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 parents often cannot freely choose among schools. We study preferences for school effectiveness and peer quality in the centralized secondary school market of Barbados, where admissions are based on a one-shot exam and parents face a binding cap on the number of schools they can list. Exploiting a policy reform that further tightened this cap, we show that parents respond by 'skipping the impossible': omitting the most selective schools from their applications. We then estimate preferences for school effectiveness, measured by impacts on test scores and adult wages, and peer quality under alternative behavioral assumptions. Under the truth-telling assumption that applications reveal parents' most preferred schools, we find no valuation of school effectiveness conditional on peer quality. In contrast, under market-stability assumptions that allow parents to omit desirable but seemingly unattainable schools, we find that parents value both school effectiveness and peer quality. The difference arises because the most selective schools are also the most effective. As a result, approaches that interpret omitted schools as less preferred understate parental demand for school effectiveness.

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 education systems use centralized mechanisms to assign students to schools (Neilson, 2024). In these systems, parents submit rank-ordered lists (ROLs) of schools, and assignments are determined by a matching mechanism. Whether submitted ROLs reveal parents' most preferred schools depends both on the incentive properties of the matching mechanism and on the institutional constraints under which parents submit their applications. Many centralized school markets are designed so that truthful reporting is optimal when application lists are unconstrained. In practice, however, the length of submitted ROLs is often constrained. Such constraints can lead parents to omit desirable but seemingly unattainable schools, making it difficult to interpret omitted schools as evidence that parents value them less than the schools they rank (Fack et al., 2019).

Understanding parental preferences is essential for evaluating policy reforms in centralized school markets. Consider a setting where seats are rationed using scores on a one-shot exam and parents face a binding cap on the number of schools they can list. Policymakers may seek to expand access to schools by changing admission rules or relaxing application constraints. The consequences of such reforms depend critically on whether parents value peer quality or school effectiveness. Recovering these preferences, however, is challenging when institutional constraints affect what submitted applications reveal about underlying demand. If parents primarily value peer quality, expanding access may mainly alter the allocation of students across schools. In contrast, if parents value school effectiveness, expanding access to more effective schools may improve educational outcomes and strengthen 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 constraint is binding. We first study how parents respond to this constraint and use the resulting evidence to determine how omitted schools should be interpreted when recovering parental preferences. We then estimate preferences for school effectiveness and peer quality under alternative behavioral assumptions. Importantly, we measure school effectiveness using both short-run academic outcomes and long-run adult wages to assess whether our conclusions are robust across alternative measures of school quality.

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 parents face a binding cap on ROL length. To assess the role of this constraint, we exploit a policy reform that further reduced the maximum application length and examine its effects on application behavior and student assignments. We then combine application and assignment data with the institutional structure of the market to estimate parental preferences under two alternative behavioral assumptions: truth-telling and market stability.[1] To measure school effectiveness, we link application records to secondary school exit examinations that determine tertiary education eligibility and to a nationally representative household survey conducted in 2016 that provides adult wage information for participants aged 25 to 40. These data allow us to estimate school effectiveness using both short-run academic outcomes and long-run adult wages.

We first show that further restricting ROL length led parents to adjust their applications in ways consistent with 'skipping the impossible': they omitted the most selective schools from their submitted ROLs. At the same time, the reform had little effect on equilibrium assignments. This pattern is consistent with parents making payoff-irrelevant omissions (Artemov et al., 2023): omitting schools that are unattainable changes submitted applications without affecting realized assignments. These findings suggest that binding application constraints can substantially weaken the link between submitted ROLs and underlying preferences while leaving assignment outcomes largely unchanged.

The evidence from the policy reform has two important implications for the estimation of parental preferences. First, it shows that, in a constrained environment, submitted ROLs may provide an incomplete picture of parental preferences because they may exclude schools that parents prefer to those that are ranked. Second, the fact that a tighter application

[1]Truth-telling assumes that parents reveal their most preferred schools in their submitted ROLs, implying that omitted schools are less preferred than those that are ranked. Market stability assumes that students are assigned to their parents' most preferred ex-post feasible school.

cap changes submitted ROLs but not assignments suggests that many of these omissions are payoff-irrelevant. This finding makes the market-stability assumption more plausible, even though, in our setting, ROLs are submitted before admission exam scores are known. Consistent with this interpretation, fewer than 10% of students who score high enough to qualify for a selective school have parents who do not apply to one.

Informed by these findings, we estimate parental preferences for schools using two approaches. The first, which we refer to as the truth-telling approach, assumes that submitted ROLs reveal parents' most preferred schools (Hastings et al., 2009; Abdulkadiroğlu et al., 2017b, 2020; Ainsworth et al., 2023; Campos and Kearns, 2024). Although our evidence suggests that this assumption may not hold when ROLs are constrained, submitted ROLs nonetheless influence student assignments and therefore contain partial information about parental preferences. The second approach assumes that the matching equilibrium is stable (Fack et al., 2019; Artemov et al., 2023). We implement this approach both by recovering preferences from assignment data alone and, as a robustness exercise, by combining the preference relations implied by market stability with those observed in submitted ROLs using moment inequalities. Our earlier findings provide empirical support for this assumption. The two approaches yield substantially different estimates of parental preferences, particularly for the most selective schools in the market.

Next, we estimate school effectiveness and derive measures of peer quality using both test scores and adult wages. Because students are not randomly assigned to schools, identification requires additional assumptions. We begin with a value-added model under selection on observables and find that, although school averages overstate effectiveness, the most selective schools are also the most effective at improving both short- and long-run outcomes. We then show that these findings are robust to relaxing the selection-on-observables assumption using a control-function approach (Dubin and McFadden, 1984; Dahl, 2002; Abdulkadiroğlu et al., 2020), validate the estimates using admission discontinuities (Angrist et al., 2017; Abdulkadiroğlu et al., 2022), and improve precision through multivariate empirical Bayes shrinkage (Walters, 2024).

Combining our preference estimates with our estimates of school effectiveness and peer quality, we decompose average parental valuations of schools into components associated with peer quality and school effectiveness. Under the truth-telling assumption, parents appear not to value school effectiveness once peer quality is taken into account. In contrast, the stability-based estimates reveal that parents place significant weight on both school effectiveness and peer quality, with remarkably similar results using both short-run academic outcomes and long-run adult wages. The difference arises because the truth-telling approach interprets omitted selective schools as less preferred, rather than as parents 'skipping the impossible'. Because the most selective schools are also among the most effective, this assumption understates parental demand for school effectiveness.

Taken together, our findings show that market design features can affect the extent to which submitted ROLs provide a complete picture of parental preferences for school effectiveness. In particular, the combination of a one-shot priority exam and a binding application cap leads many parents to omit highly effective schools that they perceive as unattainable. More broadly, our results suggest that understanding how institutional constraints shape the relationship between submitted applications and underlying preferences is essential for accurately measuring parental demand and designing policies that improve the functioning of education markets.

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 depend on whether application lists are constrained. Theoretically, constraining application lists induces strategic behavior (Haeringer and Klijn, 2009), and laboratory experiments document reduced truth-telling under such constraints (Calsamiglia et al., 2010). We complement this evidence by showing that binding application constraints alter application behavior and can substantially distort inference about what parents value in schools.

