Centralized School Choice with Unequal Outside Options*

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Abstract

This paper studies how market design choices exacerbate or mitigate pre-existing inequalities when market participants differ in their options outside of the centralized system. We introduce inequality in outside options in a canonical model of centralized school choice, and show that having an outside option confers a strategic advantage under a manipulable mechanism but not under a strategy-proof mechanism. We then test the positive predictions of the model by leveraging a unique empirical setting where we can identify students with and without outside options and observe applications under the Boston mechanism and a deferred acceptance mechanism. Consistent with our theoretical predictions, students with an outside option are more likely to list popular, highly-rated schools under the Boston mechanism, but this gap disappears after the switch to deferred acceptance. Our model and empirical findings support a new argument in favor of strategy-proof mechanisms: strategy-proofness equalizes opportunity *inside* the centralized system by neutralizing the effect of inequality in opportunities *outside* the system. Our results provide a new foundation for using strategy-proof mechanisms in practice: they provide all market participants with an equal opportunity to receive the most popular public resources.

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1 Introduction

Participants in matching markets often have access to outside options that the market designer cannot control. In markets that match individuals to public housing, participants may differ in their access to private housing. In markets that match kidney recipients to deceased donors, recipients differ in their access to family or other living donors. In markets that match job seekers to public-sector jobs, some workers may have access to private-sector alternatives. And so on. Differences in access to options *outside* the centralized market may affect the way market participants behave *inside* the system. This paper shows, using economic theory and a unique empirical setting, how market design choices can exacerbate or mitigate such pre-existing inequalities.

We focus on a canonical application of market design theory in which equity is an important concern: centralized public school choice. Students participating in the same centralized choice systems often have very different options outside the choice process. These include the option to pay for and enroll in private schools, the option to participate in parallel choice systems for different types of schools, and the option to take a guaranteed spot in a neighborhood school. To the extent that students' income and where they live are correlated with their access to outside options, heterogeneous outside options provide a channel through which socioeconomic inequalities can affect assignment outcomes.

Our central finding is that manipulable mechanisms– those that reward participants for submitting applications that do not reflect their true preferences over schools– give participants with better *outside* options an advantage *inside* the centralized system, while strategy-proof mechanisms do not. The paper makes two contributions. First, we develop a theoretical model that illustrates how manipulable mechanisms can change application behavior, so that students with outside options are more likely (relative to strategy-proof mechanisms) to attend the most popular schools. Second, we test the predictions of this model using data from a unique quasi-experiment, in which we observe choice behavior for students with and without outside options before and after a change from the manipulable Boston mechanism to the Deferred Acceptance (DA) mechanism in the New Haven, Connecticut school district. Consistent with our theoretical predictions, the change in assignment mechanism closes the gap in rates of application to high-quality, sought-after schools by outside option access.

Our findings provide a new argument for using strategy-proof mechanisms in school choice and other settings. Economists often find strategy-proof mechanisms desirable because they reduce participation costs, prevent agents from making strategic errors (Abdulkadiroglu et al., 2006; Pathak and Sonmez, 2008) and the resulting outcome does not depend on agents' higher-order beliefs (Vickrey, 1961; Wilson, 1985; Li, 2017). We show an additional benefit: strategy-proofness neutralizes the effect of inequality in outside options, and provides all market participants with an equal opportunity to receive the most popular public resources. This benefit of strategy-proofness may accrue even if participation costs are small, strategic errors are unlikely, and beliefs are accurate.

The intuition underlying our model is that under a manipulable school choice mechanism, access to an outside option for a subset of students can change application behavior, so that students with outside options are more likely to attend the most popular schools. This interaction between manipulability and the presence of the outside option makes students with outside options better off, potentially at the expense of those without outside options. By contrast, under a strategy-proof mechanism, access to an outside option does not change application behavior. We model asymmetric access to schools outside of the centralized setting as an outside option that is in the choice set of some students, but not all.[1] In this setting, two students with identical preferences for centralized school options can have different preferences for the outcome of being unassigned by the mechanism.

Our first result proves that, with asymmetric outside options, the ex ante Pareto improvement (over the strategy-proof allocation) from the Boston mechanism predicted by Abdulkadiroglu et al. (2011) need not be realized. In particular, a manipulable choice mechanism always improves outcomes for students with outside options, while potentially reducing welfare for students without outside options. In that sense, manipulable mechanisms are regressive.

Our second result considers a simpler setting, where students with the outside option are *single-minded*, meaning that they only prefer the most popular school in the centralized system to their outside option. We show that manipulable assignment mechanisms endogenously segregate the schools in the centralized system by increasing the fraction of students with the outside option who go to the most popular school.

After stating our theoretical results, we assess the empirical importance of the proposed relationship between outside options and manipulability using data from a centralized school choice system. Heterogeneous outside options are relevant for choice behavior in a wide variety of settings. Some students participating in centralized public school choice may have the option to attend private schools, while others may not. In districts that have separate applications for public, charter, and exam schools, such as New York City, students holding or anticipating admissions offers to one type of school effectively have an outside option when they apply to other systems. Students may also have heterogeneous public outside options if they have access to guaranteed neighborhood assignments outside the centralized system, or

[1]The term "outside option" is sometimes used to denote the option of declining an assigned seat. We assume this option is available to all students. Instead, we use the term "outside option" to refer to seats in schools that are not part of the centralized system. These seats are not under the control of the designer, and therefore need not be available to all students.

if some students have the option to remain enrolled in their current school.

In practice, however, understanding the effect of heterogeneous outside options and how this effect varies with the school assignment mechanism is difficult. The first challenge is data limitations. Most empirical studies of school choice rely on records of applications submitted to centralized choice systems, which are not usually informative about what students would do if they were not assigned to any school. Another challenge is that outside options are not randomly assigned. Access to desirable outside option schools may be endogenous to the availability of inside option schools, or otherwise correlated with preferences over these schools. Understanding how the effects of choice depend on the interplay between mechanism manipulability and heterogeneous outside options requires some way of inferring how otherwise similar choice participants with different outside options behave under different assignment mechanisms.

The setting we consider—Kindergarten choice in the New Haven, Connecticut public school system—has two institutional features that help us overcome these challenges. First, students have heterogeneous outside options and we can observe these options. Some pre-Kindergarten schools in New Haven allow students to continue to elementary grades without entering the centralized Kindergarten choice process, while others do not. Students in pre-Kindergartens that do not have the outside option to continue and who are not placed in the centralized process are administratively assigned to a school with excess capacity. Second, New Haven public schools changed the assignment mechanism used for centralized choice from the manipulable Boston mechanism to a (truncated) Deferred Acceptance mechanism in 2019.

These two features allow us to implement a difference-in-differences design that compares choice behavior for students with and without the outside option both before and after the change from Boston to DA. The outcome we focus on is families' propensity to list schools with high scores on state accountability measures on their applications. Though not all families agree that these schools are the most desirable, high-achieving schools consistently receive more applications.

Our findings are consistent with predictions from the model. Under Boston, choice applicants with continuation options are 18.5 percentage points more likely to list a school in the top tercile of the achievement distribution first on their choice application than applicants without a continuation option. Students with the outside option listed a first choice school with, on average, a 0.66 SD higher accountability score than students without the outside option. Under DA, we observe no difference in the rate at which students list top tercile schools first, and the gap in mean accountability score at the first-ranked school falls to 0.20. Looking across all application ranks, we see evidence that the accountability scores of schools at each rank depend less on access to the outside option under DA than under Boston.

Related Work. Abdulkadiroğlu and Sönmez (2003) first modeled school choice as a mechanism design problem. Several papers consider how outside options play a role in determining assignments by centralized school choice mechanisms. Pathak and Avery (2015) study parallel centralized markets when households make joint residential and school choices. In contrast to our paper, their focus is not on comparing assignment outcomes across choice mechanisms with different incentive properties. Calsamiglia et al. (2017) use a theoretical model that endogenizes school quality as a function of peer quality to study sorting and segregation under the Boston mechanism as a result of private school options. Our model focuses on welfare implications, and is more stripped down to highlight the interaction between outside options and incentive properties of the mechanism. Furthermore, our results hold within a more general class of school choice mechanisms.

Abdulkadiroglu et al. (2006) and Pathak and Sonmez (2008) argue in favor of deferred acceptance based on the argument that it 'levels the playing field,' in the sense that it is less likely to disadvantage 'unsophisticated' students.[2] Their model predicts that less privileged students are behavioral agents who are less sophisticated, thus they are *less* likely to strategize. In our model, students without the outside option are *more* likely to strategize. Of course, the reality could be somewhere in between. Our empirical setting, however, suggests that students without outside options are more likely to strategize.

Our model is a generalization of Abdulkadiroglu et al. (2011). We maintain their assumption that schools have no priorities over students. Troyan (2012) points out that relaxing this assumption can also change the welfare implications of switching between the Boston Mechanism and deferred acceptance. In a concurrent paper, Shorrer (2019) develops an alternate theoretical framework to analyze optimal applications in school choice settings, and proves that students with better outside options have stronger incentives to apply aggressively in his setting.

On the empirical side, the key innovation in this paper is that we study the change from Boston to DA using data from a district where such a change took place. Several recent papers predict the welfare effects of a switch from Boston to DA using data generated under the Boston mechanism (De Haan et al., 2015; Agarwal and Somaini, 2018; Kapor et al., 2020; Calsamiglia et al., 2020). These papers predict counterfactual application choices under DA using simulation approaches. Calsamiglia and Güell (2018) study the Boston mechanism in Barcelona. They find that naive application behavior is associated with richer and better educated parents. This finding is consistent with the predictions of our model, since we would generally expect higher-income students to have more access to private-school outside options. We build on this work with our theoretical and empirical examination

[2] In a new paper, Babaioff et al. (2018) show that this result relies on schools' strict priorities—with coarse priorities, the results can go either way.

of a mechanism change, which contrasts to Calsamiglia and Güell (2018)'s focus on changes in priority groups within the Boston mechanism. Our focus on the change in the mechanism also contrasts with Kapor et al. (2020), which considers the equity impacts of changes in aftermarket options holding the (strategyproof) mechanism fixed.

The only paper we are aware of that reports what happened before and after an observed change in a school assignment mechanism is Pathak (2017), which documents changes in first place assignment shares following Boston's 2005 mechanism change, and also presents descriptive statistics on changes in submitted applications following a 2009 mid-process change in the mechanism used to allocate spots in Chicago exam schools. The scarcity of empirical work describing the consequences of mechanism changes highlights the importance of using those we do observe to describe and interpret their behavioral effects.

An interesting feature of our empirical findings is that they are qualitatively consistent with model predictions even though the assumptions needed to make sharp predictions in the theoretical analysis do *not* hold in the empirical application. As in Abdulkadiroglu et al. (2011), to obtain useful theoretical predictions we assume that all students share ordinal preferences over schools inside the centralized system. While some schools in our empirical setting are more popular than others on average, there is no evidence that all students share ordinal preferences in the New Haven school system (or in any other school system). The fact that our model predictions nevertheless go through suggests that theoretical analyses of the Boston mechanism may retain empirical relevance despite invoking some unrealistic assumptions.

More broadly, we find that agents' chances of receiving the object depends jointly on their outside options as well as the allocation mechanism. In this light, our work also has kinship with the matching market design literature that is concerned with the fair allocation of objects, ensuring that who gets what is not completely determined by their ability to pay (Prendergast, 2016, 2017; Budish, 2011).

2 Model and theoretical predictions

This section lays out our theoretical model. A simple example illustrates the key intuition before we state the more general model.

