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RESEARCH DESIGN MEETS MARKET DESIGN: USING CENTRALIZED ASSIGNMENT FOR IMPACT EVALUATION

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A growing number of school districts use centralized assignment mechanisms to allocate school seats in a manner that reflects student preferences and school priorities. Many of these assignment schemes use lotteries to ration seats when schools are oversubscribed. The resulting random assignment opens the door to credible quasi-experimental research designs for the evaluation of school effectiveness. Yet the question of how best to separate the lottery-generated randomization integral to such designs from non-random preferences and priorities remains open. This paper develops easily-implemented empirical strategies that fully exploit the random assignment embedded in a wide class of mechanisms, while also revealing why seats are randomized at one school but not another. We use these methods to evaluate charter schools in Denver, one of a growing number of districts that combine charter and traditional public schools in a unified assignment system. The resulting estimates show large achievement gains from charter school attendance. Our approach generates efficiency gains over ad hoc methods, such as those that focus on schools ranked first, while also identifying a more representative average causal effect. We also show how to use centralized assignment mechanisms to identify causal effects in models with multiple school sectors.

KEYWORDS: Causal inference, propensity score, instrumental variables, unified enrollment, charter schools.

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1. INTRODUCTION

FAMILIES IN MANY LARGE URBAN DISTRICTS CAN APPLY for seats at any public school in their district. The fact that some schools are more popular than others and the need to distinguish between students who have different priorities at a given school generate a matching problem. Introduced by Gale and Shapley (1962) and Shapley and Scarf (1974), matchmaking via market design allocates scarce resources, such as seats in public schools, in markets where prices cannot be used for this purpose. The market-design approach to school choice, pioneered by Abdulkadiroğlu and Sönmez (2003), is used in a long and growing list of public school districts in America, Europe, and Asia. Most of these cities match students to schools using a mechanism known as deferred acceptance (DA).

Two benefits of centralized matching schemes like DA are efficiency and fairness: the resulting match improves welfare and transparency relative to ad hoc alternatives, while lotteries ensure that students with the same preferences and priorities have the same chance of obtaining highly-sought-after seats. The latter is sometimes called the "equal treatment of equals" (ETE) property. DA and related algorithms also have the virtue of narrowing the scope for strategic behavior that would otherwise give sophisticated families the opportunity to manipulate an assignment system at the expense of less-sophisticated participants (Abdulkadiroğlu, Pathak, Roth, and Sönmez (2006), Pathak and Sönmez (2008)). In addition to these economic considerations, centralized assignment generates valuable data for empirical research on schools. In particular, when schools are oversubscribed, lottery-based rationing induces quasi-experimental variation in school assignment that can be used for credible evaluation of individual schools and of school reform models like charters.

Previous research using the lotteries embedded in centralized assignment schemes include studies of schools in Boston (Abdulkadiroğlu, Angrist, Dynarski, Kane, and Pathak (2011)), Charlotte-Mecklenburg (Hastings, Kane, and Staiger (2009), Deming (2011), Deming, Hastings, Kane, and Staiger (2014)) and New York (Bloom and Unterman (2014), Abdulkadiroğlu, Hu, and Pathak (2013)). Causal effects in these studies are convincingly identified by quasi-experimental variation, but the research designs deployed in this work fail to exploit the full power of the random assignment embedded in centralized assignment schemes. A major stumbling block is the multi-stage nature of market-design matching. Market design weaves random assignment into an elaborate tapestry of information on student preferences and school priorities. In principle, all features of student preferences and school priorities can shape the probability of assignment to each school. Families tend to prefer schools located in their neighborhoods, for example, while schools may grant priority to children poor enough to qualify for a subsidized lunch. Conditional on preferences and priorities, however, centralized assignments are independent of potential outcomes.

This paper explains how to recover the full range of quasi-experimental variation embedded in centralized assignment. Specifically, we show how mechanisms that satisfy ETE map information on preferences, priorities, and school capacities into a conditional probability of random assignment, often referred to as the propensity score. As in other stratified randomized research designs, conditioning on the propensity score eliminates selection bias arising from the association between conditioning variables and potential outcomes (Rosenbaum and Rubin (1983)). The payoff to propensity-score conditioning turns out to be substantial in our application: full stratification on preferences and priorities reduces degrees of freedom markedly, eliminating many schools and students from consideration, while score-based stratification leaves our research sample largely intact.

The propensity score does more for us than reduce the dimensionality of preference and priority conditioning. Because all applicants with score values strictly between zero and one contribute variation that can be used for evaluation, the propensity score identifies the maximal set of applicants for whom we have a randomized school-assignment experiment. The nature of this sample is not easily seen otherwise. We show, for example, that the quasi-experimental sample includes many schools that are undersubscribed, that is, schools that have fewer applicants than seats. Intuitively, applicants are randomly assigned to undersubscribed schools when they are rejected by oversubscribed schools that they have ranked more highly. As we show here, random assignment of this sort occurs frequently.

The propensity score for any mechanism that satisfies ETE is easily estimated by simulation, that is, by repeatedly drawing lottery numbers and computing the resulting average assignment rates across draws. This amounts to sampling from the relevant permutation distribution, a natural and highly general solution to the problem of score estimation. At the same time, while any stochastic mechanism can be simulated, simulation fails to illuminate the path producing random assignment. For example, the simulated score for a given school does not reveal the proportion of applicants randomly assigned due to oversubscription of that school and the proportion randomly assigned due to oversubscription of other schools that this school's applicants have ranked more highly. We therefore develop an analytic formula for the propensity score for a broad class of DA-type mechanisms. This formula explains how and why random assignment emerges. Our formula also provides a natural smoother for estimated scores. Because the relevant covariates are discrete, unsmoothed simulated scores fail to provide the sort of dimension reduction that gives the propensity score its practical appeal (Hirano, Imbens, and Ridder (2003)).

The propensity score generated by DA-type mechanisms does not typically have a general closed-form solution. As a result, our analytic framework uses an asymptotic "large market" approximation to derive a simple formula for the score. The resulting *DA propensity score* is a function of a few easily-estimated parameters. Both the simulated and DA (analytic) propensity scores work well as far as covariate balance goes, a result that emerges in our empirical application. Importantly, however, the DA score highlights specific sources of randomness and confounding in DA-based assignment schemes. In other words, the DA propensity score reveals the nature of the stratified experimental design embedded in a particular match. The DA score is also quickly and easily computed, and can be used without the rounding or functional form restrictions (such as linear controls) required when using a simulated score.

Our test bed for the DA propensity score is an empirical analysis of charter school effects in the Denver Public School (DPS) district, a new and interesting setting for charter school impact evaluation.[1] Because DPS assigns seats at traditional and charter schools in a unified match, the population attending DPS charters is less positively selected than in large urban districts with decentralized charter lotteries. As far as we know, ours is the

[1]Charter schools operate with considerably more independence than traditional public schools. Among other differences, many charters fit more instructional hours into a year by running longer school days and providing instruction on weekends and during the summer. Because few charter schools are unionized, they hire and fire teachers and administrative staff without regard to the collectively bargained seniority and tenure provisions that constrain such decisions in many public schools. About half of Denver charters implement versions of the *No Excuses* model of urban education. No Excuses charters run a long school day and year, emphasize discipline and comportment and traditional reading and math skills, and rely heavily on data and teacher feedback to improve instruction. See Abdulkadiroğlu et al. (2011) and Angrist, Pathak, and Walters (2013) for related evidence on charter effects.

first charter evaluation to exploit an assignment scheme that simultaneously randomizes seats in both the charter and traditional public school sectors.

The next section details the class of assignment mechanisms of interest to us and describes the central role of the propensity score for impact evaluation. Following this context, Section 3 uses the theory of market design to characterize the propensity score for DA-generated offers in large markets. Section 4 uses these results to estimate charter effects. Specifically, our empirical evaluation strategy uses an indicator for DA-generated charter offers as an instrument for charter school attendance in a two-stage least squares (2SLS) setup. This 2SLS procedure eliminates bias from non-random variation in preferences and priorities by controlling for the DA propensity score. This section also shows how to estimate effects for multiple sectors, an important extension when school effects are potentially heterogeneous. Finally, Section 5 summarizes our theoretical and empirical findings and outlines an agenda for further work.

2. CENTRALIZED ASSIGNMENT IN GENERAL AND IN DENVER

2.1. *Conditional Independence and ETE*

A school choice problem is an economy defined by a set of applicants, schools, and school capacities. Applicants have strict preferences over schools while schools have priorities over applicants. Let I denote a set of applicants, indexed by i, and let $s = 0, 1, \dots, S$ index schools, where $s = 0$ represents an outside option. Let n be the number of applicants. Seats at schools are constrained by a capacity vector, $\mathbf{q} = (q_0, q_1, q_2, \dots, q_S)$; we assume $q_0 > n$.

Applicant i's preferences over schools constitute a partial ordering of schools, denoted $\succ_i$, 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 $\rho_{is} \in \{1, \dots, K, \infty\}$ 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 $i \neq j$. Let $\boldsymbol{\rho}_i = (\rho_{i1}, \dots, \rho_{iS})$ be the vector of applicant i's priorities for each school.

Applicant type is defined as $\theta_i = (\succ_i, \boldsymbol{\rho}_i)$, that is, the combination of an individual applicant's preference and priorities at all schools. We say that an applicant of type θ has preferences $\succ_\theta$ and priorities ρ_θ. The symbol Θ denotes the set of possible types.

An *assignment* for applicant i, denoted μ_i, specifies her assigned school or assignment to the outside option. $\boldsymbol{\mu}$ denotes the vector of assignments for all $i \in I$. School s is assigned at most q_s applicants. A mechanism is a set of rules determining $\boldsymbol{\mu}$ as a function of preferences, priorities, and a possible tie-breaking variable that might be randomly assigned. The mechanism known as *serial dictatorship*, for example, orders applicants by the tie-breaker, assigning the first in line his or her top choice, the second in line his or her top choice among schools with seats remaining, and so on.

Many mechanisms use randomization to break ties, inducing a distribution of assignments. Such mechanisms are said to be *stochastic*. When applicants in a serial dictatorship are ordered randomly, for example, the mechanism is called *random serial dictatorship* (RSD). Randomizers that are drawn independently from a uniform distribution for each applicant are called *lottery numbers*. The distribution of assignments induced by RSD is the permutation distribution generated by all possible lottery draws.

Formally, any stochastic mechanism maps economies characterized by $(I, S, \mathbf{q}, \Theta)$ into a distribution of possible assignments. This distribution is described by a matrix with generic element p_{is} satisfying (i) $0 \leq p_{is} \leq 1$ for all i and s, (ii) $\sum_s p_{is} = 1$ for all i, and (iii) $\sum_i p_{is} \leq q_s$ for all s. The value of p_{is} is the probability that applicant i is assigned to school s. These are collected in a vector $\boldsymbol{p}_i = (p_{i0}, p_{i1}, \dots, p_{iS})$ recording the probabilities i finds a seat for all schools. This notation covers deterministic mechanisms in which p_{is} equals either 0 or 1 and, for each i, $p_{is} = 1$ for at most one s.

We say mechanism φ satisfies the *equal treatment of equals* (ETE) property when applicants with the same preferences and priorities at all schools have the same assignment probability at each school.[2] That is, for any school choice problem and any applicants i and j with $\theta_i = \theta_j$, ETE means that $\boldsymbol{p}_i = \boldsymbol{p}_j$.

ETE allows us to use stochastic mechanisms to estimate causal effects. Specifically, we would like to estimate the causal effect of attendance at a particular school or group of schools relative to one or more alternative schools. This task is complicated by the fact that school assignment reflects preferences and priorities and these variables in turn are related to outcomes like test scores. ETE solves this problem: the distribution of offers generated by a stochastic assignment mechanism is viewed here as a stratified randomized trial, where the "strata" are defined by type.

As the notion of a stratified randomized trial suggests, ETE makes offers conditionally independent of all possible confounding variables that might otherwise generate omitted variables bias in econometric analyses of school attendance effects. To see this, pick any school s as a treatment school and let $D_i(s)$ be a dummy variable indicating when applicant i is assigned to school s by stochastic mechanism φ. For any random variable or vector of characteristics W_i, which can include covariates like race or outcome variables like test scores, let W_{0i} be the potential value of W_i that is revealed when $D_i(s) = 0$ and let W_{1i} be the potential value revealed when $D_i(s) = 1$. These two potential values might be the same, as for covariates (race is unchanged by school assignment) or for test scores when assignment has no effect on achievement. But in cases where they differ, as for outcomes affected by treatment, only one is seen in a given assignment realization. Potential variables are attributes and therefore non-stochastic in a fixed applicant population, that is, they are unchanged by school assignments (see, e.g., Rosenbaum (2002), Imbens and Rubin (2015)). We therefore say that the observed characteristic W_i is *fixed under re-randomization* if $W_{0i} = W_{1i}$ for all i.

Although assignment probabilities almost certainly vary with applicant characteristics, ETE restricts this variation to be independent of characteristics conditional on type:

PROPOSITION 1: *Consider the conditional assignment probability $P[D_i(s) = 1 | W_i = w, \theta_i = \theta]$ for all applicants i with $W_i = w$ and $\theta_i = \theta$. The probability P is the assignment rate to school s induced by stochastic mechanism φ and w is a particular value of W_i. If φ satisfies ETE and W_i is fixed under re-randomization, we have that*

$$P\bigl[D_i(s) = 1 | W_i = w, \theta_i = \theta\bigr] = P\bigl[D_i(s) = 1 | \theta_i = \theta\bigr]$$

for any w.

PROOF: Since W_i is fixed under re-randomization,

$$P\bigl[D_i(s) = 1 | W_i = w, \theta_i = \theta\bigr] = P\bigl[D_i(s) = 1 | W_{0i} = w, \theta_i = \theta\bigr].$$

[2]ETE is widely studied in allocation problems; see, for example, Moulin (2003). Shapley's (1953) axiomatization of the Shapley value appears to be the first formal statement of this concept.

Moreover, since knowledge of an individual applicant's identity implies knowledge of his type and of W_i, the law of iterated expectations implies

$$P\big[D_i(s)=1|W_{0i}=w, \theta_i=\theta\big]=E[p_{is}|W_{0i}=w, \theta_i=\theta]=E[p_{is}|\theta_i=\theta],$$

where the second equality follows from ETE: p_{is} is the same for applicants of the same type. We have therefore shown

$$P\big[D_i(s)=1|W_i=w, \theta_i=\theta\big]=P\big[D_i(s)=1|\theta_i=\theta\big]. \qquad \textit{Q.E.D.}$$

Although elementary, Proposition 1 is the foundation of our analysis, showing how centralized assignment schemes induce a stratified randomized trial. In particular, this proposition defines the conditional independence assumption that provides the foundation for causal analysis of DA-generated assignments.

Conditional Independence for DA

Many U.S. school districts implement versions of DA with a single tie-breaking lottery number, though some districts use other schemes. As we discuss below, however, the most widely-used centralized assignment mechanisms can be cast as a version of DA. Single tie-breaking DA for school assignment works like this:

> Draw an independently and identically distributed lottery number for each applicant.
>
> Each applicant applies to his most preferred school. Each school ranks these applicants first by priority, then by random number within priority groups, and *provisionally* admits the highest-ranked applicants in this order up to its capacity. Other applicants are rejected.
>
> Each rejected applicant applies to his next most preferred school. Each school ranks these new applicants *together with applicants that it admitted provisionally in the previous round*, first by priority and then by random number. From this pool, the school provisionally admits those it ranks highest up to capacity, rejecting the rest.

The algorithm terminates when there are no new applications (some applicants may remain unassigned).[3]

DA with single tie-breaking is easily seen to satisfy ETE. Stochastic assignments in this case are determined by the type distribution, which is fixed, and a particular lottery draw. Assignment differences from one realization to the next are therefore generated solely by differences in lottery draws. In particular, if we swap the lottery numbers for two applicants with the same type, DA swaps their assignments, leaving other assignments unchanged. Since all draws are equally likely for all applicants, the probability of assignment must be equal for two applicants of the same type, satisfying ETE. This argument is made formally in Appendix A.1, which also shows that the class of mechanisms satisfying ETE includes:

- DA with multiple tie-breakers (i.e., different lottery numbers at different schools)
- the immediate acceptance ("Boston") mechanism with single or multiple tie-breakers

[3]DA produces a stable allocation in the following sense: any applicant who prefers another school to the one he has been assigned must be outranked at that school, either because everyone assigned there has higher priority, or because those who share the applicant's priority at that school have higher lottery numbers. DA is also strategy-proof, meaning that families do as well as possible by submitting a truthful preference list (e.g., there is nothing to be gained by ranking undersubscribed schools highly just because they are likely to yield seats). See Roth and Sotomayor (1990) for a review of these and related theoretical results.

- random serial dictatorship
- top trading cycles with single or multiple tie-breakers.

Where does ETE fail? Some English towns use DA with distance-based tie-breaking (Burgess, Greaves, Vignoles, and Wilson (2014)). In this case, distance plays the role otherwise played by lottery numbers. DA with distance-based tie-breaking fails to satisfy ETE because applicants of the same type (as defined above) need not face the same assignment probability.

2.2. *Propensity Score Pooling*

Proposition 1 implies that for applicant i of type θ and for any variable W_i that is fixed under re-randomization,

$$P\big[D_i(s)=1|W_i=w,\theta_i=\theta\big]=P\big[D_i(s)=1|\theta_i=\theta\big].$$

In other words for any mechanism that treats equals equally, conditioning on θ_i eliminates selection bias arising from the association between type and potential outcomes. Since θ_i takes on many values, however, full-type conditioning reduces the sample available for impact evaluation. We therefore consider schemes that compare applicants while pooling types.

Rosenbaum and Rubin's (1983) propensity score theorem tells us how this pooling can be accomplished while still eliminating omitted variables bias. The *propensity score* for a market of any size, denoted $p_s(\theta)$, is the scalar function of type defined by

$$p_s(\theta)=\Pr\big[D_i(s)=1|\theta_i=\theta\big].$$

Rosenbaum and Rubin (1983) showed that propensity score conditioning is enough to ensure that offers are independent of W_i. In other words,

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

Equation (1) implies that propensity score conditioning eliminates omitted variables bias due to the dependence of offers on type.[4] The following simple example illustrates propensity score pooling in a matching market.

EXAMPLE 1: Five applicants $\{1,2,3,4,5\}$ apply to three schools $\{a,b,c\}$, each with one seat. Applicant 5 has the highest priority at c and applicant 2 has the highest priority at b; otherwise the applicants have the same priority at all schools. We are interested in measuring the effect of an offer at school a. Applicant preferences are

$$\begin{aligned}&1:a\succ b,\\&2:a\succ b,\\&3:a,\\&4:c\succ a,\\&5:c.\end{aligned}$$

Applicants 3 and 5 rank only one school.

[4] Rosenbaum and Rubin (1983) also showed that the propensity score is the coarsest balancing score, which in this case means that no coarser function of type ensures conditional independence of $D_i(s)$ and W_i. Hahn (1998), Hirano, Imbens, and Ridder (2003), and Angrist and Hahn (2004) discussed the efficiency consequences of conditioning on the score.

Note that no two applicants here have the same preferences and priorities. Consequently, full-type conditioning puts each applicant into a different stratum. This rules out research strategies that rely on full-type conditioning to eliminate selection bias. But full-type conditioning is unnecessary in this case because DA assigns each of applicants 1, 2, 3, and 4 to school a with probability 0.25. This calculation reflects the fact that 5 beats 4 at c by virtue of his priority there, leaving 1, 2, 3, and 4 all applying to a with no one advantaged there. The impact of assignment to a can therefore be analyzed in a single stratum containing four applicants with a common propensity score value of 0.25.

The propensity score for this simple example is readily determined. In real assignment problems, the score is not easily computed, but can be simulated by repeatedly drawing lottery numbers and running DA for each draw. By a conventional law of large numbers, the average offer rate across draws converges to the actual finite-market score as the number of draws increases. We illustrate this approach to score estimation by using data from the Denver Public School (DPS) district to construct a simulated propensity score for offers of a charter school seat.

2.3. *DPS Data and Descriptive Statistics*

Since the 2011 school year, DPS has used DA to assign applicants to most schools in the district, a process known as SchoolChoice. Denver school assignment involves two rounds, but only the first round uses DA. Our analysis therefore focuses on the initial round.

In the first round of SchoolChoice, parents rank up to five schools of any type, including traditional public schools, magnet schools, innovation schools, and most charters. A neighborhood school is also ranked automatically (if need be, the district inserts a neighborhood school in applicant rankings as the last choice). Schools ration seats using a mix of priorities and a single lottery number. Priorities vary across schools and typically involve siblings and neighborhoods. Seats may be reserved for a certain number of subsidized-lunch applicants and for children of school staff. Reserved seats are allocated by splitting schools and assigning the highest priority status to applicants in the reserved group at one of the sub-schools created by a split.[5] Seats grouped together in this way within a school are said to be in a "bucket."

Buckets are created when schools are split to accommodate reserved seating, but they're created for programmatic reasons as well. DPS converts applicants' preferences over schools into preferences over buckets, depending on qualification for reserved seating and/or programmatic interests. The upshot for our purposes is that DPS's version of DA assigns seats at buckets rather than schools, while the relevant propensity score captures the probability of offers at buckets. The discussion that follows nevertheless refers to propensity scores for schools, with the understanding that the fundamental unit of assignment is a bucket, from which assignment rates to schools are constructed.[6]

The data analyzed here come from files containing the information used for first-round assignment of students applying in the 2011–2012 and 2012–2013 school years for seats the following years (this information includes preference lists, priorities, random num-

[5]For more on reserve implementation via school splitting, see Dur, Kominers, Pathak, and Sönmez (2014) and Dur, Pathak, and Sönmez (2016).

[6]DPS modifies DA by recoding the lottery numbers of all siblings applying to the same school to be the best random number held by any of them. This modification (known as "family link") changes the allocation of only about 0.6% of applicants from that generated by standard DA. Our analysis incorporates family link by defining distinct types for linked applicants.

bers, assignment status, and school capacities). We use a spring dating convention, labeling these years "2013" and "2014," and focus on applicants for grades 4–10, who are in grades 3–9 in the application year. Most of our applicants are applying for a (middle school) grade 6 seat or a (high school) grade 9 seat. School-level scores were constructed by summing scores for all component buckets. Our empirical work also uses files with information on October enrollment and standardized scores from the Colorado School Assessment Program (CSAP) and the Transitional Colorado Assessment Program (TCAP) tests, given annually in grades 3–10. The Supplemental Material (Abdulkadiroğlu, Angrist, Narita, and Pathak (2017)) describes these files and the extract we created from them. For our purposes, "Charter schools" are schools identified as "charter" in DPS *2012–2013 and 2013–2014 SchoolChoice Enrollment Guide* brochures and not identified as "intensive pathways" schools, which serve applicants who are much older than is typical for their grade.

The DPS population enrolled in grades 3–9 is roughly 60% Hispanic, a fact reported in Table I, along with other descriptive statistics. The outcome scores of applicants in grades 3–9 come from TCAP tests taken in grades 4–10 in the spring of the following year.[7] The high Hispanic proportion of its student body makes DPS an especially interesting and unusual urban district. Not surprisingly in view of this, almost 30 percent of DPS students have limited English proficiency. Consistent with the high poverty rates seen in many urban districts, three quarters of DPS students are poor enough to qualify for a subsidized lunch. Roughly 20% of the DPS students in our data are identified as gifted, a designation that qualifies them for differentiated instruction and other programs.

In the two years covered in Table I, roughly 22,000 of the students enrolled in grades 3–9 sought to change their school for the following year by participating in SchoolChoice in the spring. We drop applicants for 2014 seats who also participated in the previous year's match. The sample participating in the assignment, described in column 2 of Table I, contains fewer charter school students than appear in the total DPS population, but is otherwise similar. It is also worth noting that our analysis is limited to students enrolled in DPS in the years that provide baseline (that is, pre-assignment) test scores. The sample described in column 2 is therefore a subset of that described in column 1.