Second, we contribute to the literature on parental valuation of school effectiveness and peer quality. Existing studies typically assume that submitted 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 when application constraints appear non-binding (Abdulkadiroğlu et al., 2020). Evidence on parental valuation of school effectiveness, net of peer quality, remains mixed. Some studies find that parents do not value school effectiveness once peer quality is accounted for, while others find the opposite (Angrist et al., 2023). We study a setting with a binding application constraint, where our evidence suggests that truth-telling is less plausible. Unlike Beuermann et al. (2023), who allow for misrepresentation while relying on adaptations of truth-telling estimators, 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 understates low-SES preferences for school quality. We similarly show that ignoring a binding constraint on ROL length can lead researchers to understate parental demand for school effectiveness.

Third, we contribute to the literature on measuring school effectiveness (Angrist et al., 2023). Our data allow us to estimate the effects of individual schools on both test scores and adult wages, 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 using 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 the effects of individual schools on adult wages. Altonji and Mansfield (2018) bound the contribution of schools to wage variation using a control function approach. In contrast, we exploit the structure of a centralized school market to estimate the effects of individual schools on both test scores and adult wages under alternative identifying assumptions.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background. Section 3 describes the administrative and survey data. Section 4 presents evidence on the effects of the policy change. Section 5 outlines the empirical framework and presents estimates of parental preferences, school effectiveness, and peer quality. Section 6 analyzes parental valuation of school effectiveness and peer quality in a constrained environment. Section 7 concludes.

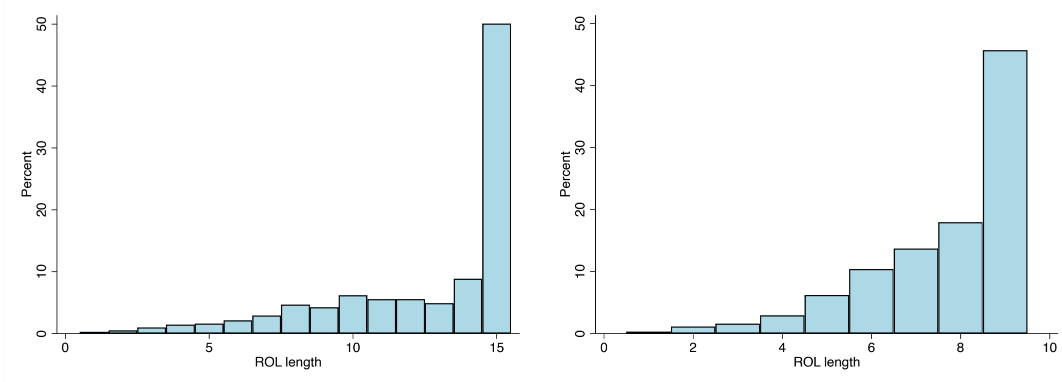
2 Institutional background

Barbados has a centralized system governing admission to public secondary schools. Near the end of primary school (sixth grade), parents submit a rank-ordered list (ROL) of secondary school choices to the Ministry of Education, and their children subsequently take the Barbados Secondary School Entrance Examination (BSSEE). Typically, ROLs are due by the end of January, while the exam is administered in early May. As a result, parents must submit school choices before observing their child's BSSEE score. The BSSEE consists of three components—mathematics, English language, and an essay—and the total score ranges from 0 to 200. Individual school capacity is predetermined by gender. With the exception of two single-sex schools (one for girls and one for boys), all schools are coeducational, with half of seats reserved for girls and half for boys.

Admissions are determined through a serial dictatorship based on BSSEE scores. Students are ranked according to their total BSSEE score and, in descending order, each student is assigned to the highest-ranked school in their parents' submitted ROL with an available seat, subject to gender-specific capacity constraints. This algorithm is a special case of the DA mechanism in which all schools share the same priority ranking of students. Once assignments have been completed, students are informed of both their BSSEE score and their assigned secondary school. Some students remain unassigned if all schools listed in their ROL are full by the time their turn in the serial dictatorship arrives. These students are subsequently assigned by the government to schools with remaining capacity.

Between 1987 and 1995, parents could rank up to fifteen schools out of the 22 schools operating in the country, with no geographical restrictions on school choice. Starting in 1996, several changes were introduced. Most importantly for our analysis, the maximum length of the ROL was reduced from fifteen to nine schools. In addition, schools ranked third through ninth were required to be located within the geographical zone in which the family resided. For this purpose, the island was divided into three geographical zones. The first two positions on the ROL remained unrestricted. Finally, each secondary school was required to admit at least 30 percent of its students from within its zone. The two single-sex schools were exempt from both the geographical choice restrictions and the minimum

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.

local intake requirement. As we show later, distance is an important determinant of school demand, suggesting that the geographic restrictions may have been less consequential than the reduction in application length.

Figure 1 shows that the ROL length constraint was binding both before and after the policy change. Before the reform, nearly half of parents ranked the maximum of fifteen schools. After the reform, nearly half ranked the maximum of nine schools. In both periods, the distribution of ROL lengths exhibits substantial bunching at the maximum permitted length. Appendix B reports the distribution of assignment positions within submitted ROLs. Before the policy change, students were assigned, on average, to their 6.68th-ranked school, whereas after the reform they were assigned, on average, to their 4.26th-ranked school. These averages indicate that assignments frequently occurred beyond the top few positions in submitted ROLs.

Secondary school in Barbados comprises five years (termed forms), beginning 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 Certificate (CSEC) examinations. These examinations are the Caribbean equivalent of the British Ordinary Level (O-level) examinations and are externally graded by the Caribbean Examinations Council (CXC). The CSEC covers 33 subjects. To be eligible for university admission, students must pass at least five subjects, including English and mathematics. In addition, many entry-level positions in the public sector require a minimum of five CSEC passes. As a result, CSEC performance is a high-stakes measure of academic achievement and provides a natural benchmark for assessing school effectiveness.

3 Data

We observe the universe of students who applied to public secondary schools in Barbados between 1987 and 2011.[2] The administrative records, obtained from the Ministry of Education, contain each applicant's BSSEE score, date of birth, gender, primary school attended, parish of residence, submitted rank-ordered list (ROL) of secondary schools, and secondary-school assignment.

To measure student performance in secondary school, we collected individual-level records from the Caribbean Secondary Education Certificate (CSEC) examinations. The CSEC data are available for all years between 1993 and 2016 and contain scores for each subject examination taken as well as the secondary school attended. We linked these records to students in the 1987–2011 BSSEE cohorts using full name (first, middle, and last), date of birth, and gender.[3]

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 information on educational attainment, employment, and wages. Importantly, the survey also collected information that facilitates linkage to the BSSEE records. Specifically, respondents were asked to report their full names at age 10 (to account for name changes), dates of birth, and gender. To study labor-market outcomes, we restrict attention to BSSEE cohorts that were at least 25 years old at the time of the survey, corresponding to the 1987–2002 cohorts. We focus on individuals aged 25 and older because survey data indicate that 99 percent had

[2]Approximately 91 percent of secondary-school students in Barbados are enrolled in the public education system.

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

completed formal schooling by that age. Using these identifiers, we matched 90 percent of surveyed individuals meeting our age criterion to the BSSEE records.[4]

Table 1 reports summary statistics by cohort. Column 1 corresponds to students who took the BSSEE before the policy change (1987–1995), while Column 2 corresponds to students who took it afterward (1996–2011). The share of female students is similar across cohorts. Average admitted-school cohort size declined slightly, from 160 students before the reform to 155 afterward, likely reflecting demographic changes over time. The table also illustrates substantial variation in educational outcomes. Relative to the earlier cohort, students in the later cohort were more likely to sit at least one CSEC examination, took more CSEC subjects, passed more subjects, and were more likely to satisfy the requirements for tertiary education. Consistent with these differences, the later cohort attained more years of schooling and was more likely to complete secondary school and obtain a university degree. Employment rates are similar across cohorts, whereas earnings are higher among the older cohort, likely reflecting greater labor-market experience.