2.1 An illustrative example

Consider a setting with three schools, $\{s_1, s_2, s_3\}$, that assign seats through a centralized school choice mechanism, and one outside option school, s_o. There is a continuum mass 1 of students, half of whom have access to s_o. Students know their own types, but have only probabilistic knowledge of others' types. Each school in the centralized system has capacity $q = 1/3$, and they break ties uniformly at random. All students agree on the desirability of the schools in the centralized system. However, students with the outside option only prefer s_1 over being unassigned (their preferences over assignments are $s_1 \succeq \emptyset \succeq s_2 \succeq s_3$), while students without outside options would prefer s_2 and s_3 over being unassigned (their preferences are $s_1 \succeq s_2 \succeq s_3 \succeq \emptyset$). We restrict attention to the case where students without outside options have the same cardinal valuations: their valuation of attending s_i is v_i. We assume $v_1 = 1$ and $v_3 = 0$, and let $v_2 = v$, where $0 \leq v \leq 1$.

Comparing outcomes under two well-known school choice mechanisms—student-proposing deferred acceptance and the Boston mechanism—illustrates how outside options interact with manipulability. Under deferred acceptance, all students report truthfully, which leads to the probability distribution over outcomes in Table 1.

School	s_1	s_2	s_3	s_o
Outside option	1/3	0	0	2/3
No outside option	1/3	2/3	0	0

Table 1: Outcomes under deferred acceptance with random tie-breaking.

Under the Boston mechanism, students with the outside option have no incentive to misreport their preferences, while students without the outside option have an incentive to report different preferences. In this example, since s_3 is available to everyone, each student without the outside option effectively has two pure strategies available to him: to be truthful and report $s_1 \succeq s_2 \succeq s_3 \succeq \emptyset$, or to be strategic and report $s_2 \succeq s_1 \succeq s_3 \succeq \emptyset$. To ease exposition, let $v \geq 1/2$. Then, solving for the symmetric (Bayesian) Nash equilibrium, we can show that the probability of reporting truthfully is

$$p = \frac{1-v}{1+v}.$$

Therefore, as long as $v \geq 1/2$, students without the outside option have an incentive to play the non-truthful strategy with positive probability. If all students play the truthful strategy, then each student without the outside option has an incentive to deviate and get assigned to s_2 with probability 1.

When students without the outside option assign a positive probability to the non-truthful strategy, they decrease their likelihood of enrolling in s_1, which, in turn, increases the likelihood of students with the outside option being offered a seat at s_1. On the other hand, in any symmetric equilibrium, students without the outside option can never increase their likelihood of being accepted to s_2, relative to their likelihood of acceptance under deferred acceptance, because they are not competing for seats with students who have access to the outside option. As a result, competition among students without the outside option only increases their likelihood of going to s_3 at the cost of decreasing their likelihood of going to s_1. Manipulability makes students with the outside option better off, and students without the outside option worse off, relative to a strategyproof mechanism.

A striking scenario occurs when students without the outside option are close to indifferent between s_1 and s_2, or $v = 1 - \epsilon$, for an arbitrarily small $\epsilon > 0$. The truthful strategy profile cannot be an equilibrium under the Boston mechanism because, under the truthful strategy profile, any of the students without the outside option have an incentive to deviate. This within-type competition for seats in s_2 continues until, as $\epsilon \to 0$, we have $p \to 0$ in equilibrium. Consequently, students with the outside option go to s_1 with probability $2/3$, and students without the outside option go to s_1 with probability zero, to s_2 with probability $2/3$, and to s_3 with probability $1/3$.

In this example, the manipulability of the Boston mechanism makes it more likely that students with the outside option attend the most popular school, at the expense of students without the outside option. In this sense, manipulability *endogenously segregates* the school system according to access to the outside option.

2.2 Model

We can generalize the intuition from the simple example in the previous section in two ways. First, we can consider strategy-proof versus manipulable mechanisms, going beyond the comparison between deferred acceptance and the Boston mechanism. Second, we can allow for more general types of preferences over schools. Our model generalizes the model in Abdulkadiroglu et al. (2011) by including outside options into their framework.

We consider a model of school choice in which each student knows his own outside option, but only has probabilistic knowledge of other students' outside options. Suppose there is a continuum mass 1 of students. A student is described by his type

$$\theta \in \Theta = \{\text{outside option}, \text{no outside option}\},$$

distributed according to

$$p(\theta) = \begin{cases} \eta & \text{if } \theta = \text{outside option} \\ 1 - \eta & \text{if } \theta = \text{no outside option} \end{cases}$$

for some $\eta > 0$ that is common knowledge. For ease of notation, we use w and w/o to denote students with and without the outside option, respectively.

There is a set $S = \{s_1, s_2, \cdots, s_M\}$ of schools that are part of the centralized system, where $M \geq 3$, and where each school j has capacity $0 < q_j < 1$. There is one school outside of the centralized system, s_o, with infinite capacity. We assume schools have no priorities over students and break ties randomly.

We assume student i has vNM utility value v_j^i when he attends school j, where $v^i = [v_1^i, v_2^i, \cdots, v_M^i, v_o^i]$ is the **valuation vector** of student i. Each student i draws a valuation vector v^i from a finite set $\mathcal{V} = \{(v_1, v_2, \cdots v_m, v_o) \in [0,1]^{M+1} | v_1 > v_2 > \cdots v_l > v_o > v_{l+1} > \cdots > v_M\}$. This setup means that all students agree on their ordinal preferences, but they may have different cardinal preferences. The probability of a valuation vector $v^i \in \mathcal{V}$ is $f(v^i)$, where $\sum_{v \in \mathcal{V}} f(v) = 1$. We assume $f(\cdot)$ is common knowledge. To make sure that the least popular school is not irrelevant, we assume $\sum_{j \in S \setminus s_M} q_j < 1$. In addition, we assume $\sum_{j \in S} q_j = 1$.

For a student with access to s_o, the truthful rank-order list over schools inside the centralized system is $s_1 \succ s_2 \succ \cdots s_l \succ \emptyset \succ s_{l+1} \succ \cdots \succ s_M$ (since he prefers his outside option to other schools in the centralized system). For a student without access to s_o, the truthful ranking is $s_1 \succ s_2 \succ \cdots \succ s_M \succ \emptyset$.

A **strategy** is a mapping $\sigma : \Theta \times \mathcal{V} \to \Delta(\Pi)$, where Π is the set of all rank-order lists of S (potentially with truncation) and $\Delta(\Pi)$ is the set of probability distributions over Π. We focus on symmetric strategies in which students (of the same type) follow the same strategy.

An (ex ante) **assignment** is a matrix $\mathbf{X} = [\mathbf{X}(\theta, j)]$, for $\theta \in \Theta$ and $j \in S$. An assignment describes the allocation of students to schools in the centralized system. In particular, for any school s it assigns a probability $\mathbf{X}(w, s)$ to students with the outside option and a probability $\mathbf{X}(w/o, s)$ to students without, which represents the ex ante probabilities that these two types of students are assigned to school s. The capacity constraints require that $\eta \mathbf{X}(w, j) + (1-\eta)\mathbf{X}(w/o, j) \leq q_j$ for all $j \in S$. An **assignment mechanism** (or simply, a mechanism) is a systematic procedure that results in an assignment.

Student-proposing Deferred Acceptance Mechanism. It is well-known that this mechanism is strategy-proof. Hence, students with the outside option report $s_1 \succ s_2 \succ \cdots s_l \succ \emptyset \succ s_{l+1} \succ \cdots \succ s_M$ (since they prefer their outside option to other schools in the centralized system), and students without the outside option report $s_1 \succ s_2 \succ \cdots \succ s_M \succ \emptyset$. Assuming that schools break ties in a symmetric way, the deferred-acceptance mechanism generates the assignment matrix

$$\mathbf{X_{DA}} = \begin{bmatrix} q_1 & q_2 & \cdots & q_l & 0 & 0 & \cdots & 0 \\ q_1 & q_2 & \cdots & q_l & \frac{q_{l+1}}{1-\eta} & \frac{q_{l+2}}{1-\eta} & \cdots & 1 - \sum_{j=1}^{l} q_j - \sum_{j=l+1}^{M-1} \frac{q_j}{1-\eta} \end{bmatrix}.$$

We consider the class of symmetric and monotone assignment mechanisms that are non-wasteful. We call a mechanism *monotone* if ranking a school higher does not decrease your chance of being admitted there. We call a mechanism *non-wasteful* if no student who would have preferred an unassigned seat from one of the schools in the centralized system is unassigned to that seat. We call a mechanism *symmetric* if it has a symmetric tie-breaking rule. An assignment mechanism is *standard* if it is monotone, non-wasteful and symmetric.

To state the main theorem, we need one more definition.

Definition 1 *A student i **always prefers** an assignment mechanism A to an assignment mechanism B if he gets a weakly higher expected utility under any symmetric equilibrium of mechanism A than under any symmetric equilibrium of mechanism B.*

We are now ready to state the main result. We prove this theorem in Appendix A.

Theorem 1 *A student i always prefers a manipulable standard mechanism to strategy-proof mechanisms if and only if he has an outside option.*

Theorem 1 shows that the result presented in Abdulkadiroglu et al. (2011) will not go through for all students; only students who have an outside option are guaranteed to be better off under manipulable mechanisms (and the Boston mechanism, in particular). In the proof of the theorem, we show that this claim is true for any manipulable standard mechanism. Our example in section 2.1, on the other hand, shows that there might be plausible cases in which students without the outside option are strictly worse off under manipulable mechanisms. Is it the case that without the outside option students are always worse off under the Boston mechanism? *No*—we provide a counterexample in example 1.

Example 1 *Suppose there are three schools in the centralized system each with capacity $1/3$, and suppose all students value those schools at $v_1 = 1$, $v_2 = 0.9$ and $v_3 = 0$. Suppose there is also a school outside the centralized system that students with the outside option value at $v_o^w = 0.9 - \epsilon$ for some $\epsilon > 0$. Let $\eta = 2/3$. Then, for sufficiently small ϵ, a symmetric equilibrium of the Boston mechanism is for students with the outside option to report $s_1 \succ s_2 \succ \emptyset$, and for students without the outside option to report $s_2 \succ s_1 \succ s_3 \succ \emptyset$. Note that under these strategies, students with the outside option go to s_1 with probability $1/2$ and to s_o with probability $1/2$, while students without the outside option go to s_2 with probability 1. For sufficiently small ϵ, no deviation can make any student better off.*

On the other hand, under deferred acceptance, all students go to s_1 with probability $1/3$ and to s_2 with probability $1/3$. Students without the outside option go to s_3 with probability $1/3$, while students with the outside option go to the outside option school with probability $1/3$. It is easy to check that all students are strictly better off under the Boston mechanism.

While our opening example in section 2.1 shows that students without the outside option are strictly worse off under the Boston mechanism, the previous example shows that this is not a necessary consequence of manipulability. Whether students without the outside option prefer deferred acceptance over the Boston mechanism depends on the structure of preferences. Asymmetry in access to outside options can therefore change conclusions about welfare improvements previously associated with manipulable school choice mechanisms by, for instance, Abdulkadiroglu et al. (2011), who show that in a world without meaningful outside options the Boston mechanism makes all students weakly better off.

One may be curious to see whether there are some plausible conditions under which students without outside options always prefer the deferred-acceptance mechanism to the Boston mechanism. To introduce one such condition, we first introduce the notion of a 'single-minded' student; in words, a student is single-minded if he only wants to attend the most popular school inside the centralized system or else prefers the school outside the centralized system.

