Constrained School Choice and the Demand for Effective Schools *

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Abstract

Choosing a school is one of the most important decisions parents make regarding their children's human capital, yet they often face restricted choice sets. We study parental preferences over school effectiveness and peer quality in the centralized education market of Barbados, where admissions are based on a one-shot exam and parents face a binding cap on the number of schools to which they can apply. Exploiting a policy reform that further tightened this cap, we show that parents responded by omitting the most selective schools from their applications, underscoring how market design shapes application behavior. We then estimate parental preferences for school effectiveness—measured by impacts on test scores and adult wages—as well as peer quality. Preference estimates under the assumption of truth-telling indicate that parents do not value effectiveness after controlling for peer quality. In contrast, preference estimates under the assumption of market stability, which allows for strategic behavior, show that parents place substantial weight on both effectiveness and peer quality. This divergence arises because the most selective schools are also the most effective, forcing parents to trade off effectiveness against admission probabilities.

JEL codes: I21, I24, I28, J24.

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1 Introduction

Schools play a critical role in shaping both the short- and long-run outcomes of students, making school choice one of the most consequential decisions parents face. An increasing number of school systems use centralized mechanisms to assign students to schools (Neilson, 2024). In these systems, parents submit rank-ordered lists (ROLs) of schools, while a central planner defines school priorities. A matching mechanism then assigns students to schools based on the submitted ROLs and school priorities. Depending on the incentive properties of the mechanism, submitted ROLs may or may not reflect parents' true preferences (Agarwal and Somaini, 2020). Many systems rely on the student-proposing deferred acceptance (DA) mechanism, which induces truthful reporting of preferences *if* ROLs are unconstrained. In practice, however, most centralized markets impose limits on ROL length. These constraints can encourage strategic behavior, as parents may omit desirable but seemingly unattainable schools, thereby weakening the link between observed ROLs and true unobserved preferences (Haeringer and Klijn, 2009).

Understanding parental preferences is essential for evaluating policy reforms in school markets. Consider a setting where seats are allocated according to scores on a one-shot exam, but policymakers aim to transition to a system with school priorities based on school zones and lotteries. Such a reform can potentially increase access to schools but its impact depends critically on what parents value in schools. A finding that parents primarily care about peer quality, implies that the reform may lead to sorting by student composition and reduce incentives for schools to improve effectiveness (Friedman et al., 1962).

In this paper, we examine whether parents value school effectiveness, after accounting for peer quality, in an environment where the number of schools they can list is capped and the cap is binding. We empirically evaluate how parents react to this type of constraint and use the evaluation results to inform our modeling choices when estimating parental preferences for school effectiveness. Importantly, we measure school effectiveness using both short-run academic outcomes and long-run adult wages.

We draw on more than two decades of administrative data (1987–2011) from the centralized secondary school market in Barbados, where admissions are determined by scores

on a one-shot exam and ROLs are subject to a binding length constraint. To assess the role of the binding constraint, we exploit a policy change that further restricted application length and examine its effects on application behavior and student assignment. The data on applications and assignments combined with the structure of the market design enables us to estimate parental preferences for schools under two alternative assumptions: truth-telling and market stability.[1] To measure school effectiveness in the short run, we link application records to performance on the exit exam that determines tertiary education eligibility. To examine effectiveness in the long run, we link applications to a nationally representative household survey conducted in 2016, which provides individual-level wage information when the participants in the centralized secondary school market are between 25 and 40 years old.

We first show that further restricting ROLs' length led parents to adjust them in ways consistent with 'skipping the impossible': they excluded the most selective schools. We also find that the reform did not alter the equilibrium assignments. This pattern is consistent with parents making payoff-irrelevant mistakes (Artemov et al., 2023): if the most selective schools are truly unattainable, omitting them from ROLs does not affect actual assignments. This result reflects broader market conditions. A one-shot exam determines admissions to secondary schools, while catchment areas determine entry into primary schools. Each year, roughly half of the seats at the most selective secondary schools are filled by students from the same ten (out of more than one hundred) primary schools, both before and after the reform. These primary schools are located in wealthier neighborhoods and consistently outperform others on the one-shot exam. Students who did not attend one of these primary schools are far less likely to qualify for admission to a selective secondary school, whether or not their parents apply to them.

Our first set of results has two important implications for modeling and estimating parental preferences. First, they demonstrate that in a constrained setting, ROLs may not be a reliable source of information for understanding parental valuation of school characteristics, as they may not include parents' most preferred schools. Second, the fact that changes in applications induced by a tighter length cap do not affect assignments provides evidence

[1]Tuth-telling implies that parents reveal their most preferred choices in their ROLs. Market stability implies that students are assigned to their parents preferred ex-post feasible school.

that, even if many parents make strategic mistakes, these are mostly payoff-irrelevant. This finding supports the stability of the market equilibrium even though, in our setting, ROLs are submitted before the admission exam score is known (Che et al., 2023).[2]

Informed by our findings, we then estimate average parental tastes (net of distance) for schools using two approaches. The first assumes that ROLs truthfully reveal parents' most preferred schools (Hastings et al., 2009; Abdulkadiroğlu et al., 2017b, 2020; Ainsworth et al., 2023; Campos and Kearns, 2024). While our first set of results implies that this assumption does not hold when ROLs are constrained, the ROLs nonetheless shape student assignments and capture the type of demand pressure the system places on schools. The second approach instead assumes that the matching equilibrium is stable (Fack et al., 2019; Artemov et al., 2023), allowing us to recover the distribution of preferences without directly using application data. Our first set of findings supports this assumption. The two approaches yield strikingly different results: the truth-telling approach suggests that parents place little value on the most selective schools, whereas the stability-based approach shows that these schools are, in fact, the most highly valued. This pattern is consistent with parents strategically omitting schools they perceive as unattainable, and illustrates how the truth-telling assumption can understate demand for selective schools.

Next, we estimate the effectiveness and peer quality of individual schools in terms of test scores and adult wages. Since students are not randomly assigned to schools, we rely on different identifying assumptions. We begin with a value-added model that assumes selection on observables, and show that while school averages overstate effectiveness, the most selective schools are also the most effective at raising test scores and wages. We show that our estimates are not sensitive to relaxing the selection-on-observables assumption by including a control function derived from our preferences model (Dubin and McFadden, 1984; Dahl, 2002; Abdulkadiroğlu et al., 2020). We further validate the effectiveness estimates by exploiting admission discontinuities embedded in the system and showing they are forecast-unbiased (Angrist et al., 2017; Abdulkadiroğlu et al., 2022). Finally, because test scores are observed for the full applicant population but wages only for the survey sample, we

[2]Importantly, fewer than 10% of children who score high enough to qualify for a selective school have parents who do not apply to one.

employ multivariate empirical Bayes shrinkage to improve precision across estimates for both outcomes (Walters, 2024).

Drawing on our full set of estimates, we decompose average parental tastes for schools into components reflecting valuations for peer quality and school effectiveness at improving test scores and adult wages. Under the truth-telling assumption, once peer quality is accounted for, parents appear not to value school effectiveness. In contrast, the stability-based estimates reveal that parents place significant weight on *both* effectiveness and peer quality—consistently across short-term academic outcomes and long-term wage outcomes. The difference in results is due to the truth-telling assumption imposing that unranked schools are less preferred than ranked ones disregarding if they were feasible or not (Che et al., 2023).

Taken together, our findings show that market design features can affect the extent to which ROLs reveal parental preferences for school effectiveness. In particular, the combination of a one-shot priority exam and an application length cap leads many parents to trade off school effectiveness for admission probabilities. An implication is that, as parents recognize and value school effectiveness, providing additional information on it would not dramatically alter assignments. Alternatively, a different approach to address school oversubscription, combined with uncapped application lists, could increase access to schools and allow parents to act on their preferences for school effectiveness.

Our paper contributes to three strands of literature. First, we contribute to the literature on the incentive properties of matching mechanisms. It is well established that, under the Boston mechanism, parents submit strategic application lists (Abdulkadiroğlu and Sönmez, 2003; Calsamiglia and Güell, 2018; Calsamiglia et al., 2020). Under the student-proposing deferred acceptance mechanism, incentive properties hinge on whether application lists are constrained. Theoretically, the constraining of application lists induces strategic behavior (Haeringer and Klijn, 2009), and laboratory experiments confirm reduced truth-telling under such constraints (Calsamiglia et al., 2010). Our empirical findings, obtained in a real-life setting, provide further evidence on the effects of list-length constraints.

Second, we build on the research on parental valuation of school effectiveness and peer quality. Existing studies typically assume that ROLs reveal parents' most preferred schools (Hastings et al., 2009; Abdulkadiroğlu et al., 2020; Ainsworth et al., 2023; Campos and

Kearns, 2024). This assumption may be justified when ROLs are unconstrained (Ainsworth et al., 2023) or have a constraint that appears non-binding (Abdulkadiroğlu et al., 2020). Thus far, evidence on parental valuation of school effectiveness, net of peer quality, remains mixed. A part of the literature finds that parents do not value school effectiveness after accounting for peer quality, while another finds they do (Angrist et al., 2023). This paper studies a setting with a binding ROL constraint, where we show that truth-telling is less plausible. Unlike Beuermann et al. (2023), who allow for misrepresentation but adapts estimators that rely on truth-telling, we employ the theoretically grounded estimator of Fack et al. (2019), which leverages market stability to recover parental preferences. Relatedly, Gazmuri (2024) shows that neglecting supply-side constraints in school choice understates low-SES preferences for school quality. We similarly demonstrate that ignoring a binding constraint in ROL length leads to underestimating the value that parents place on school effectiveness.

Third, we contribute to the literature that measures school effectiveness (Angrist et al., 2023). Our data collection allows us to estimate the effect of individual schools, complementing prior work that focuses on the effects of school sectors (Dobbie and Fryer Jr, 2014; Deming et al., 2014; Abdulkadiroğlu et al., 2017a). Moreover, while most studies measure school effectiveness through test scores or other academic outcomes (Abdulkadiroğlu et al., 2020), only a few examine non-academic, medium-term outcomes (Beuermann et al., 2023), and even less is known about individual school effectiveness on wages. Altonji and Mansfield (2018) bound the contribution of schools to the variance of wages using a control function approach. Instead, we exploit the structure of the centralized market to estimate the impacts of individual schools on test scores and adult wages under different sets of assumptions.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background. Section 3 details the administrative and survey data used for the analysis. Section 4 shows evidence regarding the effects of the policy change. Section 5 presents the empirical methods and estimates of parental preferences, school effectiveness, and peer quality. Section 6 contains the analysis of parental valuation for school effectiveness and peer quality in a constrained environment. Section 6 concludes.