4 Policy change

This section examines how the 1996 policy reform affected applications and assignments, with particular attention to the reduction in the maximum ROL length from fifteen to nine schools. 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 inclusion of the most selective schools in submitted ROLs. Finally, we investigate whether these changes translated into different equilibrium assignments.

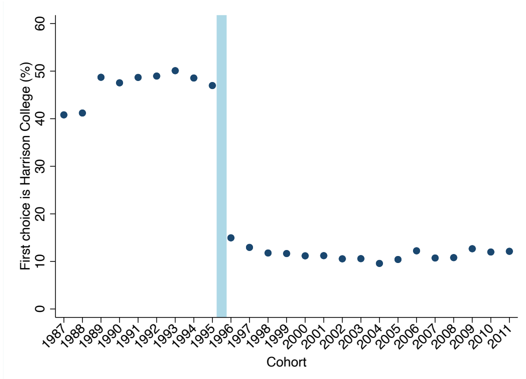
[4] Appendix Tables A.1 and A.3 show that the matched survey sample is representative of the population, as the distributions of BSSEE and CSEC scores in the survey sample closely mirror those in the broader population.

Table 1: Summary Statistics

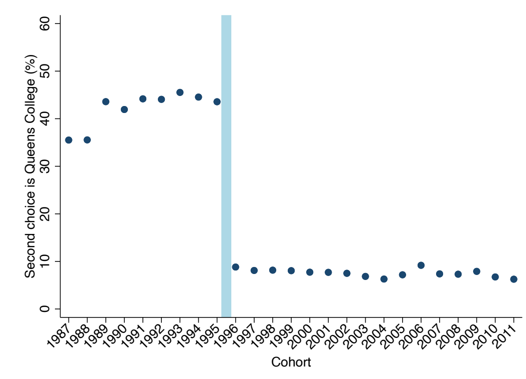
BSSEE cohorts:	1987-1995	1996-2011
	(1)	(2)
Panel A: Administrative Data		
Female	0.50	0.50
	(0.50)	(0.50)
Admitted cohort size	160.57	154.74
	(49.54)	(46.53)
Took CSEC	0.56	0.75
	(0.50)	(0.43)
Number of CSEC subjects taken	3.23	4.76
	(3.38)	(3.49)
Number of CSEC subjects passed	2.44	3.35
	(2.99)	(3.17)
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.

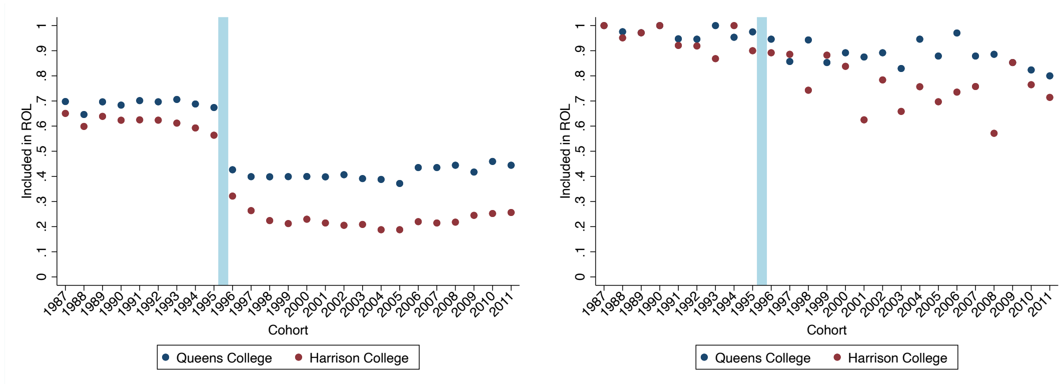
4.1 Applications

Before the policy change, Harrison College was the most popular first-choice school; 47 percent of parents selected it as their first choice. Queens College was the leading second-choice option, chosen by 42 percent of parents. Consequently, these schools were also the most selective, with the highest admission cut-off scores in the market.

As illustrated in Figure 2, panel (a), the share of parents listing Harrison College as their first choice declined by approximately 35 percentage points following the policy change. Panel (b) shows a similarly large decline for Queens College, whose share of second-choice listings also fell by about 35 percentage points. Appendix C presents formal estimates of these changes. Across specifications, the declines are statistically significant, and the most flexible specification implies reductions of nearly 40 percentage points for both outcomes.

These findings demonstrate substantial changes in application behavior. However, changes in first and second choices do not, by themselves, imply that parents stopped including the most selective schools elsewhere in their ROLs. Figure 3, panel (a), shows that the reform reduced not only the likelihood that Harrison College and Queens College were ranked first or second, but also the probability that they were included anywhere in the submitted ROL. The declines are substantial: the share of ROLs containing Harrison College fell by roughly

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.

30 percentage points, and the share containing Queens College declined by a similar amount. Panel (b) repeats the analysis for students in the top 1 percent of the BSSEE score distribution each year. Among these students, the vast majority continue to include Harrison College and Queens College in their ROLs, and there is no discernible break at the time of the reform. This pattern suggests that the reduction in applications to selective schools was concentrated among students who were unlikely to gain admission, consistent with parents 'skipping the impossible'. Appendix D reports the corresponding regression estimates and standard errors.

Although Figure 3 reveals large discontinuous changes in applications around the time of the reform, other contemporaneous changes may also have affected parents' school choices. To isolate the impact of the policy, we implement a difference-in-differences design that compares top scorers to all other applicants. We define the control group as students in the top 1 percent of the BSSEE score distribution each year and the treatment group as all remaining applicants. The intuition is that parents of top scorers face little risk that their children will be denied admission to the most selective schools and therefore have less incentive to omit such schools from their ROLs. In contrast, the reform should have a larger effect on the application behavior of lower-scoring students, for whom admission to selective schools is less likely. Appendix E reports the corresponding estimates and standard errors. Even under this conservative specification, we find statistically significant reductions of 20 and 19 percentage points, respectively, in the probability that Harrison College and Queens College are included in submitted ROLs. These results suggest that the decline in applications to selective schools was not driven solely by broader time trends and is consistent with parents omitting schools they perceived as unattainable.

Taken together, these findings suggest that parents responded to the policy change by omitting schools they perceived as unattainable. Yet such changes in application behavior need not translate into changes in realized assignments. If the omitted schools were truly out of reach, excluding them from a submitted ROL would have no effect on the school ultimately assigned. We therefore 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.[5] Figure 4, panel (a), shows

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

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.

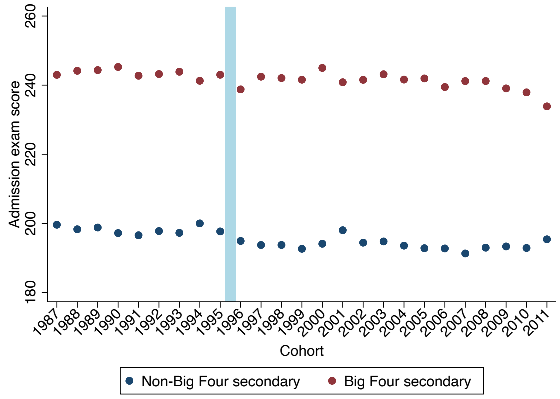
Next, we examine whether the policy affected the socioeconomic composition of students in the Big Four schools. Such effects could arise if particular subgroups—such as the parents of low-SES students—disproportionately reduced or ceased applying to these schools relative to the parents of high-SES students. 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 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.