Definition **2** *A student i is **single-minded** iff $v_1^i \geq v_o^i \geq v_2^i \geq \cdots \geq v_M^i$.*

The following theorem identifies one condition under which students without outside options always prefer deferred acceptance.

Proposition **1** *Suppose students with the outside option are single-minded, and all students without the outside option have the same valuation vectors. Then, students without the outside option always prefer the deferred-acceptance mechanism to the Boston Mechanism.*

Proof. When students with the outside option are single-minded, they will always report truthfully. Therefore, their ex ante probability of going to s_1 is at least q_1.

Next, note that all students without the outside option will play the same strategy, because we are studying the symmetric NE and they all have the same valuation vector. Now, if all students without the outside option report s_1 as their top choice with probability 1, they all have a q_1 chance of going to s_1, and by symmetry, the outcome is the same as under deferred acceptance. Suppose, on the contrary, that in the symmetric NE students without the outside option assign a non-zero chance to a rank-order list that does *not* put s_1 at the top, as in our illustrative example. Then, the probability that students without the outside option go s_1 would be strictly less than q_1 and they are all strictly worse off. ■

The above theorem states that if we assume that students without the outside option have homogeneous intensity of preferences, then single-minded students with the outside option are all better off under the Boston mechanism, while students without the outside option are all worse off.

We would like to emphasize that the assumption of same valuations for students without the outside option is essentially shutting down the channel by which the Boston mechanism enhances efficiency. Nevertheless, this is not a knife-edge result. One can in principle use the above theorem and a continuity argument to show that for "small enough" variations in preferences, the same insight goes through. In other words, the Boston mechanism hurts students without the outside option by forcing them to compete more both within type and with students of the other type. As long as the preference-signaling gains from the mechanism are smaller than the cost, students without the outside option are worse off.

This result suggests that students with better outside options are more likely to attend the most popular schools in the centralized system when the mechanism is not strategy-proof. Hence, a direct prediction of our model is that manipulability can segregate students according to the constraints they face outside the centralized school system; that is, under a manipulable system, the most popular school in the centralized system will have more students with the outside option and fewer students without the outside option.

Proposition **2 (Segregation)** *In any symmetric Nash equilibrium produced by the Boston mechanism, the fraction of students with the outside option who attend the most popular school in the centralized system is weakly higher than their population share* η*, and the fraction of students without the outside option who attend the most popular school in the centralized system is weakly lower than their population share* $1 - \eta$*. In addition, these weak inequalities hold strictly for some parameters.*

3 Empirical Application

3.1 Setting

3.1.1 School choice in New Haven

We focus our empirical analysis on the prediction that access to an outside option should lead choice participants to list more desirable schools first under the Boston Mechanism but not under DA. We employ a difference-in-differences approach in which we compare choice behavior for students with and without outside options, before and after the change from Boston to DA.

We study centralized public school choice in New Haven, Connecticut. Two features of the New Haven choice system make our empirical analysis possible. First, we are able to identify participants in the Kindergarten choice process who have access to schooling options outside the centralized system. Second, we observe choice behavior under both the Boston and DA assignment mechanisms. Each of these features is rare in empirical studies of school choice. Both are critical for evaluating predictions about behavior that relate to both the availability of an outside option and the incentive properties of the centralized assignment mechanism.

The New Haven Public School system (henceforth NHPS) uses a portfolio model of school choice. Students choose between "neighborhood" schools with geographically defined priority zones, magnet schools with themes like the arts, classical studies, or STEM, and charter school options including some run by prominent "No Excuses" brand Achievement First. NHPS uses a centralized choice system to assign students to schools in all grades, from pre-Kindergarten (henceforth pre-K) through high school. Students typically submit applications in early March, receive placement decisions by April, and enroll in school in August. Some schools (known as interdistrict magnets) also enroll students from towns outside the district; these students apply through the centralized system as well. Once enrolled in a school, students can continue their enrollment through the last grade offered without having to reapply.

3.1.2 The choice mechanism and how it changed

From 2016 through 2018, NHPS assigned students to schools using the Boston mechanism.[3] Students could list up to four schools on their application. In 2019, NHPS switched to a (truncated) DA mechanism. Students could list four schools in 2019 and six schools in 2020. The change to DA took place in consultation with researchers and was based in part on evidence presented in Kapor et al. (2020) that the change would likely be welfare-improving; see New Haven Independent (2019) for details. As part of this change, choice administrators conducted outreach with the goal of communicating to choice participants that, under the new mechanism, the best approach was to list the schools you like in the order that you like them.[4]

Under both the Boston and DA mechanisms, schools had coarse preferences over students determined by neighborhood, sibling, and zip-code priority groups, with ties broken by random lottery draws. An important feature of the choice process in elementary grades is that students do not have neighborhood schools as outside options. Students wanting to attend a school in their zoned neighborhood must list that school on their choice application. Students who are not placed are administratively assigned to schools with excess capacity.

3.1.3 Outside options and centralized choice

We identify students with options outside the centralized choice system using a unique feature of the Kindergarten choice process in New Haven. In New Haven, some elementary schools start in pre-K, while others start with Kindergarten. There are also stand-alone preschools that offer pre-K but are not affiliated with an elementary school. Students enrolled in pre-K at elementary schools have the option to continue on to Kindergarten at their school without going through the centralized

[3]Prior to 2016, NHPS used a Boston-like mechanism known as the New Haven Mechanism; see Kapor et al. (2020) for details.

[4]NHPS also provided application guidance under the Boston mechanism. This advice was necessarily less concrete. The choice platform, application forms, and informational materials that students used to submit their applications did not otherwise change over this period.

choice process. Students enrolled in standalone NHPS pre-Ks or students entering NHPS for the first time in Kindergarten do not have this continuation option. If students with the continuation option want to switch schools—for example, to a school that does not offer pre-K—they must enter the centralized choice process.

Our empirical analysis compares the Kindergarten choice behavior of students with and without the option to continue at their current school, before and after the switch to DA. There are three important things to understand about this environment. The first is why a family might want their child to change schools for Kindergarten when that student has the option to continue at their current school. In New Haven, some of the most sought-after schools do not enroll students before Kindergarten. These schools include the Achievement First charter school and the zoned neighborhood schools with the highest state accountability scores.

The second is how our setting relates to the theoretical model and to other settings in which heterogeneous outside options might affect choice behavior. We believe the theoretical exposition carries over well. The option to continue at one's current school eliminates the risk of being administratively assigned to a school with excess capacity (which may be very undesirable) if a student is not placed through the centralized process. Because this is a public outside option, students who have access to it are likely quite different from students with access to private outside options in this and other districts. Students with continuation options in our setting are likely more similar to students with other kinds of public outside options.

The third relates to timing. The plan to change from Boston to DA was announced in January 2019, roughly three months before the first set of school assignments conducted using the new mechanism. To the best of our knowledge there were no public discussions of the change before this point. Families were therefore unable to adjust their pre-kindergarten enrollment choices in anticipation of the mechanism change.

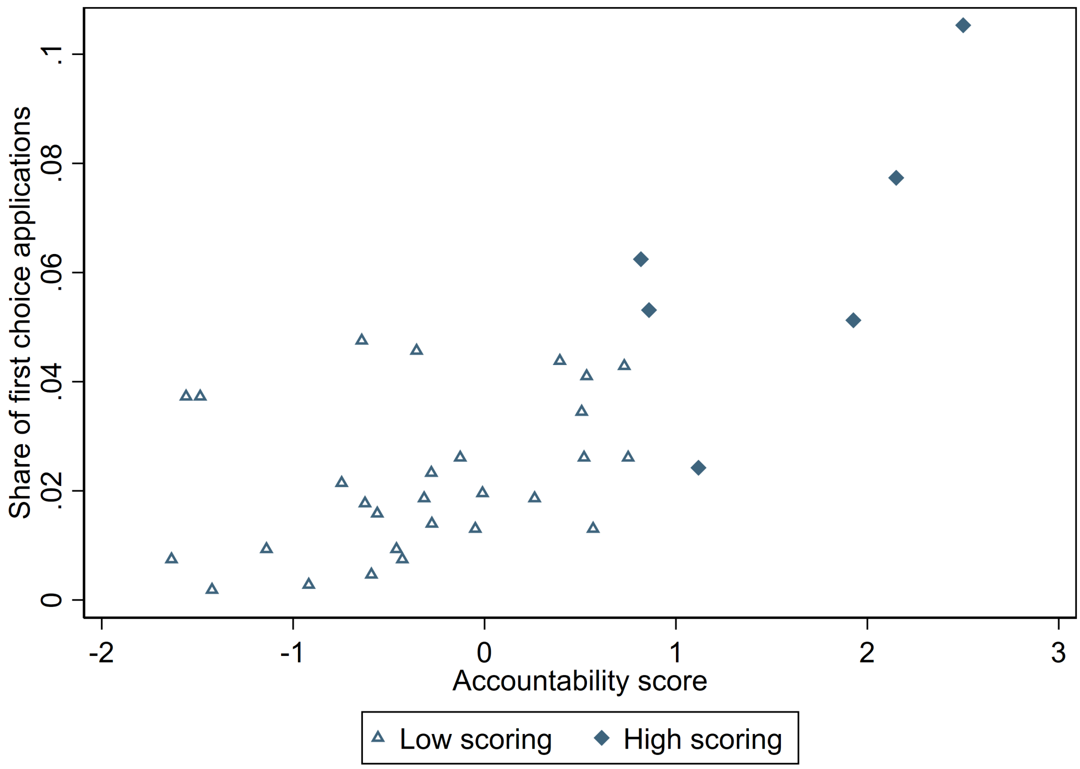
3.1.4 Describing schools

In our theoretical model, schools are vertically differentiated, with choice participants agreeing on the preference ordering. The NHPS choice system includes elements of horizontal differentiation over which we would not expect students to share preferences. However, there are also large differences in academic achievement across schools. Prior research suggests that families making school choices act as if they value school-level academic achievement, whether because they value test score production itself, because they value peer attributes associated with higher test scores, or because they observe accountability measures that result from higher test scores (Figlio and Lucas, 2004; Hastings and Weinstein, 2008; Abdulkadiroğlu et al., 2020)

We use state accountability scores to measure vertical differentiation across schools. We focus on the headline school-level scores, which are weighted averages of a number of academic and non-academic subscores, including test score levels in math and reading, test score growth in math and reading, attendance rates, and on track graduation rates. The academic growth measures receive double the weight of all other criteria, and achievement for low-income students is upweighted. See Edsight (2020) for details.

We take school score data for the years 2014 through 2018, compute the mean value for each school over the period, and then standardize values in the sample of schools offering Kindergarten to have mean zero and standard deviation one.[5] In some of our analyses we use a binary classification of schools as "high achieving," a designation that includes all schools in the top tercile of the application-weighted sample of the school achievement distribution. We emphasize that this measure of achievement, while useful for our analysis, does not reflect our judgment of the value or importance of any particular school.

Figure 1: Share of first choice applications by accountability score



This figure shows the share of first-choice applications (vertical axis) by school-level accountability scores (horizontal axis). See Section 3.1.4 for details.