2 Institutional background

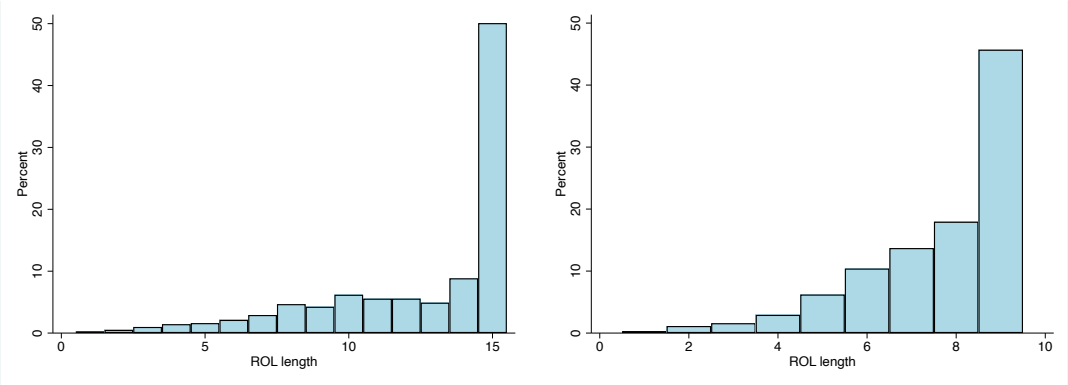
Barbados has a centralized system governing the admission process to public secondary schools. Near the end of primary school (in sixth grade), parents submit a ROL of secondary school choices to the Ministry of Education, and their children take the Barbados Secondary School Entrance Examination (BSSEE). Typically, ROLs are due by the end of January, and the exam is held in early May. The BSSEE has three components: mathematics, English language, and an essay. The total BSSEE score is the sum of the scores on the three sections and ranges from 0 to 200. Individual school capacity by gender is pre-determined. With the exceptions of the two single-sex schools (one for girls and another for boys), all schools are coed, with half of their seats reserved for girls and the other half for boys.

The matching algorithm is the serial dictatorship. The serial dictatorship algorithm ranks students by their total BSSEE scores and, in order, matches each applicant to the highest-ranked choice in their parents' submitted ROL, provided there are available seats. Seat availability respects the gender quota. This algorithm is a special case of the DA algorithm in which all schools rank students identically. After the assignment is complete, students are informed of their BSSEE score and the secondary school to which they have been assigned. At the end of the process, some students may remain unassigned if all the schools requested by their parents are full by the time it is their turn in the matching process. The government assigns these students to schools with available seats after the matching process is complete.

Between 1987 and 1995, parents could rank up to fifteen schools out of the 22 schools operating in the country, without any geographical restrictions. Starting in 1996, some restrictions were implemented. First, the ROL length was restricted to nine schools. Second, schools ranked third to ninth were restricted to those within the geographical zone where a family resided. For this purpose, the island was divided into three geographical zones. The first two school choices in the ROL continued to have no geographical restrictions. Third, each secondary school was required to take at least 30 percent of its students from within its zone. The two single-sex schools were not subject to either the zone-related choice restrictions or the minimum local student intake rule.

Figure 1 shows evidence that the ROL length constraint was binding before and after the

Figure 1: ROL length



(a) 1987-1995 (b) 1996-2011

NOTE: This figure shows the distribution of application lengths before (panel a) and after (panel b) the policy change.

policy change. Before the policy change, close to half of the parents ranked fifteen schools. After the policy change, close to half of the parents ranked nine schools. Importantly, in both cases, there is a discontinuous change in the distribution of ROL lengths at the maximum allowed. In Appendix B, we show the distribution of positions in ROLs to which students were assigned. Before the policy change, on average, students were assigned to their 6.68 ROL position. After the policy change, on average, students were assigned to their 4.26 ROL position.

Secondary school in Barbados comprises five years (termed forms), starting with first form (the equivalent of seventh grade) and ending with fifth form (the equivalent of eleventh grade). In fifth form, students take the Caribbean Secondary Education Certification (CSEC) examinations. These are the Caribbean equivalent of the British Ordinary levels (O-levels) examinations and are externally graded by the Caribbean Examinations Council (CXC). The CSEC examinations are given in 33 subjects. To be eligible for university admission, a student must pass five or more subjects, including English and mathematics. In addition, entry-level positions in the public sector require at least five CSEC subject passes. Therefore, the CSEC can be considered a high-stakes examination for much of the population.

3 Data

We observe the full population of students who applied to a public secondary school in Barbados between 1987 and 2011.[3] We have administrative data on the BSSEE scores for all student applicants as well as their date of birth, gender, primary school attended, parish of residence, ROL of secondary schools, and the secondary school assignment from the Ministry of Education.

To track student performance in secondary school, we collected individual-level data on the CSEC examinations. The CSEC data are available for all years between 1993 and 2016. These data include the scores for each subject examination taken and the secondary school attended. We linked the CSEC data to the 1987-2011 BSSEE cohorts by full name (first, middle, and last), date of birth, and gender.[4]

To track long-run outcomes, we draw on the 2016 Barbados Survey of Living Conditions. This nationally representative survey interviewed 7,098 individuals and collected data on educational attainment, employment, and wages. Importantly, the survey was purposely designed to be matched with the BSSEE data. This was achieved through the collection of full names at age 10 (to account for name changes), dates of birth, and gender. Because our interest is in tracking labor market outcomes, we focus on BSSEE cohorts aged 25 years and older at the time of the survey (which correspond to 1987-2002 BSSEE cohorts). We focus on adults aged at least 25 years because the survey data show that 99 percent of individuals had completed formal schooling by then. We matched 90 percent of surveyed individuals meeting our age criteria to the BSSSE data.[5]

Table 1 reports summary statistics by cohort. Column 1 reports on the cohort who took the BSSEE before the policy change (1987-1995); Column 2 reports on the post-policy change cohort (1996-2011). The distribution of female test-takers is the same in both cohorts. While

[3] Around 91 percent of secondary students in Barbados are enrolled in the public education system.

[4] We matched 90 percent of individuals observed in the CSEC administrative records to the BSSEE records. The 10 percent rate of unmatched individuals is similar to the enrollment rate in private secondary schools (9 percent), whose students would not have taken the BSSEE.

[5] Appendix Tables A.1 and A.3 show that the matched survey sample is representative of the population as the distributions of BSSEE and CSEC scores among the survey sample mirror those of the broader population.

the average admitted school cohort size was 160 students in the 1987-1995 period, it declined to 155 in the post-1996 period, likely reflecting reduced fertility. The younger cohort shows better CSEC outcomes. While 75 percent of students among the 1996-2011 cohort took at least 1 CSEC subject, only 56 percent of the 1987-1995 cohort did so. The younger cohort also took more CSEC subjects (4.76 versus 3.23 subjects), passed more subjects (3.35 versus 2.44 subjects), and was more likely to qualify for tertiary education (30 versus 21 percent). The survey data indicates that the younger cohort has attained more education (11.8 years versus 10.8 years), and was more likely to complete secondary school and obtain a university degree. Nonetheless, the employment rate is equivalent for both cohorts at 76 percent. In contrast, the older cohort has higher earnings than the younger cohort, likely reflecting their additional years of experience and tenure with respect to their younger counterparts.

4 Policy change

This section examines how the 1996 policy change, which lowered the cap on ROL length from fifteen to nine schools, affected applications and assignments. We begin by analyzing changes in parents' first and second choices, which were not subject to geographical restrictions after the policy change. We then assess broader shifts in application lists, examining the impact on the inclusion of the most selective schools in the ROLs. Finally, we investigate the impact of the policy on equilibrium assignments.

4.1 Applications

Before the policy change, Harrison College was the most popular first-choice school; 47% of parents selected it as their first choice. Queens College was the leading second-choice option, chosen by an average of 42% of parents. As a result, these two schools had the highest admission cut-off scores.

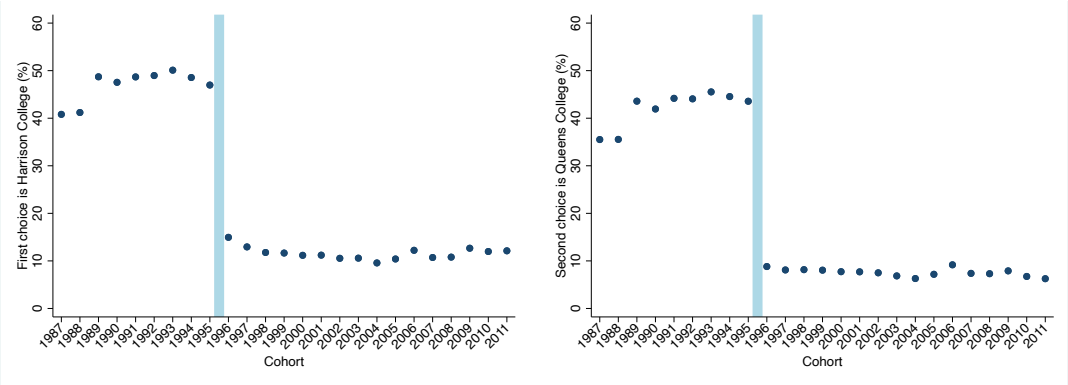
As illustrated in Figure 2, panel (a), the proportion of parents listing Harrison College as their first choice declined by approximately 35 percentage points following the policy change. Panel (b) reveals a comparable decline for Queens College, where second-choice listings also fell by about 35 percentage points. To more precisely quantify these changes, we present

Table 1: Summary Statistics

BSSEE cohorts:	1987-1995	1996-2011
	(1)	(2)
Panel A: Administrative Data		
Female	0.50	0.50
	(0.50)	(0.50)
Admitted cohort size	160.57	154.74
	(49.54)	(46.53)
Took CSEC	0.56	0.75
	(0.50)	(0.43)
Number of CSEC subjects taken	3.23	4.76
	(3.38)	(3.49)
Number of CSEC subjects passed	2.44	3.35
	(2.99)	(3.17)
Qualified for tertiary *	0.21	0.30
	(0.41)	(0.46)
Observations	37,074	58,317
Panel B: Matched Survey Data		
Years of education	10.76	11.81
	(4.52)	(4.22)
Completed secondary school	0.75	0.83
	(0.43)	(0.37)
University degree	0.16	0.20
	(0.37)	(0.40)
Employed	0.76	0.76
	(0.43)	(0.43)
Monthly wage (2016 US$)	1,423.82	1,121.37
	(1074.91)	(806.02)
Observations	516	424

NOTE: Panel A includes all individuals who took the BSSEE between 1987 and 2011. Panel B is the survey data sample that is matched with BSSEE cohorts 1987-2002 (25 - 40 years old when surveyed). Column (1) displays means and standard deviations for the BSSEE cohort that applied to secondary school before the policy change (1987-1995). Column (2) displays means and standard deviations for the BSSEE cohort who applied to secondary school after the policy change (1996-2011). Because individuals in the BSSEE cohort 2003-2011 were too young at the time of the survey to reliably observe educational attainment and labor market outcomes, statistics presented in Panel B - Column (2) only reflect the 1996-2002 BSSEE cohort. Standard deviations are reported in parentheses below the means. *Qualification for tertiary education requires passing five CSEC subjects, including English and mathematics.

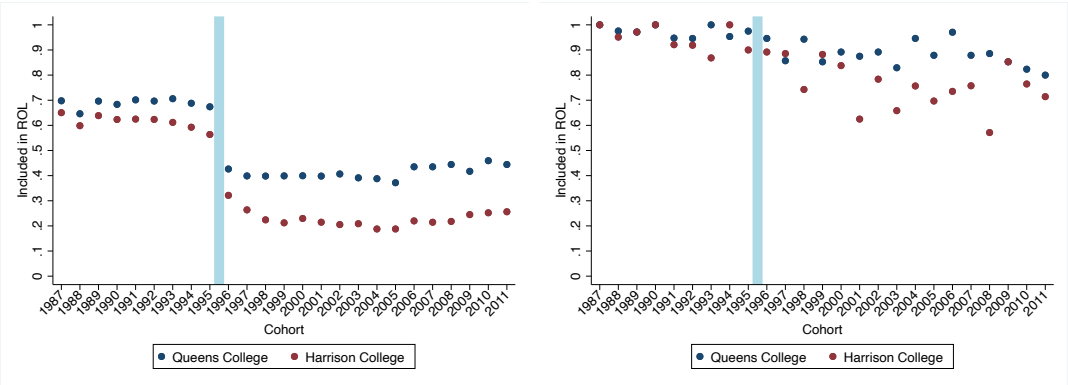
Figure 2: Most popular first and second choices



(a) Harrison College (b) Queens College

NOTE: This figure shows the percentage of parents listing Harrison College as their first choice (panel a) and Queens College as their second choice (panel b). The shaded vertical bar indicates the year of the policy change.

