that is created at screened programs with either duplicates or a gap. Estimates in column 5 are from a model that applies a stricter version of the restriction in column 4. Robust standard errors are in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%

Table A4. Grade A Effects with Alternative Bandwidths

	Benchmark	Half Bandwidth Size	Double Bandwidth Size	Using Baseline Scores for BW Computation
	(1)	(2)	(3)	(4)
SAT Math	2.23***	1.88**	1.88**	2.25**
(200-800)	(0.716)	(0.805)	(0.657)	(0.734)
SAT Reading	0.594	0.883	-0.087	1.17*
(200-800)	(0.657)	(0.733)	(0.607)	(0.670)
N	22,944	17,975	27,966	22,005
Graduated	0.029**	0.028*	0.020*	0.034**
	(0.013)	(0.016)	(0.012)	(0.014)
N	19,202	15,251	23,110	18,398

Notes. This table reports 2SLS estimates of the effects of Grade A school enrollment, computed as described in the notes to Table 4. Estimates in column 1 correspond to the estimates in column 4 in Table 4. Estimates in column 2 are from a model that halfes the size of the estimated bandwidth at screened programs. Estimates in column 3 are from a model that doubles the size of the estimated bandwidth. Estimates in column 4 are from a model that uses 6th grade baseline math and English test scores instead of outcomes to compute the IK bandwidth. Robust standard errors are in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%