The schools identified as high achieving schools via this metric correspond roughly to those perceived as most desirable by district families participating in Kinder-

[5] 2014 is the first year for which accountability scores are available in their current form, and 2018 was the most recent year available when we performed our analysis.

garten choice. Figure 1 plots school accountability scores on the horizontal axis and the share of all 2019 first choice Kindergarten applications that the school received on the vertical axis. The six schools in the high achieving group include the five schools receiving the most Kindergarten applications. The schools with the highest accountability scores include the neighborhood schools in the highest-income neighborhoods, and the Achievement First No Excuses charter school branches. See Online Appendix D.1 for more details on schools and plots similar to Figure 1 for each application year.

3.2 Empirical approach

We use a difference-in-differences approach to evaluate how the change to a DA assignment mechanism affects the gap in application behavior between students who have outside options and students who do not. Our core specifications take the form

$$Y_{it} = \beta_0 + \beta_1 OO_i + \beta_2 DA_t + \beta_3 OO_i \times DA_t + \mathbf{x}_i\beta_4 + e_{it}, \tag{1}$$

where Y_{it} is the outcome of interest for individual i in year t, OO_i is an indicator equal to one if i has an outside option, DA_t is an indicator equal to one if the choice mechanism in year t is DA, and $\mathbf{x}_i$ is a set of predetermined covariates that we allow to vary across specifications. The coefficient of interest is β_3, the effect of the interaction between the mechanism and outside option availability. The outcomes we consider are descriptors of achievement levels at the first-listed school on a student's application. We estimate these specifications in the sample of students enrolled in NHPS pre-Ks who participate in the Kindergarten choice process.

Our goal is to estimate the differential effect of outside option availability due to the mechanism change. These specifications produce unbiased estimates under the assumption that choice behaviors would have evolved in parallel for students with and without outside options in the absence of the change. This assumption will be violated if the characteristics of choice participants with and without outside options changed differentially over time in ways that affect choice behavior, and if these differential changes are not captured by $\mathbf{x}_i$. The assumption will also be violated if other aspects of the choice environment change in ways that differentially affect students with and without outside options. We evaluate these assumptions to the extent possible in our data as part of the analysis below.

3.3 Data

We use data on school enrollment and choice participation between 2016 and 2020. We focus on families with four-year old students enrolled in their final year of NHPS pre-K. These students have the option to participate in the Kindergarten choice process. See Appendix D.1 for details of sample construction.

Table 2 presents descriptive statistics for this population. We split the sample by the mechanism in place when these families make their Kindergarten choices (if they make them). The left panel, labeled “Boston,” displays statistics for students enrolled between 2016 and 2018, while the right panel, labeled “DA,” displays statistics for students enrolled in 2019 and 2020.

Table 2: Sample descriptives of NH PreK4 students

	Boston				DA			
	All	All in K Lottery	OO in K Lottery	No OO in K Lottery	All	All in K Lottery	OO in K Lottery	No OO in K Lottery
I. Demographics								
Tract poverty rate	0.249	0.254	0.218	0.257	0.251	0.255	0.217	0.259
Female	0.477	0.476	0.395	0.482	0.489	0.500	0.537	0.496
Black	0.452	0.437	0.661	0.420	0.417	0.406	0.575	0.388
White	0.105	0.088	0.113	0.086	0.105	0.099	0.163	0.092
Hispanic	0.443	0.475	0.226	0.494	0.479	0.495	0.263	0.519
Special education	0.106	0.103	0.129	0.101	0.146	0.124	0.087	0.127
II. Choice participation								
Participate in K lottery	0.666	1.000	1.000	1.000	0.578	1.000	1.000	1.000
Has OO	0.238	0.068	1.000	0.000	0.283	0.094	1.000	0.000
Participate if OO	0.191				0.192			
Participate if No OO	0.815				0.731			
List high scoring school 1^{st}		0.352	0.524	0.339		0.322	0.325	0.322
1^{st}-listed school quality		0.409	1.024	0.365		0.321	0.499	0.303
III. Placements								
Any placement		0.963	0.879	0.969		0.958	0.887	0.965
Placement at high scoring school		0.278	0.371	0.271		0.295	0.287	0.296
N	2734	1822	124	1698	1471	850	80	770

This sample includes all students that are enrolled in a NHPS Pre-K school and eligible to apply to Kindergarten between 2016 and 2020. The panel includes students who apply (or could have applied) to Kindergarten between 2016 and 2018, when the Boston mechanism was in place. The DA panel includes students who apply (or could have applied) between 2019 and 2020. See text for variable definitions.

Panel I describes student demographics. As in many urban districts, students in New Haven come from relatively low-income neighborhoods and are mostly non-white. Students live in Census tracts where 25% of families are in poverty, well above the nationwide rate of 11%. Roughly half of students are female, and nearly 90% are Black or Hispanic, with the Hispanic share rising somewhat from the Boston period to the DA period. 10–15% of students are designated as special education students by the district.

Panel II describes how students participate in the Kindergarten choice process. 67% of students participated in centralized choice in the Boston period, with that figure falling to 58% in the DA period. The second column of each panel shows statistics for participants in the choice process. Choice participants have similar demographic characteristics to the full sample in both periods. In the Boston period, 24% of all students are enrolled in pre-Ks that give them the outside option to continue through elementary grades. This figure rises slightly to 28% in the DA period. As expected, choice participation is much lower for students who have the option to continue at their current school. Roughly 19% of students with outside options participate in the centralized process in both the Boston and DA periods, compared to 82% of other students in the Boston period and 72% in the DA period.

Columns three and four of each panel display statistics for students with and without outside options, conditional on choice participation. In both the Boston and DA periods, choice participants with an outside option are more likely to be Black and less likely to be Hispanic than participants without an outside option, and students with an outside option live in Census tracts with slightly lower poverty rates. The one demographic comparison that differs across periods is for share female; outside option students are less likely to be female in the Boston period but slightly more likely in the DA period. Overall, that observable differences in student characteristics by outside option status are similar in the Boston and IA periods provides some support for the assumption of no differential changes in choice behavior that underlies the difference-in-differences analysis. This is consistent with results below showing that controlling for student observable characteristics does not affect our findings.

3.4 Analysis

The last two rows of Panel II of Table 2 describe choice behavior and placement outcomes for choice participants. Our main difference-in-difference results can be read off of these sample statistics. Under the Boston mechanism, 52% of choice participants with an outside option list a high-achieving school first, compared to 34% of students without an outside option. Under DA, the figure is 32% for both groups. The gap in rates of listing a high-achieving school as a first choice that we observe under Boston disappears under DA. We obtain similar findings when we take the achievement z-score of the first-listed school as the outcome. Under Boston, the gap in our standardized measure of school quality between students with and without an outside option is 0.66; under Boston it is 0.20.

Table 3 presents these findings using the regression framework given in Equation 1. The first panel of Table 3 reports results with no controls, reproducing the difference-in-difference findings we obtained from visual inspection of Table 2. The observed differences are statistically significant, with t-statistics around 2.5. The second panel adds controls for demographic covariates listed in the upper panel of Table 2. These controls do not affect our point estimates or inference. The third panel adds additional controls for each students' neighborhood school zone. These controls also do not affect our findings. In general, the small differences in individual attributes we observe across periods are unrelated to the kind of choice behavior we are interested in here.

Table 3: Differences in the outside-option effect before and after the mechanism change to DA – *K lottery participants*

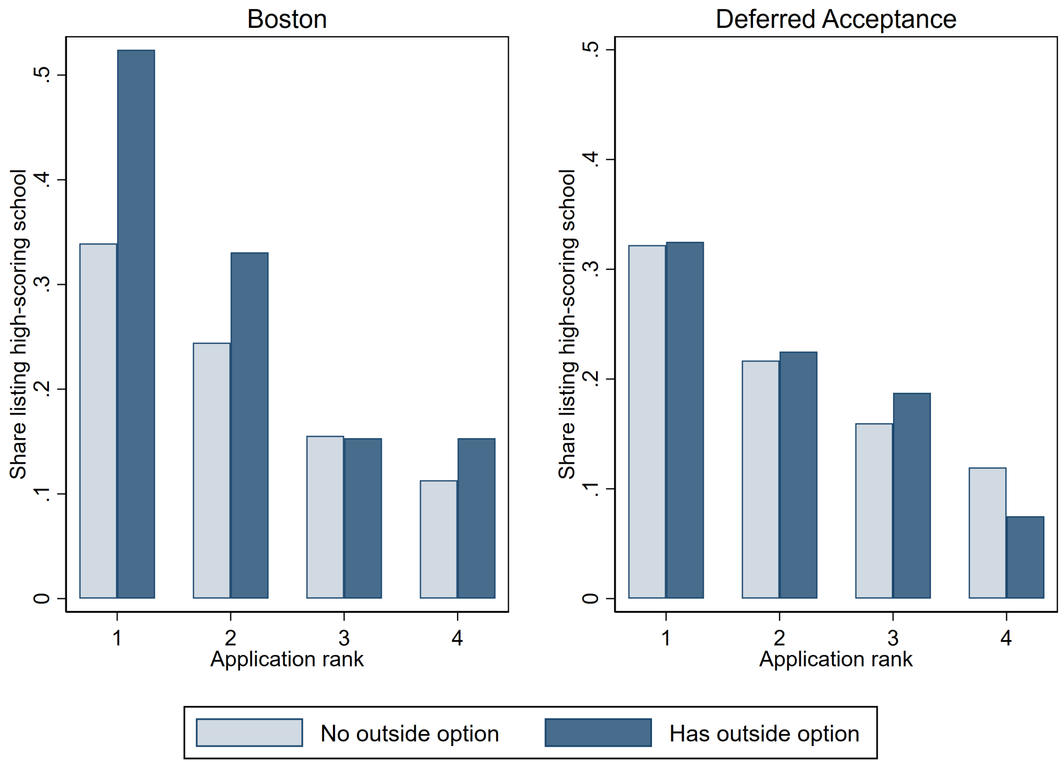
Controls	Observational sample: List high scoring school 1^{st}	Quality of 1^{st}-listed school
No controls		
Coeff.	-0.182	-0.463
Std. err.	(0.072)	(0.189)
Demographics		
Coeff.	-0.190	-0.468
Std. err.	(0.073)	(0.189)
+ *school zone*		
Coeff.	-0.195	-0.500
Std. err.	(0.072)	(0.187)
N	2672	2667

Results from difference-in-difference estimates of equation 1 for the outcome listed in each column. The coefficients reported are from the $OO_i \times DA_t$ interaction term. Sample: students who are enrolled in a NHPS Pre-K and participated in a kindergarten lottery between 2016 and 2020. Sample counts differ slightly across columns because one school does not have an accountability score; it is included in the left column as a non-high-scoring school. Robust standard errors in parentheses.

Figure 2 provides a graphical breakdown of the changing relationship between outside options and choice under the Boston and DA mechanisms. In each panel, the horizontal axis is the position on the application rank list. The left panel displays the share of students listing high-achieving schools in each rank position under the Boston mechanism, split by availability of the outside option. Students with the outside option are more likely to list high-achieving schools in both the first and second position on their rank lists (52% vs. 34% for rank 1, 33% vs. 25% for rank 2), with similar shares in the third and fourth positions. The right panel displays the same statistics, but for students applying under DA. Here, shares of high achieving schools are similar across all ranks (32% vs. 32% for rank 1, 22% vs. 23% for rank 2).[6]

[6]Both panels include blank student-rank combinations (other than the first rank, which cannot be left blank). These application-ranks are coded as zeroes: students who do not list a school do not list a high-achieving school. Online Appendix Figure B1 repeats the information in Figure 2 and also displays rates of non-application by rank. We present the simpler figure here for visual clarity.