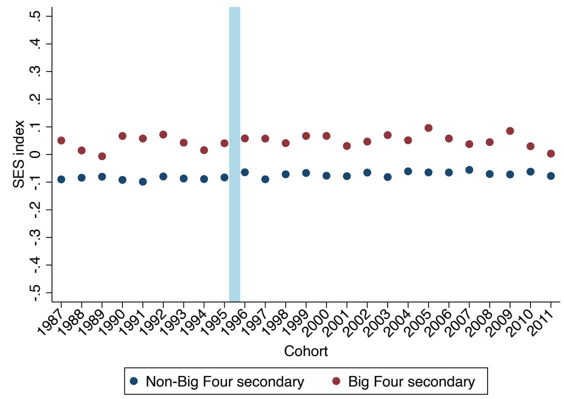
Admission to primary schools is decentralized and determined by catchment areas. Figure 5, panel (a), shows that the SES composition of students at Big Ten primary schools is substantially higher than that of students at other primary schools, reflecting their location in wealthier neighborhoods. Panel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

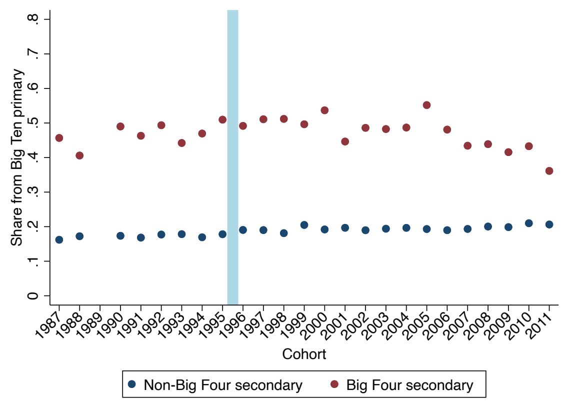
Figure 4: Composition changes



(a) Admission exam score



(b) SES index



(c) Share from Big Ten primary

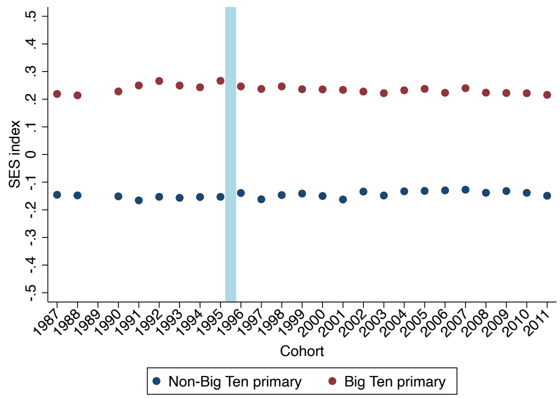
NOTE: Panel (a) shows the average admission exam score of students assigned to Big Four and non-Big Four schools. Panel (b) shows the average SES index of students assigned to Big Four and non-Big Four schools. The SES index measures average socio-economic status at the primary school level. Panel (c) 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.

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.

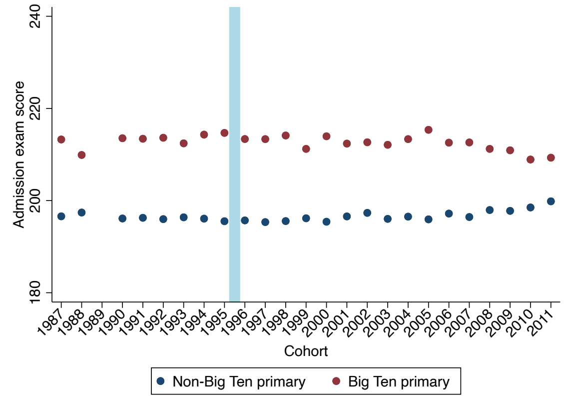
In Appendix F, we present results from a set of empirical specifications that quantify changes in assignment outcomes. The estimated coefficients and standard errors closely correspond with the graphical evidence presented in this section.

In Appendix G, we examine whether the policy change affected the distance to assigned

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.

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 develop a model of parental school preferences and describe the methods used to estimate preference parameters under alternative behavioral assumptions. We then define school effectiveness and peer quality within a potential outcomes framework and estimate these measures under alternative identifying assumptions.

For estimation, we primarily rely on the pre-policy period (1987–1995), when parents could rank up to fifteen schools. We do so for four reasons. First, the pre-reform period features a binding application-length constraint but no geographical restrictions, making it a cleaner institutional environment for estimating parental preferences. Second, admission cut-offs are remarkably stable during the pre-reform period, facilitating the measurement of school selectivity. Third, among students who score high enough to qualify for at least one selective school, only 5 percent do not apply to one, providing additional support for the market stability assumption. Finally, our wage outcomes come from the 2016 Barbados Survey of Living Conditions. Restricting attention to pre-reform cohorts also ensures that most individuals have completed their education and entered the labor market by the time of the survey. Nonetheless, we use post-policy data (1997–2011) to assess the robustness of our estimates to the institutional changes introduced 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 valuation of school j, X_j is a vector of school characteristics, 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. Later in the paper, we decompose the estimated school effects δ_j into components reflecting school effectiveness and peer quality.

We first estimate the preference parameters (δ_j, λ) under the truth-telling assumption that submitted application lists reveal parents' most preferred schools (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. This is a rank-ordered conditional logit model (Hausman and Ruud, 1987). Under the truth-telling assumption, schools omitted from an application list are interpreted as being less preferred than every school that is listed. Consequently, the estimator does not distinguish between schools that are genuinely less preferred and those omitted because they are perceived as unattainable. Importantly, it does not account for the feasible choice sets induced by admission probabilities or application constraints. We denote the resulting estimates by $(\hat{\delta}_j^{TT}, \hat{\lambda}^{TT})$.

In a constrained environment, parents may not be incentivized to reveal their most preferred schools in their applications. 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.

In a constrained environment, parents may not be incentivized to reveal their most preferred schools in their applications. 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-length constraint is binding. In this setting, some parents may omit schools they perceive as unattainable from their submitted ROLs. If these schools are interpreted as being less preferred rather than omitted because they are out of reach, preference estimates may understate parental valuation of school characteristics that are concentrated among the omitted schools, such as school effectiveness.

To address this concern, we also estimate the preference parameters using the method of Fack et al. (2019), which assumes market stability and relies on school assignment data rather than submitted application lists. Under this approach, each parent has an ex-post feasible choice set determined by their child's admission score and the realized admission cut-offs. Specifically, the feasible choice set consists of all schools whose admission cut-off is below the student's score. Market stability implies that, within this feasible set, parents obtain their most preferred school. Formally, the choice problem for parent i is:

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

where Ω_i denotes the ex-post feasible choice set. Let κ_j denote the admission cut-off for school j. Then,

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

that is, the set of schools for which student i's exam score s_i exceeds the admission cut-off. Notice that these feasible choice sets are directly observed from students' admission scores and school admission cut-offs and therefore do not need to be estimated. Intuitively, parents of higher-scoring students face larger feasible choice sets because their children qualify for admission to a greater number of schools.

