Who Gets What May Not Matter: Understanding School Match Effects*

Atila Abdulkadiroğlu[†] Parag A. Pathak[‡] Christopher R. Walters[§]

March 2025

Preliminary and Incomplete

Abstract

We examine two approaches to improving urban school systems: changing who gets to go to existing schools (market clearing) and restructuring school portfolios through closures and reconstitution (resource augmentation). Using data from New York City high schools, we estimate models of schools effects allowing for both vertical school quality differences and horizontal student-specific match effects. While sophisticated market clearing policies that optimize student-school matches can generate modest educational gains, they are constrained by limited seats at highly effective schools. Simple resource-augmentation policies targeting low-performing schools achieve comparable improvements with less systemic disruption. Analysis of NYC's school closures reveals that basic graduation rate metrics effectively identify struggling schools, suggesting complex value-added models may be unnecessary for closure decisions. Our findings indicate that capacity constraints, rather than poor school matching, primarily drive educational inequality.

*We thank seminar participants at Boston College, Brown, Carnegie Mellon, Claremont McKenna, Columbia, Harvard, the Harvard Kennedy School, MIT, Northwestern, Simon Fraser University, UC Berkeley, UCLA, Washington University in St. Louis, and Yale for input. Nati Mulat, Eli Mogel, and Kate Bradley provided expert research assistance. We also thank Eryn Heying, Talia Gerstle, and Niamh McLoughlin for invaluable administrative support. The research described here was carried out under data-use agreements between MIT and the New York City Department of Education.

[†]Duke University Department of Economics and NBER. Email: atila.abdulkadiroglu@duke.edu

[‡]MIT Department of Economics and NBER. Email: ppathak@mit.edu

[§]UC Berkeley Department of Economics and NBER. Email: crwalters@econ.berkeley.edu

1 Introduction

Urban school districts have used two main policies as part of reform efforts: changing who goes where and re-investing in schools by reconstituting them or closing them. We'll call these **market clearing** and **resource augmentation policies**, respectively.[1] Market clearing policies are about matchmaking: they reshuffle the deck of students across existing schools. Examples include changes to school assignment processes, informational interventions, and recent moves by districts to drop test scores for selective school admissions following Covid-19 disruptions in test schedules. Resource augmentation policies, in contrast, involve changing the school portfolio. With recent declines in public school enrollment, many districts are grappling with "right-sizing" reforms, deciding which buildings to close and which ones stay open.[2] Following Roth (2016), market clearing affects "who gets what" while resource augmentation determines "the what."

This paper compares the performance of best-case market-clearing policies to equally-effective resource augmentation policies. We use data from New York City's high schools, where both policy levers have been deployed. To augment resources, the district created hundreds of new small high schools of choice, while simultaneously closing down large high schools and middle schools in the early 2000s (Bloom and Unterman, 2014; Kemple, 2015; Bifulco and Schwegman, 2019). The district has also seen expansion of the charter sector, which is currently attended by 15% of students, a seven-fold increase from 2005 (NYC Charter Center, 2024). Market-clearing reforms include changes to New York's school assignment algorithms beginning in 2003-04 (Abdulkadiroğlu, Pathak and Roth, 2009; Abdulkadiroğlu, Agarwal and Pathak, 2017). Recently, New York descreened admissions criteria across numerous middle and high schools, and has toyed with proposals to adjust selective exam high school admissions (see, e.g., Shapiro (2019)). In the 2023-24 school year, more than 40 high schools used modified admissions criteria which prioritize low-income, English Language Learners, or other disadvantaged populations as part of the district's Diversity in Admissions program.[3] New York has also increased information distributed to applicants about school options (see e.g., Corcoran et al. (2018) and Corradini (2024)).

Key to our exercise is characterizing how a student's achievement depends on three factors: their own abilities, their school's quality, and how well these fit together. We define the latter as school match effects. Without match effects, redistributing students among existing schools is zero-sum: some students gain what other lose. With match effects, market-clearing policies can result in placing students in schools where they stand to benefit the most. We therefore

[1] The terminology "resource augmentation", though potentially counterintuitive in the context of school closures, derives from computer science literature on algorithmic efficiency. This framework evaluates performance improvements by comparing theoretically optimal solutions when handicapped with fewer resources (Roughgarden, 2020). Examples in economics include Bulow and Klemperer (1996), Akbarpour, Malladi and Saberi (2018), and Mohammad Akbarpour and Saberi (2023).

[2] Dee (2023) analyzes enrollment declines following COVID-19. Several districts, including Boston, Chicago, Rochester NY, are considering closing schools due to underenrollment.

[3] Idoux (2024) studies Diversity in Admissions policies in Brooklyn and Manhattan. See https://www.schools.nyc.gov/enrollment/enrollment-help/meeting-student-needs/diversity-in-admissions for additional details.

begin our analysis by estimating models of school effects that allow for match-specific effects on both observable and unobservable dimensions. The "vertical" component, which is a school's average treatment effect, represents universal quality differences that benefit all students equally. The "horizontal" component captures match effects, where certain students flourish at particular schools. We find that the variation in the vertical dimension is roughly triple the variation in the horizontal dimension.

These estimates allow us to simulate a set of market-clearing policies, which focus on how families choose schools, how schools select students, and the interaction of these forces. On the demand side, we examine what would happen if parents could be persuaded to choose schools based purely on educational effectiveness. This represents a departure from current preferences, which are shaped by factors like proximity or peer composition. The corresponding supply-side intervention has schools prioritize students based on who would benefit most from their offerings. We also compute the impact of a first-best scenario achievement-maximizing allocation that would occur if an all-knowing central planner could assign students to schools to maximize educational outcomes, constrained only by school capacities.

Simulations of these policies show that improved market-clearing can generate modest improvements, but gains from market-clearing are limited by availability of seats at high-quality schools. We find that realistic resource-augmentation policies that close the lowest quality schools and add capacity to accommodate displaced students achieve similar gains to the most sophisticated market-clearing policies. This stems from the fact that differences between schools are predominantly vertical rather than horizontal, so modest changes to the vertical dimension can match extreme horizontal reforms. Moreover, students are not sorted based on match effects in current assignment patterns, so leveraging match effects would require wholesale reorganization of school assignments. The disruption created by augmenting the school portfolio, in contrast, is more concentrated among the specific schools involved.

We conclude by investigating aspects of resource-augmentation policies based on school closures in New York. Between 2007 and 2017, New York closed 44 high schools from our analysis sample. The schools chosen for closure overlap considerably with those that would have been selected by estimates of school effectiveness. New York's closure decisions correlate strongly with high school graduation rates. We find a basic rule-of-thumb focusing solely on graduation rates would have identified many of the same schools as our more complex measures of school effects. This suggests that sophisticated value-added models might not be necessary for identifying the worst-performing schools for closure decisions. Finally, we show that closure designs are weakly correlated with school popularity, suggesting central planners with access to with graduation data may be better equipped than parents to identify failing schools.

The rest of the paper is organized as follows. Section 2 provides additional details on market-clearing and resource augmentation policies across districts. Section 3 introduces our conceptual framework for measuring school effects and computing counterfactual outcomes. Section 4 summarizes estimates of value-added and match effects. Section 5 compares market clearing and resource

agumentation policies. Section 6 analyzes some aspects of New York's closures. Section 7 concludes and offers some directions for future research.

2 Examples of Market Clearing and Resource Augmentation

2.1 Market Clearing

Table 1 describes three categories of market clearing policies: admissions criteria, matching processes, and decision aids. Several districts have shifted away from test-based admissions, with elite exam schools leading the charge. Chicago replaced racial quotas with placed-based affirmative action (Barrow, Sartain and de la Torre, 2020; Dur, Pathak and Sönmez, 2020; Ellison and Pathak, 2021; Angrist, Pathak and Zarate, 2023). Boston's Latin School followed these footsteps (Boston School Committee, 2021). Thomas Jefferson High School for Science and Technology in northern Virginia now picks top performers from each middle school, while San Francisco's Lowell High School briefly went all-in on lotteries before pivoting back to test-based admissions (Saul and Liptak, 2023; Heller, 2022; Mojadad, 2022). Changes are not limited to these elite schools. Boston scrapped zones systemwide for personalized school menus (Shi, 2015; Dur et al., 2018). Philadelphia traded principal discretion for a clear-cut system of grades, attendance, and zone-specific lotteries (Aubri, 2023).