Column 3 of Table I shows that of the 22,000 DPS-at-baseline applicants participating in SchoolChoice, about 10,000 ranked at least one charter school. We refer to these students as charter applicants; the estimated charter attendance effects that follow are for subsets of this applicant group. DPS charter applicants have baseline achievement levels and demographic characteristics broadly similar to those seen district-wide. The most noteworthy feature of the charter applicant sample is a reduced proportion white, from about 18% among SchoolChoice applicants to a little over 12% among charter applicants. It is also worth noting that charter applicants have baseline test scores close to the DPS average. This contrasts with the modest positive selection of charter applicants seen in Boston (reported in Abdulkadiroğlu et al. (2011)).

We computed simulated scores by running DA for one million lottery draws for each year. Simulated scores are the proportion these of draws in which applicants of a given type were seated at each school. The propensity score for charter offers is the sum of the scores for each individual charter school (a consequence of the fact that SchoolChoice produces a single offer for each applicant).[8] Applicants subject to random charter assign-

[7]Grade 3 is omitted from our outcome sample because third-graders have no baseline test to gauge balance.

[8]Calsamiglia, Fu, and Güell (2014) and Agarwal and Somaini (2015) simulate variants of the Boston mechanism as part of research estimating preferences over schools.

TABLE I

DPS APPLICANT CHARACTERISTICS[a]

	Denver Students (1)	SchoolChoice Applicants (2)	Charter Applicants (3)	Simulated Score in (0, 1) Charter Applicants (4)	Simulated Score in (0, 1) Charter Students (5)
Origin school is charter	0.151	0.088	0.144	0.192	0.273
Female	0.494	0.495	0.505	0.500	0.495
Race					
Hispanic	0.594	0.600	0.640	0.645	0.698
Black	0.140	0.143	0.167	0.186	0.161
White	0.193	0.184	0.126	0.102	0.082
Asian	0.034	0.033	0.028	0.027	0.029
Applied in 2013	0.490	0.488	0.487	0.445	0.445
Gifted	0.180	0.214	0.203	0.180	0.175
Bilingual	0.040	0.030	0.038	0.037	0.038
Subsidized lunch	0.752	0.763	0.804	0.815	0.827
Limited English proficient	0.297	0.301	0.344	0.364	0.419
Special education	0.119	0.122	0.091	0.087	0.084
Baseline scores					
Math	0.000	−0.003	0.008	−0.009	0.043
Reading	0.000	−0.003	−0.009	−0.032	−0.022
Writing	0.000	−0.008	−0.005	−0.012	0.019
N	51,325	22,311	10,203	3,466	1,769

[a]This table describes the population of Denver 3rd–9th graders in 2011–2012 and 2012–2013, the baseline years. Statistics in column 1 are for charter and non-charter students. Column 2 describes the subset that submitted an application to the SchoolChoice system for a seat in grades 4–10 at another DPS school in 2013 or 2014. Column 3 reports values for applicants ranking any charter school. Column 4 shows statistics for charter applicants with simulated score values strictly between zero and one. The simulated score is rounded to 0.001. Column 5 tabulates statistics for applicants in column 4 who matriculate at a charter school. Test scores are standardized to the population described in column 1.

ment are those with charter propensity scores (probabilities of assignment) between zero and one. Column 4 of Table I shows that nearly 3,500 charter applicants are subject to random assignment. This group looks like the full charter applicant pool on most dimensions, though randomized applicants are more likely to have already been enrolled at a charter at the time they entered the match. Column 5 reports statistics for the subset of the randomized group that enrolls in a charter school; these show slightly higher baseline scores among charter students.

2.4. *DPS Schools Randomized*

Table II lists charter schools in our sample, along with the number of applicants, capacities, offers, and counts of applicants subject to random assignment for each school in 2013. Three charter management organizations (CMOs), the Denver School of Science and Technology (DSST), STRIVE Preparatory Schools, and the Knowledge is Power Program (KIPP), contribute 16 of the 31 charters listed.

The proportion of applicants subject to random assignment varies markedly from school to school. This can be seen by comparing the count of applicants subject to random assignment in column 5 with the total applicant count in column 2. Column 5 documents the presence of applicants subject to random assignment at every charter in 2013, except for the Denver Language School, which offered no seats. With the exception of Venture

TABLE II

DPS CHARTER SCHOOLS (2013 APPLICANTS)[a]

School	CMO (1)	Applicants (2)	School Capacity (3)	Applicants Offered Seats (4)	Simulated Score in (0, 1): Applicants (5)	Simulated Score in (0, 1): First Choice Applicants (6)
Elementary and middle schools						
Cesar Chavez Academy Denver		62	72	9	8	3
Denver Language School		4	100	0	0	0
DSST: Cole	Yes	281	150	129	45	0
DSST: College View	Yes	299	310	130	69	0
DSST: Green Valley Ranch	Yes	1,014	146	146	358	291
DSST: Stapleton	Yes	849	156	156	231	137
Girls Athletic Leadership School		221	143	86	51	0
Highline Academy Charter School		159	93	26	84	50
KIPP Montbello College Prep	Yes	211	125	39	56	21
KIPP Sunshine Peak Academy	Yes	389	120	83	46	36
Odyssey Charter Elementary		215	32	6	22	14
Omar D. Blair Charter School		385	193	114	182	99
Pioneer Charter School		25	152	5	2	0
SIMS Fayola International Academy Denver		86	120	37	21	0
SOAR at Green Valley Ranch		85	114	9	44	37
SOAR Oakland		40	117	4	7	2
STRIVE Prep—Federal	Yes	621	138	138	193	131
STRIVE Prep—GVR	Yes	324	147	112	119	0
STRIVE Prep—Highland	Yes	263	147	112	19	0
STRIVE Prep—Lake	Yes	320	147	126	26	0
STRIVE Prep—Montbello	Yes	188	147	37	36	0
STRIVE Prep—Westwood	Yes	535	141	141	239	141
Venture Prep		100	114	50	18	0
Wyatt Edison Charter Elementary		48	300	4	2	0
High schools						
DSST: Green Valley Ranch	Yes	806	186	173	332	263
DSST: Stapleton	Yes	522	27	27	143	96
KIPP Denver Collegiate High School	Yes	268	100	60	41	24
SIMS Fayola International Academy Denver		71	130	15	23	0
Southwest Early College		265	235	76	58	0
STRIVE Prep—SMART	Yes	383	160	160	175	175
Venture Prep		140	246	39	49	0

[a]This table describes DPS charter applications for the 2012–2013 school year. Column 1 indicates CMO schools belonging to the DSST, STRIVE, and KIPP networks. Column 2 reports the number of applicants ranking each school. Column 3 reports each school's capacity. Column 4 counts the number of applicants offered a seat. Column 5 counts applicants with simulated score values strictly between zero and one. The simulated score is rounded to 0.001. Column 6 shows the number of applicants from column 5 who rank each school first.

Prep, this was also true in 2014 (see Supplemental Material Table B.V for a version of Table II for 2014 applicants).

DA randomizes seats for applicants ranking charters *first* for a smaller set of schools. This can be seen in the last column of Table II, which reports the number of applicants with a simulated charter score strictly between zero and one who also ranked each school first. The reduced scope of first-choice randomization is important for our comparison of strategies using the DA propensity score with previously-employed IV strategies using first-choice instruments. First-choice instruments applied to the DPS charter sector necessarily ignore many schools (in 2013, 15 schools had no first-choice random assignment).

A broad picture of DPS random assignment appears in Figure 1. Panel (a) summarizes the information in columns 5 and 6 of Table II by plotting the number of first-choice applicants subject to randomization as dots, with the total number randomized at each school plotted as an arrow pointing up from these (schools are indexed on the x-axis by their capacities). This representation highlights the empirical payoff to our score-based approach to the DA research design. These benefits are not limited to the charter sector, a fact documented in panel (b) of the figure, which presents the same comparisons for non-charter schools in the DPS match.

Table II reports a few surprising features of the assignment distribution. We see, for example, that only 112 applicants were offered seats at STRIVE Prep-GVR, a school with a capacity of 147. In spite of the fact that this school was undersubscribed, many of the seats there were randomly assigned. The simulated score shows that this happens, without explaining why. We therefore introduce a large-market approximation to $p_s(\theta)$ that reveals the sources of random assignment in a large class of mechanisms satisfying equal treatment of equals. The large-market score also provides a natural smoother for the raw simulated score.

3. SCORE THEORY

3.1. *A Large-Market Approximation*

Our analysis of single tie-breaking DA provides a theoretical foundation for the derivation of propensity scores for a wide class of mechanisms. Extensions to the most important of these other mechanisms (DA with multiple tie-breakers and immediate acceptance) are discussed at the end of the Appendix.

The probability of assignment to school a under DA is determined both by an applicant's failure to win a seat at schools he ranks more highly than a and by the odds he wins a seat at a in competition with those who have also ranked a and similarly failed to find seats at schools they have ranked more highly. This structure leads to a simple formula quantifying these two types of risk.[9] The following example illustrates this structure:

EXAMPLE 2: Four applicants $\{1, 2, 3, 4\}$ apply to three schools $\{a, b, c\}$, each with one seat. There are no school priorities and applicant preferences are

$$\begin{aligned} &1 : c, \\ &2 : c \succ b \succ a, \\ &3 : b \succ a, \\ &4 : a. \end{aligned}$$

As in Example 1, each applicant is of a different type.

[9]Other applications of large-market approximations include Abdulkadiroğlu, Che, and Yasuda (2015), Azevedo and Leshno (2016), Budish (2011), Che and Kojima (2010), Kesten and Ünver (2015).

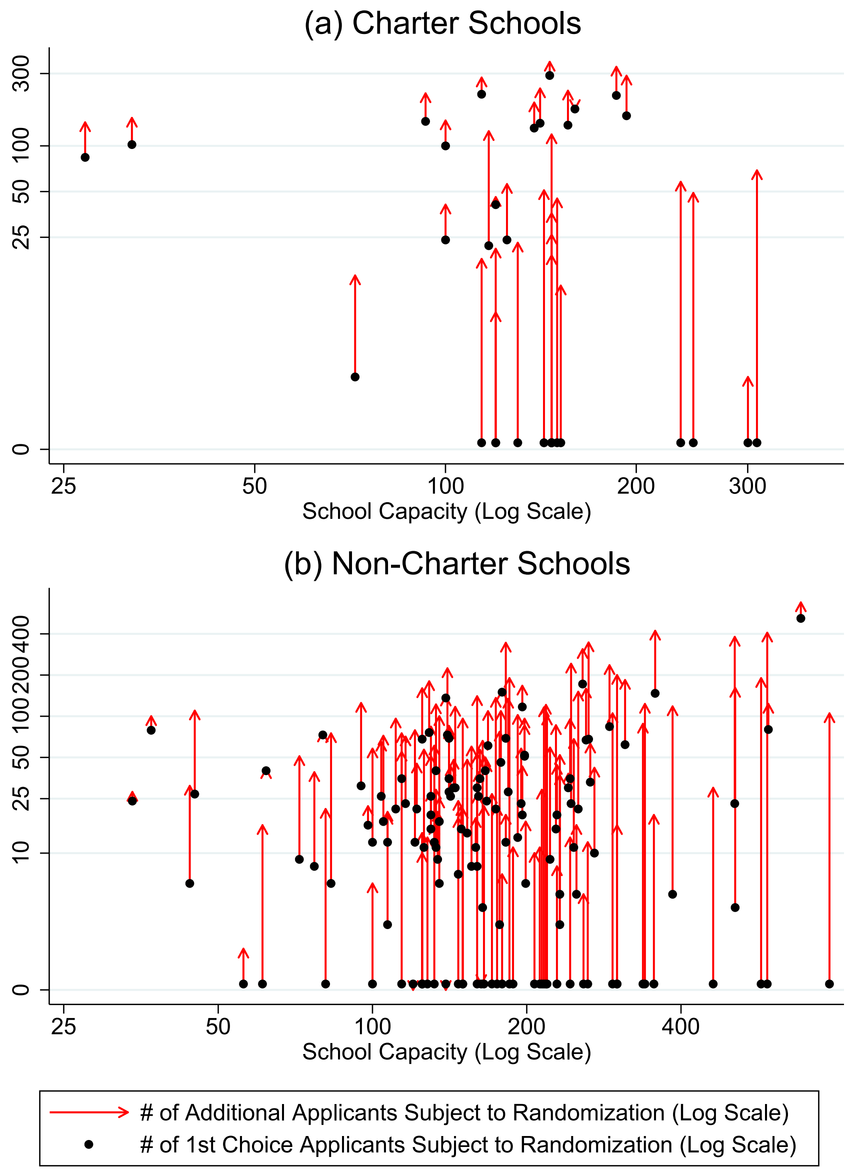

FIGURE 1.—Sample size gains from the propensity-score strategy. *Notes*: These figures compare the sample size subject to random assignment to the number of applicants randomized at their first choice school. An applicant is said to be subject to randomization at a school if the applicant has a simulated probability of assignment to that school that is neither 0 nor 1.

Let $p_a(\theta)$ denote the probability that type i is assigned to school a for $\theta = 1, 2, 3, 4$. With four applicants, $p_a(\theta)$ comes from $4! = 24$ possible orders of lottery draws, all equally likely. Given this modest number of possibilities, $p_a(\theta)$ is easily calculated by enumeration:

- Not having ranked a, type 1 is never assigned there, so $p_a(1) = 0$.
- Type 2 is seated at a when schools he has ranked ahead of a, schools b and c, are filled by others, and when he also beats type 4 in competition for a seat at a. This occurs

for the two realizations of the form $(s, t, 2, 4)$ for $s, t = 1, 3$, where the notation (s, t, u, v) means a draw with lottery number s assigned to applicant 1, lottery number t assigned to applicant 2, and so on. Therefore, $p_a(2) = 2/24 = 1/12$.

- Type 3 is seated at a when the schools he has ranked ahead of a—in this case, only b—are filled by others, while he also beats type 4 in competition for a seat at a. b can be filled by type 2 only when 2 loses to 1 in the lottery at c. Consequently, type 3 is seated at a only in a sequence of the form $(1, 2, 3, 4)$, which occurs only once. Therefore, $p_a(3) = 1/24$.
- Finally, since type 4 gets the seat at a if and only if the seat does not go to type 2 or type 3, so $p_a(4) = 21/24$.

In this example, the propensity score differs for each applicant. But in larger markets with the same distribution of types, the score is smoother. To see this, consider a market that replicates the structure of this example n times, so that n applicants of each type apply to up to three schools, each with n seats.

The relationship between simulated probabilities of assignment and market size for Example 2, plotted in Figure 2, reveals that as the market grows, the distinction between types 2 and 3 disappears. In particular, Figure 2 shows that, for large enough n,

$$p_a(2) = p_a(3) = 1/12; \qquad p_a(1) = 0; \qquad p_a(4) = 5/6,$$

with the probability of assignment at a for types 2 and 3 converging quickly. This convergence is a consequence of a result established in the next subsection, which shows that the large-market probabilities that types 2 and 3 are seated at a are both determined by failure to win a seat at b. The fact that applicant 2 ranks c ahead of b is irrelevant.

Why is the difference in preferences between applicants 2 and 3 ultimately irrelevant? Among schools that an applicant prefers to a, large-market risk is determined solely by failure to qualify—that is, by having a lottery number above the cutoff—at the school

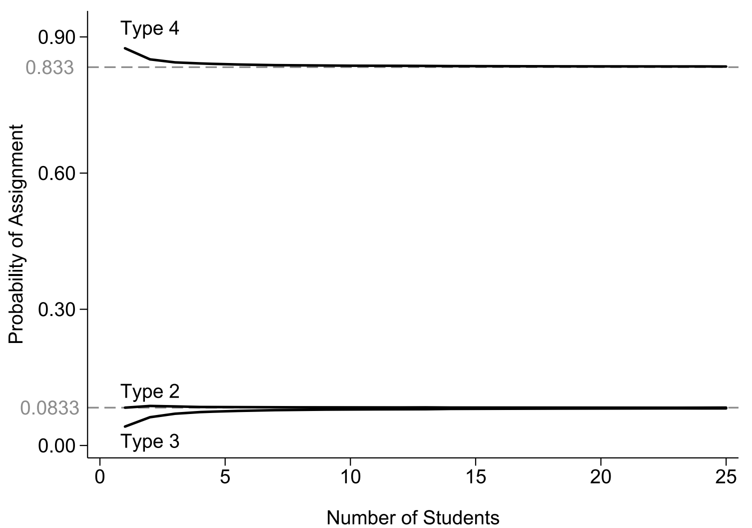

FIGURE 2.—Propensity scores and market size in Example 2. *Notes*: This figure plots finite-market propensity scores for expansions of Example 2. For each value on the x axis, we consider an expansion with x applicants of each type. The propensity scores plotted here were computed by drawing lottery numbers 100,000 times and rerunning the DA algorithm for each draw.

at which it is easiest to qualify. In general, this *most informative disqualification* (MID) determines how distributions of lottery numbers for applicants of differing types are truncated before entering the competition for seats at a. As we show below, the fact that the large-market score depends on type only through a set of parameters like MID allows us to replace full-type conditioning with something much smoother.

As a formal matter, the large-market model is built on the notion of a continuum of applicants. A *continuum economy* sets $I = [0, 1]$, with school capacities, q_s, defined as the proportion of I that can be seated at school s. Applicant i's lottery number, r_i, is drawn from a standard uniform distribution over $[0, 1]$, independently for all applicants. In particular, lottery draws are independent of type. With single tie-breaking, all schools look at the same lottery number. Extension to the less-common multiple tie-breaking case, in which applicants have different lottery numbers for each school, is discussed in the Appendix.

For any set of applicant types $\Theta_0 \subset \Theta$ and for any number $r_0 \in [0, 1]$, define the set of applicants in Θ_0 with lottery number less than r_0 to be

$$I(\Theta_0, r_0) = \{i \in I \mid \theta_i \in \Theta_0, r_i \leq r_0\}.$$

We use the shorthand notation $I_0 = I(\Theta_0, r_0)$.

In a finite economy with n applicants, denote the fraction of applicants in I_0 by

$$F(I_0) = \frac{|I_0|}{n}.$$

$F(I_0)$ for a finite economy depends on the realized lottery draw. In a continuum economy, $F(I_0)$ is defined as

$$F(I_0) = E\big[1\{\theta_i \in \Theta_0\}\big] \times r_0,$$

where $E[1\{\theta_i \in \Theta_0\}]$ is the proportion of types in set Θ_0. Either way, the applicant side of an economy is fully characterized by the distribution of types and lottery numbers, for which we sometimes use the shorthand notation, F.

Defining DA

We define DA using the notation above, nesting the finite-market and continuum cases. First, combine priority status and lottery realization into a single number for each applicant and school, called *applicant rank*:

$$\pi_{is} = \rho_{is} + r_i.$$

Since the difference between any two priorities is at least one and random numbers are between zero and one, rank is lexicographic in priority and lottery numbers.

DA proceeds in a series of rounds, indexed here by t. Denote the evolving vector of *admissions cutoffs* in round t by $\mathbf{c}^t = (c_1^t, \dots, c_S^t)$. The *demand* for seats at school s conditional on $\mathbf{c}^t$ is defined as

$$Q_s(\mathbf{c}^t) = \{i \in I \mid \pi_{is} \leq c_s^t \text{ and } s \succ_i \tilde{s} \text{ for all } \tilde{s} \in S \text{ such that } \pi_{i\tilde{s}} \leq c_{\tilde{s}}^t\}.$$

In other words, school s is demanded by applicants with rank below the school-s cutoff, who prefer school s to any other school for which they are also below the relevant cutoff.

The largest possible value of an eligible applicant's rank is $K+1$, so we can start with $c_s^1 = K+1$ for all s. Cutoffs then evolve as follows:

$$c_s^{t+1} = \begin{cases} K+1 & \text{if } F\big(Q_s(\mathbf{c}^t)\big) < q_s, \\ \max\big\{x \in [0, K+1] \mid F\big(\big\{i \in Q_s(\mathbf{c}^t) \text{ such that } \pi_{is} \leq x\big\}\big) \leq q_s\big\} & \text{otherwise;} \end{cases}$$

where, because the quantities involving F can be written in the form $F(\{i \in I | \theta_i \in \Theta_0, r_i \leq r_0\})$, the expression is well-defined. This formalizes the idea that when the demand for seats at s falls below capacity at s, the cutoff is $K+1$. Otherwise, the cutoff at s is the largest value such that demand for seats at s is less than or equal to capacity at s.

The final admissions cutoffs determined by DA for each school s are given by

$$c_s = \lim_{t\to\infty} c_s^t.$$

The set of applicants that are assigned school s under DA is the demand for seats at the limiting cutoffs: $\{i \in Q_s(\mathbf{c})\}$ where $\mathbf{c} = (c_1, \ldots, c_S)$.[10] Since $c_s \leq K+1$, an ineligible applicant (who has priority ∞) is never assigned to school s.

We write the final DA cutoffs as a limiting outcome to accommodate the continuum economy; in real markets, DA converges in a finite number of rounds. Appendix A.2 shows that the characterization of DA in this section is valid in the sense that: (a) the necessary limits exist for every economy, finite or continuum; (b) for every finite economy, the allocation upon convergence matches that produced by DA as usually described (e.g., by Gale and Shapley (1962) and the many studies building on their work).

3.2. *Characterizing the DA Propensity Score*

A key component of our large-market characterization of $p_s(\theta)$ is the notion of a *marginal priority* group at school s. The marginal priority group consists of applicants for whom seats are allocated by lottery when a school is oversubscribed. Formally, marginal priority, ρ_s, is the integer part of the cutoff, c_s. Conditional on being rejected by all more preferred schools and applying for school s, an applicant is assigned s with certainty if $\rho_{is} < \rho_s$, that is, if he clears 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 lottery numbers fall below a school-specific *lottery cutoff*. The lottery cutoff at school s, denoted τ_s, is the decimal part of the cutoff at s, that is, $\tau_s = c_s - \rho_s$.

These observations motivate a partition determined by marginal priorities at s. Let Θ_s denote the set of applicant types who rank s and partition Θ_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*).

The set Θ_s^n contains applicant types who have worse-than-marginal priority at s. No one in this *never seated* group is assigned to s. Θ_s^a contains applicant types that clear marginal priority at s. Some of these *always seated* applicants may end up seated at a school they prefer to s, but they are assigned s for sure if they fail to find a seat at any school they

[10]The characterization of DA via cutoffs has proven valuable in other studies of matching markets. See, for example, Abdulkadiroğlu, Che, and Yasuda (2015), Azevedo and Leshno (2016), and Agarwal and Somaini (2015).

have ranked more highly. Finally, Θ_s^c is the subset of Θ_s that has marginal priority at s. These *conditionally seated* applicants are assigned s when they are not assigned a higher choice *and* have a lottery number that clears the lottery cutoff at s.