Figure 3: Share of applicants to the most selective schools



(a) All applicants (b) Top scorers

NOTE: Panel (a) shows the share of students that include Harrison College or Queens College in their ROLs before and after the policy change. Panel (b) shows the share of top scoring students that include Harrison College or Queens College in their ROLs before and after the policy change. The shaded vertical bar indicates the year of the policy change. The x-axis indicates the application cohort.

a set of empirical specifications along with the corresponding standard errors in Appendix C. The most flexible specification indicates a statistically significant reduction of nearly 40 percentage points for both outcomes.

These findings demonstrate substantial shifts in parents behavior. However, they do not necessarily imply that parents completely omitted the most selective schools from the

ROLs. Figure 3, panel (a), shows that, not only did the policy change affect the first- and second-ranked schools in the ROLs, it also reduced the probability that Harrison College or Queens College were included in the ROLs at all. The declines are sizable; the proportion of ROLs including Harrison College reduced by 30 percentage points after the policy change. The proportion of ROLs including Queens College reduced by a similar amount. Panel (b) illustrates the same outcome over time but only among the top scorers in the admission exam, defined as students within the top 1% of the score distribution each year. The vast majority of the top scorers include Harrison College and Queens College on their ROLs and this is largely unaffected by the policy change. There is an overall downward trend in the inclusion of these schools, but there is no noticeable drop around the time of the policy change. A full set of empirical specifications and associated standard errors is reported in Appendix D.

Although the figures show large, discontinuous changes in ROLs after the policy change, we adopt a conservative approach to avoid confounding the effect of the policy with other contemporaneous changes by implementing a difference-in-differences design. We define the control group as the set of top scorers and the treatment group as all other applicants. Intuitively, parents of top scorers face minimal risk of their children going unassigned and therefore have little incentive to omit the most selective schools from their ROLs. The corresponding estimates and standard errors are reported in Appendix E. Even under our most conservative approach, the results indicate a statistically significant decrease of 20 and 19 percentage points, respectively, in the share of parents including Harrison College and Queens College in their ROLs.

Taken together, these findings suggest that parents responded to the policy change by omitting schools they perceived as unattainable. However, changes in application behavior may not necessarily translate into changes in realized assignments. Thus, we next examine the impact of the policy on equilibrium assignment outcomes.

4.2 Assignments

Application changes can affect equilibrium assignments when they are payoff-relevant (Artemov et al., 2023). As shown in the previous section, parents responded to the policy change

by omitting the most selective schools from their ROLs. However, if these schools were never attainable, such changes are payoff-irrelevant and need not alter assignments. For instance, whether parents of low-scoring students include the most selective schools in their ROLs may be inconsequential, as their children's test scores would have been too low to meet the admission cut-offs.

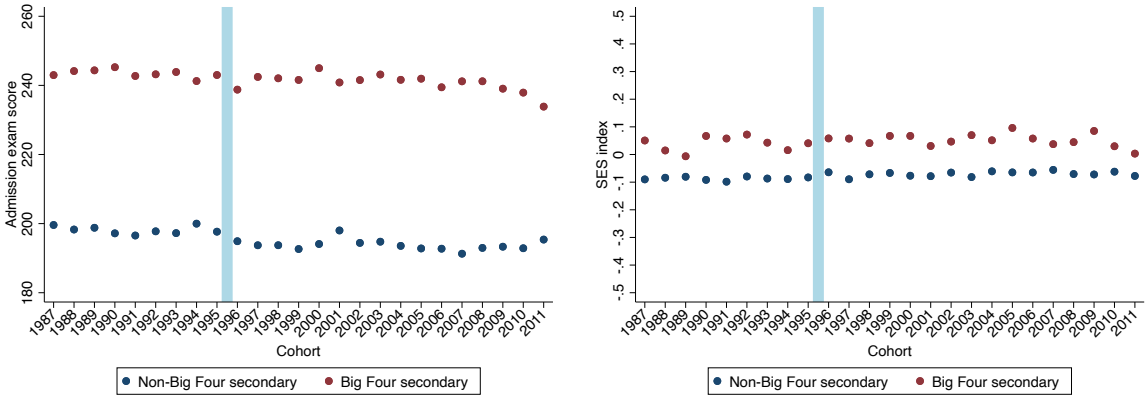
In this market, the share of girls and boys across schools is fixed due to the gender quotas. Specifically, the most selective schools always admit 50% girls, as half of the seats are reserved for them both before and after the policy change. What can potentially change, however, are the average admission scores and the socioeconomic composition (SES) of students at different schools. To assess these changes, we focus on the four most selective schools in the market, which we term the 'Big Four' and examine composition shifts over time for students at the Big Four and non–Big Four schools separately.[6] Figure 4, panel (a), shows that students assigned to Big Four schools consistently achieve substantially higher admission exam scores than those assigned to non–Big Four schools, a pattern that persists both before and after the policy change, with no discontinuous change around the policy implementation.

Next, we examine whether the policy affected the socioeconomic composition of students in the Big Four schools. Such effects could arise if particular subgroups—such as the parents of low-SES students—disproportionately reduced or ceased applying to these schools relative to the parents of high-SES students. Because family-level SES data are unavailable, we approximate socioeconomic status at the primary school level by constructing an SES index from census information corresponding to the smallest geographic unit for each primary school. Figure 4, panel (b), shows that students assigned to the Big Four schools consistently come from higher SES backgrounds than those assigned to other schools, and this pattern remains stable over time, with no discontinuous change coinciding with the policy implementation.

The absence of compositional changes at the Big Four schools is consistent with the fact that shifts in applications were largely payoff-irrelevant. Figure 4, panel (c), illustrates why this is the case: nearly half of all students assigned to the Big Four schools each year come

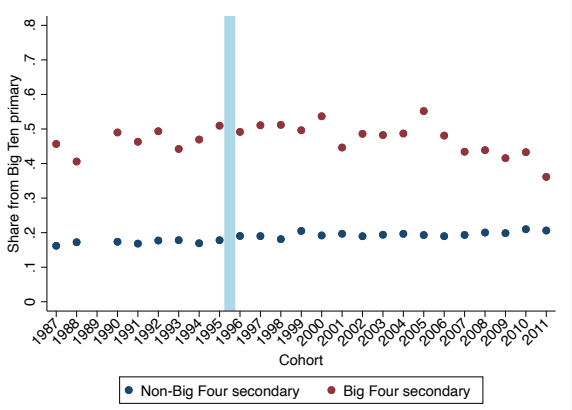
[6]The four most popular schools in the market are Harrison College (HC), Queens College (QC), Combermere School (CS), and Saint Michael's (SM).

Figure 4: Composition changes



(a) Admission exam score

(b) SES index



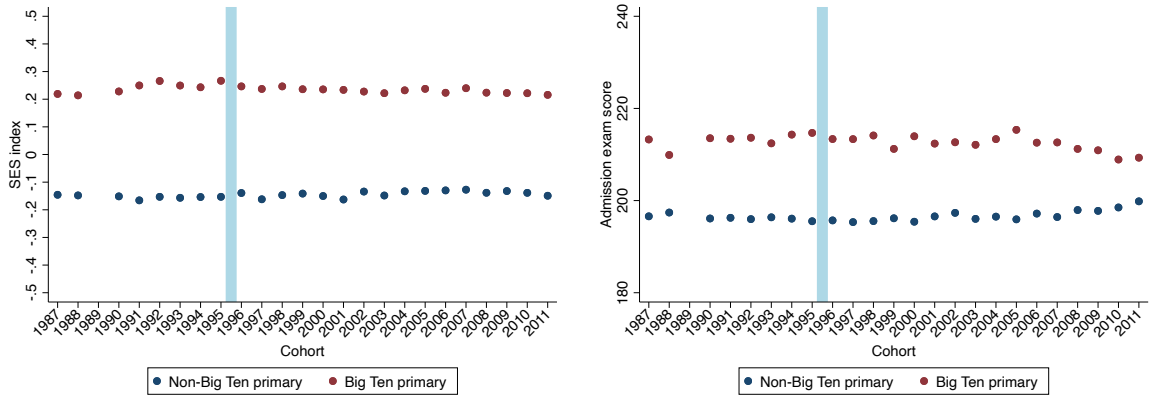
(c) Share from Big Ten primary

NOTE: Panel (a) shows the average admission exam score of students assigned to Big Four and non-Big Four schools. Panel (b) shows the average SES index of students assigned to Big Four and non-Big Four schools. The SES index measures average socio-economic status at the primary school level. Panel (c) shows the share of students from the Big Ten primary schools assigned to the Big Four and non-Big Four schools. We define the Big Ten primary schools as the set of schools that send the most students to the Big Four secondary schools. The x-axis indicates the application cohort.

from the same ten primary schools, which we label the 'Big Ten'. Consequently, students from the remaining one hundred primary schools face only a small probability of admission to a Big Four school, regardless of whether their parents apply to them.

Admission to primary schools is decentralized and determined by catchment areas. Figure 5, panel (a), shows that the SES composition of students at Big Ten primary schools is substantially higher than that of students at other primary schools, reflecting their location

Figure 5: Big Ten primary schools



(a) SES index

(b) Admission exam score

NOTE: Panel (a) shows the average SES index of students that attended Big Ten and non-Big Ten primary schools. Panel (b) shows the average admission exam score of students that attended Big Ten and non-Big Ten primary schools. The shaded vertical bar indicates the year of the policy change. The x-axis indicates the application cohort.

in wealthier neighborhoods. Panel (b) shows that students from the Big Ten schools have higher average admission exam scores than those from other schools. These patterns remain unchanged before and after the policy reform, suggesting that for many families, secondary school assignment is largely predetermined well before the application stage, and that they are aware of this fact.

Overall, the absence of changes in equilibrium assignments highlights two key points. First, the policy had no effect on equity of access—it neither improved nor worsened it. Second, the findings show that, although parents theoretically have choice over secondary schools, in practice many face tightly constrained options due to the design of the system, namely the one-shot exam to rank students and the strict cap on application list length.

In Appendix F, we present results from a set of empirical specifications that quantify changes in assignment outcomes. The estimated coefficients and standard errors closely correspond with the graphical evidence presented in this section.

In Appendix G, we examine whether the policy change affected the distance to assigned schools and the share of students who were unassigned. Distance serves as an informative proxy for welfare, as it is a key determinant of parental preferences. We find that the average

distance to assigned schools remained largely unchanged following the policy change. With respect to the share of unassigned students, we observe a statistically significant decline. This finding is consistent with parents submitting more conservative application lists, thereby reducing the risk of their children remaining unassigned.