Matching processes have also evolved towards coordination and centralization. Table 1 demonstrates that Denver, DC, Newark, and Camden replaced decentralized school-by-school application processes with unified single-application enrollment systems (Pathak, 2017). Atlanta, Buffalo, and others streamlined charter applications into a single form (Avery, Kocks and Pathak, 2024). Other cities have tweaked the rules of their computerized matching algorithms (Abdulkadiroğlu and Sönmez, 2003; Pathak and Sönmez, 2008; Abdulkadiroğlu, Pathak and Roth, 2009; Abdulkadiroğlu et al., 2020*b*; de Haan et al., 2023).[4]

Alongside these changes in assignment rules, districts increasingly provide enhanced information or decision-making aids to applicants. Examples of such efforts include online school directories, quality reports, school finder tools, and admissions fairs. Applicants in New Haven, Connecticut, receive personalized feedback on admissions chances through a "smart-matching platform" (Arteaga et al., 2022). A growing body of work evaluates households' responses to school quality information (Hastings and Weinstein, 2008; Houston and Henig, 2021; Ainsworth et al., 2023; Campos, 2024).

2.2 Resource Augmentation

In the 2000s, large-scale efforts to restart or close schools were driven by federal legislation. The 2001 No Child Left Behind Act required chronically underperforming schools to be shuttered. By 2009, the Obama administration's School Improvement Grant (SIG) program offered grants of up to two million dollars for turnaround efforts. These could mean closure, restarts, or phase-out.

[4]Studies of the potential effects of these changes include Agarwal and Somaini (2018) and Calsamiglia, Fu and Güell (2020).

More recently, closures have been driven by enrollment declines. Figure 1 shows that America's 100 largest school districts have lost 800,000 pupils since 2010, with enrollment dropping from 12 million to 11.2 million. This decline predates COVID-19, in part due to falling birth rates and high urban housing costs, but has accelerated since the pandemic. Boston's public schools, which taught 85,000 pupils in 1980, now serve just 47,000, prompting efforts to right-size the district through closures and consolidations. This effort may cut the number of school buildings in half (Ciurczak, 2023; Boston Globe, 2024). Chicago closed 50 schools in 2013 and has lost 80,000 students in the last decade. Recent political backlash has led to a moratorium on further closures until 2027 (Vevea, 2024; Mohamad, 2024).

Table 2 lists select closures in large urban school districts since 2000. Districts implementing closures typically weigh multiple factors, including enrollment, academic results, building conditions, and how closures affect children's commutes. In some cases, sophisticated metrics have factored into decisions about which schools to close. Pittsburgh created a performance index in partnership with RAND (Gill, Engberg and Booker, 2005; Kowal and Hassel, 2008). Ohio used value-added scores to determine which charter schools to close (Carlson and Lavertu, 2018). Seattle, Milwaukee, and Chicago all incorporated test score gains into their deliberations (Guin and Roza, 2007; Kowal and Hassel, 2008). Engberg et al. (2012) describe a district that targeted low-performing schools in its closure plan and sought to move their students to higher-performing schools. Barnum (2019) summarizes studies of the effects of closures.

The growth of new school models illustrates examples of resource augmentation. Seven percent of public school students attend charter schools in 2021, an increase from four percent in 2010 (NCES, 2023). A large literature studies the effects of charter schools (see, e.g., Abdulkadiroğlu et al. (2011); Dobbie and Fryer (2014); Angrist et al. (2016)). Abdulkadiroğlu et al. (2016) study new school restarts, through charter school takeovers or in-district alternatives. Schueler, Goodman and Deming (2017) find district-wide turnaround effort in Massachusetts had large math and modest reading effects. These supply-side changes haven't occurred without obstacles. Roughly half of US States have some form of a charter school cap, which restricts entry of new public school providers (IES, 2018). In Massachusetts, six new charter schools entered after a 2010 law raised the cap to 18 percent of district spending for Boston and other low-performing districts (Commonwealth of Massachusetts, 2010). In 2016, voters in Massachusetts rejected a referendum proposing to increase this cap (Scharfenberg, 2016; Walters, 2018).

3 Conceptual Framework

We analyze the effects of market clearing and resource augmentation policies with a potential outcomes model building on the framework of Abdulkadiroğlu et al. (2020*a*). Consider a population of N students, each enrolled at one of J schools. Let Y_{ij} denote student i's potential outcome at school j. Write the linear projection of the potential outcome for school j on a constant and a

vector of student characteristics X_i:

$$Y_{ij} = \alpha_j + X_i'\beta_j + \epsilon_{ij}. \tag{1}$$

Here $E[\epsilon_{ij}] = E[X_i\epsilon_{ij}] = 0$ by definition of α_j and β_j. We normalize the mean of the covariates to $E[X_i] = 0$, so that α_j is the population mean potential outcome at j: $\alpha_j = E[Y_{ij}]$. Student i's observed school attendance is $D_i \in \{1, ..., J\}$, and her observed outcome is $Y_i = \sum_j D_{ij}Y_{ij}$, where the indicator variable $D_{ij} = 1\{D_i = j\}$ equals one if student i attends school j.

Equation (1) facilitates a decomposition of potential outcomes into additively-separable student, school, and match components. We can rewrite i's potential outcome at school j as

$$Y_{ij} = \underbrace{\mu_\alpha + X_i'\mu_\beta + \bar{\epsilon}_i}_{A_i} + \underbrace{(\alpha_j - \mu_\alpha)}_{ATE_j} + \underbrace{X_i'(\beta_j - \mu_\beta) + (\epsilon_{ij} - \bar{\epsilon}_i)}_{M_{ij}}, \tag{2}$$

where $(\mu_\alpha, \mu_\beta, \bar{\epsilon}_i)$ are averages of $(\alpha_j, \beta_j, \epsilon_{ij})$ across j. The term A_i is the average of student i's potential outcomes across the J schools in the district, which we term her *ability*. The term ATE_j is the average treatment effect of school j relative to the district average school. In other words, ATE_j measures the effect of randomly selecting students from the district and sending them to school j rather than another randomly-selected district school. The term M_{ij} is student i's match effect at school j, which can arise either because of an interaction between student i's characteristics and the school-specific excess slope $(\beta_j - \mu_\beta)$ or due to an especially high (or low) unobserved component ϵ_{ij} at this school. Equation (2) demonstrates that Y_{ij} equals the sum of student i's general ability A_i, school j's overall quality ATE_j, and the match component M_{ij}.

Decomposition (2) implies that the overall average outcome in the district is given by

$$\begin{aligned}
\bar{Y} &= \frac{1}{N}\sum_i Y_i \\
&= \frac{1}{N}\sum_i \sum_j D_{ij}\left[A_i + ATE_j + M_{ij}\right] \\
&= \frac{1}{N}\sum_i A_i + \sum_j s_j ATE_j + \frac{1}{N}\sum_i \sum_j D_{ij}M_{ij},
\end{aligned} \tag{3}$$

where $s_j = N^{-1}\sum_j D_{ij}$ is the share of students attending school j and the third line uses the fact that $\sum_j D_{ij} = 1$ for each student. The first term in this equation is average ability A_i, which is a fixed characteristic of the district population invariant to school enrollment. The second term, $\sum_j s_j ATE_j$, is the enrollment-share-weighted average quality of the district's schools. The last term captures the influence of match effects on the average outcome.

This framework clarifies the channels through which market-clearing and resource-augmentation policies affect aggregate achievement. Market clearing policies seek to improve outcomes by resorting students across schools to leverage match effects. Student i's *potential match effect* at school j is M_{ij}. Student i's *realized match effect* is $\sum_j D_{ij}M_{ij}$. Market-clearing policies target realized

match effects, as reflected in the third term of equation (3). Can we change the configuration of D_{ij}'s to increase average match quality, holding fixed enrollment shares s_j? In contrast, resource-augmentation policies seek to increase educational outcomes by changing enrollment shares or school-specific parameters, captured primarily by the second term. For example, a school closure policy can be viewed as setting the enrollment share s_j to zero for a target school and increasing shares at other schools to absorb the displaced students. Equivalently, we can model such a policy as deleting ATE_j and school j's match effects from the set of available parameters in the district, thereby increasing the weight on parameters for other schools. We assess the prospects for achievement gains from these two types of policies for New York students.