Figure 2: Share of high scoring choices by application rank



Sample: Students enrolled in PreK4 at a New Haven Public School and participating in the NHPS Kindergarten lottery in a year in which either the Boston or Deferred Acceptance mechanism was in place. Under the Boston (Deferred Acceptance) mechanism 124 (80) students have the option to continue Kindergarten without application at their PreK school and 1698 (770) students don't have this option.

Panel III of Table 2 shows that differences in applications carry over to differences in placements. under Boston, 39% of students with outside options place at high-achieving schools, compared to 31% of students without an outside option. Under DA, this difference disappears.

3.5 Comparing empirical evidence to theory

Our difference-in-difference findings support the theoretical prediction that, relative to Boston assignment mechanisms, DA assignment should reduce the difference between students with and without an outside option in their propensity to list sought-after schools at the top of their applications. Applications from students with and without outside options are in fact more similar in terms of school quality across the full rank list under DA than under Boston.

We observe full or nearly-full convergence in choice behavior between students with and without outside options. This suggests that the distinction between strategy-proof DA with unlimited list length and the non-strategy-proof truncated DA mechanism that we observe in our setting (and that is common in school choice practice) may not have first-order implications for choice behavior, at least as it interacts with access to outside options.

One feature of our results that is arguably in tension with the theoretical predictions is that the gap in rates of listing high-achieving schools first between students with and without outside options closes after the switch to DA because the rate for the former group *falls*, not because the rate for the latter group *rises*. This contrasts with the example presented in Section 2.1, in which convergence takes place as students without outside options become more likely to list desirable schools first on their application. We note that the unreliability of causal inferences drawn from single-difference comparisons (such as the before-after comparison of rates of listing high-achieving schools for students without outside options) is precisely why researchers employ difference-in-differences designs in the first place. For example, a reduction in the relative preferences for high-achieving schools for all students over time would produce this type result for reasons unrelated to the test we are trying to conduct.

3.6 Additional analyses

3.6.1 Alternate approaches to inference and sample selection

In our main difference-in-differences analysis, our statistical tests are based on heteroscedasticity-robust standard errors that treat each individual as an independent observation. While we think the assumption that individual school choice applications are statistically independent is reasonable in our context, one common approach in difference-in-difference specifications is to cluster standard errors by the unit at which treatment is assigned (Bertrand et al., 2004). In our setting, that corresponds to the Pre-K school, because it is the schools where students are enrolled that determine outside option availability. Table B1 repeats the exercise in Table 3 with standard errors clustered at the level of the Pre-K school. In addition to clustered standard errors, we report p-values from a test of the null hypothesis of zero effect obtained using a clustered wild bootstrap-t procedure (Roodman et al., 2019). This procedure improves inference when the number of clusters is relatively small or clusters are of different sizes (Cameron et al., 2008; MacKinnon and Webb, 2017). Compared to our main estimates, these changes tighten our standard errors and yield p-values closer to zero.

Table B2 again repeats the exercise in Table 3, this time excluding the 2020 choice process from the sample. As noted above, applicants prior to 2020 could list four schools on their application, while in applicants in 2020 could list six. Changes in application length could alter strategic play separately from the change in assignment mechanism (Haeringer and Klijn, 2009). However, results that exclude the 2020 process are very similar to our main findings. The change in application length is not a compelling explanation for the changes in choice behavior we observe.

3.6.2 Event study

Event study specifications that compute year-by-year differences across groups before and after the change in the policy treatment are a common component of difference-in-difference analyses; in particular, they can provide a useful visual check for confounding "pre-trends" (Roth, 2018; Kahn-Lang and Lang, 2020). We observe three years of data under Boston and two years of data under DA, so this exercise is technically possible in our setting. Unfortunately, the small sample of individuals who participate in choice despite having an outside option available (204 students across all five years of data, as reported in Table 2) renders the results imprecise and uninformative. For transparency, we report our findings from this exercise in Online Appendix Table B3. We cannot reject the null hypothesis that the coefficient on OO_i in year t is equal to the coefficient on OO_i in the year preceding the policy change (2018) for any year $t \neq 2018$.

3.6.3 Random assignment of outside options

Our main difference-in-differences approach relies on the assumption that, in the absence of the mechanism change, changes in choice behavior would have been the same for students with and without outside options. The observable characteristics of the two groups are stable over the period in question, which suggests the assumption may be reasonable, but the pre-K that students attend is not randomly assigned and it is possible that group composition changes differentially in ways we cannot observe but that affect choice behavior.

To address this possibility, we considered additional analyses that exploit the random assignment of students to *pre-Ks* that arises from tiebreaking lotteries in the *pre-K* choice process. This approach compares Kindergarten choice behavior for students who were assigned to pre-Ks that give students the option to continue Kindergarten to choice behavior for students who were assigned to pre-Ks without that option, using the econometric strategy from Abdulkadiroğlu et al. (2017). This analysis is challenging because participating in Kindergarten choice is an endogenous response to pre-K placement, so we cannot condition on participation while also effectively leveraging the randomness of lottery assignment. Including non-participants in the analysis requires defining new outcome variables that do not condition on choice participation, and for which the relationship to theory is less clear. This approach also reduces statistical precision. Online Appendix B describes the exercise in detail. Our results are too imprecise to provide compelling evidence one way or the other; we report them for completeness and transparency.

4 Conclusion

This paper presents a theoretical framework under which manipulable mechanisms always make students with outside options better off, and may make students without outside options either better off or worse off. We combine this framework with data from an empirical setting that has the institutional features we need to test the positive predictions of our theory.

We think of our theoretical and empirical analyses as complements. The theoretical framework's sharp predictions clarify how to think about the role played by outside options under different mechanisms, and point to the relevant empirical setting for evaluating the role of outside options. However, in making the assumptions needed to analyze the equilibrium of manipulable mechanisms, we abstract away from some key aspects of the real world. Specifically, as in the model proposed by Abdulkadiroglu et al. (2011), we assume that students' ordinal preferences are perfectly correlated and schools have no priorities. A practical-minded reader might ask whether our theoretical results are valuable in understanding the role of outside options in real-world school choice settings. We therefore test the model's predictions in the empirical analysis, and find that in this real-world setting, the evidence is consistent with the model's predictions on application behavior when switching from a manipulable to a strategyproof mechanism.

We hope this mix of theory and data convinces the reader that there are plausible circumstances under which asymmetric access to substitutes outside of a centralized matching market can have distributional consequences. Hence, it is important to be aware of such parallel markets when designing marketplaces—"The game is always bigger than you think."

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A Proof of Theorem 1

Fix a (potentially manipulable) mechanism such as the Boston mechanism. We will refer to it as the *mechanism* in this proof. Let $\sigma_w^*(v)_{v\in\mathcal{V}}$ and $\sigma_{w/o}^*(v)_{v\in\mathcal{V}}$ be the symmetric equilibrium strategies of the unconstrained and constrained students, respectively. Let $\pi_j^w(v^i)$ be the probability that an unconstrained student with valuation v^i goes to school j in the Nash equilibrium. Define $\pi_j^{w/o}(v^i)$ similarly for constrained students. Our goal is to show that for a student i with outside option:

$$\sum_{j=1}^{l} \pi_j^w(v^i)v_j^i \geq \sum_{j=1}^{l} q_j v_j^i. \tag{2}$$

The left-hand side and the right-hand side are the expected utilities of students with outside option under the mechanism and deferred acceptance, respectively. Recall that s_l was the last public school that a student i with outside option preferred to his outside option.

Now, using a similar strategy as ACY's, suppose students with outside option follow a different strategy and 'mimic' the population: for an unconstrained student i with valuation vector v^i, with probability $\eta f(v)$ they play the strategy $\sigma_w^*(v)$ and with probability $(1-\eta)f(v)$ they play the strategy $\sigma_{w/o}^*(v)$. In playing $\sigma_{w/o}^*(v)$, students with outside option drop schools with value less than their outside option from the list. The probability of going to school j under this strategy is at least:

$$\sum_{v\in\mathcal{V}} \Big(\eta\pi_j^w(v) + (1-\eta)\pi_j^{w/o}(v)\Big) f(v) = q_j. \tag{3}$$

It is not too hard to see why this inequality holds. The left-hand side is the total number of students assigned to a school, in expectation, and in the equilibrium of our continuum economy, this should be equal to the capacity of the school.

The utility of a students with outside option from this new strategy is at least:

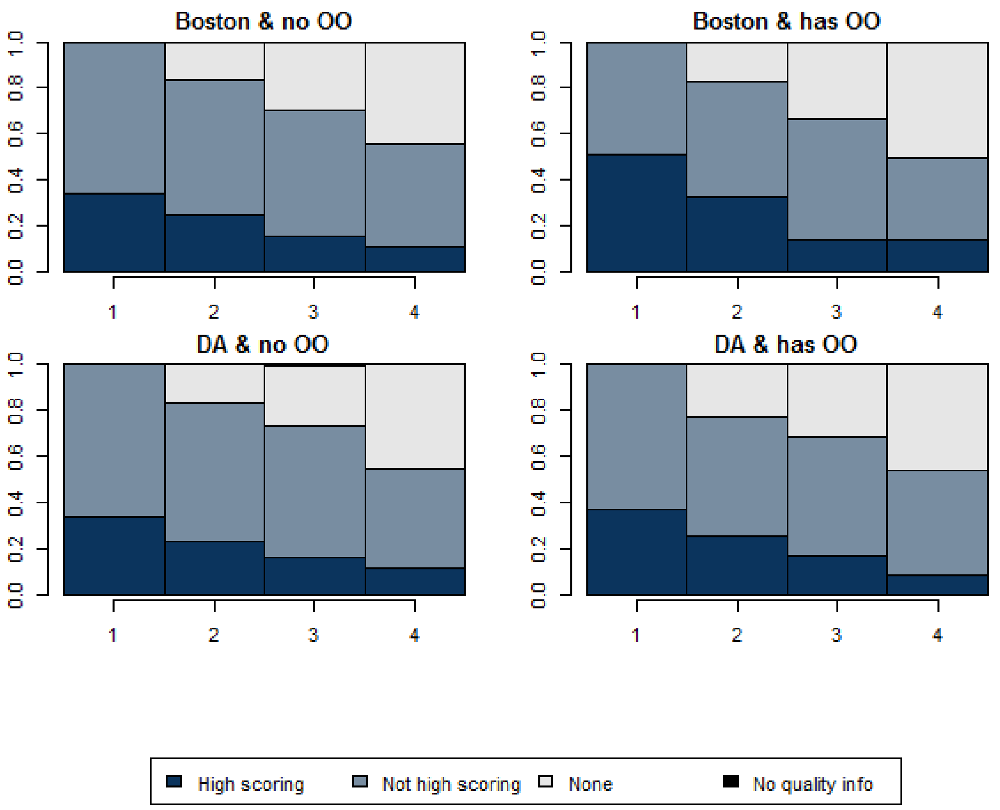
$$\sum_{i=1}^{l} v_j^i \Big(\sum_{v\in\mathcal{V}} (\eta\pi_j^w(v) + (1-\eta)\pi_j^{w/o}(v)) f(v)\Big) = \sum_{i=1}^{l} v_j^i q_j. \tag{4}$$

This is exactly their utility under deferred acceptance. Note that this is a lower bound on their utility, since by dropping those schools with value less than the outside option, they potentially increase their chances of going to schools they like.