5 Estimating preferences and school effectiveness

In this section, we present a model of (parental) school preferences, outlining the assumptions and the estimation methods used to recover preference parameters. We then employ a potential outcomes framework to define school effectiveness and peer quality, and proceed by estimating these measures under varying assumptions.

For estimation, we primarily rely on the pre-policy change data (1987–1995), when ROLs were capped at 15. We do so for two reasons. First, we want to measure school effectiveness using test scores and wages, and wage data are only available from a 2016 household survey. Pre-1996 cohorts are more likely to have completed post-secondary education and be in employment by the time of the household survey. Second, in addition to lowering the maximum cap to 9, the policy reform altered other features of the market design, such as the definition of school zones. These changes may have weakened desirable properties of the assignment mechanism—such as truth-telling and market stability—thereby complicating the estimation of preferences. Nonetheless, we use post-policy data (1997-2011) to assess whether our estimates are affected by the reform.

5.1 Preferences

We define the indirect utility U of agent i for school j as:

$$U_{ij} = X_j'\rho + \xi_j + \lambda d_{ij} + \epsilon_{ij} = \delta_j + \lambda d_{ij} + \epsilon_{ij},$$

where $\delta_j = X_j'\rho + \xi_j$ represents average parental taste for school j, X_j is a vector of school characteristics that includes school effectiveness and peer quality, ξ_j is an unobserved school

characteristic, d_{ij} is the distance (in kilometers) from agent i to school j, and ϵ_{ij} is a random component assumed to follow a type I extreme value distribution.

We begin by estimating the preference parameters (δ_j, λ) under the assumption that application lists reveal parental true preferences (Hastings et al., 2009; Burgess et al., 2015; Abdulkadiroğlu et al., 2017b, 2020; Ainsworth et al., 2023; Campos and Kearns, 2024). Given this assumption on parental behavior and our parametric assumptions, the probability of observing a particular application list is:

$$P(R_i = L) = \prod_{j \in L} \frac{\exp(\delta_j + \lambda d_{ij})}{\sum_{j' \not\succ_L j} \exp(\delta_{j'} + \lambda d_{ij'})},$$

where L denotes the observed application list and $j' \not\succ_L j$ indicates that j' is not ranked ahead of j in application list L. Notice that this is a rank-ordered conditional logit model (Hausman and Ruud, 1987). Estimating preferences under the truth-telling assumption implies that any school not included in an application list provides lower utility than a listed school. Importantly, this model does not impose any restrictions on the choice sets that agents face. We denote the truth-telling estimates $(\hat{\delta}_j^{TT}, \hat{\lambda}^{TT})$.

In a constrained environment, parents may not be incentivized to reveal their true preferences when forming their application lists. This can occur even when the matching algorithm is the deferred acceptance mechanism (Haeringer and Klijn, 2009). As shown in the previous section, although the number of secondary schools is relatively small, the application list constraint is binding. In this context, a key concern for estimating preferences is that some parents may omit schools that are out of reach, which could lead to underestimating their valuation of selective schools if we assume that all schools belong to each parent's choice set.

To address this, we also estimate preference parameters using the method proposed by (Fack et al., 2019), which assumes market stability and relies on school assignment information rather than application lists. Under this approach, each parent has a personalized choice set determined by their child's admission score and the ex-post schools admission cut-offs. Specifically, a parent's feasible choice set includes all schools with admission cut-offs below their child's score. Stability implies that within this feasible set, parents obtain their most preferred school. Formally, the choice problem for parent i is:

$$\arg\max_{j \in \Omega_i} \quad U_{ij},$$

where Ω_i denotes the feasible choice set. Let κ_j be the admission cut-off for school j, then the choice set is defined as:

$$\Omega_i = \{j : s_i \geq \kappa_j\},$$

that is, the set of schools for which student i's exam score s_i exceeds the admission cut-off. Notice that the feasible choice sets can be constructed with data on applications and cut-offs and do not need to be estimated. Intuitively, the parents of a high-scoring student have a larger choice set than the parents of a low-scoring one. Under the market stability assumption and the usual distributional assumption for ϵ_{ij}, the probability of observing student i at school j is:

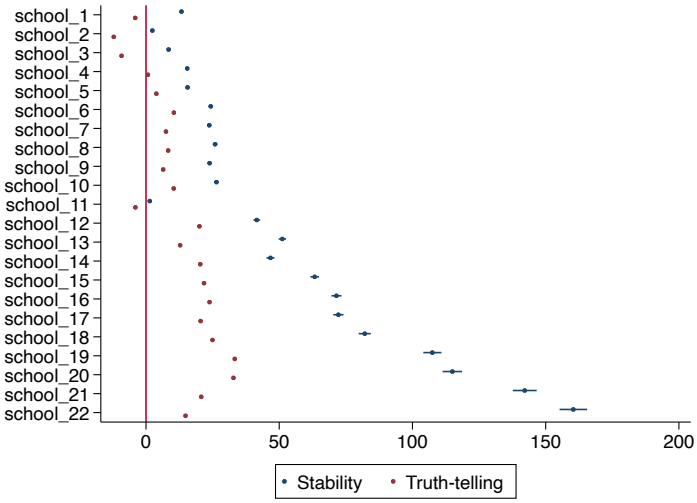
$$Pr(S_i = j) = \frac{\exp\left(\delta_j + \lambda d_{ij}\right)}{\sum_{k \in \Omega_i} \exp\left(\delta_k + \lambda d_{ik}\right)}.$$

We denote the stability based estimates $(\hat{\delta}_j^{ST}, \hat{\lambda}^{ST})$.

Figure 6 presents the estimated average parental taste for each school in the market, measured in willingness to travel, $\hat{\delta}_j/\hat{\lambda}$. For ease of exposition, we index schools by their average selectivity, defined as the average admission cut-off over the period, so that a higher index corresponds to a more selective school. Blue dots show estimates based on the stability of the market equilibrium, while red dots correspond to estimates under the truth-telling assumption.

Two features stand out from Figure 6. First, the estimates differ substantially depending on the underlying behavioral assumptions. Second, stability-based estimates assign higher tastes to the most selective schools, whereas truth-telling estimates do not. In fact, under the truth-telling assumption, parents appear not to value the most selective schools particularly highly, while the stability-based estimates indicate that these schools are precisely those parents value the most.

Figure 6: Average tastes in willingness-to-travel ($\frac{\hat{\delta}_j}{\hat{\lambda}}$)



NOTE: This figure shows preferences estimates in willingness-to-travel under two behavioral assumptions. The red dots show the estimates and associated confidence intervals we obtain when assuming truth-telling. The blue dots show the estimates and associated confidence intervals we obtain under market stability. On the y-axis, we index schools by selectivity, a higher index denotes a more selective school.

In Appendix H, we present a table with all the estimated parameters. The distance parameter is large and statistically significant under both truth-telling and stability. We also report the p-value from a Hausman test (Hausman, 1978) comparing the estimates obtained under the two assumptions. Under the null hypothesis that parents are truth-tellers, $(\hat{\delta}_j^{TT}, \hat{\lambda}^{TT})$ are consistent and efficient, while $(\hat{\delta}_j^{ST}, \hat{\lambda}^{ST})$ are consistent but not efficient. Under the alternative that not all parents are truth-tellers, only $(\hat{\delta}_j^{ST}, \hat{\lambda}^{ST})$ are consistent. Based on this test, we reject the truth-telling model (p-value=0.00).

In Appendix I, we present preference estimates in willingness-to-travel under both truth-telling and stability, using post-policy data (1997-2011). Interestingly, once parents adjust their ROLs to the tighter constraint, the truth-telling estimates suggest a distaste for the most selective school in the market and, more generally, a reduced preference for selectivity. In contrast, the stability-based estimates continue to indicate that parents place the highest value on the most selective schools.

5.2 School effectiveness

We define potential outcomes as:

$$Y_{ij} = \alpha_j + X_i'\beta + \eta_{ij},$$

where Y_{ij} denotes a post-secondary educational outcome for student i in school j. The vector X_i includes observable characteristics such as the admission exam score, year of birth, and gender. The term η_{ij} captures unobserved determinants of outcomes. From this setup, we define the quality of student i and the peer quality school j as:

$$A_i = (1/J)\sum_j Y_{ij} \qquad \text{and} \qquad Q_j = E[A_i \mid S_i = j].$$

Since students are not randomly assigned to schools, η_{ij} need not have mean zero. As a result, the parameters (α_j, β) cannot be consistently estimated without further assumptions.

Assuming selection on observables, we estimate the parameters (α_j, β) using the following empirical specification:

$$Y_i = \sum_{j=1}^{J} \alpha_j S_{ij} + X_i'\beta + \epsilon_i,$$

where S_{ij} is a dummy variable equal to one if student i attends school j. Notice that since X_i includes the admission exam score, this is a value-added model. Using the estimated parameters from the value-added model, we construct measures of the quality of student i and the peer quality school j as:

$$\hat{A}_i = \frac{1}{J}\sum_{j=1}^{J}[\hat{\alpha}_j + X_i'\hat{\beta}] \qquad \text{and} \qquad \hat{Q}_j = \frac{\sum_i \mathbb{1}\{S_i = j\}\hat{A}_i}{\sum_i \mathbb{1}\{S_i = j\}},$$

where $\hat{A}_i$ represents the average predicted outcome for student i across all schools, and $\hat{Q}_j$ is an estimate of the average student quality among those assigned to school j.

We focus on two outcomes of interest. The first measures whether an applicant qualifies for tertiary education based on her secondary school exit exam scores, and the second measures applicants' adult wages. We denote our estimates of interest as $(\hat{\alpha}_j^{test}, \hat{\alpha}_j^{wage})$ for school

Table 2: Effectiveness, peer quality, and selectivity

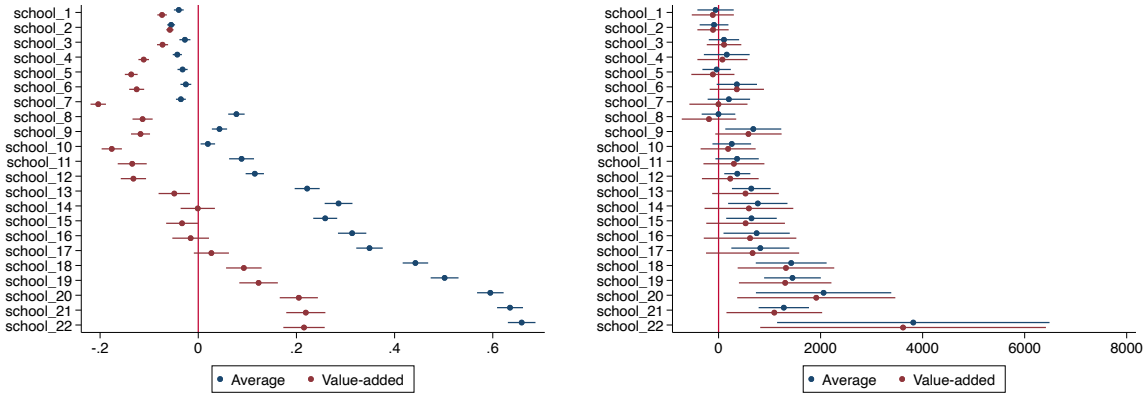
	$\hat{\alpha}_j^{test}$	$\hat{\alpha}_j^{wage}$	$\hat{Q}_j^{test}$	$\hat{Q}_j^{wage}$	κ_j
$\hat{\alpha}_j^{test}$	1.00				
$\hat{\alpha}_j^{wage}$	0.81	1.00			
$\hat{Q}_j^{test}$	0.76	0.73	1.00		
$\hat{Q}_j^{wage}$	0.65	0.61	0.94	1.00	
κ_j	0.78	0.74	0.99	0.89	1.00

NOTE: This table reports correlations across effectiveness estimates $(\hat{\alpha}_j^{test}, \hat{\alpha}_j^{wage})$, peer quality estimates $(\hat{Q}_j^{test}, \hat{Q}_j^{wage})$, and school selectivity κ_j. We measure school selectivity using equilibrium admission cut-offs.

effectiveness and $(\hat{Q}_j^{test}, \hat{Q}_j^{wage})$ for peer quality. Table 2 reports the correlations among these estimates and a measure of school selectivity, κ_j, defined as the average admission cut-off during the pre-policy period. Two patterns emerge from this table. First, school effectiveness in both outcomes is strongly correlated with school selectivity ($corr \geq 0.74$). Second, school effectiveness in test scores is also highly correlated with school effectiveness in wages ($corr = 0.81$).