Unlike the truth-telling estimator, this approach does not require omitted schools to be less preferred than ranked schools. Instead, it relies only on the implication that the assigned school is the parent's most preferred school among those that were ex-post feasible. Under the market stability assumption and the Type I extreme value distribution for ϵ_{ij}, the probability that student i is assigned to school j is

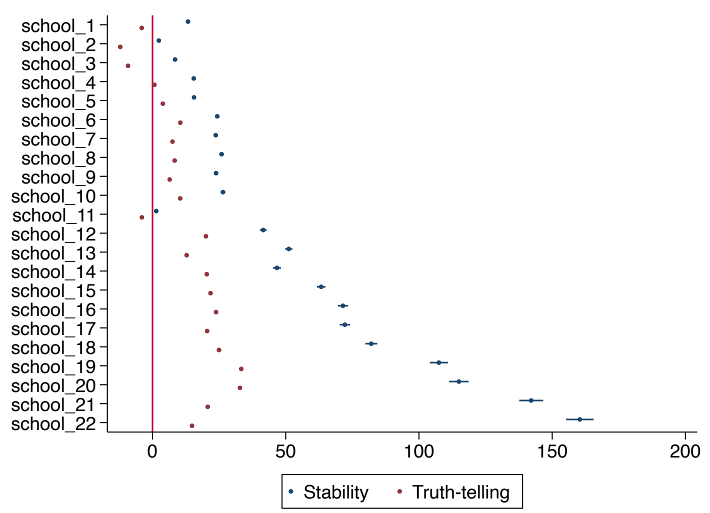
$$Pr(S_i = j) = \frac{\exp(\delta_j + \lambda d_{ij})}{\sum_{k \in \Omega_i} \exp(\delta_k + \lambda d_{ik})}.$$

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

Figure 6 presents the estimated average parental valuations for schools, expressed in willingness-to-travel units, $\hat{\delta}_j/\hat{\lambda}$. For ease of exposition, schools are ordered by their average selectivity, measured as the average admission cut-off over the sample period, so that higher indices correspond to more selective schools. Blue dots show estimates based on the market-stability approach, while red dots correspond to estimates under the truth-telling assumption.

Two features stand out from Figure 6. First, the estimated parental valuations differ substantially depending on the underlying behavioral assumptions. Second, the divergence is concentrated among the most selective schools. The market-stability estimates assign

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.

substantially higher valuations to these schools than the truth-telling estimates. Under the truth-telling assumption, the most selective schools receive relatively modest estimated valuations, whereas the stability-based estimates indicate that they are among the most highly valued schools in the market. As we show below, this difference is crucial because the most selective schools are also among the most effective, implying that the truth-telling approach understates parental valuation of school effectiveness.

Appendix H reports the complete set of parameter estimates. The estimated distance parameter is large and statistically significant under both the truth-telling and market-stability approaches. We also report a Hausman test (Hausman, 1978) comparing the preference estimates obtained under the two behavioral assumptions. Under the null hypothesis that parents are truth-tellers, the truth-telling estimator is consistent and efficient, whereas the stability estimator is consistent but inefficient. Under the alternative that not all parents are truth-tellers, only the stability estimator remains consistent. The Hausman test strongly rejects the truth-telling model ($p < 0.001$). This finding is consistent with the evidence from the policy reform, which suggests that some parents omit schools they perceive as unattainable from their submitted ROLs.

In Appendix I, we report preference estimates, expressed in willingness-to-travel units, under both the truth-telling and market-stability approaches using post-policy data (1997–2011). Once parents adjust their submitted ROLs to the tighter application constraint, the truth-telling estimates assign even lower valuations to the most selective schools. In fact, the most selective school in the market is estimated to have a negative valuation relative to the outside option. In contrast, the market-stability estimates continue to assign the highest valuations to the most selective schools.

5.2 School effectiveness

To measure school effectiveness, we adopt a potential outcomes framework. Let

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

where Y_{ij} denotes an outcome of interest (e.g., test scores or adult wages) that student i would obtain if assigned to school j. The vector X_i includes observable characteristics such as admission exam score, year of birth, and gender, while η_{ij} captures unobserved determinants of outcomes. The parameter α_j captures the causal contribution of school j to student outcomes and therefore measures school effectiveness.

Following Abdulkadiroğlu et al. (2020), we define the quality of student i and the peer quality of school j as

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

Because students are not randomly assigned to schools, $E[\eta_{ij} \mid S_i = j]$ need not equal zero. Consequently, consistently estimating the school effects α_j requires additional identifying assumptions.

As a baseline, we assume selection on observables and 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 an indicator equal to one if student i attends school j. Because X_i includes the admission exam score, this specification corresponds to a value-added model. Using the estimated parameters, we compute measures of student quality and peer quality that are used in the preference decomposition below:

$$\hat{A}_i = \frac{1}{J}\sum_{j=1}^{J}\left(\hat{\alpha}_j + X_i'\hat{\beta}\right), \qquad \hat{Q}_j = \frac{\sum_i \mathbb{1}\{S_i = j\}\hat{A}_i}{\sum_i \mathbb{1}\{S_i = j\}},$$

where $\hat{A}_i$ denotes the average predicted outcome student i would obtain across all schools, and $\hat{Q}_j$ denotes the estimated average quality of students attending school j. By averaging predicted outcomes across all schools, $\hat{A}_i$ isolates permanent student quality from school-specific effects, allowing peer quality to be measured independently of school effectiveness.

We estimate school effectiveness and peer quality for two outcomes. The first is qualification for tertiary education based on performance on the secondary school exit examination, and the second is adult wages. Accordingly, we denote the estimated school effects by $(\hat{\alpha}_j^{test}, \hat{\alpha}_j^{wage})$ and the corresponding peer-quality measures by $(\hat{Q}_j^{test}, \hat{Q}_j^{wage})$. 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 findings are particularly noteworthy. First, school effectiveness is strongly positively correlated with school selectivity for both outcomes ($corr \geq 0.74$). Second, schools that are more effective at improving test-score outcomes also tend to be more effective at improving adult 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. 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 improvements in both short- and long-term outcomes. As expected,

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

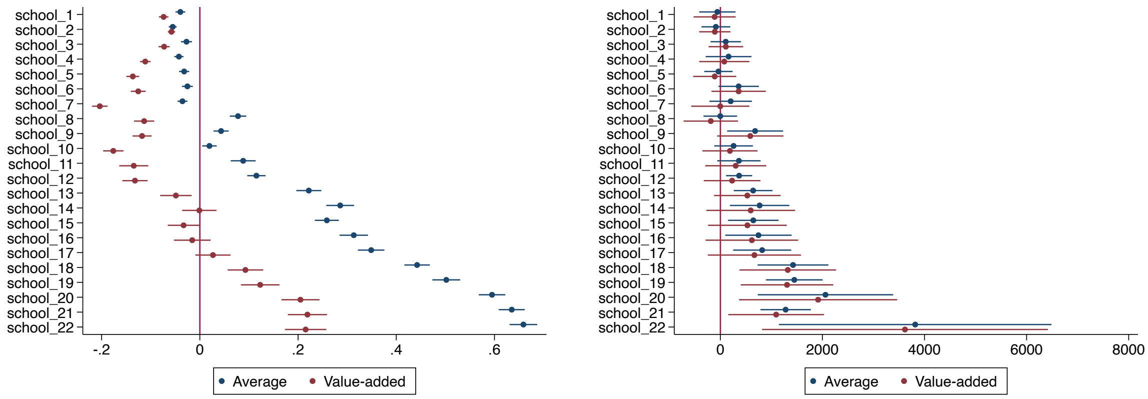
NOTE: This table reports correlations across effectiveness estimates $(\hat{\alpha}_j^{test}, \hat{\alpha}_j^{wage})$, peer quality estimates $(\hat{Q}_j^{test}, \hat{Q}_j^{wage})$, and school selectivity κ_j. We measure school selectivity using equilibrium admission cut-offs.

the wage estimates are substantially less precise than the test score estimates, reflecting that wages are observed only for a sample of applicants.