3.1 Estimating School Effects

Our empirical analysis pursues two strategies to estimate the parameters of the school potential outcome equations defined in the previous section. The first is an OLS *value-added model* (VAM) approach based on the following selection-on-observables assumption:

$$E[Y_{ij}|D_i, X_i] = \alpha_j + X_{i1}'\beta_j + X_{i2}'\gamma, \tag{4}$$

where X_{i1} and X_{i2} partition the vector of observed student covariates X_i so that $X_i = (X_{i1}', X_{i2}')'$. Assumption (4) requires the controls in X_i to eliminate all selection bias in comparisons across schools, so that potential outcomes are mean-independent of school attendance D_i conditional on these characteristics. The model of potential outcomes in (4) also imposes a constant coefficient γ on the sub-vector X_{i2}, allowing for match effects only with respect to X_{i1}. In our main specification X_{i1} includes an above-median baseline math indicator, a Black/Hispanic indicator, and a female indicator, while X_{i2} includes indicators for home borough, subsidized lunch, cubic functions of baseline math and reading scores, and the log of the median income in a student's census tract. Partitioning the covariates in this way accommodates match effects on three important dimensions while allowing for additive controls for a larger set of student characteristics. We later explore robustness with respect to this choice.

Under assumption (4), we can write the observed outcome Y_i as

$$Y_i = \sum_j D_{ij}[\alpha_j + X_{i1}'\beta_j] + X_{i2}'\gamma + e_i, \tag{5}$$

where $E[e_i|D_{i1}, ..., D_{iJ}, X_{i1}, X_{i2}] = 0$. We estimate equation (5) by OLS, which delivers unbiased estimates of causal value-added parameters if selection-on-observables holds. A recent literature evaluates selection-on-observables assumptions like (4) and generally finds that OLS VAM estimates are a reliable guide to causal effects of teachers and schools, though modestly biased (Rothstein (2010, 2017), Chetty, Friedman and Rockoff (2014), Deming (2014), Angrist et al. (2017, 2024)). Lottery-based tests indicate little selection bias in VAMs for New York's high schools (Abdulkadiroğlu et al., 2020*a*).

Our second strategy leverages information from students' rank-ordered preference lists to relax selection-on-observables. Specifically, we estimate two-step *rank-ordered control function* models that use students' preference lists to control for unobserved student tastes for each school. Suppose students rank schools based on latent utilities from a multinomial logit model of the form:

$$U_{ij} = \delta_j(X_i) - \tau(X_i)d_{ij} + \eta_{ij}. \tag{6}$$

Here X_i is a set of observed characteristics, d_{ij} is distance from student i's home to school j, and η_{ij} is a (de-meaned) *iid* extreme value type I error independent of covariates and distance. Student i's rank-ordered list is $R_i = (R_{i1}, ..., R_{i\ell(i)})'$, with $R_{i1} = \arg\max_j U_{ij}$ and $R_{ik} = \arg\max_{j \notin \{R_{i1},...,R_{i(k-1)}\}} U_{ij}$ for $k > 1$. The first step of our control function strategy estimates the parameters of model (6) by fitting rank-ordered logits in 360 covariate cells defined by values of X_i.

In the second step, we parameterize the relationship between unobserved tastes and potential outcomes as follows:

$$E[Y_{ij}|D_i, X_i, d_i, \eta_{i1}, ..., \eta_{iJ}] = \alpha_j + X_{i1}'\beta_{j1} + X_{i2}'\beta_2 + d_i'\kappa + \sum_{k=1}^{J} \psi_k \eta_{ik} + \varphi\eta_{ij}. \tag{7}$$

Equation (7) allows linear dependence of mean potential outcomes on the unobserved tastes underlying students' preference lists. Parameter ψ_j is the coefficient on the taste for school j common to every potential outcome equation, which allows levels of potential outcomes to differ for students that like or dislike specific schools (selection on levels). Parameter φ is an extra effect of taste for j at this specific school, which allows selection on gains. We include distances to each school as controls to avoid using distance as an excluded instrument. The key assumption underlying the control function strategy is that the preferences underlying students' rank-ordered lists capture all selection into school attendance (conditional on the observed controls in X_i).

Equation (7) implies observed outcomes can be written:

$$Y_i = \sum_j \{D_{ij}[\alpha_j + X_{1i}'\beta_j] + \lambda_j(X_i, d_i, R_i)[\psi_j + D_{ij}\varphi]\} + X_{2i}'\gamma + d_i'\kappa + u_i, \tag{8}$$

with $E[u_i|D_i, X_i, d_i, R_i] = 0$. Control function $\lambda_j(X_i, d_i, R_i) = E[\eta_{ij}|X_i, d_i, R_i]$ is the mean logit taste for school j conditional on covariates, distances, and the rank-ordered preference list R_i. This formulation is a rank-ordered generalization of the multinomial logit-based control functions proposed by Dubin and McFadden (1984). Variation in the control functions conditional on (X_i, d_i, D_i) comes from the fact that not all students attend their most-preferred schools due to differences in priorities, random tie-breaking, and non-compliance with school assignments. As a result, rankings R_i will differ for observably-similar students attending the same school, revealing the relationship between unobserved tastes and outcomes. We estimate equation (8) by OLS after plugging in estimated control functions based on the first-step rank-ordered logit, adjusting inference with a two-step score bootstrap procedure.

Match effects are central to our counterfactual exercises. As noted above, our value-added specification includes match effects by baseline achievement, gender, and race. Previous evidence suggests important match effects with respect to these characteristics (Deming et al., 2014; Egalite, Kisida and Winters, 2015). The control function approach adds an unobserved match component linking outcomes to preferences (captured by parameter φ). While this specification allows matching on several important dimensions, it is possible that our model misses match effects on other dimensions. As a robustness check, we estimate alternative models based on a principal components analysis (PCA), collapsing the full covariate X_i to a low dimensional list of factors and allowing match effects with respect to these factors. We use the scree eigenvalue test to select number of factors to retain, and substitute these factors in place of X_{i1} in equations (5) and (8). This approach selects three factors that put substantial weight on the three characteristics from our baseline model, resulting in similar empirical estimates. Results for the PCA analysis are reported in Appendix C.

OLS value-added estimation generates a vector of estimates for each school,$\hat{\theta}_j = (\hat{\alpha}_j, \hat{\beta}_j')$. The control function approach adds an estimate $\hat{\psi}_j$ to this vector. These estimates are unbiased but noisy estimates of underlying school quality parameters θ_j. The noise in school-specific estimates is captured by a sampling covariance matrix:

$$E[(\hat{\theta}_j - \theta_j)(\hat{\theta}_j - \theta_j)'|\theta_j] = \Omega_j.$$

We deal with this noise in a hierarchical empirical Bayes (EB) procedure. Let hyperparameters $\mu_\theta = (\mu_\alpha, \mu_\beta')'$ and $\Sigma = E[(\theta_j - \mu_\theta)(\theta_j - \mu_\theta)']$ describe the mean and variance of parameters in the population of schools, treating each θ_j as a random draw from a school-level mixing distribution. We estimate thesehyperparameters as follows:

$$\hat{\mu}_\theta = \frac{1}{J}\sum_j \hat{\theta}_j, \quad \hat{\Sigma} = \frac{1}{J}\sum_j \left[(\hat{\theta}_j - \hat{\mu_\theta})(\hat{\theta}_j - \hat{\mu}_\theta)' - \hat{\Omega}_j\right]. \tag{9}$$

Subtracting the mean $\hat{\Omega}_j$ removes excess variance in the noisy $\hat{\theta}_j$s due to sampling error, resulting in a consistent estimate of the cross-school variability of latent θ_j's.

We form empirical Bayes (EB) posterior predictions for each school's parameters with multivariate linear shrinkage:

$$\theta_j^* = (\hat{\Omega}_j^{-1} + \hat{\Sigma}^{-1})^{-1}(\hat{\Omega}_j^{-1}\hat{\theta}_j + \hat{\Sigma}^{-1}\hat{\mu}_\theta). \tag{10}$$

Here, $\theta_j^* = (\alpha_j^*, \beta_j^{*'})'$ is an EB posterior mean for θ_j when the school quality parameters follow a multivariate normal distribution, and a best linear predictor of θ_j otherwise. These posterior mean forecasts are used to form posterior predictions of average treatment effects and match effects, ATE_j^* and M_{ij}^*. Our policy counterfactuals impute treatment effects using these EB posteriors.The counterfactual simulation results therefore capture a scenaio in which parents or policymakers know *our best estimates* of VAM parameters and act on this information, rather than the true value-added parameters θ_j.

3.2 Computing Counterfactuals

Our goal is to compare the effects of various policies that change the allocation of students across schools. Let $\mathcal{P}$ be the set of policies. For any $p \in \mathcal{P}$, we'd like to compute average realized achievement in the population of N students:

$$\bar{Y}(p) = \frac{1}{N} \sum_i \sum_j D_{ij}(p) Y_{ij} = \frac{1}{N} \sum_i \sum_j D_{ij}(p) \left[A_i + ATE_j + M_{ij} \right],$$

where $D_{ij}(p)$ is 1 if i attends school j under policy p. We will focus on $\bar{Y}(p) - \bar{Y}(p_0)$ where p_0 is the policy status quo. In this case, mean student ability A_i differences out:

$$\bar{Y}(p) - \bar{Y}(p_0) = \frac{1}{N} \sum_i \sum_j \left[D_{ij}(p)(ATE_j + M_{ij}) - D_{ij}(p_0)(ATE_j + M_{ij}) \right].$$

We construct counterfactual estimates by substituting EB posterior mean predictions of average treatment effects and match effects into this expression, and computing school assignments $D_{ij}(p)$ with a simulation of the school assignment process.