Figure 7 presents the estimated school effectiveness parameters, $(\hat{\alpha}_j^{test}, \hat{\alpha}_j^{wage})$, along with their 95% confidence intervals. For comparison, we also display the corresponding school-level outcome averages. As expected, the wage estimates are substantially less precise than the test score estimates, reflecting that wages are observed only for a sample of applicants. Two main findings emerge from this figure. First, schools exhibit considerable heterogeneity in both their average outcomes and effectiveness. Second, while school averages provide biased measures of effectiveness, the most selective schools nonetheless deliver the largest

Figure 7: Value-added and average 1987-1995



(a) Qualified for tertiary

(b) Monthly wages

NOTE: Panel (a) shows uncontrolled and value-added estimates for tertiary school qualification as an outcome. Panel (b) shows uncontrolled and value-added estimates for wages as an outcome. Bars indicate 95% confidence intervals. In the y-axis, we index schools by selectivity, a higher index denotes a more selective school.

improvements in both short- and long-term outcomes.

To relax the selection-on-observables assumption, we take advantage that we have a choice model and derive a control function. We rely on the choice probabilities from the stability based estimator. Notice that our control function depends on the ex-post feasible choice set Ω_i because the choice probabilities also depend on it. We work under the following restriction:

$$E[Y_i \mid X_i, D_i, S_i = j] = \alpha_j + X_i'\beta + \varphi\lambda_j(X_i, D_i, \Omega_i),$$

where we use the distance vector $D_i = (d_{i1}, \cdots, d_{iJ})'$ as an exclusion restriction. Parameter φ measures the effect of selection on unobserved gains.

In Appendix J we provide more details on how we derive the control function, and show a comparison of our value-added and control function estimates for short- and long-term outcomes. School effectiveness estimates remain mostly unchanged for both outcomes and the ranking of effects is unaffected. We take this as evidence in favor of our value-added estimates and for the rest of the paper use these as our main estimates.

To further validate our value-added estimates we exploit the admission discontinuities

induced by the matching algorithm. Notice that a student assignment depends on only two inputs, her ROL and her admission exam score. For students with the same ROL, assignment is entirely determined by a student crossing or not the equilibrium admission cut-offs of the schools included in her ROL. We therefore exploit the admission discontinuities created by the cut-off structure for validation.

We apply the method of Abdulkadiroğlu et al. (2022) and construct a local propensity score for each student at each school that we denote p_{ij}. The propensity score only takes values of 0,0.5, and 1. Students with a propensity score of 0.5 are locally randomized into admission to a given school j. The randomization is local in the sense that it only occurs within a small bandwidth around relevant admission cut-offs. We then use the propensity score as a saturated regressor and implement the bias test proposed by Angrist et al. (2017).

$$
\begin{aligned}
Y_i =& \kappa_0 + \phi \hat{Y}_i + \sum_p \sum_j \kappa_{jp} \mathbb{1}[p_{ij} = p] + e_i \\
\hat{Y}_i =& \pi_0 + \sum_j \pi_j S_{ij} + \sum_p \sum_j \omega_{jp} \mathbb{1}[p_{ij} = p] + v_i,
\end{aligned}
$$

where $\hat{Y}_i$ is the prediction we obtain from our value-added model and S_{ij} are dummy variables that indicate admission to school j. The parameter ϕ should equal 1 if the value-added estimates used to construct $\hat{Y}_i$ correctly predict the effects of the admission discontinuities on average. We can only perform this test for the test score outcome for which we have administrative data. For the wage outcome, although we can estimate the value-added model, we do not have enough observations locally randomized into schools as we only observe wages for a sample.

In Appendix K, we present the results of our validation exercise. We first show that, in the uncontrolled model, $\hat{\phi} = 0.8$, and we reject the null hypothesis that $\phi = 1$. This indicates that the uncontrolled model is forecast biased and does not correctly predict the effects using the discontinuities on average. We also reject the overidentification test (Sargan, 1958), which implies that the value-added estimator does not have the same predictive validity across all discontinuities. Encouragingly, these results demonstrate that the test has sufficient power

to detect bias in the most naïve specification. We then turn to our value-added model obtain $\hat{\phi} = 0.96$ and do not reject that $\phi = 1$, suggesting that the value-added estimates are forecast unbiased and accurately predict the discontinuity effects on average. Regarding the overidentification test, we reject that the value-added model has the same predictive validity across all discontinuities (p-value = 0.03). However, it is important to note that the overidentification test is asymptotic and tends to over-reject in moderate sample sizes. For this reason, it is generally recommended to reject only in the case of very small p-values (Hansen, 2022).

To improve the precision of our school effectiveness estimates on wages, we take advantage of the high correlation between effectiveness estimates in test scores for which we used administrative data and effectiveness estimates in wages for which we used only a sample. Instead of estimating the test score and the wage equation separately, we estimate them as a system of equations such that we obtain estimates of the variances and covariances across effectiveness in both outcomes. Our system of equations is:

$$Y_{ik} = \sum_j \alpha_{jk} S_{ij} + X_i' \beta_k + \epsilon_{ik}, \quad k \in \{test, wage\}.$$

In this setup, school effectiveness is a vector $\alpha_j = (\alpha_{test}, \alpha_{wage})'$. We then rely on multivariate empirical Bayes shrinkage to obtain posteriors as follows:

$$\alpha_j^* = (V_j^{-1} + \Sigma_\alpha^{-1})^{-1}(V_j^{-1}\hat{\alpha}_j + \Sigma_\alpha^{-1}\mu_\alpha),$$

where V_j is a sampling variance matrix for each school, and $(\mu_\alpha, \Sigma_\alpha^{-1})$ are the parameters of the mixing distribution which we assume to be multivariate normal. The posteriors α_j^* borrow strength across our outcomes as well as across the ensemble of schools when predicting any one of the outcome-specific value-added parameters α_{jk} (Walters, 2024).

In Appendix L, we present scatter plots comparing the value-added estimates with the multivariate empirical Bayes posteriors for test scores and wages. The test score estimates are largely unaffected by shrinkage, as they rely on administrative data and are precisely estimated. In contrast, the wage estimates are more strongly influenced by shrinkage due to

their higher level of noise. In the next section, when we decompose our preference estimates into peer quality and school effectiveness, we show that our results are robust to using either the baseline value-added estimates or the posteriors after shrinkage.

6 Preferences for peer quality and school effectiveness

In this section, we combine the estimated parameters obtained in the previous section to examine whether parents value school effectiveness once peer quality is taken into account (Abdulkadiroğlu et al., 2020). Using our value-added estimates, we decompose average school-level outcomes into two components: peer quality ($\hat{Q}_j$) and school effectiveness ($\hat{\alpha}_j$). We then regress our estimates of average parental tastes on these components. Specifically, we consider two sets of average taste estimates: one derived under the truth-telling assumption ($\hat{\delta}_j^{TT}$) and the other under market stability ($\hat{\delta}_j^{ST}$). All estimates are scaled by their standard deviation, allowing us to interpret the coefficients as the change in parental tastes (in standard deviations) induced by a one-standard-deviation increase in the regressors. Our baseline specification is:

$$\hat{\delta}_{jt} = \rho_{0t} + \rho_1 \hat{Q}_{jt} + \rho_2 \hat{\alpha}_{jt} + \xi_{jt},$$

where t indexes the admission cohort. For the test score outcome, we can estimate cohort-specific parameters because this outcome is measured in the administrative data for all students. In contrast, for the wage outcome, we lack sufficient observations to estimate cohort-specific parameters, so we pool all cohorts and drop the index t.

Table 3 reports the estimation results. The first four columns use qualification to tertiary education as the outcome when estimating school effectiveness and peer quality. Under the assumption that parents report their truthful preferences in their ROLs (column 1), school effectiveness does not appear to matter once peer quality is controlled for. By contrast, the stability-based estimates (column 2) indicate that parents value both peer quality and school effectiveness: a one-standard-deviation increase in school effectiveness raises the average taste for a school by 0.56 standard deviations. The sample size is 22, corresponding to the number

Table 3: Preferences determinants: 1987-1995

	Qualified for tertiary		Qualified for tertiary (yearly)		Monthly wages	
	Truth-telling	Stability	Truth-telling	Stability	Truth-telling	Stability
$\hat{Q}_j$	0.944***	0.474***	0.978***	0.779***	0.793***	0.475***
	(0.141)	(0.070)	(0.060)	(0.049)	(0.220)	(0.120)
$\hat{\alpha}_j$	-0.117	0.555***	-0.084	0.345***	0.017	0.562***
	(0.196)	(0.101)	(0.055)	(0.054)	(0.267)	(0.093)
Observations	22	22	194	194	22	22

NOTE: This table reports estimates of the relationship between peer quality, school effectiveness, and average preferences for schools. Columns labeled 'Truth-telling' use preference estimates under the assumption of truth-telling, while columns labeled 'Stability' use preference estimates under the assumption of market stability. Columns (1) and (2) pool data across years and measure school effectiveness and peer quality using test scores. Columns (3) and (4) also use test scores but estimate cohort-specific parameters. Columns (5) and (6) measure school effectiveness and peer quality using adult wages. Standard errors are reported in parentheses.

of schools in the market. Columns (3) and (4) present pooled cohort-specific estimates, which expand the number of observations but do not alter the main findings. Again, stability-based estimates show that parents place weight on both peer quality and school effectiveness, with the latter increasing the average taste for a school by 0.35 standard deviations.

Columns (5) and (6) of Table 3 use monthly wages as the outcome when estimating school effectiveness and peer quality. The results mirror those for test scores: assuming truth-telling implies no significant valuation of school effectiveness when controlling for peer quality, whereas stability-based estimates suggest that parents value both peer quality and school effectiveness, with a one-standard-deviation improvement in effectiveness raising average school tastes by 0.56 standard deviations.

In Appendix M, we present the same decomposition results but using the estimated posteriors after shrinkage. Our main findings remain unchanged. Assuming truth-telling, parents do not seem to value school effectiveness. Under the stability estimates, parents value both peer quality and school effectiveness in terms of test scores and wages.

In Appendix N, we show results using the control function estimates instead of the value-added estimates. Regarding average parental tastes, we only use the stability-based preference estimates because we derive the control function under the assumption of market stability. Our main results are not sensitive to the use of value-added or control function estimates. Parents value both peer quality and school effectiveness.