A second key component of our score formulation reflects the fact that qualification at schools other than s may truncate the distribution of lottery numbers in the marginal priority group for s. To characterize the distribution of lottery numbers among those at risk of assignment to s, we introduce notation for the set of schools ranked above s. Specifically, applicants of type θ view the following set of schools as better than s:

$$B_{\theta s} = \{s' \in S \mid s' \succ_\theta s\}.$$

Type θ's MID at school s is defined as a function of the cutoffs at schools in $B_{\theta s}$:

$$\mathrm{MID}_{\theta s} = \begin{cases} 0 & \text{if } \rho_{\theta\tilde{s}} > \rho_{\tilde{s}} \text{ for all } \tilde{s} \in B_{\theta s} = \emptyset, \\ 1 & \text{if } \rho_{\theta\tilde{s}} < \rho_{\tilde{s}} \text{ for some } \tilde{s} \in B_{\theta s}, \\ \max\{\tau_{\tilde{s}} \mid \tilde{s} \in B_{\theta s} \text{ and } \rho_{\theta\tilde{s}} = \rho_{\tilde{s}}\} & \text{if } \rho_{\theta\tilde{s}} = \rho_{\tilde{s}} \text{ for some } \tilde{s} \in B_{\theta s} \\ & \quad \text{and } \rho_{\theta\tilde{s}} > \rho_{\tilde{s}} \text{ otherwise.} \end{cases}$$

$\mathrm{MID}_{\theta s}$ describes lottery number truncation among applicants to s disqualified at schools they prefer to s. $\mathrm{MID}_{\theta s}$ is zero when type-θ applicants have worse-than-marginal priority at all higher ranked schools: when no applicants for s can be seated at a more preferred school, there is no lottery number truncation among those at risk of assignment to s. On the other hand, when at least one school in $B_{\theta s}$ is undersubscribed, no one of type θ competes for a seat at s. Truncation in this case is complete, and $\mathrm{MID}_{\theta s} = 1$.

The definition of $\mathrm{MID}_{\theta s}$ also reflects the fact that, among applicants for whom $\rho_{\theta\tilde{s}} = \rho_{\tilde{s}}$ for some $\tilde{s} \in B_{\theta s}$, anyone who fails to clear $\tau_{\tilde{s}}$ 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 fail to qualify at schools with cutoffs below 0.5. Consequently, to keep track of the truncation induced by qualification at all schools an applicant prefers to s, we need to record only the most forgiving cutoff that an applicant fails to clear.

The following theorem uses the marginal priority and MID concepts to define an easily-computed *DA propensity score*, which coincides with the true propensity score $p_s(\theta)$ in any continuum economy:

THEOREM 1: *Consider a continuum economy populated by applicants of type $\theta \in \Theta$, to be assigned to schools indexed by $s \in S$. For all s and θ in this economy, we have*:

$$p_s(\theta) = \varphi_s(\theta) = \begin{cases} 0 & \textit{if } \theta \in \Theta_s^n, \\ (1 - \mathrm{MID}_{\theta s}) & \textit{if } \theta \in \Theta_s^a, \\ (1 - \mathrm{MID}_{\theta s}) \times \max\left\{0, \dfrac{\tau_s - \mathrm{MID}_{\theta s}}{1 - \mathrm{MID}_{\theta s}}\right\} & \textit{if } \theta \in \Theta_s^c, \end{cases} \tag{2}$$

where we also set $\varphi_s(\theta) = 0$ when $\mathrm{MID}_{\theta s} = 1$ and $\theta \in \Theta_s^c$.

The proof appears in Appendix A.3.

School choice without priorities offers a revealing simplification of this result. Without priorities, DA is the same as a random serial dictatorship (RSD), that is, a serial dictatorship with applicants ordered by lottery number (see, e.g., Abdulkadiroğlu and Sönmez

(1998), Svensson (1999), Pathak and Sethuraman (2011)).[11] Theorem 1 therefore implies the following corollary, which gives the *RSD propensity score*:

COROLLARY 1: *Consider a continuum economy with no priorities populated by applicants of type $\theta \in \Theta$, to be assigned to schools indexed by $s \in S$. For all s and θ in this economy, we have*:

$$p_s(\theta) = \varphi_s(\theta) = (1 - \text{MID}_{\theta s}) \times \max\left\{0, \frac{\tau_s - \text{MID}_{\theta s}}{1 - \text{MID}_{\theta s}}\right\} = \max\{0, \tau_s - \text{MID}_{\theta s}\}.$$

Without priorities, Θ_s^n and Θ_s^a are empty. The probability of assignment at s is therefore determined solely by draws from the truncated distribution of lottery numbers remaining after eliminating applicants seated at schools they have ranked more highly. Applicants whose most informative disqualification exceeds the cutoff at school s cannot be seated at s because disqualification at a more preferred school implies disqualification at s. Abdulkadiroğlu, Angrist, Narita, Pathak, and Zarate (2017) derive the propensity score for simple RSD with a single non-random tie-breaker.

In a match with priorities, the DA propensity score also accounts for the fact that random assignment at s occurs partly as a consequence of not being seated at a school preferred to s. Using the language and notation introduced in this section, we can explain the DA propensity score as follows:

(i) Type Θ_s^n applicants have a DA score of zero because these applicants have worse-than-marginal priority at s.

(ii) The probability of assignment at s is $1 - \text{MID}_{\theta s}$ for applicants in Θ_s^a because these applicants clear marginal priority at s, and applicants who clear marginal priority at s are guaranteed a seat there if they do not do better. Not doing better means failing to clear $\text{MID}_{\theta s}$, the most forgiving cutoff to which they are exposed in the set of schools preferred to s. Since lottery numbers are uniform, this occurs with probability $1 - \text{MID}_{\theta s}$.

(iii) Applicants in Θ_s^c are marginal at s and so are seated at s when they fail to be seated at a higher-ranked choice and win the competition for seats at s. As for applicants in Θ_s^a, the proportion in Θ_s^c given consideration at s is $1 - \text{MID}_{\theta s}$. Because applicants in Θ_s^c are marginal at s, their status at s is also determined by the lottery cutoff at s. If the cutoff at s, τ_s, falls below the truncation point, $\text{MID}_{\theta s}$, no one in this partition finds a seat at s. On the other hand, when τ_s exceeds $\text{MID}_{\theta s}$, seats are awarded by drawing from a continuous uniform distribution on $[\text{MID}_{\theta s}, 1]$. The resulting assignment probability is therefore $(\tau_s - \text{MID}_{\theta s})/(1 - \text{MID}_{\theta s})$.

The DA propensity score is a simple function of a small number of market parameters, specifically, $\text{MID}_{\theta s}$, τ_s, and marginal priority status at s and elsewhere. Because priorites are usually coarse and $\text{MID}_{\theta s}$ is drawn from the set of school-specific cutoffs, we expect many different types to share marginal priority status and $\text{MID}_{\theta s}$, coarsening the score in a manner that facilitates empirical work. In stylized examples, we can easily compute continuum values for these parameters.[12] In real markets with elaborate preferences and

[11] Seats for selective exam schools are sometimes assigned by a serial dictatorship based on admission test scores instead of random numbers (see, e.g., Abdulkadiroğlu, Angrist, and Pathak (2014), Dobbie and Fryer (2014)). A generalization of RSD, multi-category serial dictatorship, is used for Turkish college admissions (Balinski and Sönmez (1999)).

[12] Appendix A.4 uses Theorem 1 to explain the convergence of type 2 and type 3 propensity scores seen in Figure 2.

priorities, it is natural to use sample analogs for score estimation. As we show below, this generates consistent estimators of the propensity score for finite markets.[13]

3.3. *Estimating the DA Propensity Score*

We are interested in the asymptotic behavior of propensity score estimates based on Theorem 1. In particular, we show here that a sample analog of the DA score converges (almost surely) uniformly as market size grows to the propensity score for the limiting economy. Convergence in overall market size explains in part why we expect conditioning on the sample analog of the DA score to produce ignorable offers in real markets with few applicants per type. Our empirical application validates this conjectured good performance: applicant characteristics are balanced conditional on sample analogs of the DA propensity score. Other factors contributing to the empirical success of Theorem 1 are discussed briefly after documenting this balance.

The asymptotic sequence for the estimated score works as follows: randomly sample n applicants and their lottery numbers from a continuum economy, described by distribution F and school capacities, $\mathbf{q}$. Call the distribution of types and lottery numbers in this sample F_n. Fix the proportion of seats at school s in the sampled economy to be q_s and run DA with these applicants and schools. Compute $\text{MID}_{\theta s}$, τ_s, and partition Θ_s using observed cutoffs $\hat{\mathbf{c}}_n$ and assignments in this single realization, then plug these quantities into equation (2). This generates an estimated propensity score, $\hat{p}_{ns}(\theta)$, constructed by treating a size-n sample economy like its continuum analog. The actual propensity score for this finite economy, computed by repeatedly drawing lottery numbers for the sample of applicants described by F_n and the set of schools with proportional capacities $\mathbf{q}$, is denoted $p_{ns}(\theta)$. We consider the gap between $\hat{p}_{ns}(\theta)$ and $p_{ns}(\theta)$ as n grows.

The analysis here makes use of a regularity condition:

ASSUMPTION 1—Rich Support: *For any $s \in S$ and priority $\rho \in \{1, \dots, K\}$ with $F(\{i \in I : \rho_{is} = \rho\}) > 0$, we have $F(\{i \in I : \rho_{is} = \rho, i \text{ ranks } s \text{ first}\}) > 0$.*

This says that in the continuum economy, every school is ranked first by at least some applicants in every non-empty priority group defined for that school.

In this setup, the propensity score estimated by applying Theorem 1 to data drawn from a single sample and lottery realization converges almost surely to the propensity score generated by repeatedly drawing lottery numbers. This result is presented as a theorem:

THEOREM 2: *In the asymptotic sequence described by F_n with proportional school capacities fixed at $\mathbf{q}$ and maintaining Assumption 1, the DA propensity score $\hat{p}_{ns}(\theta)$ computed by applying Theorem 1 to F_n is a strongly consistent estimator of $p_{ns}(\theta)$ in the following sense: For all $\theta \in \Theta$ and $s \in S$,*

$$\left|\hat{p}_{ns}(\theta) - p_{ns}(\theta)\right| \xrightarrow{a.s.} 0.$$

Moreover, since θ has finite support, this convergence is uniform in θ.

[13] Appendices A.9 and A.10 extend Theorem 1 to DA using school-specific tie-breaking and the Boston mechanism. Theorem 1 also applies to first preference first mechanisms (discussed by Pathak and Sönmez (2013)), Chinese parallel mechanisms (discussed by Chen and Kesten (2017)), and deduction point mechanisms (discussed by Pathak, Song, and Sönmez (2016)).

PROOF: The proof uses intermediate results given as lemmas in Appendix A.5. The first lemma establishes that the vector of cutoffs computed for the sampled economy, $\hat{\mathbf{c}}_n$, converges to the vector of cutoffs in the continuum economy. That is,

$$\hat{\mathbf{c}}_n \overset{\text{a.s.}}{\to} \mathbf{c},$$

where $\mathbf{c}$ denotes the continuum economy cutoffs. This result, together with the continuous mapping theorem, implies

$$\hat{p}_{ns}(\theta) \overset{\text{a.s.}}{\to} \varphi_s(\theta).$$

In other words, the propensity score estimated by applying Theorem 1 to a sampled finite economy converges to the DA propensity score for the corresponding continuum economy.

A second lemma establishes that for all $\theta \in \Theta$ and $s \in S$,

$$p_{ns}(\theta) \overset{\text{a.s.}}{\to} \varphi_s(\theta).$$

That is, the actual (re-randomization-based) propensity score in the sampled finite economy also converges to the propensity score in the continuum economy.[14]

Combining these two results shows that for all $\theta \in \Theta$ and $s \in S$,

$$\left|\hat{p}_{ns}(\theta) - p_{ns}(\theta)\right| \overset{\text{a.s.}}{\to} \left|\varphi_s(\theta) - \varphi_s(\theta)\right| = 0,$$

completing the proof. Since both Θ and S are finite, this also implies uniform convergence, that is, $\sup_{\theta\in\Theta, s\in S} |\hat{p}_{ns}(\theta) - p_{ns}(\theta)| \overset{\text{a.s.}}{\to} 0$. *Q.E.D.*

Theorem 2 justifies our use of the formula in Theorem 1 to control for applicant type in empirical work estimating school attendance effects. This theoretical result is used for propensity score estimation in two ways. The first, which we label a "formula" calculation, applies equation (2) directly to the DPS data. Specifically, for each applicant type, school, and entry grade, we identify marginal priorities and applicants are allocated by priority status to either Θ_s^n, Θ_s^a, or Θ_s^c. The DA score is then estimated by computing the sample analog of $\text{MID}_{\theta s}$ and τ_s in the DPS assignment data and plugging these into equation (2).

Much of our empirical work uses a second application of Theorem 1, which also starts with marginal priorities, MIDs, and cutoffs in the DPS data. This score estimate takes cells defined by constant values of MID, Θ^c, Θ^a, and Θ^n estimated as in the formula calculation and tabulates the empirical offer rate in these cells. This score estimate, which we refer to as a "frequency" calculation, is closer to an estimated score of the sort discussed by Abadie and Imbens (2016) than is the formula score. The large-sample distribution theory in Abadie and Imbens (2016) suggests that conditioning on an estimated score may increase the efficiency of score-based estimates of average treatment effects.[15]

[14]See also Azevedo and Leshno (2016), who provided convergence results for the cutoffs and conditional-on-type probabilities of assignment generated by a sequence of stable matchings, showing that the empirical assignment rates for types in a finite market converge to the continuum probability of assignment. The two lemmas in Appendix A.5 differ from Azevedo and Leshno's (2016) results in that they use Assumption 1 and are proved using the Extended Continuous Mapping Theorem. The characterization of the DA propensity score in Theorem 1 does not appear to have an analog in the Azevedo and Leshno (2016) framework.

[15]Section 2.3 of Rosenbaum (1987) makes a similar argument.

TABLE III

DA SCORE ANATOMY AT STRIVE PREP SCHOOLS (2013 APPLICANTS)[a]

				Θ_s^n	Θ_s^c		Θ_s^a		
	Eligible			$0 \le \text{MID} \le 1$	$\text{MID} \ge \tau_s$	$\text{MID} < \tau_s$	$\text{MID} = 1$	$0 < \text{MID} < 1$	$\text{MID} = 0$
	Applicants	Capacity	Offers	$p_s(\theta) = 0$	$p_s(\theta) = 0$	$0 < p_s(\theta) < 1$	$p_s(\theta) = 0$	$0 < p_s(\theta) < 1$	$p_s(\theta) = 1$
Campus	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
GVR	324	147	112	0	0	0	159	116	49
Lake	274	147	126	0	0	0	132	26	116
Highland	244	147	112	0	0	0	121	21	102
Montbello	188	147	37	0	0	0	128	31	29
Federal	574	138	138	78	284	171	3	1	37
Westwood	494	141	141	53	181	238	4	0	18

[a]This table shows how formula scores are determined for STRIVE applicants in grade 6 (all 6th-grade seats at these schools are assigned in a single bucket; ineligible applicants are omitted) for applicants applying for seats in the fall 2013 school year. Column 3 records offers made to these applicants. Columns 4, 5, and 7 show the number of applicants in partitions with a score of zero. Columns 6 and 8 show the number of applicants subject to random assignment. Column 9 shows the number of applicants with certain offers.

3.4. *Explaining Random Assignment*

Earlier, we noted that STRIVE Prep-GVR had 119 applicants randomized in 2013, even though no applicant with non-degenerate offer risk ranked this school first. Random assignment at GVR is a consequence of the many GVR applicants randomized by admissions offers at schools they had ranked more highly. This and related determinants of offer risk are detailed in Table III, which explores the anatomy of the DA propensity score for 6th-grade applicants to six middle schools in the STRIVE network. Columns 6 and 8 of the table count the number of randomized applicants. We see, for example, that all 116 randomized GVR applicants were randomized by virtue of having $\text{MID}_{\theta s}$ inside the unit interval (shown in column 8), with no one randomized at GVR's own cutoff (shown in column 6).

In contrast with STRIVE's GVR school, few 2013 applicants were randomized at STRIVE's Highland, Lake, and Montbello campuses. This is a consequence of the fact that most Highland, Lake, and Montbello applicants were likely to clear marginal priority at these schools (having $\rho_{\theta s} < \rho_s$), with values of $\text{MID}_{\theta s}$ mostly equal to zero or one, eliminating random assignment at schools ranked more highly. Interestingly, the Federal and Westwood campuses are the only STRIVE schools to see applicants randomized around the cutoff in these schools' own marginal priority groups. We can therefore increase the number randomly assigned at Federal and Westwood by changing the cutoff there (e.g., by changing capacity), whereas such a change is likely to be of little consequence for evaluations of the other schools.

Table III also documents the surprisingly weak connection between applicant randomization counts and a naive definition of oversubscription based on school capacity. In particular, columns 2 and 3 reveal that four out of six schools described in the table ultimately made fewer offers than they had seats available (far fewer in the case of Montbello). Even so, assignment at these schools was far from certain. They therefore contribute to our charter school impact analysis.

3.5. *DA Score Balancing Tests*

Theorems 1 and 2 provide asymptotic approximations, the quality of which should be judged in real markets. The goal of propensity score conditioning is to eliminate omitted variables bias induced by covariates associated with treatments. Covariate balance is therefore an important measure of the score-based empirical strategy's success (a standard applied routinely; see, e.g., Dehejia and Wahba (1999) and Chapter 14 of Imbens and Rubin (2015)).

The balance measures reported in Table IV compare uncontrolled differences in average applicant characteristics by charter-offer status with balance estimates that control for the score. The latter put applicant characteristics on the left-hand side of regression models that have charter-offer status and controls for the propensity score on the right. Balance in this case is measured by regression estimates of

$$\omega = E\{[W_i|D_i = 1, \hat{p}_D(\theta_i)] - E[W_i|D_i = 0, \hat{p}_D(\theta_i)]\},$$

where W_i is a vector of applicant characteristics, including some in θ_i, and D_i indicates the offer of any charter seat. $\hat{p}_D(\theta_i)$ is an estimate of the propensity score for getting any charter seat ($D_i = 1$), computed using simulation or Theorems 1 and 2. The balance parameter, ω, is estimated over 400 runs of DA. Specifically, for each of these 400 draws, we regress W_i on a saturated model for the estimated (or simulated) propensity score along with a dummy for charter offers. The propensity score theorem leads us to expect the average charter offer coefficient from these regressions to be close to zero.

Table IV reports the average coefficient on offer in models that dummy all score values inside the unit interval. Uncontrolled comparisons by offer status, reported in column 2 of the table, show large differences in average applicant characteristics, especially for variables related to preferences. On average, those not offered a charter seat ranked an average of 1.4 charter schools, but this increases by almost half a school for applicants who were offered a charter seat. Likewise, while fewer than 30% of those not offered a charter seat had ranked a charter school first, the probability applicants ranked a charter first increases to over 0.9 (i.e., 0.28 + 0.64) for those offered a charter seat. Column 2 also reveals important demographic differences by offer status; Hispanic applicants, for example, are substantially over-represented among those offered a charter seat.

Control for the simulated propensity score balances covariates almost perfectly. This can be seen in columns 3 and 4 of Table IV, which report balance conditional on the simulated score using two rounding schemes. Rounding reflects the fact that, for example, the simulated score has 1,229 unique values. Rounding to the nearest hundredth leaves 77 points of support, while rounding to the nearest thousandth leaves 153 points of support.

Conditioning on frequency and formula estimates of the DA propensity score also reduces differences by offer status markedly, and almost as completely as does conditioning on the simulated score. The balance estimates in column 5 of Table IV, which come from regression models with nonparametric control for the DA frequency score, show only small differences by offer status. Column 6 shows that control for the formula score reduces offer gaps for some covariates even further. This evidence of balance means that the estimated DA score indeed eliminates selection bias. This is in spite of the fact that the DA propensity score is an asymptotic approximation that has been shown to provide perfect treatment-control balance only in a large-market limit.

We also report traditional statistical balance tests such as would typically be reported for a randomized trial. Specifically, Table V documents balance for the realized SchoolChoice match by reporting t- and F-statistics for charter offer gaps in covariate means. Again, we look at balance conditional on propensity scores for applicants with scores strictly between zero and one. Measured by statistical tests, covariates are about equally well-balanced by both the simulated score and the estimated DA scores. Not surprisingly,

TABLE IV

EXPECTED BALANCE[a]

			Simulated Score Controls		DA Score Controls	
Covariate	Non-Offered Mean (1)	No Controls (2)	Rounded (Hundredths) (3)	Rounded (Thousandths) (4)	Frequency (Saturated) (5)	Formula (Saturated) (6)
			A. Application variables			
Number of schools ranked	4.431	−0.533	0.013	0.005	0.049	0.012
Number of charter schools ranked	1.451	0.445	0.005	0.002	0.061	0.002
First school ranked is charter	0.276	0.641	0.000	−0.001	0.002	0.000
			B. Baseline covariates			
Origin school is charter	0.085	0.124	−0.003	−0.003	0.002	−0.002
Female	0.513	−0.021	0.003	0.003	0.002	0.003
Hispanic	0.596	0.105	0.001	0.001	−0.002	0.003
Black	0.189	−0.053	0.000	0.001	0.001	0.000
Gifted	0.207	−0.009	0.003	0.002	−0.002	−0.001
Bilingual	0.033	0.016	0.000	0.000	0.001	0.001
Subsidized lunch	0.781	0.054	0.001	0.000	0.000	0.003
Limited English proficient	0.306	0.090	0.001	0.001	−0.002	0.002
Special education	0.094	−0.007	−0.001	0.000	−0.003	−0.001
Baseline scores						
Math	−0.028	0.078	−0.003	−0.002	−0.007	−0.006
Reading	−0.001	−0.024	−0.002	−0.004	−0.007	−0.008
Writing	−0.001	−0.011	−0.002	−0.003	−0.008	−0.007
Average risk set points of support			77	153	90	104

[a] This table reports average covariate balance by charter offer status across 400 lottery draws, with DA rerun each time. The sample includes applicants for 2012–2013 and 2013–2014 charter seats in grades 4–10 who were enrolled in Denver at baseline. Balance is estimated by regressing each covariate on an any-charter offer dummy, controlling for the propensity-score variable indicated in each column and dummies for the year in which the applicant applied. The table reports averages of these balance coefficients. The charter-offer variable indicates an offer at any charter school for a given lottery draw (excluding alternative charters). Column 1 reports the average over lottery draws baseline characteristics of charter applicants who did not receive a charter offer. Column 2 reports the average coefficient when no propensity-score controls are used. The estimates in columns 3, 4, 5, and 6 control for the score variables indicated in the column heading. The average risk set points of support reported at the bottom of the table counts the average number of unique values found in the support of the relevant propensity score, excluding 0 and 1. For applicants who applied in both years, we consider only first-time applications.

a few marginally significant imbalances pop up. But the F-statistics (reported at the bottom of the table) that jointly test balance for all baseline covariates fail to reject the null hypothesis of conditional balance for any specification reported. In this case, conditioning on the frequency score produces a slight improvement in balance over the formula score.

As can be seen in the last column of Table V, full control for type reduces the sample available for estimation considerably. Models with full-type control are run on a sample of size 462. Likewise, the fact that saturated control for the simulated score requires some smoothing can be seen in the reduced sample available for estimation of models that control fully for a simulated score rounded to the nearest thousandth rather than the nearest hundreth (the sample size for baseline score balance falls from 2,678 to 2,263).