Every year in New York, about 80,000 eighth graders enter one of about 400 high schools using a centralized matching process based on the student-proposing deferred algorithm (Gale and Shapley, 1962; Abdulkadiroğlu, Pathak and Roth, 2009). Applicants report their preferences for schooling options to the assignment mechanism by submitting rank-ordered lists of up to 12 academic programs. The DA algorithm combines student preferences with program priorities to generate a single program assignment for each student. In the algorithm, students propose to their top-choice schools, schools tentatively accept their favorite students up to capacity, and rejected students apply to their next choices. The algorithm continues until every student either obtains an assignment or runs out of choices. During our study time period, students left unassigned in the main round participate in a supplementary DA round in which they rank up to 12 additional programs with available seats. Any remaining students are administratively assigned by the district. About 82 percent, 8 percent, and 10 percent of applicants are assigned in the main, supplementary, and administrative rounds, respectively (Abdulkadiroğlu, Agarwal and Pathak, 2017).

Programs order students using a combination of priorities and ranks. Our data include priorities/ranks of students at all school-programs to which they applied. To compute counterfactual policies, we must determine priorities, ranks, and eligibility for students at all schools for our counterfactuals. Priorities are determined by mechanical rules. Ranks are determined by a combination of test scores, attendance and other factors such as interviews and auditions.

We construct priorities using data on rules for each program, and predict program ranks using ordered and rank-ordered logits. DA with simulated priorities and ranks produces results close to actual assignments. We, therefore, treat our simulation as the status quo to eliminate differences due to imputations of priorities and ranks. Some counterfactuals also require extending student rankings to the full list of schools, which we do by generating preferences from the multinomial logit model described in Section 3. Appendix A has additional details on the simulation procedure

and underlying assumptions.

3.3 Threats to Validity of Counterfactuals

Our framework treats value-added parameters (α_j, β_j) as fixed school characteristics, invariant to student composition. Potential threats to this assumption include peer effects (Sacerdote, 2011), changes to curricula and other responses to student composition, new patterns of social interactions, and effects of competition (Duflo, Dupas and Kremer, 2011; Carrell, Sacerdote and West, 2013; Campos and Kearns, 2024). While we assume away all such effects for simplicity, ourcounterfactuals results are robust to certain forms of such forces. Under a standard linear-in-means peer effect specification in which peer effects equal a constant coefficient times mean peer ability, peer effects are zero-sum and wash out in aggregate, so have no impact on overall mean achievement. More complicated non-linear forms of peer effects would not wash out and could affect the validity of our counterfactuals. It's worth noting that such non-linearities would also render the optimal assignment problem computationally intractable, further complicating efforts to improve achievement through market-clearing reforms.

4 Estimates of School Effects

4.1 Data and Samples

Our data are extracted from the DOE administrative information system and cover students enrolled in New York City from 2003-04 through 2012-13 school years. Our data allow us to characterize the following aspects of a student's educational journey: where they enrolled, their demographics, New York Regents test scores, graduation status, and college attendance. We also have application records and the ingredients to recreate New York's implementation of deferred acceptance.

Our analysis focuses on four cohorts of students enrolled in New York City public schools in eight grade between 2003-04 and 2006-07. Table 3 reports characteristics of our two main samples. The VAM sample is used for estimating our measures of school effectiveness. The sample includes students with available baseline (8th grade) data, observed high school outcomes, and enrolled for ninth grade at one of 291 schools with at least 50 students with outcomes. The analysis sample restricts attention to the 2006 cohort and is used for simulating our market-clearing counterfactuals. We exclude a small group of selective New York City's exam schools that admit students in a parallel system outside the main round of the assignment process (Abdulkadiroğlu, Angrist and Pathak, 2014).

Table 3 shows that New York's student population reflects its urban character. Most students come from the Bronx, Brooklyn, or Queens, with three-quarters being Black or Hispanic. Nearly two third qualify for free or reduced price lunch. We focus on three educational outcomes: Regents math achievement, high school graduation within five years of completing 8th grade, and any college (two or four year) enrollment within two years of expected graduation date.

The city's schools sort students through four distinct methods. Some programs require auditions or portfolios, showcasing artistic or specialized talents. Screened programs rate applicants based on grades, test scores, and attendance. Unscreened programs simply use random selection, while Educational Option programs split their seats between screened and unscreened admission. Many programs also include borough or district priority.

4.2 The Distribution of School Quality

OLS VAM and control function estimates of the distribution of school value-added parameters appear in Table 4. The estimated standard deviation of Regents math average treatment effects across New York high schools equals 0.26σ in the control function model and 0.28 in the value-added specification. This is slightly higher than variation in annual school value-added found in other contexts, which may be due to the fact that students attend high school for multiple years before taking Regents tests.[5] The magnitude of these estimates aligns with previous research on Regents math among New York's high schools (Abdulkadiroğlu et al., 2020*a*).

Estimates of match effect parameters reveal that school effectiveness also varies with student characteristics. In the control function model, the estimated cross-school standard deviation in the slope coefficient on an above-median baseline Math indicator equals 0.13σ. This implies that a school one-standard-deviation above average with respect to this slope increases scores for high-baseline students by 0.13σ relative to lower-baseline peers, holding average treatment effects constant. Similar variation in treatment effects is evident by race (slope standard deviation of 0.14σ), while effect heterogeneity is more modest by gender (slope standard deviation of 0.05σ). While these match effect parameters are statistically significant, a comparison to column (1) demonstrates that variation in average treatment effects (the vertical component) is much larger than variation in match effects (the horizontal component).

The bottom rows of each panel in Table 4 report estimated correlations between dimensions of school effectiveness across schools. Schools that are highly effective overall (high ATE_j) are especially effective for low-achievers, a fact revealed by the negative correlation between the school-specific intercept and above-median baseline slope coefficient. The intercept and female slope coefficient are positively correlated, indicating that effective schools are also especially effective. The correlation between ATE_j and the school-specific preference parameter ψ_j is positive, which implies that more effective schools tend to attract positively-selected students who would do well anywhere. Results from control function and value-added specifications are similar overall, suggesting selection bias due to unobserved tastes are modest for Regents math scores.

Corresponding school quality distribution estimates for high school graduation and college attendance appear in Tables B1 and B2). As with test scores, we find substantial variation in the vertical component of school quality (as reflected by the standard deviation in the intercept),

[5] Angrist et al. (2017) find the standard deviation of value-added across Boston's middle schools is $0.15 - 0.2\sigma$. Angrist et al. (2024) find the standard deviation of value-added on city-wide standardized tests is 0.2σ across NY middle schools and is 0.15σ across NY high schools on the SAT math.

smaller but still significant standard deviations of match parameters, and positive correlations between the intercept and baseline math slope. This implies a consistent pattern of heterogeneity in which effective schools are more effective for low achievers for all outcomes. Unlike the Regents math outcome, correlations between the intercept and female slope are negative for high school and college attendance. Coupled with the positive female main effect for the longer-term outcomes (and negative female main effect for Regents scores), this again implies that more effective schools differentially help those who start at a disadvantage.

We next present an allocation-level view of school effectiveness by mapping the distribution of value-added to the actual allocation and to potential assignments. In Table 5, we compare Regents math treatment effect parameters for the realized student-school assignment and all possible student-school assignments. The former corresponds to about 60,000 students while the latter represents more than 18 million potential student-school combinations. The table summarizes distributions of raw unbiased estimates $\hat{\theta}_j$ as well as the shrunk EB estimates θ_j^*. These calculations demonstrate that dispersion of the vertical component of school effects is substantially larger than the horizontal component. This can be seen in column (1) of Table 5 where the standard deviation of the ATE is 0.25σ and the standard deviation of the match effect is 0.13σ. This ratio is 0.27σ to 0.13σ when considering all possible student-school matches in column (3). The range of the ATE is also larger than the range of match effects.