This shows that an unconstrained student's utility under this new strategy is at least equal to his utility under deferred acceptance. Clearly, they must be weakly better off under the original equilibrium strategy σ_w^*, or else they could deviate to this new strategy that we just constructed. Hence, unconstrained students are weakly better off under any standard mechanism such as the Boston mechanism than under deferred acceptance. To complete the proof, note that if a student is not unconstrained, then our opening example shows that he can be worse off under a symmetric equilibrium produced by the Boston mechanism (compared to deferred acceptance equilibrium) so he will not always prefer the Boston mechanism. This completes the proof. ■

B Additional tables and figures

Figure B1: School accountability scores by application rank, mechanism, and outside option availability



Horizontal axis in each graph is application position. Vertical axis is the share of applications. Upper two graphs show results under the Boston mechanism, lower two graphs show results under the DA mechanism. Sample: Pre-K students applying to Kindergarten.

Table B1: Differences in the outside-option effect before and after the mechanism change to DA – *Standard errors clustered at PreK school level*

Controls	Observational sample: List high scoring school 1^{st}	Observational sample: Quality of 1^{st}-listed school
No controls		
Coeff.	-0.182	-0.463
Std. err.	(0.056)	(0.129)
p-value	[0.004]	[0.002]
Demographics		
Coeff.	-0.190	-0.468
Std. err.	(0.060)	(0.127)
p-value	[0.007]	[0.002]
+ school zone		
Coeff.	-0.195	-0.500
Std. err.	(0.049)	(0.105)
p-value	[0.001]	[0.000]
N	2672	2667

Results from difference-in-difference estimates of equation 1 for the outcome listed in each column. The coefficients reported are from the $OO_i \times DA_t$ interaction term. Sample: students who are enrolled in a NHPS Pre-K and participated in a kindergarten lottery between 2016 and 2020. Sample counts differ slightly across columns because one school does not have an accountability score; it is included in the left column as a non-high-scoring school. Standard errors in parentheses are clustered at the level of the PreK enrollment school. p-values are in brackets and are obtained using a wild clustered bootstrap-t procedure with 1999 resamplings (Roodman et al., 2019).

Table B2: Differences in the outside-option effect before and after the mechanism change to DA – *K lottery participants (excl. 2020 lottery)*

Controls	Observational sample: List high scoring school 1^{st}	Observational sample: Quality of 1^{st}-listed school
No controls		
Coeff.	-0.180	-0.618
Std. err.	(0.089)	(0.236)
Demographics		
Coeff.	-0.189	-0.626
Std. err.	(0.091)	(0.237)
+ school zone		
Coeff.	-0.214	-0.701
Std. err.	(0.089)	(0.227)
N	2313	2313

Results from difference-in-difference estimates of equation 1 for the outcome listed in each column. The coefficients reported are from the $OO_i \times DA_t$ interaction term. Sample: students who are enrolled in a NHPS Pre-K and participated in a kindergarten lottery between 2016 and 2019. Robust standard errors in parentheses.

Table B3: Event Study estimates of the differences in the outside-option effect by year – *K lottery participants*

Controls	Observational sample: List high scoring school 1^{st}	Observational sample: Quality of 1^{st}-listed school
Demographics + school zone		
OO $\times$2016	0.117	0.521
Std. Err.	(0.117)	(0.295)
OO $\times$2017	0.144	0.369
Std. Err.	(0.109)	(0.294)
OO $\times$2019	-0.123	-0.406
Std. Err.	(0.110)	(0.292)
OO $\times$2020	-0.078	0.054
Std. Err.	(0.113)	(0.305)
N	2672	2667

Event study estimates of equation 1 for the outcome listed in each column. The coefficients reported are from interactions between OO_i and year, with the 2018 interaction term normalized to zero. Sample: students who are enrolled in a NHPS Pre-K and participated in a kindergarten lottery between 2016 and 2020. Sample counts differ slightly across columns because one school does not have an accountability score; it is included in the left column as a non-high-scoring school. Robust standard errors in parentheses.

C Random assignment to outside options

C.1 Empirical framework

This section uses variation in assignment to schools with and without outside options generated by the pre-K application process to conduct additional tests of model predictions. The intuition is as follows: to gain admission to pre-K schools with Kindergarten continuation options, students must apply through the centralized assignment process in either their age 3 or age 4 year. We can exploit random variation in assignment outcome from the pre-K lottery to estimate the effect of having an outside option on Kindergarten choice behavior.

To conduct this analysis we use data on the universe of Pre-K applications to the centralized choice system in the years 2015 through 2019. Following Abdulkadiroğlu et al. (2020), we estimate instrumental variables specifications of the form

$$\begin{aligned} Y_i &= \beta D_i + \Gamma_1 \mathbf{P}_i + e_i \\ D_i &= \tau Z_i + \Gamma_0 \mathbf{P}_i + v_i \end{aligned} \tag{5}$$

where Y_i is a kindergarten choice behavior of interest, D_i is an indicator variable for enrollment in a school with an outside option in a student's age-4 pre-K year, Z_i is an indicator for assignment to a school with an outside option in the choice lottery, and $\mathbf{P}_i$ is a vector of dummies for each value of a student's propensity to be placed in a school with an outside option, given their application and the rules of the choice lottery, rounded to the nearest 0.001. The intuition is that assignment to an outside option school is random within groups defined by assignment propensity, as in Rosenbaum and Rubin (1983).

We compute propensity scores P_i using a resampling process that follows Agarwal and Somaini (2018) and Kapor et al. (2020), and builds on insights from Azevedo and Leshno (2016). This procedure relies on the observation that the IA and DA mechanisms are Report Specific Priority + Cutoff (RSP+C) mechanisms in the Agarwal and Somaini (2018) sense. In RSP+C mechanisms, the admissions chances for a given application to a given school depend on that application's report specific priority (i.e., a student's admissions priority group at the school) and the cutoff value for a school, which is chosen to reflect the school's capacity constraint.

The resampling process works as follows. Within each market (defined here by year and grade) we draw a large number ($N = 201$) of resampled markets from the population iid with replacement. Each resampled market is a list of individuals with a participation decision, a report if they participated in the lottery, and a priority at each school. In each resampled market, we solve for market-clearing cutoffs by running the assignment mechanism. Using j to index schools, the cutoffs $\left\{\pi_j^{(k)}\right\}_{k=1,\dots,N}$ allow us to calculate admissions chances for each individual i at each school j in each resampling k. See Kapor et al. (2020) for details of this procedure. We obtain our estimates of the propensity to be placed in a school with an outside option (the $\mathbf{P}_i$) by averaging school-specific placement probabilities over the resampled market-clearing cutoffs and then adding the placement probabilities for all outside-option schools.

With propensity scores $\mathbf{P}_i$ in hand, we compute estimates of Equation 5 separately in two samples, defined by the assignment mechanism used when applicants are scheduled to enter the Kindergarten choice process if they follow the normal grade progression. Students applying to age-4 pre-K programs in 2018 and earlier are in the IA sample, as are students applying to age-3 pre-K programs in 2017 and earlier. Other students are in the DA sample. Students who have not reached the age of scheduled kindergarten application by the time our panel ends in 2020 are excluded from the sample. This exclusion eliminates, for example, students applying to age-3 pre-K in 2019, because they are not yet eligible for Kindergarten choice in 2020, the last year of our data. We split the sample on the basis of the choice mechanism students experience in their Kindergarten choice process because that is age at which difference in continuation options across schools emerge.

C.2 Data description

Table C1 describes the data used for the lottery-based analysis. 3,363 individuals participate in a pre-K lottery during our sample period. Students occasionally participate in a pre-K lottery more than once if they want to switch schools between their age-3 and age-4 years, so the count of lottery participants at the student-year level is larger, equal to 3,941. 51% of participants are applying for placement in their age 3 year, denoted PK3 in the table. In what follows, all observations are at the level of the pre-K *application*. In our regression analysis we account for the multi-time applicants in inference by clustering standard errors at the person level.

Compared to statistics for our enrollment sample reported in Table 2, pre-K choice applicants are somewhat likely to be white, though Black and Hispanic students still make up more than 80% of the sample.

All pre-Ks to which students apply through the choice process are magnet schools that offer outside options. Students may enroll in the remaining stand-alone pre-Ks outside the choice process.[7] 26.7% of students who participate in choice receive a placement, and 22.1% enroll in a magnet (outside option) school the following year. 47.4% of participants enroll in some NHPS pre-K program; the additional students beyond those enrolled in magnets are attending stand-alone pre-Ks without continuation options. Remaining students attend non-NHPS pre-Ks or receive care of outside the formal schooling system.

[7] Non-magnet pre-K enrollment is decentralized; this contrasts with magnet pre-K choice and with all Kindergarten choice, as described in the main text.

22.9% of students enroll in a pre-K with an outside option in their age-4 year. These are students we define as having outside options in the Kindergarten choice process. The number differs from the 22.1% year-after enrollment share because some students applying to pre-K for the age-3 year may apply again in their age 4 year.

48.5% of applicants have interior placement probabilities. Our lottery-based analysis focuses on these students. Consistent with descriptive statistics for aggregate placement rates, the mean estimated placement propensity is 0.256; the mean for students with interior placement probabilities is 0.257.

Panel III of Table C1 describes how students in the sample participate in Kindergarten choice. 54% of Pre-K lottery participants go on to participate in Kindergarten choice. This figure rises to 64% for those without outside options.

The remaining rows of Panel III describe the outcome variables for Equation 5. A challenge here is how to define Kindergarten choice outcomes in a sensible way for the nearly half of pre-K choice participants who do not go on to participate in Kindergarten choice. This is critical for our analysis because, as we discuss in the main text, access to outside options dramatically changes rates of choice participation. This contrasts with our difference-in-differences tests in Section 3 of the main text, where we *condition* on participation in Kindergarten choice. We cannot do that here because Kindergarten participation is an outcome of pre-K lottery assignment, and conditioning on it would undo the benefits we get from randomization.

We define three outcomes of interest. The first is an indicator for listing a high scoring school first. This is equal to one for students who participate in Kindergarten choice and rank a high-scoring school first on their application. It is zero for other students, including non-participants. The second is an indicator for either applying and listing a high-scoring school first, or not applying and having a high-scoring school as your outside option. The third describes an outcome of the choice process–either placing at a high-scoring school through the process, or not placing and being defaulted in to a high-scoring outside option. We display descriptive statistics for these variables in the bottom three rows of Table C1.

C.3 Results

Table C2 reports findings estimates from Equation 5. The left three columns report results for students who reach Kindergarten age during the IA period, the middle three columns report results for students who reach the Kindergarten age during the DA period, and the third panel reports the difference between effect estimates in the two periods.

Panel I reports balance tests in which we estimate the first-stage equation with placement as the independent variable and predetermined student covariates as outcomes. We observe some imbalance in student observables in the IA period, with Black students more likely to be placed at a school with an outside option and Hispanic students less likely to be placed.

Our concerns about how any imbalance may impact our findings are limited, for several reasons. First, lottery placements are balanced on observables in the DA period, and we cannot reject the joint null of no difference in estimated effects of placement on observables across periods. Second, balance concerns arise only in lottery-reported race indicators, which have some missing data. We do not observe imbalance in census measures of tract-level demographics or poverty, where no data is missing. Third, the lack of balance we observe appears to arise solely from pre-K4 lotteries in two years, 2016 and 2017.[8] Supplementary analyses that exclude these years return results similar to what we report here.

Panel II reports first stage estimates of the effect of placement Z_i on enrollment in a (magnet) school with an outside option in students' age-4 year. In both the IA period and the DA period, placement raises the probability of age-4 enrollment by between 36 and 37 percentage points, on a base of 0.229 in the full sample.