Figure 7 presents the estimated school effects, $(\hat{\alpha}_j^{test}, \hat{\alpha}_j^{wage})$, together with their 95% confidence intervals. For comparison, the figure also reports the corresponding observed school-level outcome averages. Two findings stand out. First, schools exhibit substantial heterogeneity in both average outcomes and effectiveness. Second, while school averages provide biased measures of effectiveness, the most selective schools are also the most effective in both short- and long-run outcomes. As expected, the wage estimates are considerably less precise than the test-score estimates, reflecting that wage outcomes are observed only for the survey sample.

To relax the selection-on-observables assumption, we exploit the estimated choice model to construct a control function based on the market-stability model. Intuitively, conditional on observed characteristics, the estimated choice probabilities summarize how students sort

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.

into schools and therefore provide a control for unobserved determinants of student outcomes. Because these probabilities depend on the student's ex-post feasible choice set, the resulting control function also depends on Ω_i. Under this approach, we assume that

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

where the distance vector $D_i = (d_{i1}, \dots, d_{iJ})'$ serves as the exclusion restriction. The parameter φ captures the extent of selection on unobserved school-specific components of potential outcomes.

In Appendix J, we provide additional details on the derivation of the control function and compare the resulting estimates with our baseline value-added estimates for both short- and long-run outcomes. The estimated school effects are remarkably similar across the two approaches, and the ranking of schools is largely unchanged. This suggests that selection on unobserved components of potential outcomes is unlikely to be an important source of bias in our setting. We therefore use the value-added estimates as our measures of school effectiveness throughout the remainder of the paper.

To further validate our value-added estimates, we exploit the admission discontinuities generated by the centralized assignment mechanism. Notice that a student's assignment depends on only two inputs: her submitted ROL and her admission exam score. Conditional on a given ROL, assignment changes discontinuously when a student's score crosses the equilibrium admission cut-off of one of the schools on that list. We exploit these discontinuities to assess whether our estimated school effects are forecast-unbiased measures of the causal effects of attending different schools.

We apply the method of Abdulkadiroğlu et al. (2022) to test whether our estimated school effects correctly predict the causal effects identified by admission discontinuities. For each student and school, we construct a local propensity score, denoted p_{ij}, which takes values of 0, 0.5, or 1. Students with $p_{ij} = 0.5$ are locally randomized into admission to school j, where the randomization arises within a narrow bandwidth around the relevant admission cut-off. We then use these propensity scores as saturated controls and implement the forecast-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 $\hat{Y}_i$ denotes the outcome predicted by the value-added model and S_{ij} is an indicator equal to one if student i attends school j. If the value-added estimates correctly predict the causal effects of school attendance, then the forecast-bias parameter satisfies $\phi = 1$. We implement this validation exercise for the test-score outcome, for which administrative data are available for the full applicant population. We do not perform the test for adult wages because wages are observed only for a survey sample, leaving too few locally randomized students around the admission cut-offs.

In Appendix K, we report the results of the validation exercise. We begin with an uncontrolled model, for which we estimate $\hat{\phi} = 0.80$ and reject the null hypothesis that $\phi = 1$. This indicates that the uncontrolled model is forecast biased and does not accurately predict the causal effects identified by the admission discontinuities. We also reject the associated overidentification test (Sargan, 1958), implying that the model's predictive validity differs across discontinuities. These findings demonstrate that the validation exercise has sufficient power to detect bias in a naïve specification.

We then turn to our value-added model. We estimate $\hat{\phi} = 0.96$ and fail to reject the null hypothesis that $\phi = 1$, indicating that the value-added estimates are forecast unbiased and accurately predict the discontinuity effects on average. While the overidentification test rejects the null of equal predictive validity across discontinuities ($p = 0.03$), this test is asymptotic and is known to over-reject in moderate samples. For this reason, Hansen (2022) recommends interpreting only very small p-values as evidence against the model.

To improve the precision of the wage effectiveness estimates, we exploit the strong correlation between school effectiveness in test scores, which is estimated using administrative data for the full applicant population, and school effectiveness in adult wages, which is estimated using only the survey sample. Rather than estimating the two outcomes separately, we estimate them jointly as a system of equations, allowing school effects to be correlated across outcomes:

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

In this setup, school effectiveness is a vector $\alpha_j = (\alpha_{j,test}, \alpha_{j,wage})'$. We then apply multivariate empirical Bayes shrinkage to obtain the posterior estimates

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

where V_j is the sampling variance matrix for school j, and $(\mu_\alpha, \Sigma_\alpha)$ denote the mean and covariance matrix of the assumed multivariate normal prior distribution. The resulting posterior estimates borrow strength both across outcomes and across schools, improving the precision of the school-effect estimates, particularly for adult wages (Walters, 2024).

In Appendix L, we compare the value-added estimates with the multivariate empirical Bayes posterior estimates for both test scores and adult wages. As expected, the test score estimates are largely unaffected by shrinkage because they are based on administrative data for the full applicant population and are therefore estimated precisely. In contrast, the wage estimates are more strongly affected by shrinkage because they are estimated from the survey sample and are consequently much noisier. In the next section, we show that our decomposition of parental preferences into peer quality and school effectiveness is robust to whether school effectiveness is measured using the baseline value-added estimates or the empirical Bayes posterior estimates.

6 Preferences for peer quality and school effectiveness

In this section, we combine our preference estimates with our estimates of school effectiveness and peer quality to examine whether parents value school effectiveness once peer quality is taken into account (Abdulkadiroğlu et al., 2020). Specifically, we relate estimated parental valuations of schools to peer quality ($\hat{Q}_j$) and school effectiveness ($\hat{\alpha}_j$). We perform this decomposition using both the truth-telling estimates ($\hat{\delta}_j^{TT}$) and the market-stability estimates ($\hat{\delta}_j^{ST}$). All estimates are scaled by their standard deviation, so that the coefficients can be interpreted as the change in parental valuations (in standard deviations) associated with a one-standard-deviation increase in peer quality or school effectiveness. Our baseline specification is

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

where t indexes admission cohorts. For test-score effectiveness, we estimate cohort-specific specifications because the administrative data cover the full population of applicants. For wage effectiveness, the survey sample is too small to estimate cohort-specific effects, so we pool all cohorts and omit the index t.

Table 3 reports estimates from regressing parental valuations on peer quality and school effectiveness. Columns (1)–(4) use qualification for tertiary education to measure school effectiveness and peer quality. Under the truth-telling assumption (column 1), school effectiveness has no statistically significant association with parental valuations once peer quality is taken into account. In contrast, the market-stability estimates (column 2) indicate that parents value both peer quality and school effectiveness: a one-standard-deviation increase in school effectiveness raises average parental valuations by 0.56 standard deviations. Because the sample contains only the 22 schools in the market, columns (3) and (4) report pooled cohort-specific specifications, which substantially increase the number of observations while leaving the main conclusions unchanged. Under market stability, parents continue to place significant weight on both peer quality and school effectiveness, with a one-standard-deviation increase in effectiveness raising average parental valuations by 0.35 standard deviations.

Columns (5) and (6) repeat the analysis using adult wages to measure school effectiveness and peer quality. The results closely mirror those based on test scores. Under the truth-telling assumption, school effectiveness is not significantly associated with parental valuations once peer quality is controlled for. Under market stability, however, parents again place significant weight on both peer quality and school effectiveness. A one-standard-deviation increase in wage effectiveness raises average parental valuations by 0.56 standard deviations.