6.1 Discussion

Consider a school choice market where parents can rank an unlimited number of schools, and school priorities are lotteries. For simplicity, assume all schools use the same priorities and that the matching algorithm is the random serial dictatorship. In such an idealized market, a finding that parents do not choose schools based on school effectiveness would imply that parents do not value this school characteristic. However, such a market does not exist in practice. Instead, many markets ration seats using skill measures and impose constraints on application length. These additional features in the implementation of centralized markets blur the connection between preferences and choices, even under a matching algorithm that is theoretically strategy-proof.

Our results may have implications for previous findings in other markets. A finding that parental observed choices do not weight school effectiveness after controlling for peer quality (Abdulkadiroğlu et al., 2020; Ainsworth et al., 2023) only implies that parents do not value effectiveness if we take ROLs as literally representing parental most preferred schools. This is an arguably strong behavioral assumption. As a counterpoint, our results show an example of a market where parents do value school effectiveness in terms of test scores and wages, yet their ROLs do not reflect this.

Furthermore, evidence that only high-SES parents choose schools based on effectiveness (Ainsworth et al., 2023; Beuermann et al., 2023) is also consistent with our results, insofar as selective schools may be part of the feasible choice sets of these parents but not of low-SES parents. Our findings could also help explain the lack of effects on school assignments observed when providing information on school effectiveness (Ainsworth et al., 2023). If the most effective schools are out of reach for many parents, then providing information may alter ROLs without changing feasibility—and thus need not affect final assignments.

7 Conclusions

This paper examines whether parents value school effectiveness, while controlling for peer quality, in a centralized school market where ROLs are capped and a one-shot exam determines admissions. Using two decades of administrative data from Barbados, we exploit a policy change that restricted the application length by two-fifths. After the reform, many parents responded by 'skipping the impossible'—omitting the most selective schools from their ROLs. On the other hand, equilibrium assignments were unaffected. These findings show that binding constraints induce strategic behavior but also that parents anticipate admission probabilities well. Together, this evidence informs our modeling choices: in a constrained choice environment, truth-telling is unlikely to hold, but equilibrium stability proves a reasonable assumption.

Combining application data, school assignments, short-run test scores, and long-run wages, we estimate parental preferences for school effectiveness and peer quality under alternative assumptions. We find that application caps distort the link between observed ROLs and underlying preferences, biasing truth-telling estimates toward suggesting that parents do not value school effectiveness. In contrast, stability-based estimates, which are robust to strategic behavior, reveal that parents place significant weight on both school effectiveness and peer quality, consistently across short- and long-run outcomes.

Our findings carry important implications for policy and research. From a policy perspective, relaxing list-length constraints or providing additional information on school effectiveness is unlikely to substantially change student assignments, since parents mostly skip unattainable schools. Moreover, as our findings show that they value school effectiveness, it must be that they are aware of this school characteristic. At the same time, failing to account for list-length constraints can lead researchers to underestimate demand for effectiveness, thereby understating the role of increasing access to schools in fostering school competition.

Finally, although our data is specific to Barbados, the mechanisms we highlight—strategic omission under binding list-length caps and constrained effective choice sets—are common to many centralized school markets. Future work could explore how such constraints shape

the relationship between what parents value in schools and what their applications demand in other contexts. More broadly, our results suggest that assessing the potential of school choice reforms requires accounting not only for parental observed choices but also for the institutional features that limit families' ability to act on their preferences.

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A Survey Representativeness

Table A.1: Survey Representativeness: BSSEE Cohorts 1987 - 1995

Survey Status:	Not Surveyed	Matched Face to Face Survey	(1) = (2)
	(1)	(2)	(3)
Panel A: Sociodemographics			
Female	0.50	0.47	0.27
	(0.50)	(0.50)	
Month of birth: Jan - Mar	0.24	0.24	0.87
	(0.43)	(0.42)	
Month of birth: Apr - Jun	0.22	0.24	0.34
	(0.42)	(0.43)	
Month of birth: Jul - Sep	0.25	0.23	0.31
	(0.43)	(0.43)	
Month of birth: Oct - Dec	0.28	0.28	0.84
	(0.45)	(0.45)	
Panel B: Selectivity of Secondary School Choices (BSSEE score of incoming class)			
Choice 1	1.35	1.30	0.09
	(0.58)	(0.64)	
Choice 2	1.18	1.11	0.04
	(0.58)	(0.64)	
Choice 3	0.99	0.95	0.17
	(0.58)	(0.60)	
Choice 4	0.86	0.80	0.11
	(0.61)	(0.64)	
Choice 5	0.65	0.62	0.27
	(0.58)	(0.58)	
Choice 6	0.52	0.47	0.15
	(0.59)	(0.61)	
Choice 7	0.37	0.32	0.10
	(0.60)	(0.61)	
Choice 8	0.24	0.26	0.62
	(0.62)	(0.64)	
Choice 9	0.12	0.12	0.84
	(0.64)	(0.65)	
Panel C: Parish of Residency (before admission to secondary school)			
Parish 1	0.03	0.02	0.94
	(0.16)	(0.20)	
Parish 2	0.04	0.05	0.44
	(0.20)	(0.23)	
Parish 3	0.06	0.06	0.52
	(0.25)	(0.23)	

Table A.2: cont'd. A.1 Survey Representativeness: BSSEE Cohorts 1987 - 1995

Parish 4	0.04	0.03	0.69
	(0.19)	(0.23)	
Parish 5	0.04	0.05	0.35
	(0.19)	(0.23)	
Parish 6	0.41	0.43	0.49
	(0.49)	(0.48)	
Parish 7	0.03	0.03	0.75
	(0.16)	(0.20)	
Parish 8	0.08	0.06	0.09
	(0.27)	(0.26)	
Parish 9	0.07	0.08	0.43
	(0.26)	(0.28)	
Parish 10	0.04	0.05	0.62
	(0.20)	(0.23)	
Parish 11	0.16	0.14	0.41
	(0.37)	(0.33)	
Panel D: CSEC Outcomes (after 5 years of secondary school)			
Took CSEC	0.56	0.61	0.05
	(0.50)	(0.49)	
Number of CSEC subjects taken	3.23	3.35	0.48
	(3.38)	(3.29)	
Number of CSEC subjects passed	2.44	2.51	0.65
	(2.99)	(2.92)	
Qualified for tertiary *	0.21	0.23	0.38
	(0.41)	(0.41)	
Observations	36,558	516	

Notes: This table includes all individuals who took the BSSEE before the policy change (i.e., between 1987 and 1995). Standard deviations are reported in parentheses below the means. Column (1) reports means and standard deviations of individuals who were not surveyed. Column (2) reports means and standard deviations of individuals who were surveyed and matched with the BSSEE administrative dataset. Estimates in column (2) are weighted by the inverse of sampling probability to reflect survey design. Column (3) reports the p-value of a test for the equality of means reported in columns (1) and (2) adjusting for BSSEE cohorts fixed effects. * Qualification for tertiary education requires passing five CSEC subjects including English and mathematics.

Table A.3: Survey Representativeness: BSSEE Cohorts 1996 - 2011

Survey Status:	Not Surveyed	Matched Face to Face Survey	(1) = (2)
	(1)	(2)	(3)
Panel A: Sociodemographics			
Female	0.50	0.49	0.75
	(0.50)	(0.50)	
Month of birth: Jan - Mar	0.24	0.24	0.89
	(0.43)	(0.43)	
Month of birth: Apr - Jun	0.22	0.19	0.14
	(0.41)	(0.40)	
Month of birth: Jul - Sep	0.25	0.26	0.59
	(0.43)	(0.43)	
Month of birth: Oct - Dec	0.30	0.31	0.40
	(0.46)	(0.46)	
Panel B: Selectivity of Secondary School Choices (BSSEE score of incoming class)			
Choice 1	1.04	1.04	0.84
	(0.62)	(0.60)	
Choice 2	0.78	0.79	0.49
	(0.68)	(0.67)	
Choice 3	0.82	0.81	0.53
	(0.58)	(0.60)	
Choice 4	0.45	0.44	0.67
	(0.58)	(0.60)	
Choice 5	0.12	0.12	0.89
	(0.63)	(0.62)	
Choice 6	-0.23	-0.22	0.53
	(0.65)	(0.62)	
Choice 7	-0.55	-0.54	0.36
	(0.63)	(0.63)	
Choice 8	-0.76	-0.77	0.92
	(0.72)	(0.72)	
Choice 9	-1.02	-1.05	0.83
	(0.74)	(0.71)	
Panel C: Parish of Residency (before admission to secondary school)			
Parish 1	0.02	0.01	¡0.01
	(0.14)	(0.16)	
Parish 2	0.04	0.05	0.40
	(0.20)	(0.26)	
Parish 3	0.07	0.08	0.24
	(0.25)	(0.27)	

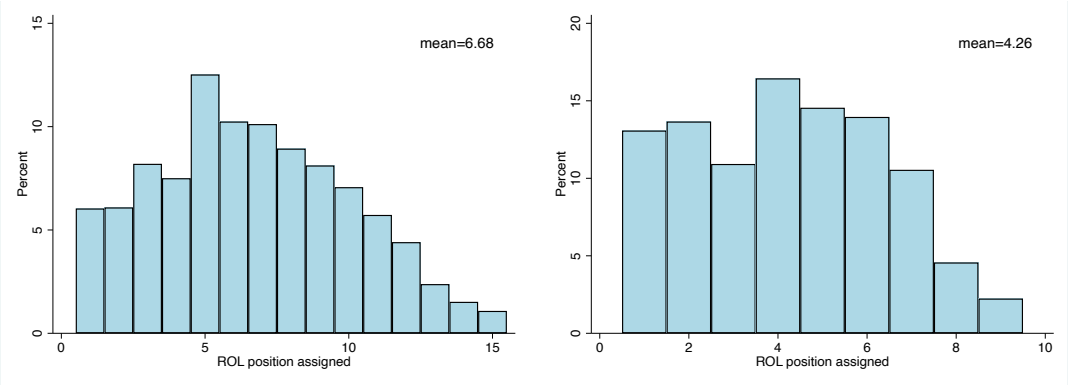
Table A.4: cont'd. A.3 Survey Representativeness: BSSEE Cohorts 1996 - 2011

Parish 4	0.04	0.03	0.22
	(0.19)	(0.20)	
Parish 5	0.03	0.05	0.08
	(0.18)	(0.22)	
Parish 6	0.33	0.32	0.35
	(0.47)	(0.43)	
Parish 7	0.02	0.03	0.42
	(0.14)	(0.19)	
Parish 8	0.09	0.10	0.56
	(0.29)	(0.31)	
Parish 9	0.08	0.09	0.29
	(0.27)	(0.29)	
Parish 10	0.05	0.04	0.46
	(0.22)	(0.24)	
Parish 11	0.22	0.21	0.44
	(0.42)	(0.39)	
Panel D: CSEC Outcomes (after 5 years of secondary school)			
Took CSEC	0.75	0.81	<0.01
	(0.43)	(0.39)	
Number of CSEC subjects taken	4.75	5.22	<0.01
	(3.49)	(3.35)	
Number of CSEC subjects passed	3.35	3.55	0.11
	(3.17)	(3.04)	
Qualified for tertiary *	0.30	0.31	0.77
	(0.46)	(0.46)	
Observations	57,288	1,029	

Notes: This table includes all individuals who took the BSSEE after the policy change (i.e., between 1996 and 2011). Standard deviations are reported in parentheses below the means. Column (1) reports means and standard deviations of individuals who were not surveyed. Column (2) reports means and standard deviations of individuals who were surveyed and matched with the BSSEE administrative dataset. Estimates in column (2) are weighted by the inverse of sampling probability to reflect survey design. Column (3) reports the p-value of a test for the equality of means reported in columns (1) and (2) adjusting for BSSEE cohorts fixed effects. * Qualification for tertiary education requires passing five CSEC subjects including English and mathematics.