Shrinkage reduces the dispersion of the estimates, more so for match effects than average treatment effects. This stems from greater statistical imprecision in the heterogeneous component of treatment effects than in overall school effects, resulting in more noise and therefore more shrinkage. Table 5 shows the standard deviation of the shrunk ATE for math is 0.23σ, compared to the raw estimate of 0.24σ. The shrunk estimate of the standard deviation of the match effect is 0.13σ, compared to 0.08σ for the raw estimate. Shrinkage also narrows the range of estimates of match effects more than for the average treatment effect. A comparison of panels A and B shows little difference between OLS VAM and control function estimates.

To illustrate sources of potential misallocation relative to a goal of maximizing educational outcomes, we next ask two questions: (1) How does observed demand for schools compare to demand based on effectiveness? (2) How do admissions policies that select students based on the potential outcomes compare to the status quo? Both exercises tell us what the average student stands to gain, but do not take market clearing into account.

Figure 2 compares Regents math effects for observed preference rankings compared to potential rankings in which parents order schools according to their effectiveness. The average treatment effect of first-choice schools would improve from 0.09σ to about 0.70σ, indicating that many parents leave substantial school value-added on the table when choosing schools.[6] The average first-choice match effect is slightly negative for observed preference rankings. This reflects the fact that better schools are especially effective for low-achievers, but tend to attract slightly more high-achievers. A ranking based on school effectiveness would result in an average first-choice match effect of 0.008σ,

[6] Abdulkadiroğlu et al. (2020*a*) and Ainsworth et al. (2023) explore these trade-offs further.

which is positive but very small in magnitude. Since average treatment effects are much more variable than match effects, rankings based on effectiveness are dominated by differences in ATE_j across schools, leading to weak positive sorting on match quality (recall that overall effectiveness equals ATE_j plus M_{ij}).

Figure 3 reports estimates of optimal school admissions which select students based on potential outcomes. For each school, we rank applicants in order of predicted potential outcomes, admit students in order of this prediction, then compute the school-average outcome. The table then reports the average of this prediction across schools. Since average treatment effects are mean-zero across schools by definition, the result can be interpreted as the average match effect for the highest potential match effect students at each school. This exercise produces estimates of 0.11σ for Regents math scores and roughly 5 percentage points for high school graduation and college attendance.

The analyses shown in Figure 2 and Figure 3 suggest there may be potential gains from aligning student preferences and school priorities with educational effectiveness. However, these exercises consider potential gains for individual students or schools in isolation, and therefore fail to account for capacity constraints and congestion effects that emerge in market equilibrium. When many students rank the same effective school highly, or when multiple schools target the same high-potential students, capacity constraints bind. A student who could benefit from several schools can ultimately attend only one, and schools can only admit up to their capacity regardless of how many high potential applicants they attract. Understanding these market-clearing dynamics requires examining how demand and supply interventions work together, which is the focus of the next analysis.

5 Market Clearing vs. Resource Augmentation

5.1 Market-Clearing Policies

We consider on different idealized market-clearing policies that focus on how families choose schools (demand-side), how schools select students (supply-side), and the combination, taking capacity constraints into account. Each counterfactual only changes the ordinal information submitted to the mechanism, either student rankings or school rankings. We also explore a benchmark using cardinal information about the magnitude of school effects. Throughout, school capacities are unchanged.

The four scenarios we consider are:

1. **Aligned Demand**: students rank schools in order of effectiveness, with school-side priorities maintained as in the status quo. The market clears with student-proposing DA.

2. **Aligned Supply**: Schools rank students in order of effectiveness,with student preferences maintained as in the status quo. The market clears with student-proposing DA.[7]

[7] Ties are broken in school rankings using the random number observed in the assignment files.

3. **Aligned Demand and Supply**: Both student preferences and school priorities are based on educational effectiveness.

4. **Treatment Effects Maximization (TEMA)**: Maximize overall average achievement in the district subject to school capacities.

Scenarios (1)-(3) use ordinal information to align preferences and priorities with educational effectiveness, but continue to clear the market with DA. Scenario (4) represents a first-best benchmark, which can becomputed by solving the Shapley-Shubik assignment problem with the objective of maximizing overall educational outcomes (Shapley and Shubik, 1971).

The counterfactual results reveal that capacity constraints limit the aggregate gains that can be achieved with market-clearing policies. Table 6 shows the impact of market-clearing policies on student achievement, computed based on control function estimates of school effectiveness. Aligned Demand boosts Regents math achievement by 0.04σ across all students in the district. This is notably smaller than the average gap of 0.70σ between students' observed choices and the most effective available school, highlighting how capacity constraints limit demand-side interventions. In particular, since there is substantial vertical quality variation across schools, Aligned Demand leads most students to rank schools in roughly the same order, with little role for match effects. But capacity at the most effective schools is fixed, so changes in aggregate achievement are driven by changes in average match quality. As a result, aggregate achievement increases only modestly relative to the status quo.

Aligning school-side priorities with achievement leads to slightly larger increases in mean achivement. Aligned Supply is predicted to increase mean Regents scores by 0.06σ, about half the average effect from optimal school admissions for each school in isolation. Combining optimized demand and supply increases achievement leads to a small increase in this effect, resulting in an overall predicted gain of 0.07σ. Maximizing aggregate achievement with TEMA boosts this effect further, to 0.09σ.

The effects of market-clearing policies on long-term outcomes parallel the math score patterns. Aligned Demand improves high school graduation and college attendance by 1.5 and 1.9 percentage points. Aligned Supply shows similar effects, at 1.8 percentage points for both outcomes, and aligning both sides of the market leads to gains of 2.1 percentage points for graduation and 2.5 percentage points for college attendance. Treatment effect maximization achieves the largest gain at 3.4 percentage points for both outcomes. Panel B of Table 6 shows that counterfactuals based on OLS VAM rather than control function estimates leads to similar results for all outcomes, with slightly smaller predicted gains in some cases due to the narrower range of match effects illustrated in Table Table 5.

The debate over market-clearing policies often centers on equity and access for specific groups. This is particularly the case in the debate about policies that scale back test-based admissions. Capacity constraints create inherent trade-offs: expanding access for one group necessarily reduces access for others. A prominent example is Thomas Jefferson High School's shift from test-based

admissions to a system favoring low-income and English language learners, which sparked ongoing litigation from Asian-American parents claiming discrimination (Saul, 2023).[8]

Table 7 reveals that treatment effects maximization (TEMA) produces larger achievement gains for students with low baseline math scores compared to those in the top three quartiles. For instance, bottom quartile students see an improvement of 0.17σ and top three quartile students see an improvement of 0.06σ. This is an artifact of the positive correlation between overall school effectiveness and differential advantage in teaching low-performing students. As a result, the optimal TEMA assignment sends low-performing students to the best overall schools. This pattern suggests that targeted market-clearing policies focusing on lower-achieving students could potentially yield greater improvements than approaches across all students.

A modified TEMA approach that prioritizes bottom-quartile math students before allocating remaining seats shows the inherent trade-offs in targeted assignment policies. Specifically, we first maximize treatment effects for the bottom quartile math students and then maximize treatment effects for the top three quartiles using the remaining seats. While producing significant gains for low-achieving students (0.42σ in Regents Math and 14 percentage points in both graduation and college attendance), it *reduces* math performance for higher-achieving students with minimal impact on their long-term outcomes. This zero-sum dynamic reflects fixed capacity constraints and the positive correlation between baseline achievement and school effectiveness. Improving outcomes for disadvantaged students requires redistributing access to effective schools away from high-achieving students.

An alternative policy approach segments the market and maximizes outcomes for bottom quartile students using only their currently assigned seats. Column 7 ofTable 7 shows this constrained version produces minimal improvements in the achievement of low-performing students, and significantly smaller gains than unconstrained TEMA. This reveals that TEMA's effectiveness for disadvantaged students largely comes from redistributing access by moving low-achieving students to higher ATE schools, while shifting high-achieving students to lower ATE ones. When reallocation across these two groups is prohibited, treatment effect maximization loses almost all impact for the low-performing group, demonstrating that meaningful improvements for disadvantaged students require taking away seats from the other students.

5.2 Resource Augmentation

Our approach to simulating closure policies begins by closing the lowest-performing schools based on posterior mean predictions of ATE. Next, we reallocate capacity from closed schools to the remaining open schools, to hold total capacity fixed. Finally, DA is used to clear the market.

We imagine three scenarios which differ depending on where students are re-assigned:

1) **Effective Replacement**: re-allocate seats to the top X schools, where X is the same as the number of schools we closed.

[8]Similarly, New York's Discovery program, which reserves seats for low-income students at specialized schools, faces legal challenges due to its impact on Asian-American enrollment (Roper and Thompson, 2024).

2) **Typical Replacement**: re-allocate seats to all schools.

3) **Neighborhood Replacement**: re-allocate seats to five closest schools.