These results are consistent with our earlier finding that parents omit highly selective schools from their submitted ROLs. Because the most selective schools are also the most effective, interpreting omitted schools as less preferred mechanically attenuates the estimated valuation of school effectiveness.

As a robustness exercise, we augment the market-stability framework with the undominated strategies restrictions proposed by Fack et al. (2019). Whereas the baseline stability estimator identifies preferences using assignment outcomes alone, this alternative specification additionally exploits the ordering of schools in submitted ROLs. Specifically, it imposes that the ranking of schools in a submitted application is consistent with the underlying utility ordering implied by the model. The resulting moment inequalities complement the stability moment equalities and therefore provide additional identifying information.

The results, reported in Appendix M, continue to indicate that parents value both peer quality and school effectiveness. Relative to the baseline stability estimates, incorporating the ROL-based restrictions increases the estimated importance of peer quality while attenuating the estimated valuation of school effectiveness. For example, in the pooled specification based on tertiary qualification rates, the coefficient on peer quality increases from 0.474 to 0.781, whereas the coefficient on school effectiveness declines from 0.555 to 0.193. Neverthe-

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.

less, the estimated effect of school effectiveness remains positive and, in the cohort-specific specification, statistically significant. Similarly, when effectiveness is measured using adult wages, we continue to find that parents value more effective schools: a one-standard-deviation increase in wage effectiveness raises average parental valuations by 0.321 standard deviations and is statistically significant at the 5 percent level.

Overall, these findings indicate that our main conclusion—that parents value school effectiveness once preferences are recovered from assignment outcomes rather than inferred directly from submitted ROLs—is not driven by the exclusion of information contained in submitted applications.

In Appendix N, we repeat the decomposition using the multivariate empirical Bayes posterior estimates of school effectiveness. The results are virtually unchanged. Under the truth-telling assumption, school effectiveness remains unrelated to parental valuations once peer quality is taken into account. In contrast, the market-stability estimates continue to indicate that parents value both peer quality and school effectiveness in terms of both test scores and adult wages.

In Appendix O, we repeat the decomposition using the control-function estimates of school effectiveness. Because the control function is derived from the market-stability model, we restrict attention to the stability-based preference estimates. The results closely mirror those obtained using the baseline value-added estimates, indicating that parents continue to value both peer quality and school effectiveness.

6.1 Discussion

Consider a school choice market in which parents can rank an unlimited number of schools and priorities are determined by a common lottery. For simplicity, suppose assignments are made through random serial dictatorship. In such an idealized setting, a finding that parents' ROLs do not reflect parental demand for school effectiveness would imply that they do not value school effectiveness. In practice, however, many centralized school markets ration seats using skill measures and impose constraints on application length. These institutional features complicate the interpretation of submitted ROLs as measures of parental preferences, particularly when omitted schools are interpreted as being less preferred than those that are ranked.

Our findings also inform the interpretation of results from other school choice markets. Evidence that parents' observed choices do not place weight on school effectiveness after controlling for peer quality (Abdulkadiroğlu et al., 2020; Ainsworth et al., 2023) can be interpreted as evidence that parents do not value school effectiveness only if schools omitted from submitted ROLs are genuinely less preferred than those that are ranked. This assumption may be reasonable in settings where application constraints are absent or non-binding. Our evidence suggests that, when application constraints bind, observed choices may understate parental demand for school effectiveness. In our setting, parents value school effectiveness measured by both test scores and adult wages, yet this demand is only partially reflected in their submitted ROLs because desirable but seemingly unattainable schools are often omitted.

Furthermore, evidence that high-SES parents appear more likely than low-SES parents to choose schools based on effectiveness (Ainsworth et al., 2023; Beuermann et al., 2023) is consistent with the mechanism documented in our setting. If selective schools are more likely to lie within the feasible choice sets of high-SES families than those of low-SES families, submitted ROLs may reveal stronger demand for school effectiveness among the former, even if underlying preferences are similar across groups. Our findings may also help explain the modest effects on school assignments observed when parents are provided information on school effectiveness (Ainsworth et al., 2023). If the most effective schools remain inaccessible to many parents, additional information may alter submitted ROLs without substantially changing assignment outcomes.

7 Conclusions

This paper examines whether parents value school effectiveness, after accounting 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 reduced the maximum application length by two-fifths. Following the reform, many parents responded by 'skipping the impossible', omitting the most selective schools from their submitted ROLs, yet equilibrium assignments were largely unchanged. These findings show that binding application constraints can weaken the link between submitted ROLs and underlying preferences while leaving assignment outcomes largely unaffected. Together, they suggest that, in constrained environments, submitted ROLs may not reliably reveal parents' most preferred schools, whereas the stability of the matching outcome remains a plausible basis for recovering parental preferences.

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 behavioral assumptions. We find that binding application constraints weaken the link between submitted ROLs and underlying preferences, causing truth-telling estimates to understate parental valuation of school effectiveness. In contrast, stability-based estimates, which are robust to the omission of desirable but seemingly unattainable schools, reveal that parents place significant weight on both school effectiveness and peer quality, with remarkably similar results for short- and long-run outcomes.

Our findings carry important implications for both policy and research. From a policy perspective, the evidence suggests that changes in application constraints may have limited effects on assignment outcomes, as many parents appear to skip schools they perceive as unattainable. Moreover, the fact that parents value school effectiveness indicates that they possess meaningful information about this school characteristic. From a research perspective, failing to account for binding application constraints can lead researchers to underestimate parental demand for school effectiveness. More generally, our results suggest that market design features can substantially affect the extent to which submitted ROLs reveal underlying preferences. Understanding these interactions is therefore important for accurately measuring parental demand and designing policies that expand access to high-quality schools.

Finally, although our empirical setting is specific to Barbados, the mechanisms we identify—strategic omission under binding application constraints and the limited ability of families to access the schools they value most—are common to many centralized school markets. Future work could examine how these constraints shape the relationship between parents' underlying preferences and what their submitted ROLs reveal in other institutional settings. More broadly, our results suggest that evaluating school choice reforms requires accounting not only for observed parental choices but also for the institutional features that shape the relationship between underlying preferences and submitted applications.

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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)	

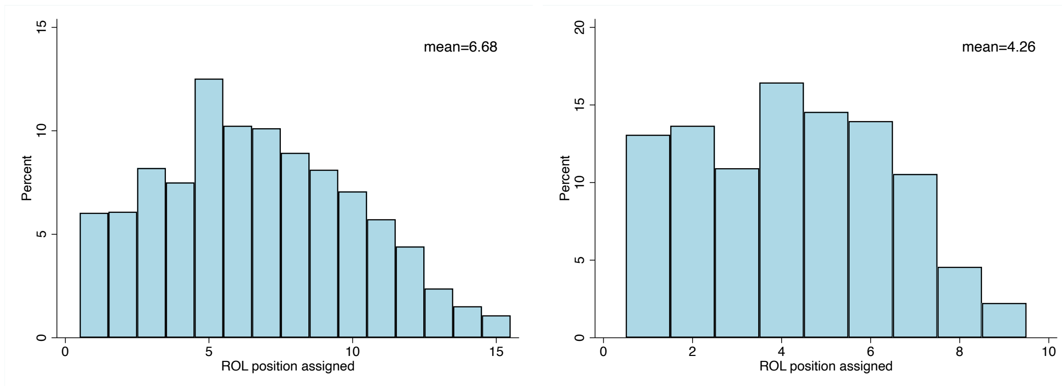
Table A.4: cont'd. A.3 Survey Representativeness: BSSEE Cohorts 1996 - 2011

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.