A few marginally significant baseline score gaps appear in some of the score-controlled comparisons at the bottom of the table. The F-test results and the fact that these gaps are not mirrored in the comparisons in Table IV suggest the differences in Table V are due to chance. Still, we can mitigate the effect of chance differences on 2SLS estimates of charter effects by adding baseline score controls (and other covariates) to empirical models. The inclusion of these additional controls also has the salutary effect of making

TABLE V

STATISTICAL TESTS FOR BALANCE[a]

			Simulated Score Controls		DA Score Controls		
	Non-Offered Mean (1)	No Controls (2)	Rounded (Hundredths) (3)	Rounded (Thousandths) (4)	Frequency (Saturated) (5)	Formula (Saturated) (6)	Full Applicant Type Controls (7)
			A. Application variables				
Number of schools ranked	4.438	−0.544***	−0.076	−0.084	−0.075	−0.104	−0.046
		(0.031)	(0.070)	(0.073)	(0.071)	(0.069)	(0.041)
Number of charter schools ranked	1.450	0.443***	−0.027	−0.029	0.036	−0.020	0.001
		(0.018)	(0.035)	(0.037)	(0.037)	(0.035)	(0.018)
First school ranked is charter	0.275	0.639***	−0.026	−0.022	−0.005	−0.003	0.000
		(0.007)	(0.017)	(0.015)	(0.015)	(0.014)	(0.000)
			B. Baseline covariates				
Origin school is charter	0.087	0.118***	−0.027*	−0.029**	−0.028**	−0.038***	0.024
		(0.007)	(0.014)	(0.014)	(0.012)	(0.012)	(0.015)
Female	0.512	−0.017*	0.030	0.023	0.020	0.020	0.027
		(0.010)	(0.025)	(0.027)	(0.026)	(0.026)	(0.055)
Hispanic	0.597	0.102***	−0.011	−0.013	−0.014	−0.007	0.025
		(0.010)	(0.021)	(0.023)	(0.021)	(0.022)	(0.034)
Black	0.188	−0.052***	0.004	0.000	0.006	0.003	−0.020
		(0.007)	(0.019)	(0.020)	(0.019)	(0.019)	(0.028)
Subsidized lunch	0.782	0.052***	−0.007	−0.003	0.004	0.010	0.031
		(0.008)	(0.018)	(0.019)	(0.018)	(0.018)	(0.031)
Limited English proficient	0.305	0.089***	0.006	0.017	0.002	0.019	0.007
		(0.010)	(0.023)	(0.026)	(0.024)	(0.025)	(0.051)
Special education	0.093	−0.005	0.014	0.009	0.006	0.012	0.036
		(0.006)	(0.014)	(0.016)	(0.014)	(0.015)	(0.023)
N	5,674	9,879	2,714	2,291	2,445	2,404	464

(*Continues*)

TABLE V—*Continued*

	Non-Offered Mean (1)	No Controls (2)	Simulated Score Controls: Rounded (Hundredths) (3)	Simulated Score Controls: Rounded (Thousandths) (4)	DA Score Controls: Frequency (Saturated) (5)	DA Score Controls: Formula (Saturated) (6)	Full Applicant Type Controls (7)
Baseline scores							
Math	−0.025	0.073***	−0.038	−0.035	−0.037	−0.052	−0.205**
		(0.019)	(0.044)	(0.049)	(0.045)	(0.046)	(0.093)
Reading	0.003	−0.032*	−0.069	−0.079*	−0.077*	−0.080*	−0.140
		(0.019)	(0.043)	(0.048)	(0.045)	(0.045)	(0.086)
Writing	0.002	−0.016	−0.055	−0.049	−0.057	−0.069	−0.122
		(0.018)	(0.043)	(0.046)	(0.044)	(0.044)	(0.089)
N	5,586	9,743	2,678	2,263	2,415	2,375	462
Risk set points of support			75	141	80	92	89
F-test for joint significance (mvreg)		483.3	0.78	0.82	1.02	1.25	0.92
P-value		0.000	0.702	0.660	0.434	0.226	0.538

[a]This table reports coefficients from regressions of the application variables and baseline covariates in each row on a dummy for charter offers. The sample includes applicants for 2012–2014 charter seats in grades 4–10 who were enrolled in Denver at baseline. Columns 2–6 are from regressions like those used to construct expected balance in Table IV, except that the tests reported here use realized DA offers, with test statistics and standard errors computed in the usual way. Column 7 reports the balance test generated by a regression with saturated controls for applicant type (i.e., unique combinations of applicant preferences over school programs and school priorities in those programs). In columns 3–7, N is the number of applicants with a propensity score between zero and one. Robust standard errors are reported in parentheses. p-values for joint significance tests are estimated with stata's mvreg command. The sample used here and for the following tables omits repeat applications. *significant at 10%; **significant at 5%; ***significant at 1%.

the 2SLS estimates of interest considerably more precise (covariates used for this purpose include dummies for grade tested, gender, origin school charter status, race, gifted status, bilingual status, subsidized lunch eligibility, special education, limited English proficiency status, and baseline test scores; baseline score controls are responsible for most of the resulting precision gain).[16]

The DA score provides effective control for covariates in spite of the fact that the DPS SchoolChoice market includes almost as many types as applicants. This is consistent with Theorem 2, which establishes uniform almost sure convergence at a rate determined by overall market size. The good performance of the estimated DA score is also in line with earlier evidence on the accuracy of large-market approximations in matching markets from Azevedo and Leshno (2016), who used simulation to show the rapid convergence of empirical DA cutoffs to large-market values. The Azevedo–Leshno results are relevant because our DA score is determined by cutoffs.

We conclude this section by noting that when lottery numbers are independent of cutoffs, the DA score described by Theorems 1 and 2 is both unbiased and sufficient for type. This, too, helps explain the success of an empirical strategy based on these theoretical results. Formally, we have the following finite-sample result:

PROPOSITION 2: *Let $\tilde{p}_s(\theta)$ be the estimated DA propensity score obtained by computing* $\mathrm{MID}_{\theta s}$, τ_s, *and* Θ_s *for a lottery realization in a finite economy and plugging these quantities into equation* (2).[17] *Suppose that individual lottery numbers are independent of DA cutoffs generated by each lottery number realization, that is,* $r_i \perp\!\!\!\perp \mathbf{c}$ *for every applicant* i. *Then the estimated DA propensity score is unbiased for the true propensity score, that is,*

$$E\big[\tilde{p}_s(\theta)\big] = P\big[D_i(s) = 1 | \theta_i = \theta\big],$$

for every applicant type θ, *where* P *denotes the probability induced by DA with random lottery numbers. Moreover, assignment is independent of type conditional on the estimated DA propensity score:*

$$P\big[D_i(s) = 1 | \tilde{p}_s(\theta_i), \theta_i\big] = P\big[D_i(s) = 1 | \tilde{p}_s(\theta_i)\big].$$

These unbiasedness and conditional independence properties also hold for the frequency version of $\tilde{p}_s(\theta)$.

This result (proved in Appendix A.6) applies to the continuum since continuum cutoffs are constant. In finite economies, cutoffs are correlated with individual lottery numbers, so the premise of the proposition is false. Even so, our simulations of DPS SchoolChoice show that lottery numbers are close to uniformly distributed conditional on cutoffs, suggesting the premise is a reasonable approximation for this market. Proposition 2 therefore provides a finite-sample rationale for the use of the estimated DA propensity score in our application, and suggests the score may apply under approximation sequences that increase the number of types along with market size (as in Kojima and Pathak (2009),

[16]Supplemental Material Table B.II reports score-controlled estimates of differential attrition by offer status. Applicants who receive charter offers are 3–4 percent more likely to have follow-up scores, a modest difference that seems unlikely to bias the 2SLS charter estimates reported below. This is confirmed by an analysis that omits the 5% of applicants for whom conditional-on-score imbalance is greatest. Results in this trimmed sample are virtually unchanged from those in the full sample.

[17]Here, $\tilde{p}_s(\theta)$ is the estimated DA score for a fixed finite economy. In contrast, $\hat{p}_{ns}(\theta)$ in Section 3.3 is the estimated DA score for finite economies randomly sampled from a continuum economy.

Lee (2014), Ashlagi, Kanoria, and Leshno (2017)). The near-unbiasedness of DPS charter score estimates constructed using Theorems 1 and 2 is documented in Figure 3. This figure plots the average frequency and formula scores across 2,000 lottery draws against the corresponding values of the simulated score (computed using one million draws and rounded to 0.01). The figure shows a close fit.

4. USING THE SCORE

4.1. *Empirical Strategies*

We use DPS's first-round charter offers to construct instrumental variables estimates of the effects of charter enrollment on achievement. How should the resulting IV estimates be interpreted? Our IV procedure identifies causal effects for applicants enrolling in a charter when DA produces a charter offer but not otherwise; in the local average treatment effects (LATE) framework of Imbens and Angrist (1994) and Angrist, Imbens, and Rubin (1996), these are charter-offer compliers. IV fails to reveal average causal effects for applicants who decline a first-round DA charter offer and are assigned another type of school in round 2 (in the LATE framework, these are never-takers). Likewise, IV methods are not directly informative about the effects of charter enrollment on applicants not offered a charter seat in round 1, but who nevertheless find their way into a charter school in the second round (LATE always-takers).

To flesh out this interpretation and the assumptions on which it rests, let C_i be a charter enrollment indicator and let D_i indicate the offer of a charter seat. These variables indicate attendance and offers at *any* charter school, rather than at a specific school. Since DA produces a single offer, offers of seats at particular schools are mutually exclusive. We therefore construct D_i by summing individual charter-offer dummies. Likewise, the propensity score for this variable, $p_D(\theta) \equiv E[D_i|\theta_i = \theta]$, is obtained by summing the scores for all charter schools.

The population of charter-offer compliers is defined by *potential* charter enrollment status. This is indexed against the charter-offer instrument, D_i. In particular, we observe potential enrollment C_{1i} when D_i is switched on and potential enrollment C_{0i} otherwise (both of these are assumed to exist for all i). Observed enrollment is therefore

$$C_i = C_{0i} + (C_{1i} - C_{0i})D_i.$$

Compliers have $C_{1i} - C_{0i} = 1$, an event that happens when $C_{1i} = 1$ and $C_{0i} = 0$.

Causal effects are determined by potential outcomes, indexed against C_i. These are written as Y_{1i} and Y_{0i}. When $C_i = c$, we see Y_{ci}, so the observed outcome (a test score) is

$$Y_i = Y_{0i} + (Y_{1i} - Y_{0i})C_i.$$

Proposition 1 implies that charter offers are independent of potential assignments conditional on type. Given an exclusion restriction, the conditional random assignment of D_i also makes D_i conditionally independent of potential outcomes. The exclusion restriction in this case means that charter offers have no effect on outcomes other than by boosting charter attendance. The conceptual distinction between random assignment and instrument exclusion is discussed in Angrist, Imbens, and Rubin (1996) and, for the case of charter offers, in our working paper (Abdulkadiroglu, Angrist, Narita, and Pathak (2015)). As a practical matter, the exclusion restriction fails when charter offers change school quality *within* charter and non-charter sectors. This most likely occurs when charter offers change the type of school attended on margins other than charter attendance.

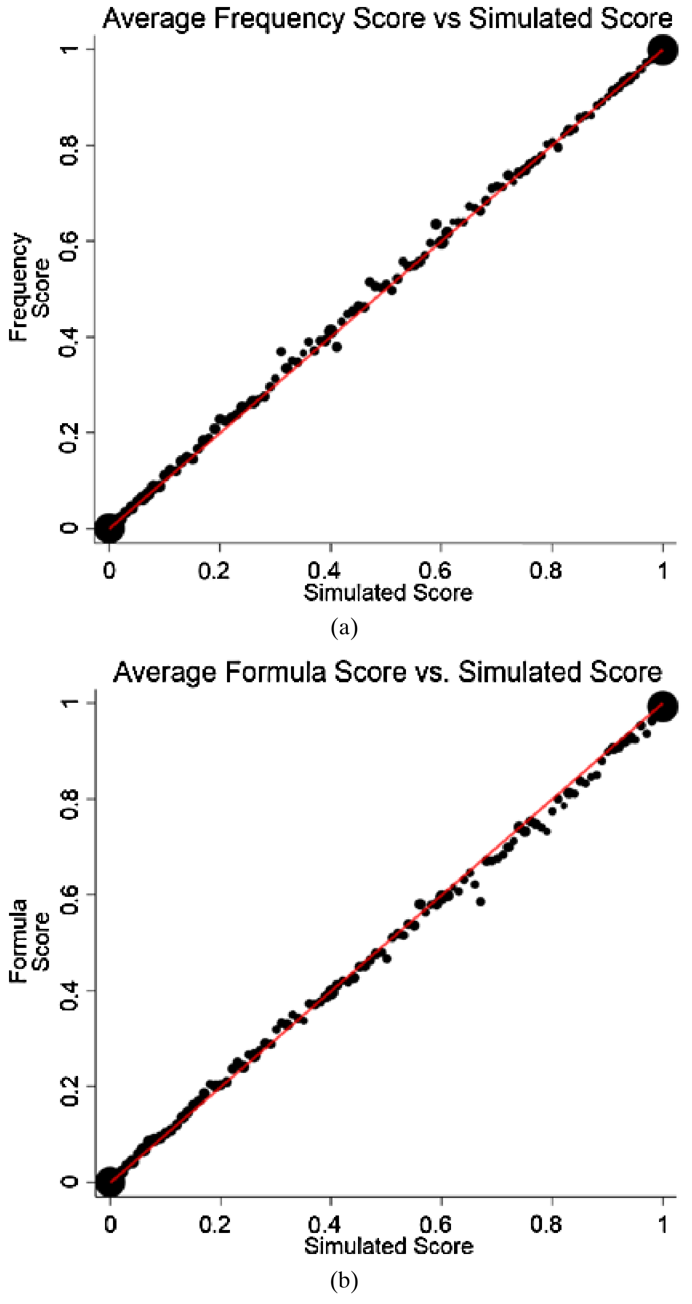

FIGURE 3.—Comparison of frequency and formula scores with simulated scores for 2013 applicants. *Notes*: This figure plots frequency and formula scores averaged over 2,000 simulations against the simulated score computed from 1,000,000 lottery draws, for each school bucket. Simulated scores are rounded to 0.01. The plot symbols are dots with radius proportional to the square root of the number of students in each 0.01 bin.

We therefore explore multi-sector models that identify the causal effects of attendance at different types of charter and non-charter schools. Estimates of multi-sector models are reported following 2SLS estimates of overall charter effects.

As with the conditional independence of single-school offers described by Proposition 1, the conditional independence and exclusion assumptions motivating 2SLS estimation of an overall charter effect can be written

$$P\big[D_i = 1|\{Y_{1i}, Y_{0i}, C_{1i}, C_{0i}\}, \theta_i = \theta\big] = P[D_i = 1|\theta_i = \theta] = p_D(\theta), \tag{3}$$

where the vector of potentials, $\{Y_{1i}, Y_{0i}, C_{1i}, C_{0i}\}$, plays the role of W_i. Likewise, as for single-school offers in equation (1), the propensity score theorem implies

$$P\big[D_i = 1|\{Y_{1i}, Y_{0i}, C_{1i}, C_{0i}\}, p_D(\theta_i) = p\big] = p, \tag{4}$$

where $p_D(\theta_i)$ is the charter-offer propensity score associated with applicant i's type.

Equations (3) and (4) allow us to estimate causal effects of charter *offers*, that is, the effect of D_i. In practice, however, we are interested in the effects of charter *attendance*, the treatment indicated by C_i. To complete the causal chain from charter offers to charter enrollment and finally to outcomes, we assume that charter offers change charter enrollment for at least some applicants, and that charter offers can only make charter enrollment more likely, so that $C_{1i} \geq C_{0i}$ for all i. With these first-stage and monotonicity assumptions supplementing (4), the conditional-on-score IV estimand is a conditional average causal affect for compliers at that score value.[18] That is, for all θ_i with $p_D(\theta_i) \in (0, 1)$,

$$\begin{aligned} &\frac{E\big[Y_i|D_i = 1, p_D(\theta_i) = x\big] - E\big[Y_i|D_i = 0, p_D(\theta_i) = x\big]}{E\big[C_i|D_i = 1, p_D(\theta_i) = x\big] - E\big[C_i|D_i = 0, p_D(\theta_i) = x\big]} \\ &\quad = E\big[Y_{1i} - Y_{0i}|p_D(\theta_i) = x, C_{1i} > C_{0i}\big], \end{aligned} \tag{5}$$

where x indexes values in the support of $p_D(\theta)$.

In view of the fact that (5) generates a distinct causal effect for each score value, it is natural to consider parsimonious models that use data from all propensity-score cells to estimate a single average causal effect. We marginalize conditional effects by estimating a 2SLS specification with first- and second-stage equations that can be written

$$C_i = \sum_x \gamma(x) d_i(x) + \delta D_i + X_i'\lambda + \nu_i, \tag{6}$$

$$Y_i = \sum_x \alpha(x) d_i(x) + \beta C_i + X_i'\mu + \varepsilon_i, \tag{7}$$

where the $d_i(x)$'s are dummies indicating values of the estimated score, $\hat{p}_D(\theta_i)$, indexed by x, and $\gamma(x)$ and $\alpha(x)$ are the associated "score effects" in the first and second stages. The coefficient δ in (6) is the first-stage effect of charter offers on charter enrollment, while the coefficient β in (7) is the causal effect of interest. These first- and second-stage

[18]Monotonicity is plausible because noncompliance of any sort arises through post-match appeals. Specifically, applicants can appeal SchoolChoice offers after the match, no matter what they are offered in SchoolChoice. But the offer of a charter seat through SchoolChoice produces a charter option that remains available regardless of the result of the appeal. The appeals process therefore seems unlikely to reduce charter enrollment for applicants effectively guaranteed a charter seat.

equations include baseline covariates, X_i, to increase precision and adjust for chance imbalances in applicant characteristics.

As a check on the 2SLS specification, we also report semiparametric estimates of $E[Y_{1i} - Y_{0i}|C_{1i} > C_{0i}]$. In contrast with the additive 2SLS setup, the semiparametric procedure requires only correct specification of the propensity score to generate a single average causal effect for all compliers. Our semiparametric strategy uses Abadie's (2003) observation that the conditional independence and exclusion restrictions imply

$$E[Y_{1i}|C_{1i} > C_{0i}] = \frac{1}{\Pr(C_{1i} > C_{0i})} E\left[\frac{C_i Y_i\big(D_i - p_D(\theta_i)\big)}{\big(1 - p_D(\theta_i)\big) p_D(\theta_i)}\right],$$

$$E[Y_{0i}|C_{1i} > C_{0i}] = \frac{1}{\Pr(C_{1i} > C_{0i})} E\left[\frac{(1 - C_i) Y_i\big((1 - D_i) - \big(1 - p_D(\theta_i)\big)\big)}{\big(1 - p_D(\theta_i)\big) p_D(\theta_i)}\right].$$

Subtracting and rearranging, we have

$$E[Y_{1i} - Y_{0i}|C_{1i} > C_{0i}] = \frac{1}{\Pr(C_{1i} > C_{0i})} E\left[\frac{Y_i\big(D_i - p_D(\theta)\big)}{\big(1 - p_D(\theta_i)\big) p_D(\theta_i)}\right]. \tag{8}$$

The first stage in the denominator, $P[C_{1i} > C_{0i}]$, is constructed using

$$P[C_{1i} > C_{0i}] = E\left[\frac{C_i\big(D_i - p_D(\theta_i)\big)}{\big(1 - p_D(\theta_i)\big) p_D(\theta_i)}\right]. \tag{9}$$

The semiparametric IV estimator used here is the sample analog of the right-hand side of (8) divided by the sample analog of (9). The semiparametric estimator plugs score estimates directly into (8) and (9), while our 2SLS strategies use saturated models to control for the frequency and formula scores. It's worth noting, however, that when the only covariates are score controls, and the estimated score is an empirical relative frequency like our frequency score, 2SLS models that control for a full set of score dummies produce estimated treatment effects identical to those generated by 2SLS with linear control for the estimated score. This is a consequence of the regression algebra detailed in the proof of Proposition 3, below.

4.2. *Effects of Charter Enrollment*

As can be seen in Table VI, 2SLS estimates of charter attendance effects are similar to the corresponding semiparametric estimates. Compare, for instance, the semiparametric estimates of effects on math and writing scores of 0.37 and 0.22 in column 1 with the 2SLS estimates of 0.35 and 0.18 in column 2. Both of these control for simulated scores. The standard errors for the semiparametric estimates using the simulated score are higher than those for 2SLS (semiparametric precision is estimated using a Bayesian bootstrap that randomly reweights observations; see, e.g., Shao and Tu (1995)). There are further substantial precision gains in 2SLS estimates using models that control for covariates beyond the score, reported in column 3. The similarity of 2SLS and semiparametric estimates and the relative simplicity and precision of 2SLS leads us to focus on

TABLE VI

CHARTER EFFECTS ESTIMATED USING ALTERNATIVE SCORE CONTROLS[a]

	Simulated Score Controls (Rounded to Hundredths)			DA Score Controls (With Covariates)		No Score Controls (With Covariates)	
	Semiparametric (1)	2SLS (No Covariates) (2)	2SLS (With Covariates) (3)	Frequency (Saturated) (4)	Formula (Saturated) (5)	2SLS (6)	OLS (7)
First stage	0.389***	0.415***	0.420***	0.443***	0.435***	0.561***	
	{0.053}	(0.024)	(0.024)	(0.024)	(0.024)	(0.016)	
Math	0.372***	0.351***	0.415***	0.417***	0.409***	0.231***	0.230***
	{0.133}	(0.108)	(0.052)	(0.050)	(0.051)	(0.030)	(0.010)
Reading	0.180	0.083	0.166***	0.174***	0.166***	0.066**	0.094***
	{0.162}	(0.108)	(0.053)	(0.050)	(0.052)	(0.029)	(0.010)
Writing	0.217	0.184*	0.274***	0.295***	0.315***	0.141***	0.171***
	{0.136}	(0.105)	(0.058)	(0.056)	(0.058)	(0.032)	(0.011)
N	2,229	2,308	2,308	2,099	2,058	2,947	8,528

[a]This table compares semiparametric, 2SLS, and OLS estimates of charter attendance effects on the 2013 and 2014 TCAP scores of Denver 4th–10th graders. For columns 2, 3, 4, 5, and 6, the instrument is an any-charter offer dummy. Models generating columns 2, 3, 4, and 5 include propensity-score controls, while models for 6 and 7 do not. All 2SLS and OLS estimates include controls for grade tested, gender, origin school charter status, race, gifted status, bilingual status, subsidized school lunch eligibility, special education, limited English proficiency status, baseline test scores, and year of application. Columns 1, 2, and 3 use the simulated score rounded to 0.01. The semiparametric estimator is described in Section 4.2. The semiparametric model excludes applicants with a rounded simulated score larger than 0.975 or smaller than 0.025. Standard errors, reported in parentheses, are robust for 2SLS and from a Bayesian bootstrap for semiparametric estimates. *significant at 10%; **significant at 5%; ***significant at 1%.

2SLS estimates with covariates in what follows.[19] It is also worth noting that 2SLS can be interpreted as a "doubly robust" variation on the semiparametric IV strategy; see, for example, Robins (2000) and Okui, Small, Tan, and Robins (2012).[20]

A DA-generated charter offer boosts charter school attendance rates by about 0.4. These first-stage estimates, shown in the first row of Table VI, are computed by estimating equation (6) or equation (9). The first stage of 0.4 reflects the fact that many charter applicants who are not offered a seat in the SchoolChoice first round ultimately find their way into a charter school by applying to schools directly in the second round (specifically, 44% of the charter applicants analyzed in Table VI are always-takers who enroll in charters even without a first-round charter offer, while fewer than 20% of the analysis sample are never-takers who decline charter offers). First-stage estimates of around 0.56 computed without score controls, shown in column 6 of the table, are clearly biased upwards.

2SLS estimates of charter attendance effects on test scores, reported below the first-stage estimates in Table VI, show remarkably large gains in math, with smaller effects on reading. The math gains reported here are similar to those found for charter students in Boston (see, e.g., Abdulkadiroğlu et al. (2011)). Previous lottery-based studies of charter schools likewise report much larger gains in math than in reading. Here, however, we also see large and statistically significant gains in writing scores.

[19]Controls include baseline test scores and the covariates described earlier. Estimates are for test scores on exams taken in grades 4–10. The sample used for IV estimation is limited to charter applicants with the relevant propensity score in the unit interval, in score cells with offer variation in the sample.