B ROL position assigned

Figure B.1: ROL position assigned



(a) 1987-1995 (b) 1996-2011

NOTE: This figure shows the distribution of ROL position assignments before (panel a) and after (panel b) the policy change.

C First and second choices

Table C.1: Policy change effect on first choice

	Harrison College is first		
Post	-0.352***	-0.362***	-0.389***
	(0.003)	(0.005)	(0.006)
Time		0.001***	0.009***
		(0.000)	(0.001)
PostxTime			-0.009***
			(0.001)
Constant	0.468***	0.472***	0.511***
	(0.003)	(0.003)	(0.006)
N	95,391	95,391	95,391

NOTE: This table shows regression estimates of the policy change effect. Post is a dummy variable that indicates the post-policy period. Time is the admission cohort centered at the year of the policy change (1996). Each column shows a different specification. Standard errors in parenthesis.

Table C.2: Policy change effect on second choice

	Queens College is second		
Post	-0.344***	-0.358***	-0.392***
	(0.003)	(0.004)	(0.006)
Time		0.001***	0.011***
		(0.000)	(0.001)
PostxTime			-0.012***
			(0.001)
Constant	0.420***	0.425***	0.475***
	(0.003)	(0.003)	(0.006)
N	95,391	95,391	95,391

NOTE: This table shows regression estimates of the policy change effect. Post is a dummy variable that indicates the post-policy period. Time is the admission cohort centered at the year of the policy change (1996). Each column shows a different specification. Standard errors in parenthesis.

D Harrison College or Queens College included in ROL

Table D.1: Policy change effect on including Harrison College

	Harrison College in ROL		
Post	-0.386***	-0.355***	-0.339***
	(0.003)	(0.006)	(0.007)
Time		-0.002***	-0.007***
		(0.000)	(0.001)
PostxTime			0.005***
			(0.001)
Constant	0.614***	0.602***	0.579***
	(0.003)	(0.003)	(0.006)
N	95,391	95,391	95,391

NOTE: This table shows regression estimates of the policy change effect. Post is a dummy variable that indicates the post-policy period. Time is the admission cohort centered at the year of the policy change (1996). Each column shows a different specification. Standard errors in parenthesis.

Table D.2: Policy change effect on including Queens College

	Queens College in ROL		
Post	-0.275***	-0.307***	-0.301***
	(0.003)	(0.006)	(0.007)
Time		0.003***	0.001
		(0.000)	(0.001)
PostxTime			0.002*
			(0.001)
Constant	0.688***	0.701***	0.693***
	(0.002)	(0.003)	(0.005)
N	95,391	95,391	95,391

NOTE: This table shows regression estimates of the policy change effect. Post is a dummy variable that indicates the post-policy period. Time is the admission cohort centered at the year of the policy change (1996). Each column shows a different specification. Standard errors in parenthesis.

Table D.3: Policy change effect on including Harrison College for top scorers

	Harrison College in ROL (top scorers)		
Post	-0.191***	-0.082*	-0.083**
	(0.021)	(0.043)	(0.042)
Time		-0.009***	-0.009**
		(0.003)	(0.004)
PostxTime			-0.000
			(0.006)
Constant	0.949***	0.905***	0.906***
	(0.012)	(0.021)	(0.028)
N	926	926	926

NOTE: This table shows regression estimates of the policy change effect. Post is a dummy variable that indicates the post-policy period. Time is the admission cohort centered at the year of the policy change (1996). Each column shows a different specification. Standard errors in parenthesis.

Table D.4: Policy change effect on including Queens College for top scorers

	Queens College in ROL (top scorers)		
Post	-0.092***	-0.042	-0.046
	(0.016)	(0.034)	(0.031)
Time		-0.004	-0.003
		(0.003)	(0.003)
PostxTime			-0.001
			(0.004)
Constant	0.975***	0.955***	0.960***
	(0.008)	(0.016)	(0.019)
N	926	926	926

NOTE: This table shows regression estimates of the policy change effect. Post is a dummy variable that indicates the post-policy period. Time is the admission cohort centered at the year of the policy change (1996). Each column shows a different specification. Standard errors in parenthesis.

E Difference-in-Differences: top scorers vs others

Table E.1: DiD estimates

	Harrison College	Queens College
Post	-0.191***	-0.092***
	(0.021)	(0.016)
Treat	-0.338***	-0.290***
	(0.012)	(0.009)
TreatxPost	-0.197***	-0.185***
	(0.022)	(0.016)
Constant	0.949***	0.975***
	(0.012)	(0.008)
N	95,391	95,391

NOTE: This table shows estimates from a difference in differences specification that uses the top scorers as the control group and all other students as the treatment group. Standard errors in parenthesis.

F Assignments

Table F.1: Policy change effect on average admission score at Big Four schools

	Admission exam score		
Post	-2.792***	1.018***	0.637***
	(0.106)	(0.199)	(0.217)
Time		-0.299***	-0.195***
		(0.013)	(0.030)
PostxTime			-0.121***
			(0.033)
Constant	243.407***	241.936***	242.448***
	(0.082)	(0.104)	(0.172)
N	14,218	14,218	14,218

NOTE: This table shows regression estimates of the policy change effect. Post is a dummy variable that indicates the post-policy period. Time is the admission cohort centered at the year of the policy change (1996). Each column shows a different specification. Standard errors in parenthesis.

Table F.2: Policy change effect on average admission score at non-Big Four schools

	Admission exam score		
Post	-4.281***	-3.205***	-3.210***
	(0.175)	(0.330)	(0.364)
Time		-0.086***	-0.085*
		(0.023)	(0.052)
PostxTime			-0.002
			(0.057)
Constant	198.158***	197.721***	197.728***
	(0.135)	(0.176)	(0.293)
N	74,265	74,265	74,265

NOTE: This table shows regression estimates of the policy change effect. Post is a dummy variable that indicates the post-policy period. Time is the admission cohort centered at the year of the policy change (1996). Each column shows a different specification. Standard errors in parenthesis.

Table F.3: Policy change effect on SES index at Big Four schools

	SES index		
Post	0.009	0.024**	0.020
	(0.006)	(0.011)	(0.013)
Time		-0.001*	-0.000
		(0.001)	(0.002)
PostxTime			-0.001
			(0.002)
Constant	0.043***	0.037***	0.043***
	(0.005)	(0.006)	(0.011)
N	13,886	13,886	13,886

NOTE: This table shows regression estimates of the policy change effect. Post is a dummy variable that indicates the post-policy period. Time is the admission cohort centered at the year of the policy change (1996). Each column shows a different specification. Standard errors in parenthesis.

Table F.4: Policy change effect on SES index at non-Big Four schools

	SES index		
Post	0.017***	0.011**	0.012**
	(0.003)	(0.005)	(0.006)
Time		0.000	0.000
		(0.000)	(0.001)
PostxTime			0.000
			(0.001)
Constant	-0.087***	-0.085***	-0.086***
	(0.002)	(0.003)	(0.005)
N	72,843	72,843	72,843

NOTE: This table shows regression estimates of the policy change effect. Post is a dummy variable that indicates the post-policy period. Time is the admission cohort centered at the year of the policy change (1996). Each column shows a different specification. Standard errors in parenthesis.

Table F.5: Policy change effect on the share of Big Ten students at Big Four schools

	Big Ten		
Post	0.041***	0.091***	0.012
	(0.009)	(0.016)	(0.018)
Time		-0.004***	0.018***
		(0.001)	(0.003)
PostxTime			-0.025***
			(0.003)
Constant	0.429***	0.410***	0.516***
	(0.007)	(0.009)	(0.015)
N	14,218	14,218	14,218

NOTE: This table shows regression estimates of the policy change effect. Post is a dummy variable that indicates the post-policy period. Time is the admission cohort centered at the year of the policy change (1996). Each column shows a different specification. Standard errors in parenthesis.

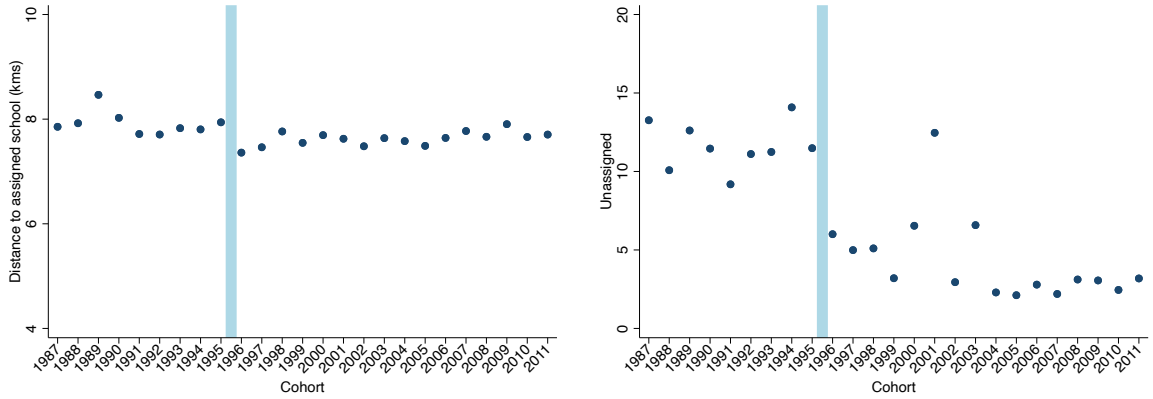
Table F.6: Policy change effect on the share of Big Ten students at non-Big Four schools

	Big Ten		
Post	0.039***	0.017***	0.003
	(0.003)	(0.005)	(0.006)
Time		0.002***	0.006***
		(0.000)	(0.001)
PostxTime			-0.005***
			(0.001)
Constant	0.157***	0.165***	0.185***
	(0.002)	(0.003)	(0.005)
N	74,265	74,265	74,265

NOTE: This table shows regression estimates of the policy change effect. Post is a dummy variable that indicates the post-policy period. Time is the admission cohort centered at the year of the policy change (1996). Each column shows a different specification. Standard errors in parenthesis.

G Distance to assigned school and share of unassigned

Figure G.1: Distance to assigned school and share of unassigned



(a) Distance to assigned secondary

(b) Unassigned

Note: Panel (a) plots the average distance to the assigned secondary school before and after the policy change. Panel (b) plots the share of unassigned students before and after the policy change. The vertical shaded bar marks the year of the policy change, and the x-axis denotes the application cohort.

Table G.1: Policy change effect on distance to assigned school

	Distance to assigned school		
Post	-0.265***	-0.398***	-0.296***
	(0.040)	(0.071)	(0.082)
Time		0.011**	-0.018
		(0.005)	(0.012)
PostxTime			0.034**
			(0.013)
Constant	7.888***	7.941***	7.800***
	(0.033)	(0.040)	(0.069)
N	86,729	86,729	86,729

NOTE: This table shows regression estimates of the policy change effect. Post is a dummy variable that indicates the post-policy period. Time is the admission cohort centered at the year of the policy change (1996). Each column shows a different specification. Standard errors in parenthesis.