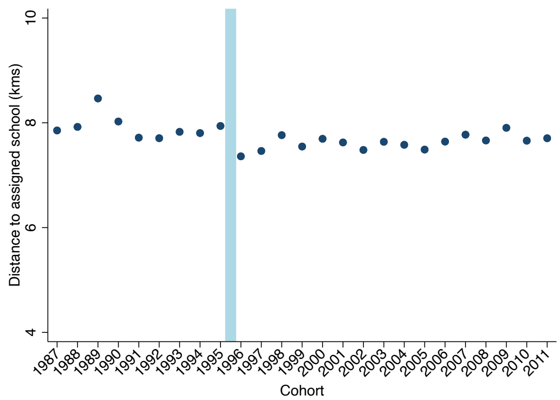
Table F.6: Policy change effect on the share of Big Ten students at non-Big Four schools

	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

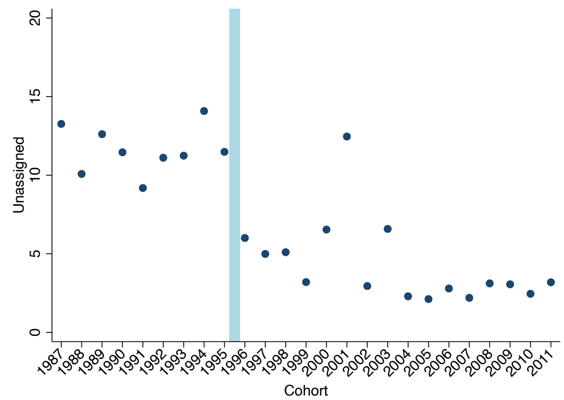
NOTE: This table shows regression estimates of the policy change effect. Post is a dummy variable that indicates the post-policy period. Time is the admission cohort centered at the year of the policy change (1996). Each column shows a different specification. Standard errors in parenthesis.

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

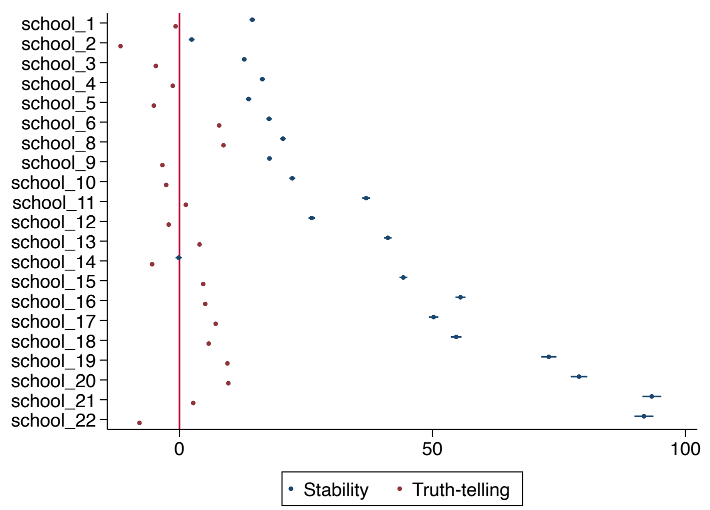
Table H.1: Preferences estimates

	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:

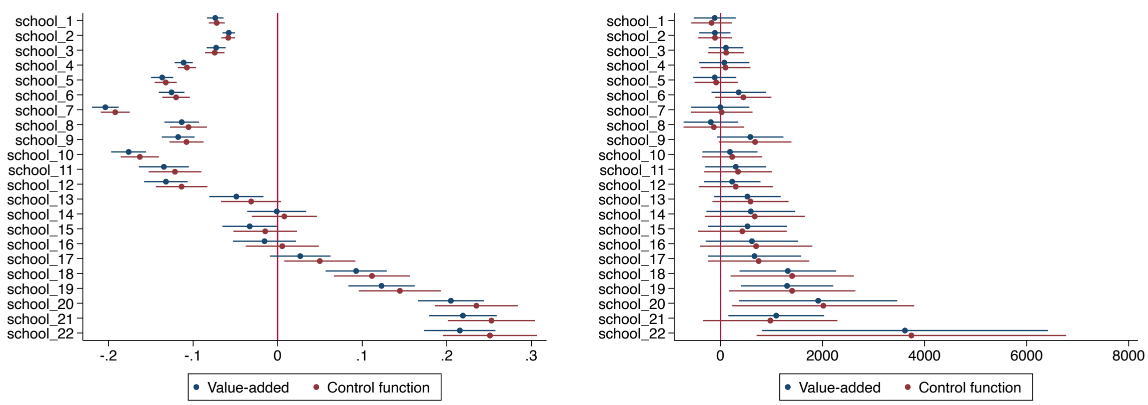
$$E[Y_i \mid X_i, D_i, S_i = j] = \alpha_j + X_i'\beta + \sum_{k=1}^{J} \psi_k \lambda_k(X_i, D_i, \Omega_i) + \varphi \lambda_j(X_i, D_i, \Omega_i).$$

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

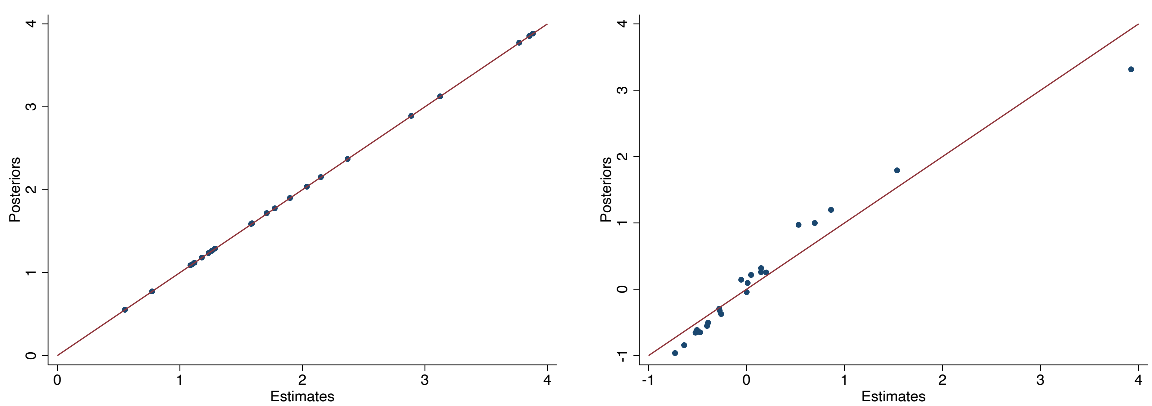
Table K.1: RDD-based tests for bias in estimates of school effectiveness

	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 Stability and undominated strategies

Table M.1: Stability and undominated strategies: 1987-1995

	Qualified for tertiary		Monthly wages
	Pooled	Yearly	Pooled
$\hat{Q}_j$	0.781***	0.904***	0.666***
	(0.118)	(0.041)	(0.146)
$\hat{\alpha}_j$	0.193	0.156***	0.321**
	(0.131)	(0.049)	(0.132)
Observations	22	194	22

NOTE: This table reports estimates of the relationship between peer quality, school effectiveness, and average preferences for schools. We estimate preferences using moment equalities from stability and moment inequalities implied by the observed ROLs. Qualified for tertiary and Monthly wages, indicate the outcome we use when estimating school effectiveness ($\hat{\alpha}_j$) and peer quality ($\hat{Q}_j$). Pooled indicates that we pool data across cohorts. Yearly indicates that we to the estimation and decomposition cohort by cohort. Standard errors are reported in parentheses.

N Decomposition using MEB posteriors

Table N.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.

O Decomposition using control function estimates

Table O.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.