[20]2SLS also obviates the need for judgments regarding bootstrap methods or implementation. We found, for example, that a conventional nonparametric bootstrap for the semiparametric estimator requires trimming or tuning to eliminate the influence of occasional small first-stage estimates. Appendix A.7 discusses alternative modes of inference for the balance tests in Table V and the 2SLS estimates in Table VI.

Importantly the estimated charter attendance effects reported in Table VI are largely invariant to whether the propensity score is estimated by simulation or by a frequency or formula calculation that uses Theorem 1. Compare, for example, math impact estimates of 0.415, 0.417, and 0.415 using simulation-, frequency-, or formula-based score controls, all estimated with similar precision (these appear in columns 3–5). This alignment further validates the use of Theorem 1 to control for applicant type.

Estimates that omit propensity-score controls highlight the risk of selection bias in a naive 2SLS empirical strategy. This selection bias is documented in column 6 of Table VI, which shows that 2SLS estimates of math and writing effects constructed using DA offer instruments while omitting propensity-score controls are too small by about half. A corresponding set of OLS estimates without propensity-score controls, reported in column 7 of the table, also tends to underestimate the gains from charter attendance.[21]

4.3. *Unbundling Hetereogeneity*

Many evaluations of charter schools emphasize charter sector heterogeneity, estimating separate charter attendance effects for different sorts of schools (see, e.g., Angrist, Pathak, and Walters (2013)). Since just over half of the schools listed in Table VI belong to one of three Denver Charter Management Organizations (CMOs), we split the charter sector by CMO affiliation. Charters run by CMOs implement common practices across school sites, and CMOs similar to those operating in Denver have produced especially large achievement gains (Teh, McCullough, and Gill (2010), Gleason, Tuttle, Gill, Nichols-Barrer, and Teh (2014), Angrist, Dynarski, Kane, Pathak, and Walters (2012)).

The 2SLS estimates in Table VI also contrast charter outcomes with potential outcomes generated by attendance at a mix of traditional public schools and schools from other non-charter sectors. We would like to unbundle this mix so as to produce something closer to a pure sector-to-sector comparison. Allowance for more than one treatment channel also addresses concerns about changes in counterfactual outcomes that might cause violations of the exclusion restriction.

The first step in our effort to unbundle school sector effects is to describe the distribution of charter and non-charter school choices for applicants who were and were not offered a charter seat in the SchoolChoice match. We then identify the distribution of school sectors for the group of charter-lottery compliers. Finally, we use the DA mechanism to jointly estimate causal effects of attendance at schools in different sectors, thereby making the groups of schools compared in our 2SLS strategy more homogeneous.

Important DPS sectors besides charters are traditional public schools, innovation schools, magnet schools, and alternative schools. Innovation and magnet schools are managed by DPS. Innovation schools design and implement innovative practices meant to improve student outcomes. Innovation schools operate under an innovation plan that waives some provisions of the relevant collective bargaining agreements (for more background on these schools, see Connors, Moldow, Challender, and Walters (2013)).[22] Magnet schools serve students with particular styles of learning. Alternative schools serve older students who have struggled in a traditional school environment. Smaller school

[21]The OLS estimation sample includes most charter applicants, ignoring the score- and cell-variation restrictions relevant for 2SLS.

[22]Innovation waivers are subject to approval by the Denver Classroom Teachers Association (which organizes Denver public school teachers' bargaining unit), and they allow, for example, increased instruction time. DPS innovation schools appear to have much in common with Boston's pilot schools, a model examined in Abdulkadiroğlu et al. (2011).

TABLE VII

ENROLLMENT DESTINIES FOR CHARTER APPLICANTS[a]

	All Charter Applicants		Charter Applicants With DA Score (Frequency) in (0, 1)			
			All Applicants		Compliers	
	No Charter Offer (1)	Charter Offer (2)	Non-Offered Mean (3)	First Stage + Col 3 (4)	No Charter Offer (5)	Charter Offer (6)
			A. Decomposing Y_1			
Any charter	0.129	0.884	0.310	0.754		1.000
CMO Charter	0.095	0.764	0.248	0.673		0.958
Non-CMO Charter	0.034	0.120	0.061	0.080		0.042
			B. Decomposing Y_0			
Traditional public	0.380	0.066	0.244	0.065	0.405	
Innovation school	0.283	0.023	0.289	0.109	0.405	
Magnet school	0.191	0.018	0.126	0.056	0.157	
Alternative school	0.009	0.005	0.021	0.014	0.015	
Contract school	0.007	0.002	0.009	0.000	0.022	
N	4,917	3,805	962	2,098		

[a]This table describes school enrollment outcomes for charter applicants. Columns 1–2 show enrollment by sector for all applicants without and with a charter offer. The remaining columns look only at those with a DA (frequency) score strictly between zero and one. Column 4 adds the non-offered mean in column 3 to the first-stage estimate of the effect of charter offers on charter enrollment. School sectors are classified by grade. CMO charters are listed in Table II. Innovation schools design and implement innovative practices to improve applicant outcomes. Magnet schools serve applicants with particular styles of learning. Alternative schools serve applicants struggling with academics, behavior, attendance, or other factors that may prevent them from succeeding in a traditional school environment. The table omits a charter school that closed in May 2013. Complier means in columns 5 and 6 were estimated using the 2SLS procedures described by Abadie (2002), with the same propensity score and controls as were used to construct the estimates in Table VI.

sectors include a single charter middle school outside the centralized DPS assignment process (now closed) and a private school contracted to serve DPS students.

The distribution of enrollment sectors for applicants who do and do not receive a charter offer is described in the first two columns of Table VII. These columns show a charter enrollment rate of 88% in the group offered a charter seat, with roughly 76% enrolling in a CMO charter. Perhaps surprisingly, only around 38% of those not offered a charter seat enroll in a traditional public school, with the rest of the non-offered group distributed over a variety of sectors. Innovation schools are the leading non-charter alternative to traditional public schools.

The sector distribution for non-offered applicants with nontrivial charter risk (meaning a charter-offer score strictly between zero and one) appears in column 3 of Table VII, alongside the sum of the non-offered mean and a charter-offer treatment effect on enrollment in each sector in column 4. These first-stage estimates, computed by putting indicators $1(S_i = j)$ for when applicant i enrolls in sector j on the left-hand side of equation (6), control for the DA propensity score and therefore have a causal interpretation. The number of applicants not offered a seat who end up in a charter school is higher for those with nontrivial charter-offer risk than in the full applicant sample, as can be seen by comparing columns 3 and 1. The charter enrollment first stage that is implicit in the column 4-versus-3 comparison matches the first stage in column 4 of Table VI. The distinction between CMO and non-CMO charters reveals that the charter-offer instrument mostly moves applicants into CMO charters. First stages for other sectors show that charter offers reduce innovation and traditional public school enrollment.

The 2SLS estimates reported in Table VI capture causal effect for charter lottery compliers. We describe the distribution of school sectors for compliers by defining *potential* school sectors, S_{1i} and S_{0i}, indexed against charter offers, D_i. Potential and observed school sectors are related by

$$S_i = S_{0i} + (S_{1i} - S_{0i})D_i.$$

In the population of charter-offer compliers, $S_{1i} = charter$ for all i: by definition, charter-offer compliers attend a charter school when the DPS assignment offers them the opportunity to do so. The top panel of Table VII reports the breakdown of charter sector for charter-offer compliers, showing (in the last column) that 96% of offered compliers attend CMO charters. We are also interested in $E[1(S_{0i} = k)|C_{1i} > C_{0i}]$ for sectors indexed by k, that is, the sector type distribution for charter-offer compliers in the scenario where they are not offered a charter seat. We refer to this distribution as describing counterfactual *enrollment destinies* for compliers.

Enrollment destinies are marginal potential outcome distributions for compliers. As shown by Abadie (2002), these are identified by a simple 2SLS estimand. The details of our implementation of this identification strategy follow those in Angrist, Cohodes, Dynarski, Pathak, and Walters (2016), with the modification that instead of estimating marginal potential outcome densities for a continuous variable, the outcomes of interest here are Bernoulli.[23]

Column 5 of Table VII reveals that only about 41% of charter lottery compliers are destined to end up in a traditional public school if they are not offered a charter seat. Moreover, an innovation school enrollment destiny is just as likely as a traditional public school. By contrast, the likelihood of an enrollment destiny outside the charter, traditional, and innovation sectors is much smaller.

4.4. *Additional School Sector Effects*

The contribution of Denver's CMO charters to our first stage and the outsize role of innovation schools in counterfactual destinies motivate an empirical strategy that distinguishes the effects of CMO and non-CMO charters and allows for separate innovation school treatment effects. By pulling innovation schools out of the non-charter counterfactual, we capture charter treatment effects driven mainly by the contrast between charter and traditional public schools. Models with a more homogeneous non-charter counterfactual also mitigate bias that might arise from violations of the exclusion restriction (discussed in Section 4.1). The innovation treatment effect is also of interest in its own right.

To facilitate the causal analysis of multiple school sectors, we write the potential outcome for sector k as Y_{ki}, representing the latent outcome when $S_i = k$, for school sectors coded by $k \in \{0, 1, \dots, K\}$. This leads to $K - 1$ heterogeneous causal effects: $Y_{Ki} - Y_{0i}, \dots, Y_{2i} - Y_{0i}$, and $Y_{1i} - Y_{0i}$. Identification of multiple LATEs with unrestricted heterogeneity is challenging and raises issues that go beyond the scope of this paper.[24] We therefore

[23]Briefly, our procedure puts $(1 - C_i)1(S_i = k)$ on the left-hand side of a version of equation (7) with endogenous variable $1 - C_i$. The coefficient on this endogenous variable is an estimate of $E[1(S_{0i} = k)|C_{1i} > C_{0i}, X_i]$. The covariates and sample used here are the same as those used to construct the 2SLS impact estimates reported in column 4 of Table VI.

[24]See Behaghel, Crépon, and Gurgand (2013), Blackwell (2017), and Hull (2016) for recent progress on multi-treatment IV models with heterogeneous effects.

assume constant effects for each sector:

$$Y_{ki} - Y_{0i} = \beta_k. \tag{10}$$

With constant effects, a multi-sector identification strategy can be motivated by the simple conditional independence assumption,

$$Y_{0i} \perp\!\!\!\perp Z_i | \theta_i, \tag{11}$$

where $Z_i = k \in \{0, 1, \dots, K\}$ is a categorical variable that records DA-generated offers in each sector.

The instruments for the multi-sector model are the full set of indicators for offers in each sector, indexed by ℓ: $\{D_i^\ell = 1[Z_i = \ell]; \ell = 1, \dots, K\}$. These dummy instruments are used in a 2SLS procedure with endogenous variables $C_i^k = 1[S_i = k]$ indicating sector k enrollment. As in Imbens's (2000) extension of the propensity-score method to multiple treatments, propensity-score conditioning to make charter offer instruments ignorable is justified by the fact that (11) implies

$$Y_{0i} \perp\!\!\!\perp Z_i | p^1(\theta), p^2(\theta), \dots, p^K(\theta), \tag{12}$$

where $p^l(\theta) = E[D_i^\ell | \theta]$ for $\ell = 1, \dots, K$.[25]

The 2SLS setup in this case consists of the second- and first-stage equations,

$$Y_i = \sum_{\ell=1}^{K} \sum_{x} \alpha_\ell(x) d_i^\ell(x) + \sum_{k=1}^{K} \beta_k C_i^k + \varepsilon_i, \tag{13}$$

$$C_i^k = \sum_{\ell=1}^{K} \sum_{x} \gamma_{k\ell}(x) d_i^\ell(x) + \sum_{\ell=1}^{K} \delta_{k\ell} D_i^\ell + \nu_{ik} \quad \text{for } k = 1, \dots, K, \tag{14}$$

where the dummy control variables, $d_i^\ell(x)$, saturate estimates of the propensity scores for each offer dummy, D_i^ℓ, with corresponding score effects denoted by the γ's and α's in the first- and second-stage models. Note that there are as many first stages as there are sectors (minus one) and that each offer dummy appears in each first-stage equation, with an associated set of score controls for that offer. The sample used for this analysis contains the union of the sets of charter and innovation school applicants, including all applicants with assignment risk in any sector in the model.

The conditional independence relation (12) suggests we should control for conditional probabilities of assignment for all treatment levels jointly. Joint score control replaces the additive score controls in equations (13) and (14) with saturated score controls of the form

$$d_i^K(x^1, \dots, x^K) = 1[\hat{p}^1(\theta_i) = x^1, \hat{p}^2(\theta_i) = x^2, \dots, \hat{p}^K(\theta_i) = x^K],$$

where hats denote score estimates and the indices, $(x^1, x^2, \dots, x^K)$ run independently over all values in the support for each score. This model generates far more score fixed effects than appear in equation (13). Fortunately, however, the algebra of 2SLS obviates the need for joint score control; additive control as in (13) is enough, a conclusion that follows from the next proposition.

[25] Imbens (2000) and Yang et al. (2016) call the score for indicators of values of a multi-level treatment the *generalized propensity score*.

PROPOSITION 3: *Let $q(\theta_i)$ be an arbitrary function of type. Consider the 2SLS estimator of the vector of β_k^q for $k = 1, \dots, K$ constructed by estimating equations (13) and (14) with additional controls $q(\theta_i)$ in each equation. Then,*

$$\beta_k^q = \beta_k.$$

PROOF: Note first that 2SLS estimates of (13)–(14) can be obtained by regressing first-stage fitted values on the controls in these two equations (the full set of score dummies and any other covariates in the model) and then using the residuals from this regression as instruments for a model that omits the score dummies and additional covariates (see, e.g., Section 4.1 in Angrist and Pischke (2009)). Equivalently, since first-stage fitted values are a linear combination of offer dummies, we can regress each of the offer dummies on these same controls and use the resulting residuals as instruments.

Now consider the set of auxiliary regressions that produce these residualized instruments: they have D_i^k on the left-hand side, with a saturating set of dummies for $p^k(\theta_i)$ and a vector of additional controls, $q(\theta_i)$, on the right. By the law of iterated expectations, the conditional expectation function (CEF) associated with this auxiliary regression is

$$E\big[D_i^k | p^l(\theta_i), q(\theta_i)\big] = E\big\{E\big[D_i^k | \theta_i\big] \big| p^k(\theta_i), q(\theta_i)\big\} = p^k(\theta_i).$$

In other words, having conditioned on $p^k(\theta_i)$, other functions of θ_i drop out of the CEF (this is a restatement of the propensity-score theorem). Moreover, because our 2SLS procedure includes a saturating set of dummies for the own-score, $p^k(\theta_i)$, the CEF $E[D_i^k | p^k(\theta_i), q(\theta_i)]$ is linear in regressors, so it and the associated auxiliary regression function coincide. This completes the proof. *Q.E.D.*

The argument for additive score control is completed by observing that both the additive and joint models implicitly control for a full set of own-score dummies and additional functions of θ_i. For both models, therefore, the auxiliary regression that generates the instruments used by 2SLS has residual $D_i^k - p^k(\theta_i)$.[26]

Multi-Sector Estimates

As a benchmark, columns 1–3 of Table VIII report three sets of single-sector 2SLS estimates, comparing CMO charter-only, non-CMO charter-only, and innovation-only estimates computed using DA (frequency) score controls. Each sample is limited to applicants to the relevant sector.[27] The CMO charter first stage (the effect of a CMO charter offer on CMO charter enrollment) is around 0.49. The non-CMO charter first stage is 0.33. It is worth noting that these two columns use different instruments, one indicating CMO charter offers and one indicating non-CMO offers. This fact makes it possible to identify distinct within-charter sector effects. The innovation school first stage (the effect of an innovation school offer on innovation school enrollment) is around 0.37. Not

[26]As noted at the end of Section 4.1, a further implication of Proposition 3 is that in models with no covariates other than score controls, and propensity score estimates using empirical offer rates, we can substitute linear score control for saturating dummy controls. In this case, the fitted values and hence the residuals for the auxiliary partialing-out regression generated by both linear and saturated score control are the same.

[27]Supplemental Material Table B.III lists innovation schools and describes the random assignment pattern at these schools along the lines of Table II for charter schools. Covariate balance and differential attrition results for innovation schools are reported in Supplemental Material Table B.IV.

TABLE VIII

SCHOOL SECTOR EFFECTS[a]

	Single Sector Models			Multi-Sector Models	
	CMO (1)	Non-CMO (2)	Innovation (3)	Additive Score Controls (4)	Joint Score Controls (5)
CMO First Stage	0.491***	0.333***	0.368***		
	(0.024)	(0.056)	(0.034)		
			A. Math		
CMO Charter	0.441***			0.472***	0.442***
	(0.047)			(0.059)	(0.065)
Non-CMO Charter		−0.090		0.057	−0.035
		(0.166)		(0.164)	(0.174)
Innovation school			−0.137	0.168	0.132
			(0.091)	(0.102)	(0.108)
N	1,940	394	923	2,663	2,326
			B. Reading		
CMO Charter	0.214***			0.152**	0.127*
	(0.047)			(0.066)	(0.072)
Non-CMO Charter		−0.248		−0.257	−0.128
		(0.188)		(0.191)	(0.210)
Innovation school			−0.100	−0.038	−0.091
			(0.101)	(0.117)	(0.126)
N	1,944	394	924	2,668	2,332
			C. Writing		
CMO Charter	0.299***			0.354***	0.343***
	(0.052)			(0.070)	(0.077)
Non-CMO Charter		0.016		0.087	0.072
		(0.171)		(0.175)	(0.198)
Innovation school			−0.075	0.147	0.094
			(0.102)	(0.120)	(0.126)
N	1,952	393	928	2,676	2,338

[a]This table reports 2SLS estimates of CMO, non-CMO charter, and innovation attendance effects for applicants to schools in these sectors. CMO charters are listed in Table II. Column 1 reports attendance effects of CMO charters, estimated in models using a CMO offer instrument. Column 2 reports attendance effects of non-CMO charters, estimated in models using a non-CMO offer instrument and non-CMO specific saturated score controls constructed like those used for charter applicants. Column 3 reports attendance effects of innovation schools, estimated in models using an innovation school offer instrument and innovation-specific saturated score controls constructed like those used for charter applicants. Column 4 reports coefficients from a three-endogenous-variable/three-instrument 2SLS model for the attendance effects of CMOs, non-CMO charters, and innovation schools, conditioning additively on CMO, non-CMO, and innovation saturated score controls. Column 5 shows results from joint-effect models that add interactions between the three scores to the specification that generates column 4. The first stage estimates reported above Panel A are for math scores. Robust standard errors are reported in parentheses. *significant at 10%; **significant at 5% ; ***significant at 1%.

surprisingly in view of the substantially reduced number of applicants with nontrivial non-CMO and innovation offer risk (401 and 942) and the corresponding smaller first stages, both the non-CMO and innovation attendance estimates are less precise than the CMO effects. Even so, it is noteworthy that in all subjects the estimated effects of non-CMO and innovation school attendance are negative or close to zero.

2SLS estimates of equation (13) appear in columns 4 and 5 of Table VIII. The large CMO charter school effects reported in column 1 remain substantial in this specification, but (insignificant) negative innovation school estimates for math flip to positive when estimated using a model that also isolates the two charter treatment effects. The negative innovation school effects on reading seen in column 3 also shrink in the three-endogenous-variables models. Most interestingly, perhaps, the large significant positive CMO charter school effect on reading in column 1 is smaller and only marginally significant in columns 4 and 5. While charter applicants' reading performance exceeds what we can expect to see were these applicants to enroll in a mix of traditional and (low-performing) innovation schools, the reading gap between CMO charters and traditional public schools appears to be a little smaller.

As the theoretical discussion above leads us to expect, the results of estimation with joint score controls, reported in column 5 of Table VIII, differ little from the estimates constructed using additive score controls. Overall, it seems fair to say that the findings showing substantial charter effectiveness in Table VI are driven entirely by CMO charters, and that these findings hold up when effects are estimated using a procedure that removes the innovation sector from the charter enrollment counterfactual.

4.5. *Alternative IV Strategies*

Previous research using centralized assignment to eliminate the selection bias arising from the dependence of assignments on preferences and priorities focuses either on offers of seats at applicants' first-choice schools, or uses instrumental variables (IVs) indicating whether an applicant's lottery number falls below the highest number offered a seat at all schools he has ranked (we call this a qualification instrument). The first-choice strategy conditions on the identity of the school ranked first, while qualification instruments condition on the set of schools ranked. These IV strategies are likely to produce estimates of school attendance free of omitted variables bias. At the same time, both first-choice and qualification instruments discard much of the variation induced by centralized assignment.

We are interested in comparing 2SLS estimates of charter effects constructed using offer dummies as instruments while controlling for the DA propensity score with suitably-controlled estimates using first-choice and qualification instruments. We expect DA-offer instruments to yield a precision gain while also increasing the number of schools represented in the estimation sample relative to these two previously-employed IV strategies.[28]

Let $X(\theta_i)$ be a variable identifying the charter school that applicant i ranks first, along with his priority status at this school, defined for applicants whose first choice is indeed a charter school. $X(\theta_i)$ ignores other schools that might have been ranked. The first-choice strategy is implemented by the following 2SLS setup:

$$Y_i = \sum_x \alpha(x) d_i(x) + \beta C_i + \varepsilon_i,$$

$$C_i = \sum_x \gamma(x) d_i(x) + \delta D_i^f + \nu_i,$$

[28]Studies using first-choice instruments to evaluate schools in districts with centralized assignment include Abdulkadiroğlu, Hu, and Pathak (2013), Deming (2011), Deming et al. (2014), and Hastings, Kane, and Staiger (2009). First-choice instruments have also been used with decentralized assignment mechanisms (Abdulkadiroğlu et al. (2011), Cullen, Jacob, and Levitt (2006), Dobbie and Fryer (2011), and Hoxby, Murarka, and Kang (2009)). Dobbie and Fryer (2014), Lucas and Mbiti (2014), and Pop-Eleches and Urquiola (2013) use qualification instruments.

where the $d_i(x)$'s are dummies indicating values of $X(\theta_i)$, indexed by x, and $\gamma(x)$ and $\alpha(x)$ are the associated "risk set effects" in the first and second stages. The first-choice instrument, D_i^f, is a dummy variable indicating i's qualification at his or her first-choice school. In other words,

$$D_i^f = 1[\pi_{is} \leq c_s \text{ for charter } s \text{ that } i \text{ has ranked first}].$$

First-choice qualification is the same as first-choice offer since, under DA, applicants who rank a first are offered a seat there if and only if they qualify at a.[29]

The qualification strategy expands the sample to include all charter applicants, with the risk sets for qualification instruments identifying the set of all charter schools that i ranks, along with his or her priority status at each of these schools (again, these risk sets are denoted $X(\theta_i)$).[30] The qualification instrument, D_i^q, indicates qualification at *any* charter he or she has ranked. In other words,

$$D_i^q = 1[\pi_{is} \leq c_s \text{ for at least one charter } s \text{ that } i \text{ has ranked}].$$

In large markets, the instruments D_i^f and D_i^q are independent of type conditional on $X(\theta_i)$; see Appendix A.8 for details.

A primary source of inefficiency in the first-choice and qualification strategies is apparent in Panel A of Table IX. This panel reports two sorts of first-stage estimates for each instrument: the first of these regresses a dummy indicating any charter *offer*—that is, our DA charter offer instrument, D_i—on each of the three instruments under consideration. A regression of D_i on itself necessarily produces a coefficient of 1. By contrast, a first-choice offer boosts the probability of any charter offer by only around 0.73 in the sample of those who have ranked a charter first. This reflects the fact that, while anyone receiving a first-choice charter offer has surely been offered a charter seat, roughly 27% of the sample ranking a charter first is offered a charter seat at schools other than their first choice. The relationship between D_i^q and charter offers is even weaker, at around 0.46. This reflects the fact that, for schools below the one ranked first, charter qualification is insufficient for a charter offer.