Table G.2: Policy change effect on the probability of being unassigned

	Unassigned		
Post	-0.072***	-0.044***	-0.053***
	(0.002)	(0.003)	(0.004)
Time		-0.002***	0.000
		(0.000)	(0.001)
PostxTime			-0.003***
			(0.001)
Constant	0.117***	0.105***	0.118***
	(0.002)	(0.002)	(0.004)
N	95,391	95,391	95,391

NOTE: This table shows regression estimates of the policy change effect. Post is a dummy variable that indicates the post-policy period. Time is the admission cohort centered at the year of the policy change (1996). Each column shows a different specification. Standard errors in parenthesis.

H Preferences estimates

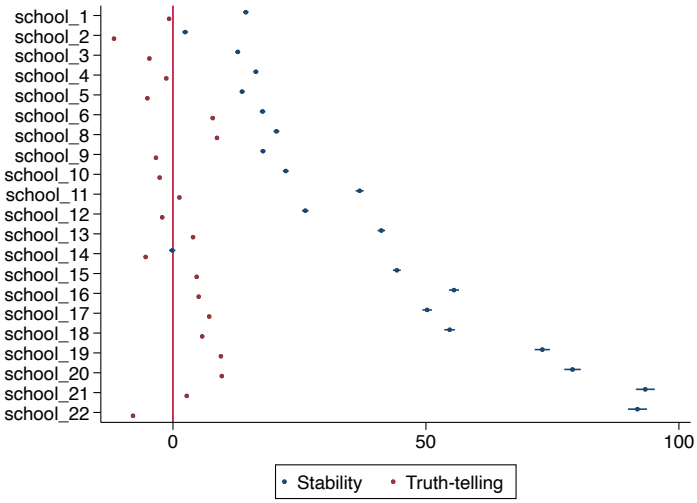
Table H.1: Preferences estimates

	Truth-telling	Stability
school_1	-0.247 (0.014)	1.382 (0.040)
school_2	-0.740 (0.012)	0.247 (0.037)
school_3	-0.562 (0.012)	0.881 (0.037)
school_4	0.044 (0.011)	1.606 (0.039)
school_5	0.237 (0.009)	1.619 (0.037)
school_6	0.641 (0.011)	2.523 (0.041)
school_7	0.459 (0.011)	2.463 (0.040)
school_8	0.509 (0.011)	2.692 (0.040)
school_9	0.395 (0.010)	2.478 (0.040)
school_10	0.638 (0.009)	2.745 (0.041)
school_11	-0.244 (0.011)	0.149 (0.043)
school_12	1.227 (0.009)	4.315 (0.046)
school_13	0.784 (0.010)	5.310 (0.056)
school_14	1.247 (0.008)	4.849 (0.054)
school_15	1.333 (0.010)	6.571 (0.060)
school_16	1.460 (0.010)	7.420 (0.066)
school_17	1.255 (0.010)	7.494 (0.065)
school_18	1.528 (0.010)	8.523 (0.069)
school_19	2.040 (0.009)	11.156 (0.118)
school_20	2.009 (0.009)	11.936 (0.122)
school_21	1.269 (0.009)	14.754 (0.149)
school_22	0.908 (0.009)	16.649 (0.170)
Distance	-0.061 (0.000)	-0.104 (0.001)
N	754,486	390,606
Hausman test (p-value)		0.000

NOTE: This table reports estimated preference parameters. Column (1) presents estimates under the assumption of truth-telling, while Column (2) presents estimates under the assumption of market stability. Schools are indexed by selectivity, with higher values indicating greater selectivity. The Hausman test compares the stability estimates, which are consistent but less efficient, with the truth-telling estimates, which are efficient but potentially inconsistent. Standard errors are reported in parentheses.

I Preferences estimates after policy change

Figure I.1: Average tastes in willingness-to-travel ($\frac{\delta_j}{\lambda}$): 1997-2011



NOTE: This figure shows preferences estimates in willingness-to-travel under two behavioral assumptions. The red dots show the estimates and associated confidence intervals we obtain when assuming truth-telling. The blue dots show the estimates and associated confidence intervals we obtain under market stability. On the y-axis, we index schools by selectivity, a higher index denotes a more selective school.

J Control function

Given our choice model, under the linearity assumption of Dubin and McFadden (1984) we can derive control function terms as follows:

$$\lambda_j = -ln(P_j) \qquad \text{for } j \text{ chosen,}$$

$$\lambda_{j'} = \frac{P_{j'} ln(P_{j'})}{1 - P_{j'}} \qquad \text{for } j' \text{ not chosen.}$$

Notice that in our case the choice probabilities depend on the feasible choice sets Ω_i so our control function terms also depend on them. Abdulkadiroğlu et al. (2020) work under the following restriction:

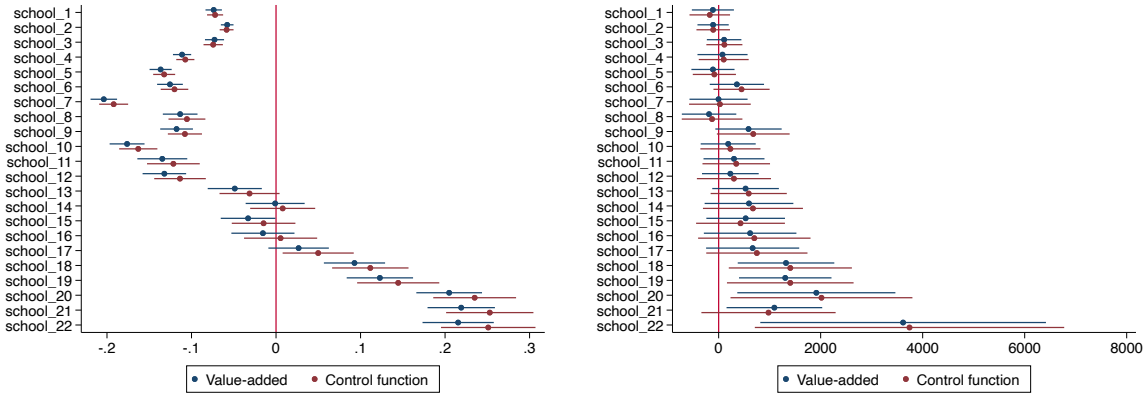
$$E[Y_i \mid X_i, D_i, S_i = j] = \alpha_j + X_i'\beta + \sum_{k=1}^{J} \psi_k \lambda_k(X_i, D_i, \Omega_i) + \varphi\lambda_j(X_i, D_i, \Omega_i).$$

We do not have enough observations to include $\sum_{k=1}^{J} \psi_k \lambda_k(X_i, D_i, \Omega_i)$ in our wage equation, so we simplify the restriction as:

$$E[Y_i \mid X_i, D_i, S_i = j] = \alpha_j + X_i'\beta + \varphi\lambda_j(X_i, D_i, \Omega_i),$$

which relies on an index sufficiency assumption similar to Dahl (2002). In this case the index sufficiency assumption is that the control function only depends on the highest choice probability. We use the vector of distances from each student to each school D_i as the exclusion restriction.

Figure J.1: Value-added and control function estimates, 1987-1995



(a) Qualified for tertiary

(b) Monthly wages

NOTE: Panel (a) shows value-added and control function estimates for tertiary school qualification as an outcome. Panel (b) shows value-added and control function estimates for wages as an outcome. Bars indicate 95% confidence intervals. In the y-axis, we index schools by selectivity, a higher index denotes a more selective school.

K Value-added validation

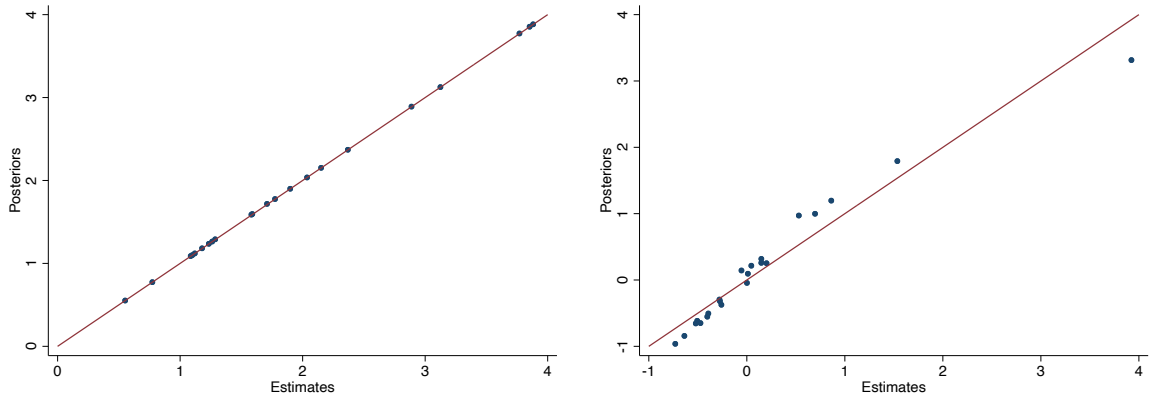
Table K.1: RDD-based tests for bias in estimates of school effectiveness

	Uncontrolled	Value-added
$\hat{\phi}$	0.827	0.961
	(0.024)	(0.027)
Forecast (p-val)	0.000	0.144
Overid. (p-val)	0.000	0.030

NOTE: This table reports estimated forecast coefficients for the uncontrolled and value-added models. The forecast p-value corresponds to a test of the null hypothesis that the forecast coefficient equals one. The overidentification test p-value is from the Sargan (1958) test.

L MEB posteriors

Figure L.1: Shrinkage



(a) Qualified for tertiary

(b) Monthly wages

NOTE: Panel (a) compares value-added estimates with posterior estimates using test scores as the outcome. Panel (b) compares value-added estimates with posterior estimates using wages as the outcome. The red line indicates the 45-degree line.

M Decomposition using MEB posteriors

Table M.1: Preferences determinants using posteriors: 1987-1995

	Qualified for tertiary		Monthly wages	
	Truth-telling	Stability	Truth-telling	Stability
Q_j^*	1.089***	0.586***	0.970***	0.532***
	(0.103)	(0.040)	(0.216)	(0.089)
α_j^*	-0.284	0.461***	-0.174	0.506***
	(0.166)	(0.058)	(0.283)	(0.060)
Observations	22	22	22	22

NOTE: This table reports estimates of the relationship between peer quality, school effectiveness, and average preferences for schools. Columns labeled 'Truth-telling' use preference estimates under the assumption of truth-telling, while columns labeled 'Stability' use preference estimates under the assumption of market stability. Columns (1) and (2) pool data across years and measure school effectiveness and peer quality using test scores. Columns (3) and (4) also pool data across years but measure school effectiveness and peer quality using adult wages. Standard errors are reported in parentheses.

N Decomposition using control function estimates

Table N.1: Preferences determinants using control function estimates: 1987-1995

	Qualified for tertiary	Monthly wages
$\hat{Q}_j$	0.466***	0.304**
	(0.071)	(0.112)
$\hat{\alpha}_j$	0.563***	0.733***
	(0.102)	(0.109)
Observations	22	22

NOTE: This table reports estimates of the relationship between peer quality, school effectiveness, and average preferences for schools. Peer quality and school effectiveness are estimated using a control function to deal with selection on unobservables. Average school preferences are estimated under the assumption of market stability. Standard errors are reported in parentheses.