In each scenario, school capacity expands proportionally, rounded so that remainders go to largest schools first.[9]

To compare market clearing and resource allocation policies, we determine how many school closures would be needed to match the achievement gains from either Aligned Demand or TEMA. We focus on these two market-clearing policies as benchmarks since other market-clearing policies produce intermediate results between these two extremes.

Figure 4 illustrates how many schools must be closed under Effective Replacement to match the achievement gains from various market-clearing policies. The process begins by closing the school with the lowest ATE, followed by the next lowest, while reallocating capacity equal to the number of displaced students and using DA to clear the market.[10] Figure 4 shows to achieve the same 0.04σ improvement in Regents Math as Aligned Demand, 7% of schools must close. Matching Aligned Supply requires 8% of closures, while Aligned Demand and Supply needs 11%. The most aggressive policy, TEMA, requires closing 13% of schools to achieve equivalent gains.

Replicating market-clearing policy effects on long-term outcomes requires fewer school closures than for math achievement. For high school graduation, matching Aligned Demand requires closing just 2% of schools, while matching TEMA needs 7% closures. For college enrollment, both Aligned Demand and Aligned Supply effects can be matched by closing 5% of schools, while matching TEMA's college enrollment gains requires closing 10% of schools.

Figure 5 contrasts Typical Replacement with Effective Replacement to address concerns about supply constraints at high ATE schools. While Effective Replacement optimistically assumes capacity can be added at the most effective schools, Typical Replacement provides a more conservative scenario. This approach requires closing significantly more schools to achieve the same gains. For instance, matching Aligned Demand's effects requires closing 11% of schools under Typical Replacement compared to 7% under Effective Replacement.

Table 8 analyzes the disruptive impacts of different closure policies, revealing varying patterns across outcomes. Under Effective Replacement, matching Aligned Demand's math effects requires more school closures than matching its college effects. However, this relationship reverses under Typical or Neighborhood replacement, where matching college outcome effects requires more closures than matching math effects. Across all scenarios, fewer schools need to be closed to match graduation effects compared to either math or college outcomes. Typical Replacement and Neighborhood Replacement create comparable levels of disruption. For example, to match Aligned Demand's effects on high school graduation, Typical Replacement requires closing 21 schools while Neighborhood Replacement needs 32 schools.

[9] For example, if 100 seats need to be reallocated to two schools with capacities of 90 and 10, then the first school would get 90 and the second school would get 10 seats.

[10] The occasional non-monotonic pattern in the results stems from the nature of reassignment as each closure triggers a new round of DA matching with updated capacities, creating potentially complex ripple effects throughout the system.

When aiming to match TEMA's more ambitious outcomes, Effective Replacement remains relatively efficient, requiring reallocation of less than 20% of school seats across all outcomes (math, graduation, and college). However, Typical and Neighborhood Replacement approaches become dramatically more disruptive, with over 90 schools needing closure just to match the TEMA college effects. This stark difference highlights the feasibility of matching extreme market-clearing policy gains depends heavily on the ability to expand capacity at higher-performing schools.

5.3 Implementation Considerations

The market-clearing policies we've discussed represent idealized scenarios that would face significant implementation challenges in practice. Our analysis in Figure 2 reveals only a weak relationship between applicants' school preferences and school effectiveness, suggesting that implementing Aligned Demand would require substantial changes in how families rank schools. Research on educational decision-making suggests caution about the potential for such large-scale behavioral changes. Studies of information interventions and decision aids consistently show limited impact.[11] For example, Corcoran et al. (2018) conducted a field experiment in New York City, providing high school applicants with customized information about graduation rates. The results showed no significant shift towards schools with higher graduation rates. Similar studies examining value-added metrics have found modest effects at best. Ainsworth et al. (2023) tested an intervention in Romania using informational fliers to explain school value-added measures. While their intervention slightly increased the likelihood of low-achieving students choosing higher-value added academic tracks, their analysis suggests that even eliminating all information barriers would not lead familes to maximize value-added in their choices. These interventions may also have unintended effects on educational equity.[12]

Implementing Aligned Supply would require substantial changes to current school admissions practices. It uses multiple factors in prioritizing students, including race (Black/Hispanic status), baseline math performance, and gender. While New York City high schools have recently moved towards simplified admissions criteria and have increased the lottery component in admissions, these changes were primarily driven by integration rather than achievement considerations (Shapiro, 2019). Furthermore, the legal landscape around race-based admissions remains uncertain. Research shows that using alternative metric as proxies for diversity often introduces substantial inefficiencies and statistical noise in the admissions process (Ellison and Pathak, 2021).

TEMA is a stark departure from current practice, effectively eliminating both parental choice and school autonomy in admissions. While some oversight over school admissions practice is common, completely removing families' ability to express preferences would likely face strong resistance, particularly in communities accustomed to school choice. Historical parallels provide important con-

[11] Information interventions have shown more promise outside of the United States in mixed public-private delivery systems, as demonstrated by studies in Pakistan (Andrabi and Khwaja, 2017) and Chile (Allende, Gallego and Neilson, 2019).

[12] Corradini (2024) found that New York's school letter grade system elicited differential reactions from applicants from different racial groups.

text: studies of mandatory court-ordered busing reveal that forced assignments led to significant backlash and increased exodus from school districts (Baum-Snow and Lutz, 2011; Boustan, 2012). Moreover, assignment mechanisms like TEMA that rely on cardinal information in settings without monetary transfers are rare in practice. Districts would likely struggle to explain the complex calculations behind student assignments in a way that families could understand and trust. This communication challenge presents another significant barrier to implementation.

Beyond the implementation hurdles, the market-clearing policies would produce student assignments that differ substantially from the current system. Table 9 illustrates these differences across three key dimensions: school segregation patterns, the relationship between family preferences and actual assignments, and student travel distances. The market-clearing policies all lead to increased racial segregation in schools. In the present system (Status Quo), 46% of New York City high school students attend schools where Black and Hispanic students make up at least 90% of the student body. Aligned Demand would increase this proportion to 47%, while Aligned Supply further increase this proportion to 54%. The trend becomes even more pronounced with policies that integrate both demand and supply considerations: under the TEMA system, the percentage of students in predominantly (90%+) Black and Hispanic schools would rise dramatically to 77%.

Most market-clearing policies would require significant changes to how students choose schools, with Aligned Supply being the sole exception as it maintains current student preferences. As Figure 2 demonstrates, getting students to rank schools according to their effectiveness would require substantial shifts in application behavior. The magnitude of these changes is striking: Under Aligned Demand, students would typically be assigned to their 350th choice school-program (out of a total of 845 school-programs). Since New York's Deferred Acceptance system only allows students to rank up to 12 school-programs, we used rank-order logit models to extrapolate preferences for unranked school-programs. Looking at actual rankings, only 20% of students would receive assignments to schools they originally included on their preference lists under Aligned Demand, and just 12% would attend the same school they would under the current system. TEMA produces similarly dramatic changes, though slightly less extreme: students would on average receive their 115th choice school-program, with even fewer students assigned to their status quo schools.

The market-clearing policies also would significantly increase student travel times and distances compared to the current system. Under the Status Quo, only 12% of students attend schools outside their home borough. Aligned Demand would more than triple this proportion, with 41% of students crossing borough boundaries for school, and would increase average travel distance by 2.8 miles to an average of 5.91 miles. TEMA would create even more dramatic changes to student travel patterns. Under this policy, three out of four students (75%) would attend schools outside their home borough, the average travel distance would increase to 9.48 miles, and more than 40% of students would face commutes exceeding 10 miles.

Resource augmentation policies have more modest effects on assignment patterns than market-clearing approaches, primarily because they affect fewer schools. Both Effective Replacement and Typical Replacement maintain current levels of racial segregation, students are more likely to be

assigned to schools they prefer, and average travel distances are similar compared to the Status Quo.

6 Closing Schools

6.1 New York's School Closures

In the early 2000s, New York City's Department of Education (DOE) implemented a two-pronged strategy: closing underperforming high schools while simultaneously expanding better options through new small high schools. According to Kemple (2015), this period saw the closure of 29 low-performing high schools and the creation of more than 200 new ones.

The closure decisions were made by the DOE's portfolio office, which evaluated schools based on historical performance and the likelihood of a successful turnaround with additional resources. The typical closure process involved a gradual phase-out, where schools stopped accepting new freshmen while allowing current students to either complete their education there or transfer to other schools.