In the IA period, students placed in schools with outside options are less likely to enroll in other NHPS schools. We do not see this tradeoff in the DA period. From the perspective of students' NHPS continuation options in the Kindergarten choice process, this distinction is not an important one. Both students who are enrolled in an NHPS school without a continuation option and students who are not enrolled in an NHPS school at all need to find a placement through the centralized process if they want to avoid an administrative placement.

Panel III reports instrumental variables estimates of the effects of enrollment in a school that offers an outside option during the age-4 year on students' Kindergarten choice behaviors. As expected, the option to continue in one's current school without having to go through the centralized choice process dramatically reduces the likelihood a student participates in the Kindergarten choice process. Magnet enrollment reduces participation in the centralized system by 71 percentage points in the IA period and 61 percentage in the DA period. These effects are each highly statistically significant. We cannot reject that the difference between them is zero at conventional levels.

Enrollment in a school with an outside option reduces the rate at which students *both* apply *and* list a high-achieving school first under both IA and DA. This is unsurprising given how much enrollment reduces application rates. We cannot reject a null of no difference across the IA and DA periods. Turning to the outcomes of primary interest, we cannot reject the null hypothesis of no effect across any specification. Standard errors are in the 0.08 to 0.09 range in all cases, so we cannot rule out effects that are large relative to sample means. Consistent with theory,

[8] We do not see any evidence of imbalance in the Kindergarten choice lotteries.

DA effects are smaller (more negative) than IA effects across each outcome, but we cannot rule out a null of no difference.

Table C3 presents an identical table that adds controls for demographic covariates to equation 5. There is no evidence that this increases precision. As in our main specifications we cannot reject the nulls of no effect and no difference between IA and DA effects at conventional levels.

Table C1: Sample descriptives of PreK Lottery participants

	Any PreK			PreK3			PreK4		
	All	Boston	DA	All	Boston	DA	All	Boston	DA
I. Demographics									
Tract poverty rate	0.228	0.228	0.227	0.226	0.227	0.225	0.230	0.230	0.230
Female	0.510	0.508	0.514	0.508	0.511	0.505	0.512	0.505	0.526
Black	0.433	0.439	0.424	0.437	0.444	0.428	0.429	0.434	0.419
White	0.173	0.163	0.188	0.181	0.171	0.193	0.164	0.155	0.181
Hispanic	0.392	0.396	0.387	0.379	0.381	0.377	0.405	0.408	0.399
Special education	0.047	0.051	0.040	0.044	0.046	0.041	0.050	0.055	0.039
Share PreK3 applicants	0.509	0.461	0.582						
II. Schooling & Choice									
Any magnet placement	0.267	0.229	0.327	0.338	0.298	0.388	0.194	0.170	0.243
Enroll magnet	0.221	0.202	0.252	0.264	0.248	0.285	0.202	0.191	0.223
Enroll any	0.474	0.481	0.463	0.452	0.448	0.457	0.494	0.506	0.468
Enroll magnet in PreK4	0.229	0.213	0.254	0.255	0.239	0.276	0.202	0.191	0.223
Interior placement chance	0.485	0.425	0.579	0.543	0.529	0.560	0.426	0.336	0.606
Magnet placement probability	0.256	0.220	0.311	0.327	0.290	0.373	0.182	0.159	0.226
Magnet placement probability – interior	0.257	0.247	0.269	0.289	0.249	0.335	0.216	0.244	0.184
III. Choices in K *Lottery*									
Participate	0.540	0.557	0.514	0.463	0.481	0.440	0.620	0.622	0.615
Participate | no OO	0.639	0.652	0.619	0.556	0.567	0.542	0.719	0.720	0.718
List high scoring school 1^{st}	0.171	0.179	0.160	0.154	0.161	0.146	0.189	0.194	0.179
High scoring 1^{st} choice or OO	0.195	0.203	0.182	0.175	0.182	0.167	0.215	0.221	0.204
Placed or OO at high scoring school	0.166	0.161	0.174	0.152	0.144	0.162	0.180	0.175	0.190
No. of obs.	3941	2395	1546	2004	1105	899	1937	1290	647
No. of students	3363	2067	1296	2004	1105	899	1937	1290	647

This table describes all applicants to PreK3 and PreK4 grades who are of age to enter the Kindergarten lottery between 2016 and 2020. and panels show results for students appyling to different pre-K grades. Panel I describes student demographics. Panel II describes Pre-K choice behavior. is an indicator for placement in a magnet Pre-K (a pre-K with a continuation option). and describe enrollment outcomes in a magnet pre-K and any pre-K in the year following the lottery. is an indicator for magnet enrollment in PreK4. is an indicator for having a magnet placement probability strictly between 0 and 1. is the probability of magnet placement for applicants with interior placement probabilities. Panel III describes students' participation in Kindergarten choice. is a dummy for submitting an application. is dummy for submitting an application in the sample of students without continuation options. is equal to one if a student participates and lists a high scoring school first. is an indicator for having a high scoring school as a listed first choice or as an OO. is an indicator for either placing at a high scoring school or not placing and having such a school as a continuation option.

Table C2: RDMD Estimates on PK4 enrollment

	Sample: Participates in any PreK lottery								
	Boston			DA			DiD		
	β	SE	N	β	SE	N	β	SE	N
I. Demographics									
Tract poverty rate	-0.003	(0.009)	1010	-0.013	(0.010)	868	-0.010	(0.014)	1878
Female	-0.010	(0.042)	1011	0.049	(0.045)	871	0.058	(0.061)	1882
Has Race	0.022	(0.023)	1011	0.016	(0.017)	871	-0.005	(0.028)	1882
Black	0.105	(0.042)	929	0.032	(0.045)	840	-0.073	(0.061)	1769
White	-0.002	(0.036)	929	0.012	(0.036)	840	0.014	(0.051)	1769
Hispanic	-0.106	(0.038)	934	-0.042	(0.043)	843	0.064	(0.057)	1777
Black – census tract share	-0.011	(0.016)	1010	-0.001	(0.019)	868	0.010	(0.024)	1878
White – census tract share	0.022	(0.016)	1010	0.020	(0.020)	868	-0.001	(0.026)	1878
Hispanic – census tract share	-0.010	(0.014)	1010	-0.020	(0.016)	868	-0.009	(0.021)	1878
Joint Test			0.022			0.656			0.463
II. First Stage									
Enroll Magnet	0.362	(0.035)	1011	0.367	(0.036)	871	0.005	(0.050)	1882
Enroll any	0.181	(0.038)	1011	0.315	(0.038)	871	0.135	(0.054)	1882
Enroll other	-0.182	(0.024)	1011	-0.052	(0.031)	871	0.130	(0.039)	1882
III. Choices K - IV									
Participate	-0.709	(0.112)	1011	-0.609	(0.123)	871	0.100	(0.166)	1882
List high scoring school 1^{st}	-0.122	(0.082)	1011	-0.165	(0.091)	871	-0.043	(0.123)	1882
High scoring 1^{st} choice or OO	0.011	(0.087)	1011	-0.060	(0.094)	871	-0.070	(0.128)	1882
Placed or OO at high scoring school	0.016	(0.080)	1011	-0.053	(0.091)	871	-0.069	(0.121)	1882

Coefficients from reduced form and IV estimates of equation 5. Sample: PreK applicants at NHPS schools old enough to participate in the Kindergarten lottery under Boston mechanism (first set of columns) and DA mechanism (second set of columns). Panel I: effects of lottery placement on predetermined covariates. Race data missing for some observations. Panel II: First stage effects of placement on magnet enrollment (schools with outside options), any enrollment, and non-magnet enrollment in students Pre-K4 year. Panel III: IV estimates of the effect of magnet enrollment (and the option to continue in current school through elementary grades) on listed outcome. is a dummy for submitting an application. is dummy for submitting an application in the sample of students without continuation options. is equal to one if a student participates and lists a high scoring school first. is an indicator for having a high scoring school as a listed first choice or as an OO. is an indicator for either placing at a high scoring school or not placing and having such a school as a continuation option.

Table C3: RDMD Estimates on PK4 enrollment with Gender and Race covariates

	Sample: Participates in any PreK lottery								
	Boston			DA			DiD		
	β	SE	N	β	SE	N	β	SE	N
I. First Stage									
Enroll Magnet	0.352	(0.034)	1011	0.366	(0.036)	871	0.013	(0.049)	1882
Enroll any	0.159	(0.037)	1011	0.313	(0.038)	871	0.154	(0.053)	1882
Enroll other	-0.193	(0.024)	1011	-0.053	(0.031)	871	0.140	(0.040)	1882
II. Choices K - IV									
Participate	-0.766	(0.113)	1011	-0.621	(0.123)	871	0.145	(0.166)	1882
List high scoring school 1^{st}	-0.157	(0.085)	1011	-0.170	(0.092)	871	-0.013	(0.125)	1882
High scoring 1^{st} choice or OO	-0.028	(0.089)	1011	-0.063	(0.095)	871	-0.035	(0.130)	1882
Placed or OO at high scoring school	-0.016	(0.082)	1011	-0.058	(0.090)	871	-0.043	(0.122)	1882

Coefficients from reduced form and IV estimates of equation 5. Sample: PreK applicants at NHPS schools old enough to participate in the Kindergarten lottery under Boston mechanism (first set of columns) and DA mechanism (second set of columns). Panel I: effects of lottery placement on predetermined covariates. Race data missing for some observations. Panel II: First stage effects of placement on magnet enrollment (schools with outside options), any enrollment, and non-magnet enrollment in students Pre-K4 year. Panel III: IV estimates of the effect of magnet enrollment (and the option to continue in current school through elementary grades) on listed outcome. is a dummy for submitting an application. is dummy for submitting an application in the sample of students without continuation options. is equal to one if a student participates and lists a high scoring school first. is an indicator for having a high scoring school as a listed first choice or as an OO. is an indicator for either placing at a high scoring school or not placing and having such a school as a continuation option. All specifications include controls for race and gender.

D Data construction

D.1 Data sources

Data for this project come from the New Haven Public Schools. We use two main data sources. The first are records from the choice process for assignment to school starting in the Fall of academic years 2015 through 2020. The second are student enrollment records from the fall of each year from 2014 through 2019.

We construct the dataset used in our main difference-in-differences analysis by merging the choice records on to the enrollment dataset. The sample universe is defined as students enrolled in NHPS pre-Ks in their age-4 year. These are students eligible for advancing to Kindergarten. Student covariates in this analysis come from school enrollment files. We match enrollment records to choice records using unique district student identifiers.

In our supplemental school-lottery based analysis (reported in Online Appendix C) the sample universe is defined by participation in a Pre-Kindergarten lottery. We then merge on both enrollment records (to obtain first stage estimates of enrollment effects) and Kindergarten choice behavior (to obtain IV estimates of the effects of enrollment on choice behavior). Student covariates in this analysis come from the school lottery files. District student IDs are not always available for students in the lottery dataset who have not previously enrolled in the NHPS system. Students for whom district identifiers are not available are merged by name and birthdate to enrollment and choice outcomes.

D.2 School classification

We identify and classify schools using NHPS records and state accountability data. Table D1 lists all all the schools available to students participating in Kindergarten choice. The "Available years" column lists the years in which the school accepted Kindergarten applications. All schools are available in all years except Highville Charter, which accepted students for the first time in 2020. Figure D 2 reproduces main text Figure 1, comparing school accountability measures to first choice shares, for each application year between 2016 and 2020.