The diminished impact of the two alternative instruments on charter offers translates into a weakened first stage for charter *enrollment*. The best-case scenario, using all DA-generated offers (i.e., D_i) as a source of quasi-experimental variation, produces a first stage of around 0.44. But first-choice offers boost charter enrollment by only 0.35, while qualification at any charter yields a charter enrollment gain of only 0.23. As always, the size of the first stage is a primary determinant of the precision of an IV estimate.[31]

At 0.050, the standard error of the DA-offer estimate for effects on math scores is lower than the standard error of 0.064 yielded by a first-choice strategy and well below the standard error of 0.092 generated by qualification instruments. The precision loss here is similar to the decline in the intermediate first stages recorded in the first row of the table (compare 0.73 with 0.050/0.064 = 0.78 and 0.46 with 0.050/0.092 = 0.54).

[29]DPS divides each school into buckets, as explained in Section 2.3. Our first-choice risk set therefore identifies applicants to all buckets at the first-choice school.

[30]For example, an applicant who ranks A and B with marginal priority only at A is distinguished from an applicant who ranks A and B with marginal priority only at B.

[31]The sample used to construct the estimates in columns 1–3 of Table IX is limited to those who have variation in the instrument at hand conditional on the relevant risk set.

TABLE IX

OTHER IV STRATEGIES[a]

	Instrument			Equivalent Sample Increase	
	Offer Instrument With DA Score Controls (1)	First Choice Charter Offer With Risk Set Controls (2)	Qualification Instrument With Risk Set Controls (3)	Col 2 vs. Col 1 (4)	Col 3 vs. Col 1 (5)
	A. First stage estimates				
First stage for charter offers	1.000	0.731***	0.457***		
		(0.017)	(0.018)		
First stage for charter enrollment	0.443***	0.347***	0.227***		
	(0.024)	(0.022)	(0.021)		
	B. 2SLS estimates				
Math	0.417***	0.515***	0.379***	1.64	3.46
	(0.050)	(0.064)	(0.092)		
Reading	0.174***	0.258***	0.198**	1.56	2.96
	(0.050)	(0.062)	(0.086)		
Writing	0.295***	0.316***	0.344***	1.61	2.81
	(0.056)	(0.071)	(0.094)		
N	2,099	2,222	3,502		

[a]This table compares alternative 2SLS estimates of charter attendance effects using the same sample and control variables used to construct the estimates in Table VI. Column 1 repeats the estimates from column 4 in Table VI. The row labeled "First stage for charter offers" reports the coefficient from a regression of an any-charter offer dummy (the instrument used in column 1) on other instruments, conditioning on the controls used in the corresponding first-stage estimates for charter enrollment. Column 2 reports 2SLS estimates estimated using a first-choice charter offer instrument. Column 3 reports charter attendance effects estimated using an any-charter qualification instrument. These alternative IV models control for risk sets making the first-choice and qualification instruments conditionally random; see Section 4.5 for details. Columns 4 and 5 report the sample size increase needed to achieve a precision gain equivalent to the gain from using the any-charter offer instrument. Robust standard errors are reported in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Columns 4 and 5 show the sample size increase needed to undo the damage done by a smaller first stage for each alternative instrument.[32]

The precision loss from alternative IV strategies is much worse in multi-sector models. For example, when estimating sector effects jointly, with additive score controls (as in column 4 of Table VIII), the innovation school math effect estimated using a first-choice offer instrument has a standard error of 0.737, while the corresponding standard error using a qualification instrument is 1.879. These can be compared with the standard error of 0.097 using DA offers and the DA score. It seems fair to say that multi-sector estimators with these other IV strategies are uninformative.

First-choice analyses lose schools because many lotteries fail to randomize first-choice applicants (as seen in Table II). It is therefore worth noting that the first-choice estimate of effects on math and reading scores are noticeably larger than the estimates generated using DA-offer and qualification instruments (compare the estimate of 0.42 using DA offers with estimates of 0.52 and 0.38 using first-choice and qualification instruments).

[32]This pattern is consistent with theoretical econometric results in Newey (1990) and Hong and Nekipelov (2010), which show that the semiparametric efficiency bound for LATE-type estimates is proportional to the number of compliers, that is, to the size of the first stage. Hong and Nekipelov (2010) also show that the efficient estimator of marginal-over-covariates LATE ($E[Y_1 - Y_0|D_1 > D_0] = E\{E[Y_1 - Y_0|D_1 > D_0, X]|D_1 > D_0\}$) is a weighted average of empirical covariate-specific Wald estimators, with weights proportional to the corresponding covariate-specific first stage. See also Frölich (2007).

This finding may reflect an advantage for those awarded a seat at their first-choice school (Hastings, Kane, and Staiger (2009), Deming (2011), Deming et al. (2014) report a general "first-choice advantage" in analyses of school attendance effects). By contrast, the DA-offer instrument yields an estimand that is more representative of the full complement of charter schools in the SchoolChoice match.

Motivated by the possibility of a "first-choice" advantage, we conclude our empirical analysis with estimates from models allowing separate effects of first-choice charters and other-choice charters. As for the estimates in Table VIII, the instruments for enrollment in more narrowly defined sectors are dummies indicating offers of seats in these sectors, controlling for the corresponding narrow-sector propensity score. For first-choice charters, for example, the instrument indicates offers at a charter ranked first and the propensity score is the probability of receiving an offer at a charter ranked first.

Consistent with the hypothesis of "first-choice" advantage, the estimates in Table X suggest first-choice charters generate achievement effects beyond those of charters ranked lower. Compare, for example, the 0.40 math estimate for first-choice charters with the 0.24 estimate for other-choice charters, both of which are reported in column 1. Likewise, for reading, the estimates in column 3 show a gain around 0.14 at first-choice charters, with an estimated zero reading effect elsewhere. As can be seen in column 5, estimates of effects on writing are similar at both types of schools.

CMO charters drive positive overall charter effects. It is natural, therefore, to ask whether the first-choice advantage is visible within CMO and non-CMO sectors. The estimates reported in even-numbered columns in Table X are from a model with four endogenous variables, distinguishing first-choice and other charters by their CMO status. The evidence of CMO charter quality remains impressive in this parameterization. We see, for example (in column 2), that among charters ranked first, CMO charters boost math

TABLE X

DPS CHARTER SCHOOL ATTENDANCE EFFECTS BY FIRST CHOICE AND CMO STATUS[a]

	Math		Reading		Writing	
	(1)	(2)	(3)	(4)	(5)	(6)
First-choice charters	0.403***		0.141***		0.291***	
	(0.049)		(0.049)		(0.056)	
First-choice charters, CMO		0.428***		0.166***		0.307***
		(0.051)		(0.052)		(0.060)
First-choice charters, non-CMO		0.015		−0.270		0.048
		(0.173)		(0.235)		(0.220)
Other-choice charters	0.239***		−0.025		0.289***	
	(0.076)		(0.078)		(0.086)	
Other-choice charters, CMO		0.294***		0.038		0.296***
		(0.073)		(0.071)		(0.082)
Other-choice charters, non-CMO		0.033		−0.253		0.250
		(0.309)		(0.355)		(0.358)
N	2,522	2,519	2,522	2,519	2,522	2,519

[a]This table reports 2SLS estimates for a two-endogenous/two-instrument model using saturated frequency score controls. The two endogenous variables indicate attendance at first-choice charters and other-choice charters. Estimates are also presented for a four-endogenous/four-instrument model. The four endogenous variables indicate attendance at a first-choice CMO, first-choice non-CMO, other-choice CMO, and other-choice non-CMO. The first-choice charter instrument is a dummy indicating an offer from a first-choice charter. The other-choice charter instrument is a dummy indicating an offer from a charter ranked below the applicant's first choice. The four first-choice-by-CMO instruments are defined similarly. Robust standard errors are reported in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

scores by 0.43, while non-CMO charters ranked first have essentially no effect. A similar contrast in favor of CMOs appears for other subjects as well, though it should be noted that the non-CMO estimates here are imprecise.

5. SUMMARY AND DIRECTIONS FOR FURTHER WORK

We have shown how to analyze the stratified randomized trial induced by any centralized assignment mechanism satisfying the equal treatment of equals property. DA with lottery tie-breaking is the leading mechanism in the ETE class. Our main theoretical result is an analytical formula for the DA propensity score, derived using a large-market approximation. This approximation works well in our DPS application in the sense of producing the covariate balance promised by the propensity-score theorem.

The theoretical results developed here extend to other widely-used matching schemes, including immediate acceptance, as well as to matches using multiple tie-breakers. The DA propensity score also reveals the nature of the experimental design embedded in DA, as well as suggesting modifications (such as to priorities) that might boost the research value of school assignment or other matching schemes. Finally, as a theoretical matter, the DA score provides a natural data driven smoother for the finite-market propensity score. In ongoing work, we are extending the framework in this paper to cover centralized assignment schemes using top trading cycles.

A score-based analysis of data from Denver's unified school match reveals substantial gains from attendance at one of Denver's many charter schools. The resulting charter effects are similar to those computed using single-school lottery strategies for Boston's charters reported in Abdulkadiroğlu et al. (2011) and to estimates computed using charter takeovers in New Orleans (Abdulkadiroğlu, Angrist, Hull, and Pathak (2016)). At the same time, as with previously reported results for Boston Pilot schools, Denver's Innovation school model does not appear to boost achievement. As always, econometric estimates need not predict the effects of policy changes. But the track record for charter lottery estimates is encouraging. In addition to the fact that lottery estimates have been replicated in many large urban districts, Cohodes, Setren, and Walters (2016) showed that a recent wave of Boston charter expansions, a policy innovation prompted by a legislative change, produced achievement gains very much in line with earlier lottery estimates.

Our analysis focuses on defining and estimating the DA propensity score, giving less attention to the problem of how best to use the score for estimation. Still, simple 2SLS procedures seem to work well, and the resulting estimates of DPS charter effects differ little from those generated by semiparametric alternatives. Estimates using DA-offer instruments also generate noteworthy precision gains relative to qualification and first-choice instruments, mostly as a consequence of an increased first stage, though also (in the case of first choice) by exploiting randomization at a larger set of schools.

Our DPS analysis shows how centralized assignment schemes can be used to unbundle school heterogeneity. We see, for example, that CMO-affiliated charters are much stronger than others in DPS, and achievement gains are larger when applicants are offered seats at charters they rank first. In principle, the empirical strategy demonstrated here can be used to construct single-school value-added estimates, though the VAM agenda raises unique challenges. In a related paper, Angrist, Hull, Pathak, and Walters (2017) show how lottery estimates can be embedded in an empirical Bayes framework that identifies value added for schools that are undersubscribed or for which there is no randomization. A natural direction for future work is the combination of the strategy outlined here with the empirical Bayes VAM framework.

Finally, it is worth noting that matching schemes for selective exam schools (analyzed by Jackson (2010), Dobbie and Fryer (2014), Abdulkadiroğlu, Angrist, and Pathak (2014), Lucas and Mbiti (2014), Pop-Eleches and Urquiola (2013)) and the U.S. medical match use non-randomly-assigned tie-breakers rather than a lottery. These schemes embed regression discontinuity designs inside a market design rather than embedding a randomized trial. The question of how best to define and exploit the DA propensity score for markets that combine regression-discontinuity tie-breaking with market-design matchmaking is an important next step on the market-design-meets-research-design agenda. Abdulkadiroğlu et al. (2017) report initial results on this piece of our agenda.

APPENDIX A: THEORETICAL APPENDIX

A.1. *Equal Treatment of Equals*

This section describes a broad class of mechanisms satisfying ETE. Fix the set of applicants and suppose that each applicant is assigned L lottery numbers. For example, L is equal to 1 if every applicant is assigned a single random number as in a DA with single tie-breakers, and L is equal to the number of schools if every applicant is assigned a different lottery number at every school. Generalizing the notation of Section 3, let $\mathbf{r}_i = (r_{i1}, \dots, r_{iL})$ be the vector of applicant i's realized lottery numbers and $\mathbf{r} = (\mathbf{r}_i : i \in I)$. Assume that, for any $\ell \in \{1, \dots, L\}$, $r_{i\ell} = r_{j\ell}$ if and only if $i = j$.

Recall that a stochastic mechanism maps a school-choice problem into a distribution of possible assignments. Given lottery numbers $\mathbf{r}$, we can be more explicit about how a stochastic mechanism is constructed by defining a function ϕ which maps the set of applicants and their random numbers to an assignment. ϕ is the allocation produced for a particular lottery realization. Let $\phi_i(\mathbf{r})$ denote i's assignment when lottery numbers are given by $\mathbf{r}$ and define $\phi(\mathbf{r}) = (\phi_i(\mathbf{r}) : i \in I)$.

Given $\mathbf{r}$ and $i, j \in I$, let i and j swap lottery numbers and denote the resulting lottery vector by $(\mathbf{r}_{(i,j)}, \mathbf{r}_j, \mathbf{r}_i)$. We say that ϕ is *anonymous* if, for any i, j, and $\mathbf{r}$ such that $i \neq j$ and $\theta_i = \theta_j$, we have

$$
\begin{aligned}
&\phi_i(\mathbf{r}_{(i,j)}, \mathbf{r}_j, \mathbf{r}_i) = \phi_j(\mathbf{r}), \\
&\phi_j(\mathbf{r}_{(i,j)}, \mathbf{r}_j, \mathbf{r}_i) = \phi_i(\mathbf{r}), \quad \text{and} \\
&\phi_k(\mathbf{r}_{(i,j)}, \mathbf{r}_j, \mathbf{r}_i) = \phi_k(\mathbf{r}) \quad \text{for all } k \in I \setminus \{i, j\}.
\end{aligned}
$$

We construct a stochastic mechanism by drawing lottery numbers. Let h be a probability distribution over $\mathbf{r}$ with support R^h. Given (ϕ, h), we can construct the corresponding stochastic mechanism, which we denote ϕ^h. Let ϕ^h_{is} be the probability that i is assigned s, that is,

$$\phi^h_{is} = \sum_{\mathbf{r} \in R^h} h(\mathbf{r}) 1\big(\phi_i(\mathbf{r}) = s\big)$$

when the support of h is countable, and

$$\phi^h_{is} = \int_{R^h} h(\mathbf{r}) 1\big(\phi_i(\mathbf{r}) = s\big)\, dr$$

otherwise. We say that a random lottery h is *symmetric* if $h(\mathbf{r}) = h(\mathbf{r}_{(i,j)}, \mathbf{r}_j, \mathbf{r}_i)$ for any i, j, and $\mathbf{r}$ such that $i \neq j$ and $\theta_i = \theta_j$.

LEMMA 1: *If ϕ is anonymous and h is symmetric, then stochastic mechanism ϕ^h satisfies Equal Treatment of Equals, that is,*

$$\phi^h_{is} = \phi^h_{js}$$

for all s and i, j such that $\theta_i = \theta_j$.

PROOF: Assume that ϕ is anonymous and h is symmetric. Consider any i and j such that $i \neq j$, $\theta_i = \theta_j$.

The set of possible assignments is finite. So, by grouping together the sets of random numbers that yield the same assignment and redefining h, we can assume without of loss of generality that R^h has finite cardinality. Formally, let $M = \{\phi(\mathbf{r}) : \mathbf{r} \in R^h\}$ be the set of all assignments generated by (ϕ, h). Construct a new lottery g as follows: Let R^g denote the support of g. For each assignment $m \in M$, pick some $\mathbf{r} \in R^h$ such that $\phi(\mathbf{r}) = m$, include it in R^g, and set

$$g(\mathbf{r}) = \int_{R^h} h(\mathbf{z}) 1\big(\phi(\mathbf{z}) = m\big)\, dz.$$

Then, by construction,

$$\phi^g_{is} = \phi^h_{is}$$

for all s and i. R^g can be constructed in a way that g is symmetric. So we assume without loss of generality that R^h has finite cardinality.

Let $\{R, R^{ij}\}$ be a partition of R^h such that $r \in R$ if and only if $(\mathbf{r}_{(i,j)}, \mathbf{r}_j, \mathbf{r}_i) \in R^{ij}$. Such partition exists by the symmetry of h and finite cardinality of R^h. Then

$$\begin{aligned}
\phi^h_{is} &= \sum_{r \in R \cup R^{ij}} h(\mathbf{r}) 1\big(\phi_i(\mathbf{r}) = s\big) \\
&= \sum_{r \in R} h(\mathbf{r}) 1\big(\phi_i(\mathbf{r}) = s\big) + h(\mathbf{r}_{(i,j)}, \mathbf{r}_j, \mathbf{r}_i) 1\big(\phi_i(\mathbf{r}_{(i,j)}, \mathbf{r}_j, \mathbf{r}_i) = s\big) \\
&= \sum_{r \in R} h(\mathbf{r}_{(i,j)}, \mathbf{r}_j, \mathbf{r}_i) 1\big(\phi_j(\mathbf{r}_{(i,j)}, \mathbf{r}_j, \mathbf{r}_i) = s\big) + h(\mathbf{r}) 1\big(\phi_j(\mathbf{r}) = s\big) \\
&= \sum_{r \in R \cup R^{ij}} h(\mathbf{r}) 1\big(\phi_j(\mathbf{r}) = s\big) \\
&= \phi^h_{js},
\end{aligned}$$

where the first equality is the definition of ϕ^h_{is}, the second follows from the way the partition is constructed, the third follows from (i) $h(\mathbf{r}) = h(\mathbf{r}_{(i,j)}, \mathbf{r}_j, \mathbf{r}_i)$ by symmetry of h, and (ii) $\phi_i(\mathbf{r}) = s \Leftrightarrow \phi_j(\mathbf{r}_{(i,j)}, \mathbf{r}_j, \mathbf{r}_i) = s$ by ϕ being anonymous. The fourth equality follows from the way the partition is constructed. Finally, the fifth equality is by definition. This completes the proof. *Q.E.D.*

This result implies that the following mechanisms with a symmetric lottery satisfy ETE: DA, the immediate acceptance ("Boston") mechanism, random serial dictatorship, and TTC, since each is anonymous. To see why, consider any i and j with $\theta_i = \theta_j$. When i and j swap lottery numbers, they swap roles in the implementation of each mechanism as well; consequently, they swap assignments. Lemma 1 also allows us to conclude that versions of these mechanisms using school-specific tie-breaking satisfy ETE when tie-breaking lotteries are symmetric.

A.2. *Defining DA: Details*

Our general formulation defines the DA match as determined by cutoffs found in the limit of a sequence. Recall that these cutoffs evolve according to

$$c_s^{t+1} = \begin{cases} K+1 & \text{if } F\big(Q_s(\mathbf{c}^t)\big) < q_s, \\ \max\big\{x \in [0, K+1] \mid F\big(\{i \in Q_s(\mathbf{c}^t) \text{ such that } \pi_{is} \le x\}\big) \le q_s\big\} & \text{otherwise,} \end{cases}$$

where $Q_s(\mathbf{c}^t)$ is the demand for seats at school s for a given vector of cutoffs $\mathbf{c}^t$ and is defined as

$$Q_s(\mathbf{c}^t) = \{i \in I \mid \pi_{is} \le c_s^t \text{ and } s \succ_i \tilde{s} \text{ for all } \tilde{s} \in S \text{ such that } \pi_{i\tilde{s}} \le c_{\tilde{s}}^t\}. \tag{15}$$

The following result confirms that these limiting cutoffs exist, that is, that the sequence $\mathbf{c}^t$ converges.

LEMMA 2: *Consider an economy described by a distribution of applicants F and school capacities as defined in Section 3.1. Construct a sequence of cutoffs*, c_s^t, *for this economy as described above. Then*, $\lim_{t\to\infty} c_s^t$ *exists.*

PROOF: c_s^t is well-defined for all $t \ge 1$ and all $s \in S$ since it is either $K+1$ or the maximizer of a continuous function over a compact set. We will show by induction that $\{c_s^t\}$ is a decreasing sequence for all s.

For the base case, $c_s^2 \le c_s^1$ for all s since $c_s^1 = K+1$ and $c_s^2 \le K+1$ by construction. For the inductive step, suppose that $c_s^t \le c_s^{t-1}$ for all s and all $t = 1, \dots, T$. For each s, if $c_s^T = K+1$, then $c_s^{T+1} \le c_s^T$ since $c_s^t \le K+1$ for all t by construction. Otherwise, suppose to the contrary that $c_s^{T+1} > c_s^T$. Since $c_s^T < K+1$, $F(\{i \in Q_s(\mathbf{c}^{T-1}) \text{ such that } \pi_{is} \le c_s^T\}) = q_s$. Then,

$$\begin{aligned} F\big(\{i \in Q_s(\mathbf{c}^T) \text{ such that } \pi_{is} \le c_s^{T+1}\}\big) &= F\big(\{i \in Q_s(\mathbf{c}^T) \text{ such that } \pi_{is} \le c_s^T\}\big) \\ &\quad + F\big(\{i \in Q_s(\mathbf{c}^T) \text{ such that } c_s^T < \pi_{is} \le c_s^{T+1}\}\big) \\ &\ge F\big(\{i \in Q_s(\mathbf{c}^{T-1}) \text{ such that } \pi_{is} \le c_s^T\}\big) \\ &\quad + F\big(\{i \in Q_s(\mathbf{c}^T) \text{ such that } c_s^T < \pi_{is} \le c_s^{T+1}\}\big) \qquad (16) \\ &\ge q_s + F\big(\{i \in Q_s(\mathbf{c}^T) \text{ such that } c_s^T < \pi_{is} \le c_s^{T+1}\}\big) \qquad (17) \\ &> q_s. \qquad (18) \end{aligned}$$

Expression (16) follows because

$$\begin{aligned} &\{i \in Q_s(\mathbf{c}^T) \text{ such that } \pi_{is} \le c_s^T\} \\ &\quad = \{i \in I \mid \pi_{is} \le c_s^T \text{ and } s \succ_i \tilde{s} \text{ for all } \tilde{s} \in S \text{ such that } \pi_{i\tilde{s}} \le c_{\tilde{s}}^T\} \\ &\quad \supseteq \{i \in I \mid \pi_{is} \le c_s^T \text{ and } s \succ_i \tilde{s} \text{ for all } \tilde{s} \in S \text{ such that } \pi_{i\tilde{s}} \le c_{\tilde{s}}^{T-1}\} \quad \big(\text{by } c_{\tilde{s}}^T \le c_{\tilde{s}}^{T-1}\big) \\ &\quad = \{i \in Q_s(\mathbf{c}^{T-1}) \text{ such that } \pi_{is} \le c_s^T\}. \end{aligned}$$

Expression (17) follows by the inductive assumption and since $c_s^T < K+1$.

Expression (18) follows since if $F(\{i \in Q_s(\mathbf{c}^T) \text{ such that } c_s^T < \pi_{is} \leq c_s^{T+1}\}) = 0$, then

$$F\big(\{i \in Q_s(\mathbf{c}^{T-1}) \text{ such that } \pi_{is} \leq c_s^{T+1}\}\big) = F\big(\{i \in Q_s(\mathbf{c}^{T-1}) \text{ such that } \pi_{is} \leq c_s^{T}\}\big) \leq q_s,$$

while $c_s^{T+1} > c_s^T$, contradicting the definition of c_s^T.

Expression (18) contradicts the definition of c^{T+1} since the cutoff at step $T+1$ results in an allocation that exceeds the capacity of school s. This therefore establishes the inductive step that $c_s^{T+1} \leq c_s^T$.