Research has shown largely positive outcomes from these interventions. Kemple (2015) found that students who would have attended the closed schools benefited by instead attending better-performing schools, while students already enrolled during the phase-out process experienced no significant negative effects. Similar benefits were observed at the middle school level, as studied by Bifulco and Schwegman (2019). They found that students who would have attended closed middle schools instead attended institutions with better standardized test performance and higher value-added measures. High-achieving students in this group showed particular improvement in their test scores. However, unlike Kemple's high school findings, Bifulco and Schwegman (2019) found that middle school closures negatively impacted students who were currently attending the schools during closure.

Our analysis examines 44 high school closures in New York City between 2007-2017. We compared these actual closures with schools that would have been selected for closure using a posterior mean math ATE rule (matching Aligned Demand under Typical Replacement). As shown in Table 11, schools identified by the posterior mean ATE rule share several characteristics: low achievement-related treatment effects (ATEs), poor overall academic performance, higher proportions of Black and Hispanic students, and locations in lower-income neighborhoods. Of the 36 schools closed under Typical Replacement, 9 overlap with the 44 high schools that were actually closed.

When comparing our optimal closure selections with New York's actual closures (columns 2 and 4), we find many similarities across these characteristics. However, there are two key differences: First, the actual closures were more concentrated in Brooklyn (28%) and the Bronx (46%), while simulated closures are more concentrated in Queens (40%). Second, while the closed schools had negative Math ATEs, these effects were not as large as those predicted by our simulated closure policy (by necessity, since the simulated closures are selected based on ATEs).

6.2 Who Decides?

We next examine whether policymakers effectively identify underperforming schools for closure and whether market demand signals could improve these decisions. Figure 6 reveals that actual school closures strongly correlate with graduation rates, and schools with low graduation rates typically have low graduation ATEs (achievement-related treatment effects). This suggests that a portfolio office using even a simple rule based on graduation rates would identify many of the same schools as a more sophisticated posterior mean ATE approach. In contrast, Figure 7 shows only a weak relationship between household demand and school effectiveness or closure decisions.

Table 11 provides a multivariate analysis of school closure decisions, examining the relationships between closures and various school characteristics. These results come from a logit model predicting closure decisions among the 291 high schools in our analysis sample. The logit estimates confirm that schools with lower graduation rates were more likely to be closed. In addition, schools with lower graduation treatment effects (ATEs) and average realized match effects were more likely to be closed. In contrast, there's only a weak correlation between closure decisions and school popularity, as measured by the school mean utility from a rank-ordered logit model predicting student preferences. When controlling for all these factors simultaneously in column (4), we find that both low graduation rates and low ATEs independently predict school closures, while the coefficient on school popularity is small and statistically significant.

The weak relationship between closure outcomes and preferences suggests that many households continued to rank schools that the district ultimately closed. This disconnect helps explain two important phenomena. First, it sheds light on why closure decisions often face community resistance. Second, it suggests that using parental demand as a primary metric for closure decisions would be less effective than the district's current approach of focusing on graduation rates, if the ultimate goal is improving student outcomes. A demand-based strategy would likely miss many ineffective schools, while the central office's closure rule appears more successful at identifying schools that need intervention to improve overall graduation outcomes.

7 Conclusion

Policies pursued by America's urban school reformers can be divided into those focused on market clearing and those focused on resource augmentation. We've shown that while ambitious market-clearing policies can yield modest gains, they stumble on a simple constraint: there are too few good schools to go around. As a result, our estimates indicate that realistic resource augmentation policies, such as closing underperformers and expanding others, prove equally effective with less disruption. Graduation rates appear to provide an accurate guide to causal school effectiveness among NYC high schools, suggesting rough heuristics may be sufficient to identify low-performing schools in the absence of more sophisticated value-added metrics. Our results show that educational inequality stems not from poor matching but from capacity constraints at the most effective schools.

The comparison between market clearing and resource augmentation depends on what happens

after a school is closed. Comparing Effective and Typical Replacement scenarios, our findings show that replacing failing schools with high value-added schools requires about half as many closures as replacing them with average schools. Effective Replacement may not be too fanciful. Cohodes, Setren and Walters (2021) provide evidence that highly effective charter schools can replicate. Evidence from New York presented in Kemple (2015) and Bifulco and Schwegman (2019) suggests that students who would have attending closed schools attended higher value-added schools. Yet, achieving this ideal may face practical hurdles. While some districts have successfully expanded their most effective schools, others remain constrained by factors such as geography and the condition of facilities as well as political constraints such as moratoriums on closures.

Many districts are likely to face important resource augmentation questions in the coming years. Enrollment has fallen among America's largest school districts, forcing inevitable closures. This enrollment shift offers an opportunity to restructure systems around educational outcomes rather than merely cutting costs. Yet New York's experience highlights political challenges: the schools that data suggests are least effective often differ from those that parents want to preserve. This tension helps explain the fierce community resistance to closure policies. Some districts have found partial solutions. New Orleans, for instance, gives priority admission to students from closed schools, offering affected communities a tangible benefit (Valant, 2022). But more research is needed on how to balance achievement imperatives with community perceptions.

References

Abdulkadiroğlu, Atila, and Tayfun Sönmez. 2003. "School Choice: A Mechanism Design Approach." *American Economic Review*, 93(3): 729–747.

Abdulkadiroğlu, Atila, Joshua D. Angrist, and Parag A. Pathak. 2014. "The Elite Illusion: Achievement Effects at Boston and New York Exam Schools." *Econometrica*, 82(1): 137–196.

Abdulkadiroğlu, Atila, Joshua D. Angrist, Peter D. Hull, and Parag A. Pathak. 2016. "Charters without Lotteries: Testing Takeovers in New Orleans and Boston." *American Economic Review*, 106(7): 1878–1920.

Abdulkadiroğlu, Atila, Joshua D. Angrist, Susan M. Dynarski, Thomas J. Kane, and Parag A. Pathak. 2011. "Accountability and Flexibility in Public Schools: Evidence from Boston's Charters and Pilots." *Quarterly Journal of Economics*, 126(2): 699–748.

Abdulkadiroğlu, Atila, Nikhil Agarwal, and Parag A. Pathak. 2017. "The Welfare Effects of Coordinated Assignment: Evidence from the NYC HS Match." *American Economic Review*, 107(12): 3635–3689.

Abdulkadiroğlu, Atila, Parag A. Pathak, and Alvin E. Roth. 2009. "Strategy-Proofness versus Efficiency in Matching with Indifferences: Redesigning the New York City High School Match." *American Economic Review*, 99(5): 1954–1978.

Abdulkadiroğlu, Atila, Parag A. Pathak, Jon Schellenberg, and Christopher R. Walters. 2020*a*. "Do Parents Value School Effectiveness?" *American Economic Review*, 110(5): 1502–1539.

Abdulkadiroğlu, Atila, Yeon-Koo Che, Parag A. Pathak, Alvin Roth, and Olivier Tercieux. 2020*b*. "Efficiency, Justified Envy, and Incentives in Prioritized Matching." *American Economic Review: Insights*, 2(4).

Agarwal, Nikhil, and Paulo Somaini. 2018. "Demand Analysis Using Strategic Reports: An Application to a School Choice Mechanism." *Econometrica*, 86(2): 391–444.

Ainsworth, Robert, Rajeev Dehejia, Cristian Pop-Eleches, and Miguel Urquiola. 2023. "Why Do Households Leave School Value Added on the Table? The Roles of Information and Preferences." *American Economic Review*, 113(2): 456–490.

Akbarpour, Mohammad, Suraj Malladi, and Amin Saberi. 2018. "Just a Few Seeds More: Value of Network Information for Diffusion." Working paper, Stanford University.

Allende, Claudia, Francisco Gallego, and Christopher Neilson. 2019. "Approximating the Equilibrium Effects of Informed School Choice." Working paper, Yale University.

Andrabi, Tahir, Jishnu Das, and Asim Ijaz Khwaja. 2017. "Report Cards: The Impact of Providing School and Child Test Scores on Educational Markets." *American Economic Review*, 107(6): 1535–63.

Angrist, Joshua D., Parag A. Pathak, and Roman Zarate. 2023. "Choice and Consequence: Assessing Mismatch at Chicago's Exam Schools." *Journal of Public Economics*, 223.

Angrist, Joshua D., Peter Hull, Parag A. Pathak, and Christopher R. Walters. 2017. "Leveraging Lotteries for School Value-Added: Testing and Estimation." *Quarterly Journal of Economics*, 132(2): 871–919.

Angrist, Joshua D., Peter Hull, Parag A. Pathak, and Christopher R. Walters. 2024. "Credible School Value-Added with Undersubscribed School Lotteries." *Review of Economics and Statistics*, 106(1): 1–46.