Table D2 lists students' enrollment options for Pre-K, the year those options were available (between 2014 and 2019), and whether the school offered a continuation option into Kindergarten.

Table D1: Available lottery options in Kindergarten

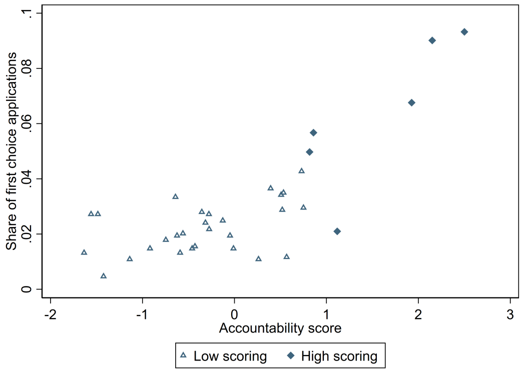
	Availabe in years
Amistad Academy Elementary & Middle Charter	2016-2020
Augusta Lewis Troup School	2016-2020
Barnard Environmental Studies Interdistrict Magnet	2016-2020
Benjamin Jepson Multi-Age Interdistrict Magnet	2016-2020
Bishop Woods Executive Academy	2016-2020
Booker T. Washington Academy Charter	2016-2020
Brennan-Rogers: The Art of Communication & Media Magnet	2016-2020
Celentano Biotech Health & Medical Magnet	2016-2020
Christopher Columbus Family Academy	2016-2020
Clinton Avenue School	2016-2020
Davis Street Arts & Academics Interdistrict Magnet	2016-2020
East Rock Community Magnet	2016-2020
Edgewood Magnet	2016-2020
Elm City College Preparatory Charter	2016-2020
Elm City Montessori Magnet	2016-2020
Fair Haven School	2016-2020
Harry A. Conte-West Hills Magnet: A School of Exploration & Innovation	2016-2020
Highville Charter School and Change Academy	2020
Hill Central School	2016-2020
John C. Daniels School of International Communication Interdistrict Magnet	2016-2020
John S. Martinez Magnet School	2016-2020
King/Robinson Interdistrict Magnet: An International Baccalaureate World School	2016-2020
L.W. Beecher Museum School of Arts & Sciences Interdistrict Magnet	2016-2020
Lincoln-Bassett Community School	2016-2020
Mauro-Sheridan Science, Technology & Communications Interdistrict Magnet	2016-2020
Nathan Hale School	2016-2020
Quinnipiac Real World Math STEM Magnet	2016-2020
Roberto Clemente Leadership Academy	2016-2020
Ross Woodward Classical Studies Interdistrict Magnet	2016-2020
Strong/Obama Magnet	2016-2020
Truman School	2016-2020
West Rock Author's Academy Interdistrict Magnet	2016-2020
Wexler-Grant Community School	2016-2020
Wintergreen Interdistrict Magnet	2016-2020
Worthington Hooker School	2016-2020

Table D2: Available enrollment options in PreK

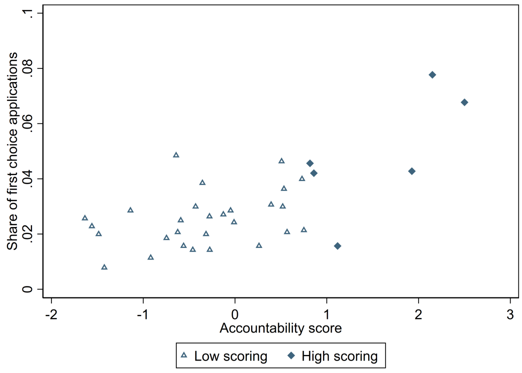
	Open in years	OO
Augusta Lewis Troup School	2014-2019	No
Barnard Environmental Studies Interdistrict Magnet	2014-2019	Yes
Benjamin Jepson Head Start	2014-2019	No
Benjamin Jepson Multi-Age Interdistrict Magnet	2014-2019	Yes
Bishop Woods Executive Academy	2014-2016	No
Brennan-Rogers: The Art of Communication & Media Magnet	2014-2019	Yes
Celentano Biotech Health & Medical Magnet	2014-2019	Yes
Christopher Columbus Family Academy	2014-2019	No
Davis Street Arts & Academics Interdistrict Magnet	2014-2019	Yes
Dr. Reginald Mayo Early Learning Center	2015-2019	No
East Rock Community Magnet	2014-2019	No
Fair Haven School	2014-2019	No
Harry A. Conte-West Hills Magnet: A School of Exploration & Innovation	2014-2019	Yes
Hill Central Music Academy	2014-2019	No
John C. Daniels School of International Communication Interdistrict Magnet	2014-2019	Yes
John S. Martinez Magnet School	2014-2019	No
King/Robinson Interdistrict Magnet School: An International Baccalaureate World School	2014-2019	Yes
L.W. Beecher Museum School of Arts & Sciences Interdistrict Magnet	2014-2019	Yes
Lincoln-Bassett School	2014-2019	No
Lulac	2014-2019	No
Mauro-Sheridan Science, Technology & Communictions Interdistrict Magnet	2014-2019	Yes
Nathan Hale School	2014-2019	No
Ross Woodward Classical Studies Interdistrict Magnet	2014-2019	Yes
Truman School	2014-2019	No
West Rock Author's Academy Interdistrict Magnet	2014-2019	Yes
Wexler-Grant School	2014-2019	No
Zigler PreK Center	2014-2018	No

OO refers to the option of continuing in the same school in kindergarten without another application. Bishop Woods was a PreK school until 2016 but stopped admitting PreK students thereafter.

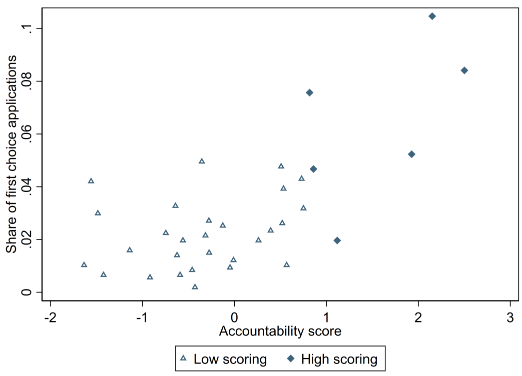
Figure D 1: Share of first choice applications by accountability score by year



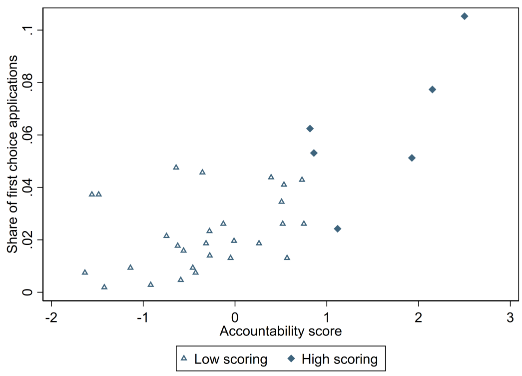
(a) 2016



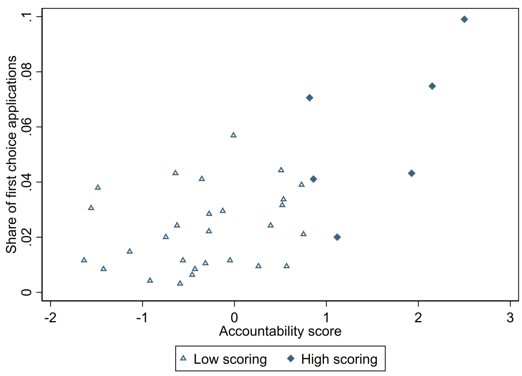
(b) 2017



(c) 2018



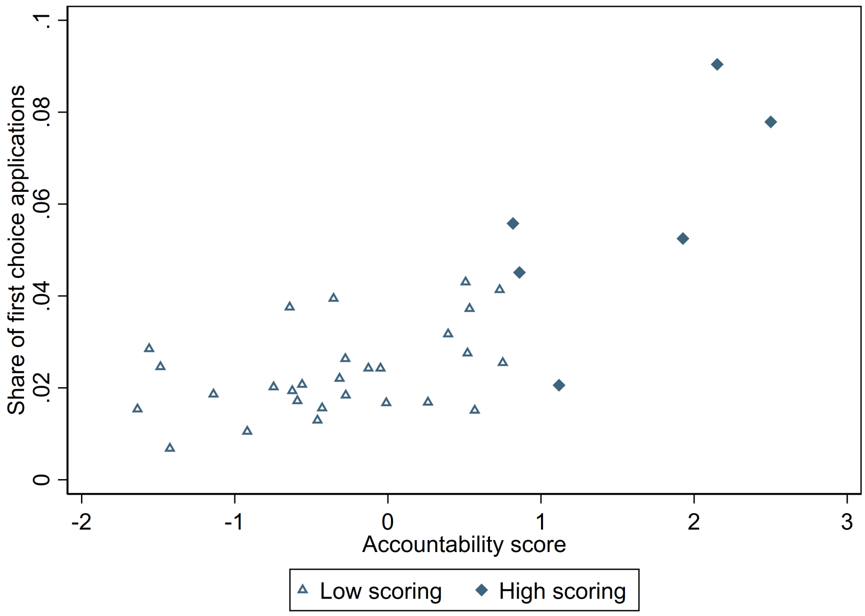
(d) 2019



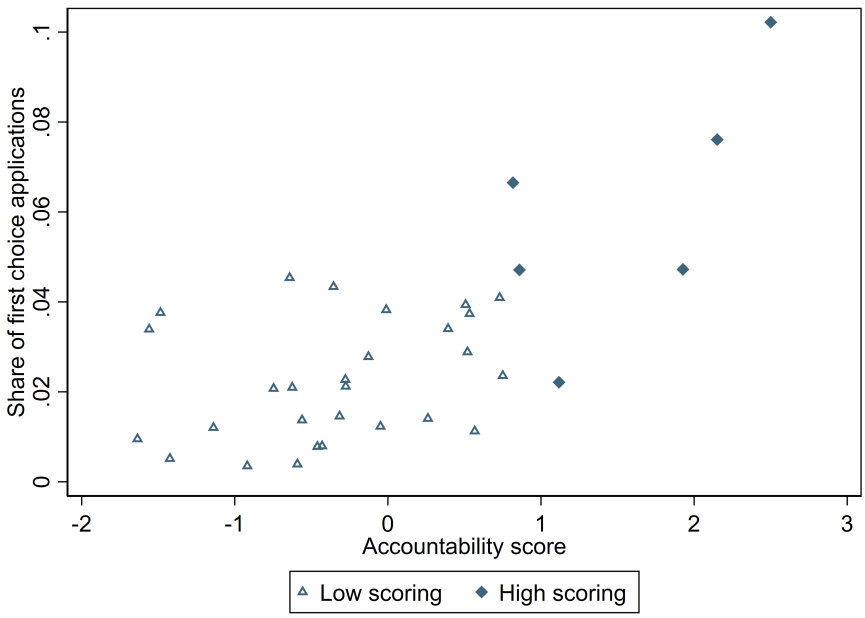
(e) 2020

Figure displays the share of first-choice applications (vertical axis) by school-level accountability scores (horizontal axis). Panels refer to application statistics for the year listed in the title. See Section 3.1.4 for details.

Figure D 2: Share of first choice applications by accountability score by mechanism



(a) Boston



(b) DA

Figure displays the share of first-choice applications (vertical axis) by school-level accountability scores (horizontal axis). Panels refer to application statistics for the year listed in the title. See Section 3.1.4 for details.