To complete the proof of the proposition, observe that since $\{c_s^t\}$ is a decreasing sequence in the compact interval $[0, K+1]$, c_s^t converges by the monotone convergence theorem. *Q.E.D.*

Note that this result applies to the cutoffs for both finite and continuum economies. In finite markets, at convergence, these cutoffs produce the allocation we get from the usual definition of DA (e.g., as in Gale and Shapley (1962)). This can be seen by noting that

$$\begin{aligned}&\max\big\{x \in [0, K+1] \mid F\big(\{i \in Q_s(\mathbf{c}^t) \text{ such that } \pi_{is} \leq x\}\big) \leq q_s\big\}\\ &\quad = \max\big\{x \in [0, K+1] \mid \big|\{j \in Q_s(\mathbf{c}^t) : \pi_{js} \leq x\}\big| \leq k_s\big\},\end{aligned}$$

implying that the provisional cutoff at school s in step t in our DA formulation, which is determined by the left-hand side of this equality, is the same as that in Gale and Shapley's (1962) DA formulation, which is determined by the right-hand side of the equality. Our DA formulation and the Gale and Shapley (1962) formulation therefore produce the same cutoff at each step. This also implies that, in finite markets, our DA cutoffs are found in a finite number of iterations, since DA as described by Gale and Shapley (1962) converges in a finite number of steps.

A.3. *Proof of Theorem 1*

Note first that admissions cutoffs $\mathbf{c}$ in a continuum economy are invariant to lottery outcomes r_i for each i: DA in the continuum depends only on $F(I_0)$ for sets $I_0 = \{i \in I \mid \theta_i \in \Theta_0\}$ with various choices of Θ_0. In particular, $F(I_0)$ does not depend on lottery realizations. Likewise, marginal priority ρ_s is uniquely determined for every school, s.

Now, consider the propensity score for school s. Applicants who do not rank s have $p_s(\theta) = 0$. Among those who do rank s, those of type $\theta \in \Theta_s^n$ have $\rho_{\theta s} > \rho_s$. Therefore, $p_s(\theta) = 0$ for every $\theta \in \Theta_s^n \cup (\Theta \setminus \Theta_s)$.

Applicants of type $\theta \in \Theta_s^a \cup \Theta_s^c$ may be assigned $\tilde{s} \in B_{\theta s}$, where $\rho_{\theta\tilde{s}} = \rho_{\tilde{s}}$. Since lottery numbers are uniform, the proportion of type-θ applicants assigned some $\tilde{s} \in B_{\theta s}$ where $\rho_{\theta\tilde{s}} = \rho_{\tilde{s}}$ is $\text{MID}_{\theta\tilde{s}}$. In other words, 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 $1 - \text{MID}_{\theta s}$. Every applicant of type $\theta \in \Theta_s^a$ who is not assigned a higher choice is assigned s because $\rho_{\theta s} < \rho_s$, and so

$$p_s(\theta) = (1 - \text{MID}_{\theta s}) \quad \text{for all } \theta \in \Theta_s^a.$$

Finally, consider applicants of type $\theta \in \Theta_s^c$ who are not assigned a higher choice. The fraction of applicants $\theta \in \Theta_s^c$ who are not assigned a higher choice is $1 - \text{MID}_{\theta s}$. Also, the random numbers of these applicants is larger than $\text{MID}_{\theta s}$. If $\tau_s < \text{MID}_{\theta s}$, then no such applicant is assigned s. If $\tau_s \geq \text{MID}_{\theta s}$, then the ratio of applicants that are assigned s within this set is given by $\frac{\tau_s - \text{MID}_{\theta s}}{1 - \text{MID}_{\theta s}}$. 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 $\max\{0, \frac{\tau_s - \mathrm{MID}_{\theta s}}{1-\mathrm{MID}_{\theta s}}\}$. Therefore,

$$p_s(\theta) = (1 - \mathrm{MID}_{\theta s}) \times \max\left\{0, \frac{\tau_s - \mathrm{MID}_{\theta s}}{1 - \mathrm{MID}_{\theta s}}\right\} \quad \text{for all } \theta \in \Theta_s^c.$$

A.4. *Example 2 in the Continuum*

In the large-market analog of Example 2, we can model lottery numbers as distributed according to a continuous uniform distribution over $[0, 1]$. Types 2 and 3 rank different schools ahead of a, so the sets of schools preferred to a by types 2 and 3 are

$$B_{3a} = \{b\} \quad \text{and} \quad B_{2a} = \{b, c\}.$$

Nevertheless, because $\tau_c = 0.5 < 0.75 = \tau_b$, we have that

$$\mathrm{MID}_{2a} = \mathrm{MID}_{3a} = \tau_b = 0.75.$$

To see where these cutoffs come from, note first that among the $2n$ type 1 and type 2 applicants who rank c first in this large market, those with lottery numbers lower (better) than 0.5 are assigned to c since it has a capacity of n: $\tau_c = 0.5$. The remaining type 2 applicants (half of the original mass of type 2), all of whom have lottery numbers higher (worse) than 0.5, must compete with all type 3 applicants for seats at b. We therefore have $1.5n$ school-b hopefuls but only n seats at b. All type 3 applicants with lottery numbers below 0.5 get seated at b (the type 2 applicants all have lottery numbers above 0.5), but this does not fill b. The remaining seats are therefore split equally between type 2 and type 3 applicants in the upper half of the lottery distribution, implying that the highest lottery number seated at b is $\tau_b = 0.75$.

Since there are no priorities, type 2 and type 3 are in Θ_s^c and type 2 and type 3 applicants seated at a must have lottery numbers above 0.75. It remains to compute the cutoff, τ_a. Types 2 and 3 compete only with type 4 at a, and are surely beaten out there by type 4's with lottery numbers below 0.75. The remaining 0.25 seats are shared equally between types 2, 3, and 4, going to the best lottery numbers in $[0.75, 1]$, without regard to type. The lottery cutoff at a, τ_a, is therefore $0.75 + 0.25/3 = 5/6$. Plugging these into equation (2) gives the DA score for types 2 and 3:

$$\begin{aligned}
\varphi_a(\theta) &= (1 - \mathrm{MID}_{\theta a}) \times \max\left\{0, \frac{\tau_a - \mathrm{MID}_{\theta a}}{1 - \mathrm{MID}_{\theta a}}\right\} \\
&= (1 - 0.75) \times \max\left\{0, \frac{5/6 - 0.75}{1 - 0.75}\right\} \\
&= \frac{1}{12}.
\end{aligned}$$

The score for type 4 is the remaining probability, $1 - (2 \times \frac{1}{12}) = \frac{5}{6}$.

A.5. *Proof of Theorem 2*

We complete the proof of Theorem 2 in Section 3.3 by proving the following two intermediate results.

LEMMA 3—Cutoff Almost Sure Convergence: $\hat{\mathbf{c}}_n \overset{a.s.}{\to} \mathbf{c}$.

LEMMA 4—Propensity-Score Almost Sure Convergence: *For all* $\theta \in \Theta$ *and* $s \in S$, $p_{ns}(\theta) \overset{a.s.}{\to} \varphi_s(\theta)$.

A.5.1. *Proof of Lemma 3*

We use the Extended Continuous Mapping Theorem (Theorem 19.1 in van der Vaart (2000)) to prove the lemma. We first show deterministic convergence of cutoffs in order to verify the assumptions of the Continuous Mapping theorem.

Let

$$I(\Theta_0, r_0, r_1) = \{i \in I \mid \theta_i \in \Theta_0, r_0 < r_i \leq r_1\}.$$

In a continuum economy,

$$F\big(I(\Theta_0, r_0, r_1)\big) = E\big[1\{\theta_i \in \Theta_0\}\big] \times (r_1 - r_0),$$

where the expectation is assumed to exist. In a finite economy with n applicants,

$$F\big(I(\Theta_0, r_0, r_1)\big) = \frac{\big|I(\Theta_0, r_0, r_1)\big|}{n}.$$

Let $\mathcal{F}$ be the set of possible F's defined above. For any two distributions F and F', the supnorm metric is defined by

$$d\big(F, F'\big) = \sup_{\Theta_0 \subset \Theta, r_0, r_1 \in [0,1]} \big|F\big(I(\Theta_0, r_0, r_1)\big) - F'\big(I(\Theta_0, r_0, r_1)\big)\big|.$$

The notation is otherwise as in the text.

PROOF OF LEMMA 3: Consider a deterministic sequence of economies described by a sequence of distributions $\{f_n\}$ over applicants, together with associated school capacities, so that for all n, $f_n \in \mathcal{F}$ is a potential realization produced by randomly drawing n applicants and their lottery numbers from F. Assume that $f_n \to F$ in metric space $(\mathcal{F}, d)$. Let $\mathbf{c}_n$ denote the admissions cutoffs in f_n. Note the $\mathbf{c}_n$ is constant because this is the cutoff for a particular realized economy f_n.

The proof first shows deterministic convergence of cutoffs for any convergent subsequence of f_n. Let $\{\tilde{f}_n\}$ be a subsequence of realized economies $\{f_n\}$. The corresponding cutoffs are denoted $\{\tilde{\mathbf{c}}_n\}$. Let $\tilde{\mathbf{c}} \equiv (\tilde{c}_s)$ be the limit of $\tilde{\mathbf{c}}_n$. The following two claims establish that $\tilde{\mathbf{c}}_n \to \mathbf{c}$, the cutoff associated with F.

CLAIM 1: $\tilde{c}_s \geq c_s$ *for every* $s \in S$.

PROOF: This is proved by contradiction in three steps. Suppose to the contrary that $\tilde{c}_s < c_s$ for some s. Let $S' \subset S$ be the set of schools the cutoffs of which are strictly lower under $\tilde{\mathbf{c}}$. For any $s \in S'$, define $I_n^s = \{i \in I \mid \tilde{c}_{ns} < \pi_{is} \leq c_s$ and i ranks s first$\}$ where I is the set of applicants in F, which contains the set of applicants in f_n for all n. In other words, I_n^s are the set of applicants ranking school s first who have an applicant rank in between $\tilde{c}_{ns}$ and c_s.

Step (a): We first show that for our subsequence, when the market is large enough, there must be some applicants who are in I_n^s. That is, there exists N such that for any $n > N$, we have $\tilde{f}_n(I_n^s) > 0$ for all $s \in S'$.

To see this, we begin by showing that for all $s \in S'$, there exists N such that for any $n > N$, we have $F(I_n^s) > 0$. Suppose, to the contrary, that there exists $s \in S'$ such that for all N, there exists $n > N$ such that $F(I_n^s) = 0$. When we consider the subsequence of realized economies $\{\tilde{f}_n\}$, we find that

$$\begin{aligned}
&\tilde{f}_n\big(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \pi_{is} \le c_s\}\big) \\
&\quad = \tilde{f}_n\big(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \pi_{is} \le \tilde{c}_{ns}\}\big) + \tilde{f}_n\big(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \tilde{c}_{ns} < \pi_{is} \le c_s\}\big) \\
&\quad = \tilde{f}_n\big(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \pi_{is} \le \tilde{c}_{ns}\}\big) && (19)\\
&\quad \le q_s. && (20)
\end{aligned}$$

Expression (19) follows from Assumption 1 by the following reason. Equation (19) does not hold, that is, $\tilde{f}_n(\{i \in Q_s(\mathbf{c}_n) \text{ such that } \tilde{c}_{ns} < \pi_{is} \le c_s\}) > 0$ only if $F(\{i \in I \mid \tilde{c}_{ns} < \pi_{is} \le c_s\}) > 0$. This and Assumption 1 imply $F(\{i \in I \mid \tilde{c}_{ns} < \pi_{is} \le c_s \text{ and } i \text{ ranks } s \text{ first}\}) \equiv F(I_n^s) > 0$, a contradiction to $F(I_n^s) = 0$. Since $\tilde{f}_n$ is realized as n i.i.d. samples from F, $\tilde{f}_n(\{i \in I \mid \tilde{c}_{ns} < \pi_{is} \le c_s\}) = 0$. Expression (20) follows by our definition of DA, which can never assign more applicants to a school than its capacity for each of the n samples. We obtain our contradiction since $\tilde{c}_{ns}$ is not maximal at s in $\tilde{f}_n$ since expression (20) means it is possible to increase the cutoff $\tilde{c}_{ns}$ to c_s without violating the capacity constraint.

Given that we have just shown that for each $s \in S'$, $F(I_n^s) > 0$ for some n, it is possible to find an n such that $F(I_n^s) > \varepsilon > 0$. Since $f_n \to F$ and so $\tilde{f}_n \to F$, there exists N such that for all $n > N$, we have $\tilde{f}_n(I_n^s) > F(I_n^s) - \varepsilon > 0$. Since the number of schools is finite, such N can be taken uniformly over all $s \in S$. This completes the argument for Step (a).

Step (a) allows us to find some N such that for any $n > N$, $\tilde{f}_n(I_n^s) > 0$ for all $s' \in S'$. Let $\tilde{s}_n \in S$ and t be such that $\tilde{c}_{ns}^{t-1} \ge c_s$ for all $s \in S$ and $\tilde{c}_{n\tilde{s}_n}^t < c_{\tilde{s}_n}$. That is, $\tilde{s}_n$ is one of the first schools the cutoff of which falls strictly below $c_{\tilde{s}_n}$ under the DA algorithm in $\tilde{f}_n$, which happens in round t of the DA algorithm. Such $\tilde{s}_n$ and t exist since the choice of n guarantees $\tilde{f}_n(I_n^s) > 0$ and so $\tilde{c}_{ns} < c_s$ for all $s \in S'$.

Step (b): We next show that there exist infinitely many values of n such that the associated $\tilde{s}_n$ is in S' and $\tilde{f}_n(I_n^s) > 0$ for all $s \in S'$. This is true because otherwise, by Step (a), there exists N such that for all $n > N$, we have $\tilde{s}_n \notin S'$. Since there are only finitely many schools, $\{\tilde{s}_n\}$ has a subsequence $\{\tilde{s}_m\}$ such that $\tilde{s}_m$ is the same school outside S' for all m. By definition of $\tilde{s}_n$, $\tilde{c}_{m\tilde{s}_m} \le \tilde{c}_{m\tilde{s}_m}^t < c_{\tilde{s}_m}$ for all m and so $\tilde{c}_{\tilde{s}_m} < c_{\tilde{s}_m}$, a contradiction to $\tilde{s}_m \notin S'$.

Fix some n such that the associated $\tilde{s}_n$ is in S' and $\tilde{f}_n(l_n^s) > 0$ for all $s \in S'$. Step (b) guarantees that such n exists. Let $\tilde{A}_{n\tilde{s}_n}$ and $A_{\tilde{s}_n}$ be the sets of applicants assigned $\tilde{s}_n$ under $\tilde{f}_n$ and F, respectively. All applicants in $I_n^{\tilde{s}_n}$ are assigned $\tilde{s}_n$ in F and rejected by $\tilde{s}_n$ in $\tilde{f}_n$. Since these applicants rank $\tilde{s}_n$ first, there must exist a positive measure (with respect to $\tilde{f}_n$) of applicants outside $I_n^{\tilde{s}_n}$ who are assigned $\tilde{s}_n$ in $\tilde{f}_n$ and some other school in F; denote the set of them by $\tilde{A}_{n\tilde{s}_n} \setminus A_{\tilde{s}_n}$. $\tilde{f}_n(\tilde{A}_{n\tilde{s}_n} \setminus A_{\tilde{s}_n}) > 0$ since otherwise, for any n such that Step (b) applies,

$$\tilde{f}_n(\tilde{A}_{n\tilde{s}_n}) \leq \tilde{f}_n(A_{\tilde{s}_n} \setminus I_n^{\tilde{s}_n}) = \tilde{f}_n(A_{\tilde{s}_n}) - \tilde{f}_n(I_n^{\tilde{s}_n}),$$

which by Step (a) converges to something strictly smaller than $F(A_{\tilde{s}_n})$ since $\tilde{f}_n(A_{\tilde{s}_n}) \to F(A_{\tilde{s}_n})$ and $\tilde{f}_n(I_n^{\tilde{s}_n}) > 0$ for all large enough n by Step (a). Note that $F(A_{\tilde{s}_n})$ is weakly smaller than $q_{\tilde{s}_n}$. This implies that for large enough n, $\tilde{f}_n(\tilde{A}_{n\tilde{s}_n}) < q_{\tilde{s}_n}$, a contradiction to $\tilde{A}_{n\tilde{s}_n}$'s being the set of applicants assigned $\tilde{s}_n$ at a cutoff strictly smaller than the largest possible value $K+1$. For each $i \in \tilde{A}_{n\tilde{s}_n} \setminus A_{\tilde{s}_n}$, let s_i be the school to which i is assigned under F.

Step (c): To complete the argument for Claim 1, we show that some $i \in \tilde{A}_{n\tilde{s}_n} \setminus A_{\tilde{s}_n}$ must have been rejected by s_i in some step $\tilde{t} \leq t-1$ of the DA algorithm in $\tilde{f}_n$. That is, there exists $i \in \tilde{A}_{n\tilde{s}_n} \setminus A_{\tilde{s}_n}$ and $\tilde{t} \leq t-1$ such that $\pi_{is_i} > \tilde{c}_{ns_i}^{\tilde{t}}$. Suppose to the contrary that for all $i \in \tilde{A}_{n\tilde{s}_n} \setminus A_{\tilde{s}_n}$ and $\tilde{t} \leq t-1$, we have $\pi_{is_i} \leq \tilde{c}_{ns_i}^{\tilde{t}}$. Each such applicant i must prefer s_i to $\tilde{s}_n$ because i is assigned $s_i \neq \tilde{s}_n$ under F though $\pi_{i\tilde{s}_n} \leq \tilde{c}_{n\tilde{s}_n} < c_{\tilde{s}_n}$, where the first inequality holds because i is assigned $\tilde{s}_n$ in $\tilde{F}_n$ while the second inequality does because $\tilde{s}_n \in S'$. This implies none of $\tilde{A}_{n\tilde{s}_n} \setminus A_{\tilde{s}_n}$ is rejected by s_i, applies for $\tilde{s}$, and contributes to decreasing $\tilde{c}_{n\tilde{s}_n}^t$ at least until step t and so $\tilde{c}_{n\tilde{s}_n}^t < c_{\tilde{s}_n}$ cannot be the case, a contradiction. Therefore, we have our desired conclusion of Step (c).

Claim 1 can now be established by showing that Step (c) implies there are $i \in \tilde{A}_{n\tilde{s}_n} \setminus A_{\tilde{s}_n}$ and $\tilde{t} \leq t-1$ such that $\pi_{is_i} > \tilde{c}_{ns_i}^{\tilde{t}} \geq \tilde{c}_{ns_i}$, where the last inequality is implied by the fact that in every economy, for all $s \in S$ and $t \geq 0$, we have $c_s^{t+1} \leq c_s^t$. Also, they are assigned s_i in F so that $\pi_{is_i} \leq c_{s_i}$. These imply $c_{s_i} > \tilde{c}_{ns_i}^{\tilde{t}} \geq \tilde{c}_{ns_i}$. That is, the cutoff of s_i falls below c_{si} in step $\tilde{t} \leq t-1 < t$ of the DA algorithm in $\tilde{f}_n$. This contradicts the definition of $\tilde{s}_n$ and t. Therefore, $\tilde{c}_s \geq c_s$ for all $s \in S$, as desired. This completes the proof of Claim 1. *Q.E.D.*

CLAIM 2: *By a similar argument*, $\tilde{c}_s \leq c_s$ *for every* $s \in S$.

Since $\tilde{c}_s \geq c_s$ and $\tilde{c}_s \leq c_s$ for all s, it must be the case that $\tilde{\mathbf{c}}_n \to \mathbf{c}$. The following claim uses this to show that $\mathbf{c}_n \to \mathbf{c}$.

CLAIM 3: *If* $\tilde{\mathbf{c}}_n \to \mathbf{c}$ *for every convergent subsequence* $\{\tilde{\mathbf{c}}_n\}$ *of* $\{\mathbf{c}_n\}$, *then* $\mathbf{c}_n \to \mathbf{c}$.

PROOF: Since $\{\mathbf{c}_n\}$ is bounded in $[0, K+1]^{|S|}$, it has a convergent subsequence by the Bolzano–Weierstrass theorem. Suppose to the contrary that for every convergent subsequence $\{\tilde{\mathbf{c}}_n\}$, we have $\tilde{\mathbf{c}}_n \to \mathbf{c}$, but $\mathbf{c}_n \nrightarrow \mathbf{c}$. Then there exists $\varepsilon > 0$ such that for all $k > 0$, there exists $n_k > k$ such that $\|\mathbf{c}_{n_k} - \mathbf{c}\| \geq \varepsilon$. Then the subsequence $\{\mathbf{c}_{n_k}\}_k \subset \{\mathbf{c}_n\}$ has a convergent subsequence that does not converge to $\mathbf{c}$ (since $\|\mathbf{c}_{n_k} - \mathbf{c}\| \geq \varepsilon$ for all k), which contradicts the supposition that every convergent subsequence of $\{\mathbf{c}_n\}$ converges to $\mathbf{c}$, completing the proof of the claim. *Q.E.D.*

The last step in the proof of Lemma 3 relates this fact to stochastic convergence.

CLAIM 4: $\mathbf{c}_n \to \mathbf{c}$ *implies* $\hat{\mathbf{c}}_n \xrightarrow{a.s.} \mathbf{c}$.

PROOF: This proof is based on two off-the-shelf asymptotic results from mathematical statics. First, let F_n be the distribution over $I(\Theta_0, r_0, r_1)$'s generated by randomly drawing n applicants from F. Note that F_n is random since it involves randomly drawing n

applicants. $F_n \overset{\text{a.s.}}{\to} F$ by the Glivenko–Cantelli theorem (Theorem 19.1 in van der Vaart (2000)). Next, since $F_n \overset{\text{a.s.}}{\to} F$ and $\mathbf{c}_n \to \mathbf{c}$, the Extended Continuous Mapping Theorem (Theorem 18.11 in van der Vaart (2000)) implies that $\hat{\mathbf{c}}_n \overset{\text{a.s.}}{\to} \mathbf{c}$, establishing the claim and completing the proof of Lemma 3. *Q.E.D.*

Q.E.D.

A.5.2. *Proof of Lemma 4*

Consider any deterministic sequence of economies $\{f_n\}$ such that $f_n \in \mathcal{F}$ for all n and $f_n \to F$ in the $(\mathcal{F}, d)$ metric space. Let $p_{ns}(\theta)$ be the (finite-market, deterministic) propensity score for a particular f_n. Note that this modifies the definition of $p_{ns}(\theta)$ from that in the text. The change here is that the propensity score for f_n is not a random quantity, because economy f_n is viewed as fixed.

For Lemma 4, it is enough to show deterministic convergence of this finite-market score, that is, $p_{ns}(\theta) \to \varphi_s(\theta)$ as $f_n \to F$. To see this, let F_n be the distribution over $I(\Theta_0, r_0, r_1)$'s induced by randomly drawing n applicants from F. Note that F_n is random and that $F_n \overset{\text{a.s.}}{\to} F$ by the Glivenko–Cantelli theorem (Theorem 19.1 in van der Vaart (2000)). $F_n \overset{\text{a.s.}}{\to} F$ and $p_{ns}(\theta) \to \varphi_s(\theta)$ allow us to apply the Extended Continuous Mapping Theorem (Theorem 18.11 in van der Vaart (2000)) to obtain $\tilde{p}_{ns}(\theta) \overset{\text{a.s.}}{\to} \varphi_s(\theta)$.

We prove convergence of $p_{ns}(\theta) \to \varphi_s(\theta)$ as follows. Let $\tilde{c}_{ns}$ and $\tilde{c}_{ns'}$ be the random cutoffs at s and s', respectively, in f_n, and

$$
\begin{aligned}
\tau_{\theta s} &\equiv c_s - \rho_{\theta s}, \\
\tau_{\theta s_-} &\equiv \max_{s' \succ_\theta s}\{c_{s'} - \rho_{\theta s'}\}, \\
\tilde{\tau}_{n\theta s} &\equiv \tilde{c}_{ns} - \rho_{\theta s}, \quad \text{and} \\
\tilde{\tau}_{n\theta s_-} &\equiv \max_{s' \succ_\theta s}\{\tilde{c}_{ns'} - \rho_{\theta s'}\}.
\end{aligned}
$$

We can express $\varphi_s(\theta)$ and $p_{ns}(\theta)$ as follows:

$$
\begin{aligned}
\varphi_s(\theta) &= \max\{0, \tau_{\theta s} - \tau_{\theta s_-}\}, \\
p_{ns}(\theta) &= P_n(\tilde{\tau}_{n\theta s} \geq R > \tilde{\tau}_{n\theta s_-}),
\end{aligned}
$$

where P_n is the probability induced by randomly drawing lottery numbers given f_n, and R is a random (not realized) lottery number for any type-θ applicant, where we omit an applicant subscript for simplicity. R's marginal distribution is $U[0, 1]$.

By Lemma 3, with probability 1, for all $\varepsilon_1 > 0$, there exists N_1 such that for all $n > N_1$,

$$
|\tilde{c}_{ns'} - c_{s'}| < \varepsilon_1 \quad \text{for all } s',
$$

which implies that with probability 1,

$$
\begin{aligned}
&|\tilde{\tau}_{n\theta s_-} - \tau_{\theta s_-}| \\
&\quad = \big|\{\tilde{c}_{ns_1} - \rho_{\theta s_1}\} - \{c_{s_2} - \rho_{\theta s_2}\}\big| \\
&\quad < \begin{cases} \big|\{\tilde{c}_{ns_1} - \rho_{\theta s_1}\} - \big(\{\tilde{c}_{ns_1} - \rho_{\theta s_2}\} + \varepsilon_1\big)\big| & \text{if } c_{s_2} - \rho_{\theta s_2} \geq \tilde{c}_{ns_1} - \rho_{\theta s_1}, \\ \big|\{\tilde{c}_{ns_1} - \rho_{\theta s_1}\} - \big(\{\tilde{c}_{ns_1} - \rho_{\theta s_2}\} - \varepsilon_1\big)\big| & \text{if } c_{s_2} - \rho_{\theta s_2} < \tilde{c}_{ns_1} - \rho_{\theta s_1} \end{cases} \\
&\quad = \varepsilon_1,
\end{aligned}
$$

where in the first equality, $s_1 \equiv \arg\max_{s' \succ_\theta s}\{\tilde{c}_{ns'} - \rho_{\theta s'}\}$ and $s_2 \equiv \arg\max\{c_{s'} - \rho_{\theta s'}\}$. The inequality is by $|\tilde{c}_{ns'} - c_{s'}| < \varepsilon_1$ for all s'. For all $\varepsilon > 0$, the above argument with setting $\varepsilon_1 < \varepsilon/2$ implies that there exists N such that for all $n > N$,

$$\begin{aligned} p_{ns}(\theta) &= P_n(\tilde{\tau}_{n\theta s} \geq R > \tilde{\tau}_{n\theta s_-}) \\ &\in \big(\max\{0, \tau_{\theta s} - \tau_{\theta s_-} - \varepsilon\}, \max\{0, \tau_{\theta s} - \tau_{\theta s_-} + \varepsilon\}\big) \\ &\in \big(\varphi_s(\theta) - \varepsilon, \varphi_s(\theta) + \varepsilon\big), \end{aligned}$$

where the second-to-last inclusion is because with probability 1, there exists N such that for all $n > N$ such that $|\tilde{\tau}_{n\theta s} - \tau_{\theta s}|, |\tilde{\tau}_{n\theta s_-} - \tau_{\theta s_-}| < \varepsilon_1$ and $R \sim U[0, 1]$. This means $p_{ns}(\theta) \to \varphi_s(\theta)$, completing the proof of Lemma 4.

A.6. *Proof of Proposition 2*

By the definition of MID, for any θ and s, there exists $\tilde{s}$ such that $\mathrm{MID}_{\theta s} = \tau_{\tilde{s}}$, which is the decimal part of $c_{\tilde{s}}$. Cutoff vector $\mathbf{c}$ also pins down Θ_s^a and Θ_s^c. Thus, the assumption ($r_i \perp\!\!\!\perp \mathbf{c}$) implies that individual lottery numbers r_i are uniformly distributed over $[0, 1]$ (not only unconditionally but also) conditional on any cutoff, MID, Θ_s^a, Θ_s^c, and type. This gives us both unbiasedness and conditional independence. When $\tilde{p}_s(\theta)$ is the formula version of the estimated DA propensity score, the DA propensity score is unbiased for the true propensity score, that is, $E(\tilde{p}_s(\theta)) = p_s(\theta)$ for every applicant type θ since

$$\begin{aligned} &E\big(\tilde{p}_s(\theta)\big) &&\text{(a)} \\ &= E\left(1\{\theta_i \in \Theta_s^a\}(1 - \mathrm{MID}_{\theta_i s}) + 1\{\theta_i \in \Theta_s^c\}(1 - \mathrm{MID}_{\theta_i s}) \max\left\{0, \frac{\tau_s - \mathrm{MID}_{\theta_i s}}{1 - \mathrm{MID}_{\theta_i s}}\right\} \middle| \right. &&\text{(b)} \\ &\qquad \left. \theta_i = \theta \right) \\ &= E\big(E\big(1\{\theta_i \in \Theta_s^a\}1\{\mathrm{MID}_{\theta_i s} < r_i\} + 1\{\theta_i \in \Theta_s^c\}1\{\mathrm{MID}_{\theta_i s} < r_i \leq \tau_s\}| \\ &\qquad \theta_i = \theta, \tau_s, \mathrm{MID}_{\theta_i s}, \Theta_s^a, \Theta_s^c\big)\big|\theta_i = \theta\big) \\ &= E\big(E\big(1\{\theta_i \in \Theta_s^a \text{ and } \mathrm{MID}_{\theta_i s} < r_i\} + 1\{\theta_i \in \Theta_s^c \text{ and } \mathrm{MID}_{\theta_i s} < r_i \leq \tau_s\}| \\ &\qquad \theta_i = \theta, \tau_s, \mathrm{MID}_{\theta_i s}, \Theta_s^a, \Theta_s^c\big)\big|\theta_i = \theta\big) \\ &= E\big(1\{\theta_i \in \Theta_s^a \text{ and } \mathrm{MID}_{\theta_i s} < r_i\} + 1\{\theta_i \in \Theta_s^c \text{ and } \mathrm{MID}_{\theta_i s} < r_i \leq \tau_s\}|\theta_i = \theta\big) \\ &= E\big(1\big\{\big(\theta_i \in \Theta_s^a \text{ and } \mathrm{MID}_{\theta_i s} < r_i\big) \text{ or } \big(\theta_i \in \Theta_s^c \text{ and } \mathrm{MID}_{\theta_i s} < r_i \leq \tau_s\big)\big\}|\theta_i = \theta\big) \\ &= P\big(D_i(s) = 1|\theta_i = \theta\big), \end{aligned}$$

where the first and fourth equalities are by the law of iterated expectation, and the second equality is by $r_i \sim U(0, 1)$ conditional on any cutoff, MID, Θ_s^a, Θ_s^c, and type. To obtain this result for the frequency version of the estimated DA propensity score, insert the following lines between equations (a) and (b):

$$
\begin{aligned}
(a) &= E\left(\sum_{\delta=a,c,n} 1\{\theta_i \in \Theta_s^\delta\} \frac{\sum_{j\in I} D_j(s) 1\{1\{\theta_j \in \Theta_s^\delta\} = 1\{\theta_i \in \Theta_s^\delta\}, \mathrm{MID}_{\theta_j s} = \mathrm{MID}_{\theta_i s}\}}{\sum_{j\in I} 1\{1\{\theta_j \in \Theta_s^\delta\} = 1\{\theta_i \in \Theta_s^\delta\}, \mathrm{MID}_{\theta_j s} = \mathrm{MID}_{\theta_i s}\}} \right| \\
&\qquad \left. \theta_i = \theta \right) \\
&= E\left(E\left(\sum_{\delta=a,c,n} 1\{\theta_i \in \Theta_s^\delta\} \frac{\sum_{j\in I} D_j(s) 1\{1\{\theta_j \in \Theta_s^\delta\} = 1\{\theta_i \in \Theta_s^\delta\}, \mathrm{MID}_{\theta_j s} = \mathrm{MID}_{\theta_i s}\}}{\sum_{j\in I} 1\{1\{\theta_j \in \Theta_s^\delta\} = 1\{\theta_i \in \Theta_s^\delta\}, \mathrm{MID}_{\theta_j s} = \mathrm{MID}_{\theta_i s}\}} \right| \right. \\
&\qquad \left. \left. \theta_i = \theta, \tau_s, \mathrm{MID}_{\theta_i s}, \Theta_s^a, \Theta_s^c \right) \right| \theta_i = \theta \right) \\
&= E\left(E\left(1\{\theta_i \in \Theta_s^a\}(1 - \mathrm{MID}_{\theta_i s}) + 1\{\theta_i \in \Theta_s^c\}(1 - \mathrm{MID}_{\theta_i s}) \max\left\{0, \frac{\tau_s - \mathrm{MID}_{\theta_i s}}{1 - \mathrm{MID}_{\theta_i s}}\right\} \right| \right. \\
&\qquad \left. \left. \theta_i = \theta, \tau_s, \mathrm{MID}_{\theta_i s}, \Theta_s^a, \Theta_s^c \right) \right| \theta_i = \theta \right) \\
&= (b),
\end{aligned}
$$

where the first equality is by the definition of the frequency DA score, the second equality is by the law of iterated expectation, and the third equality is by $r_i \sim U[0, 1]$ conditional on any cutoff, MID, Θ_s^a, Θ_s^c, and type.

Assignment is independent conditional on the formula version of the estimated DA propensity score, that is, $P(D_i(s) = 1 | \tilde{p}_s(\theta_i), \theta_i) = P(D_i(s) = 1 | \tilde{p}_s(\theta_i))$ by the following reason:

$$
\begin{aligned}
& P\big(D_i(s) = 1 | \tilde{p}_s(\theta_i) = p, \theta_i\big) \\
&\quad = E\big(1\{(\theta_i \in \Theta_s^a \text{ and } \mathrm{MID}_{\theta_i s} < r_i) \text{ or } (\theta_i \in \Theta_s^c \text{ and } \mathrm{MID}_{\theta_i s} < r_i \le \tau_s)\} | \tilde{p}_s(\theta_i) = p, \theta_i\big) \\
&\quad = E\big(E\big(1\{(\theta_i \in \Theta_s^a \text{ and } \mathrm{MID}_{\theta_i s} < r_i) \text{ or } (\theta_i \in \Theta_s^c \text{ and } \mathrm{MID}_{\theta_i s} < r_i \le \tau_s)\} | \\
&\qquad \tau_s, \mathrm{MID}_{\theta_i s}, \Theta_s^a, \Theta_s^c, \tilde{p}_s(\theta_i) = p, \theta_i\big) \big| \tilde{p}_s(\theta_i) = p, \theta_i\big) \qquad \text{(c)} \\
&\quad = E\big(E\big(1\{(\theta_i \in \Theta_s^a \text{ and } \mathrm{MID}_{\theta_i s} < r_i) \text{ or } (\theta_i \in \Theta_s^c \text{ and } \mathrm{MID}_{\theta_i s} < r_i \le \tau_s)\} | \\
&\qquad \tau_s, \mathrm{MID}_{\theta_i s}, \Theta_s^a, \Theta_s^c, \theta_i\big) \big| \tilde{p}_s(\theta_i) = p, \theta_i\big)
\end{aligned}
$$

$$
\begin{aligned}
&= E\Bigg(1\{\theta_i \in \Theta_s^a\}(1-\mathrm{MID}_{\theta_i s}) + 1\{\theta_i \in \Theta_s^c\}(1-\mathrm{MID}_{\theta_i s}) \max\left\{0, \frac{\tau_s - \mathrm{MID}_{\theta_i s}}{1-\mathrm{MID}_{\theta_i s}}\right\} \Bigg| \\
&\qquad \tilde{p}_s(\theta_i) = p, \theta_i\Bigg) \\
&= E\big(\tilde{p}_s(\theta_i) | \tilde{p}_s(\theta_i) = p, \theta_i\big) \qquad \text{(d)} \\
&= p,
\end{aligned}
$$

which is independent from θ_i conditional on $\tilde{p}_s(\theta_i) = p$. In the above calculations, the first equality is by the definition of $D_i(s)$, the second equality is by the law of iterated expectation, the third equality is by the fact that τ_s, $\mathrm{MID}_{\theta_i s}$, Θ_s^a, and Θ_s^c pin down $\tilde{p}_s(\theta_i)$, the fourth equality is by $r_i \sim U[0,1]$ conditional on any cutoff, MID, Θ_s^a, Θ_s^c, and type, and the fifth equality is by the definition of $\tilde{p}_s(\theta_i)$. Assignment is also independent conditional on the frequency version of the estimated DA propensity score since, for the frequency version, equation (c) directly implies (d). *Q.E.D.*

A.7. *Modes of Inference*

Econometric inference typically tries to quantify the uncertainty due to *random sampling*. What then, to make of the fact that the analysis reported here uses data on all DPS applicants from 2012? On one hand, we might imagine that the applicants we happen to be studying constitute a random sample from some larger population of possible applicants. At the same time, the statistical uncertainty in our empirical work can also be seen as a consequence of *random assignment*: we see only a single lottery draw for each applicant, one of many possibilities even when the sample of applicants is viewed as fixed.

In an effort to determine whether the distinction between sampling inference and randomization inference matters for our purposes, we computed randomization p-values by repeatedly drawing lottery numbers and calculating offer gaps in covariates conditional on the simulated propensity score. Regression conditioning on the simulated score produces near-perfect balance in Table IV so this distribution is what we should expect to see under the null hypothesis of no difference by treatment assignment. Randomization p-values are therefore given by quantiles of the t-statistics in the distribution resulting from these repeated draws.

The p-values associated with conventional robust t-statistics for covariate balance turn out to be close to the corresponding randomization p-values. For the number of charter schools an applicant has ranked, for example, the conventional p-value for balance is 0.885 while the corresponding randomization p-value is 0.850. This is consistent with a classic result on the asymptotic equivalence of randomization and sampling tests for differences in means (see, e.g., Chapter 15.2 in Lehmann and Romano (2005)).

Abadie, Athey, Imbens, and Woolridge (2014) generalized results on the large-sample equivalence of randomization and sampling inference to cover regression estimates of treatment effects and tests for covariate balance of the sort reported here. If the regression function is linear and the regression of treatment on controls is linear, the usual robust covariance matrix associated with random sampling is asymptotically valid for the sampling distribution induced by random assignment.[33] The treatment in our case is an

[33]This is Theorem 3, in Abadie, Athey, Imbens, and Woolridge (2014), a result predicated on independent treatment assignments. In practice, DA assignments are correlated. Here too, however, the large-market

offer dummy, while the controls are dummies or a linear model for the propensity score. The second of these requirements holds here when the controls fully saturate the propensity score (ignoring any additional covariates). The first requires constant offer effects given a saturated model for the score. The models estimated here do not quite satisfy these conditions (they are not fully saturated) but do not seem to be so far off that this matters for inference.

A related issue arises from the fact that the empirical strategy used here conditions on estimates of the propensity score (the simulated score is also an estimate since it is based on a finite number of draws). As noted by Hirano, Imbens, and Ridder (2003) and Abadie and Imbens (2016), conditioning on an estimated as opposed to a nonstochastic known score may affect sampling distributions of the resulting estimated causal effects. We therefore checked conventional large-sample p-values against randomization p-values for the reduced-form charter-offer effects associated with the 2SLS estimates reported in Table VI. Robust asymptotic sampling formulas again generate p-values close to a randomization-inference benchmark, regardless of how the score behind these estimates was constructed. In view of these findings, we rely on the usual robust standard errors and test statistics for inference about 2SLS estimates of treatment effects.

A.8. *First-Choice and Qualification Instruments: Details*

Let D_i^f be the first-choice instrument defined in Section 4.5 and let $\tilde{s}_i$ be i's first-choice school. The first-choice risk set is $X(\theta_i) \equiv (\tilde{s}_i, \rho_{i\tilde{s}})$.

PROPOSITION 4: *In any continuum economy, D_i^f is independent of θ_i conditional on $X(\theta_i)$.*

PROOF: In general,

$$\begin{aligned}\Pr\bigl(D_i^f = 1|\theta_i = \theta\bigr) &= \Pr(\pi_{i\tilde{s}_i} \leq c_{\tilde{s}_i}|\theta_i = \theta) \\ &= \Pr(\rho_{i\tilde{s}_i} + r_i \leq c_{\tilde{s}_i}|\theta_i = \theta) \\ &= \Pr(r_i \leq c_{\tilde{s}_i} - \rho_{i\tilde{s}_i}|\theta_i = \theta) \\ &= c_{\tilde{s}_i} - \rho_{i\tilde{s}_i},\end{aligned}$$

which depends on θ_i only through $X(\theta_i)$ because cutoffs are fixed in the continuum. *Q.E.D.*

Let D_i^q and $X(\theta_i)$ be the qualification instrument and the associated risk set defined in Section 4.5. The latter is given by the list of schools i ranks and his priority status at each, that is, $X(\theta_i) \equiv (S_i, (\rho_{is})_{s\in S_i})$ where S_i is the set of charter schools i ranks.

PROPOSITION 5: *In any continuum economy, D_i^q is independent of θ_i conditional on $X(\theta_i)$.*

approximation smooths things out. In the continuum, cutoffs are fixed, and treatments are determined by individual independently drawn lottery numbers. We can therefore think of the asymptotic equivalence of randomization and conventional inference as a further consequence of our large-market approximation.

PROOF: In general, we have

$$\begin{aligned}\Pr\bigl(D_i^q = 1 | \theta_i = \theta\bigr) &= \Pr(\pi_{is} \le c_s \text{ for some } s \in S_i | \theta_i = \theta) \\ &= \Pr(\rho_{is} + r_i \le c_s \text{ for some } s \in S_i | \theta_i = \theta) \\ &= \Pr(r_i \le c_s - \rho_{is} \text{ for some } s \in S_i | \theta_i = \theta) \\ &= \Pr\Bigl(r_i \le \max_{s \in S_i}(c_s - \rho_{is}) \big| \theta_i = \theta\Bigr) \\ &= \max_{s \in S_i}(c_s - \rho_{is}),\end{aligned}$$

which depends on θ_i only through $X(\theta_i)$ because cutoffs are fixed in the continuum. *Q.E.D.*

A.9. *The DA Propensity Score With School-Specific Lotteries*

Washington, DC, New Orleans, and Amsterdam use DA with multiple lottery numbers, one for each school (see, e.g., de Haan, Gautier, Oosterbeek, and van der Klaauw (2015)). Washington, DC uses a version of DA that uses a mixture of shared and individual school lotteries. This section derives the DA propensity score for a mechanism with multiple tie-breaking.

Let random variable R_{is} denote applicant i's lottery number at school s. Assume that each R_{is} is distributed uniformly and independently over i, but $R_{is} \ne R_{is'}$ for some (not necessarily all) $s, s' \in S$. When R_{is} and $R_{is'}$ differ, they're assumed to be independent.

Recall $B_{\theta s}$ is defined as $\{s' \in S \mid s' \succ_\theta s\}$. Partition $B_{\theta s}$ into $\bar{m}$ disjoint sets $B^1_{\theta s}, \dots, B^{\bar{m}}_{\theta s}$, so that s' and s'' use the same lottery if and only if $s', s'' \in B^m_{\theta s}$ for some m. Note that this partition is specific to type θ. With single-school lotteries, $\bar{m}$ simplifies to $|B_{\theta s}|$, the number of schools type θ ranks ahead of s.

Most information disqualification, $\mathrm{MID}^m_{\theta s}$, is defined for each m as

$$\mathrm{MID}^m_{\theta s} \equiv \begin{cases} 0 & \text{if } \rho_{\theta\tilde{s}} > \rho_{\tilde{s}} \text{ for all } \tilde{s} \in B^m_{\theta s}, \\ 1 & \text{if } \rho_{\theta\tilde{s}} < \rho_{\tilde{s}} \text{ for some } \tilde{s} \in B^m_{\theta s}, \\ \max\bigl\{\tau_{\tilde{s}} \mid \tilde{s} \in B^m_{\theta s} \text{ and } \rho_{\theta\tilde{s}} = \rho_{\tilde{s}}\bigr\} & \text{if } \rho_{\theta\tilde{s}} = \rho_{\tilde{s}} \text{ for } \tilde{s} \in B^m_{\theta s} \\ & \text{and } \rho_{\theta\tilde{s}} > \rho_{\tilde{s}} \text{ otherwise.} \end{cases}$$

Let m^* be the value of m for schools in the partition that use the same lottery as s. Denote the associated MID by $\mathrm{MID}^*_{\theta s}$. We define $\mathrm{MID}^*_{\theta s} = 0$ when the lottery at s is unique and there is no m^*. The following result extends Theorem 1 to a general lottery structure. The proof is omitted.

THEOREM 1—Generalization: *For all s and θ in any continuum economy, we have*

$$\Pr\bigl[D_i(s) = 1 | \theta_i = \theta\bigr] = \varphi_s(\theta) \equiv \begin{cases} 0 & \textit{if } \theta \in \Theta^n_s, \\ \displaystyle\prod_{m=1}^{\bar{m}}\bigl(1 - \mathrm{MID}^m_{\theta s}\bigr) & \textit{if } \theta \in \Theta^a_s, \\ \displaystyle\prod_{m=1}^{\bar{m}}\bigl(1 - \mathrm{MID}^m_{\theta s}\bigr) \times \max\left\{0, \frac{\tau_s - \mathrm{MID}^*_{\theta s}}{1 - \mathrm{MID}^*_{\theta s}}\right\} & \textit{if } \theta \in \Theta^c_s, \end{cases}$$

where we set $\varphi_s(\theta) = 0$ when $\mathrm{MID}^*_{\theta s} = 1$ and $\theta \in \Theta^c_s$.

Note that in the single-tie-breaker case, the expression for $\varphi_s(\theta)$ reduces to that in Theorem 1 since $\bar{m} = 1$ in that case.

A.10. *The Boston (Immediate Acceptance) Mechanism*

Studies by Hastings–Kane–Staiger (2009), Hastings–Neilson–Zimmerman (2012), and Deming–Hastings–Kane–Staiger (2013), among others, use data generated from versions of the Boston mechanism. Given strict preferences of applicants and schools, the Boston mechanism is defined as follows:

- Step 1: Each applicant applies to her most preferred acceptable school (if any). Each school accepts its most-preferred applicants up to its capacity and rejects every other applicant.

In general, for any step $t \geq 2$,

- Step t: Each applicant who has not been accepted by any school applies to her most preferred acceptable school that has not rejected her (if any). Each school accepts its most-preferred applicants up to its remaining capacity and rejects every other applicant.

This algorithm terminates at the first step in which no applicant applies to a school. Boston assignments differ from DA in that any offer at any step is fixed; applicants receiving offers cannot be displaced later. This important difference notwithstanding, the Boston mechanism can be represented as a special case of DA by redefining priorities as follows:

PROPOSITION 6—Ergin and Sönmez (2006): *The Boston mechanism applied to $(\succ_i)_i$ and $(\succ_s)_s$ produces the same assignment as DA applied to $(\succ_i)_i$ and $(\succ_s^*)_s$ where $\succ_s^*$ is defined as follows*:

1. *For $k = 1, 2, \ldots$, {applicants who rank s kth} $\succ_s^*$ {applicants who rank s $k + 1$th}.*
2. *Within each priority group, $\succ_s^*$ ranks the applicants in the same order as original $\succ_s$.*

This equivalence allows us to construct a propensity score for the Boston mechanism by redefining priorities so that priority groups at a given school consist of applicants who share original priority status at this school and rank it the same way.

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