Angrist, Joshua D., Sarah Cohodes, Susan Dynarski, Parag A. Pathak, and Christopher R. Walters. 2016. "Stand and Deliver: Effects of Boston's Charter High Schools on College Preparation, Entry, and Choice." *Journal of Labor Economics*, 34(2): 275–318.

Arteaga, Felipe, Adam J. Kapor, Christopher A. Neilson, and Seth D. Zimmerman. 2022. "Smart Matching Platforms and Heterogeneous Beliefs in Centralized School Choice." *Quarterly Journal of Economics*, 137(3): 1791–1848.

Aubri. 2023. "Lottery results for Philly's special-admissions schools to come by month end." PBS WHYY, January 23, Available at: https://whyy.org/articles/philly-special-admissions-schools-lottery/.

Avery, Christopher N., Geoffrey Kocks, and Parag A. Pathak. 2024. "Does a Common Application Increase Access? Theory and Evidence from Boston's Charters." Working paper, MIT.

Barnum, Matthew. 2019. "17 studies that tell us something about how school closures affect students." Chalkbeat, Available: https://www.chalkbeat.org/2019/2/5/21106771/17-studies-that-tell-us-something-about-how-school-closures-affect-students/.

Barrow, Lisa, Lauren Sartain, and Marisa de la Torre. 2020. "Increasing Access to Selective High Schools through Place-Based Affirmative Action: Unintended Consequences." *American Economic Journal: Applied Econoimcs*, 12(4): 135–163.

Baum-Snow, Nathaniel, and Byron F. Lutz. 2011. "School Desegregation, School Choice, and Changes in Residential Location Patterns by Race." *American Economic Review*, 101(7): 3019–46.

Bifulco, Robert, and David J. Schwegman. 2019. "Who Benefits from Accountability-Driven School Closure? Evidence from New York City." *Journal of Policy Analysis and Management*, 39(1): 96–130.

Bloom, Howard S, and Rebecca Unterman. 2014. "Can Small High Schools of Choice Improve Educational Prospects for Disadvantaged Students?" *Journal of Policy Analysis and Management*, 33(2): 290–319.

Boston Globe. 2024. "Closing schools is painful. But in Boston, it's essential." Boston Globe, Available at: https://www.bostonglobe.com/2024/09/22/opinion/boston-public-schools-closing-consolidation-michelle-wu/.

Boston School Committee. 2021. "Exam School Admissions Policy." Available at: https://www.bostonpublicschools.org/Page/8838.

Boustan, Leah Platt. 2012. "School Desegregation and Urban Change: Evidence from City Boundaries." *American Economic Journal: Applied Economics*, 4(2): 85–108.

Bulow, Jeremy, and Paul Klemperer. 1996. "Auctions vs. Negotiations." *American Economic Review*, 86(1): 180–194.

Calsamiglia, Caterina, Chao Fu, and Maia Güell. 2020. "Structural Estimation of a Model of School Choices: The Boston Mechanism versus Its Alternatives." *Journal of Political Economy*, 128(2): 642–680.

Campos, Christopher. 2024. "Social Interactions, Information, and Preferences for Schools: Experimental Evidence from Los Angeles." Working paper, Chicago Booth.

Campos, Christopher, and Caitlin Kearns. 2024. "The Impact of Neighborhood School Choice: Evidence from Los Angeles' Zones of Choice." *Quarterly Journal of Economics*, 39(2).

Carlson, Deven, and Stéphane Lavertu. 2018. "School Improvement Grants in Ohio: Effects on Student Achievement and School Administration." *Educational Evaluation and Policy Analysis*, 40(3).

Carrell, Scott, Bruce Sacerdote, and J West. 2013. "From Natural Variation to Optimal Policy? The Importance of Endogenous Peer Group Formation." *Econometrica*, 81(3): 855–882.

Chetty, Raj, John Friedman, and Jonah Rockoff. 2014. "Measuring the Impact of Teachers I: Evaluating Bias in Teacher Value-Added Estimates." *American Economic Review*, 104(9): 2593–2632.

Ciurczak, Peter. 2023. "Empty Desks: The Growing Mismatch Between City and School Demographics in Boston." Boston Indicators, Available at: https://www.bostonindicators.org/article-pages/2024/july/empty_desks_pt_2.

Cohodes, Sarah R., Elizabeth Setren, and Christopher R. Walters. 2021. "Can Successful Schools Replicate? Scaling Up Boston's Charter School Sector." *American Economic Journal: Economic Policy*, 13(4): 138–170.

Commonwealth of Massachusetts. 2010. "An Act Relative to the Achievement Gap: Turning Around Low-Performing Schools and Promoting Innovation for All." http://www.mass.gov/Eoedu/docs/legislation$_p$*olicy*/20100125$_e$*d*$_l$*aw*$_f$*act*$_s$*heet.pdf*.

Corcoran, Sean, Jennifer L. Jennings, Sarah R. Cohodes, and Carolyn Sattin-Bajaj. 2018. "Leveling the Playing Field for High School Choice: Results from a Field Experiment of Informational Interventions." NBER Working Paper 24471.

Corradini, Viola. 2024. "Information and Access in School Choice Systems: Evidence from New York City." Unpublished working paper, University of Wisconsin; Available at: https://sites.google.com/view/violacorradini/home.

Dee, Thomas. 2023. "Where the Kids Went: Nonpublic Schooling and Demographic Change during the Pandemic Exodus from Public Schools." *Teachers College Record*, 125(6): 119–129.

de Haan, Monique, Pieter A. Gautier, Hessel Oosterbeek, and Bas van der Klaauw. 2023. "The Performance of School Assignment Mechanisms in Practice." *Journal of Political Economy*, 131(1): 1–45.

Deming, David J. 2014. "Using School Choice Lotteries to Test Measures of School Effectiveness." *American Economic Review: Papers and Proceedings*, 104(5): 406–11.

Deming, David, Justine Hastings, Thomas Kane, and Douglas Staiger. 2014. "School Choice, School Quality and Postsecondary Attainment." *American Economic Review*, 104(3): 991–1013.

Dobbie, Will, and Roland G. Fryer. 2014. "Exam High Schools and Academic Achievement: Evidence from New York City." *American Economic Journal: Applied Economics*, 6(3): 58–75.

Dubin, Jeffrey, and Dan McFadden. 1984. "An Econometric Analysis of Residential Electric Appliance Holdings and Consumption." *Econometrica*, 52(2): 345–362.

Duflo, Esther, Pascaline Dupas, and Michael Kremer. 2011. "Peer Effects and the Impacts of Tracking: Evidence from a Randomized Evaluation in Kenya." *American Economic Review*, 101(5): 1739–1774.

Dur, Umut, Parag A. Pathak, and Tayfun Sönmez. 2020. "Explicit vs. Statistical Targeting in Affirmative Action: Theory and Evidence from Chicago's Exam Schools." *Journal of Economic Theory*, 187.

Dur, Umut, Scott Duke Kominers, Parag A. Pathak, and Tayfun Sönmez. 2018. "Reserve Design: Unintended Consequences and the Demise of Boston's Walk Zones." *Journal of Political Economy*, 126(6): 2457–2479.

Egalite, Anna, Brian Kisida, and Marcus Winters. 2015. "Representation in the classroom: The effect of own-race teachers on student achievement." *Economics of Education Review*.

Ellison, Glenn, and Parag A. Pathak. 2021. "The Efficiency of Race-Neutral Alternatives to Race-Based Affirmative Action: Evidence from Chicago's Exam Schools." *American Economic Review*, 111(3): 943–975.

Engberg, John, Brian Gill, Gema Zamarro, and Ron Zimmer. 2012. "Closing schools in a shrinking district: Do student outcomes depend on which schools are closed?" *Journal of Urban Economics*, 71(2): 189–203.

Gale, David, and Lloyd S. Shapley. 1962. "College Admissions and the Stability of Marriage." *American Mathematical Monthly*, 69: 9–15.

Gill, Brian, John Engberg, and Kevin Booker. 2005. "Assessing the Performance of Public Schools in Pittsburgh." RAND Education Working Paper WR-315-1-EDU.

Guin, Kacey, and Marguerite Roza. 2007. "Lessons from the Headlines: Key Questions for Districts Attempting to Close Schools." American School Board Journal.

Hastings, Justine, and Jeffrey M. Weinstein. 2008. "Information, School Choice and Academic Achievement: Evidence from Two Experiments." *Quarterly Journal of Economics*, 123(4): 1373–1414.

Heller, Nathan. 2022. "What Happens When an Elite Public School Becomes Open to All?" New Yorker, March 7.

Houston, David M., and Jeffrey R Henig. 2021. "The effects of student growth data on school district choice: Evidence from a survey experiment." *American Journal of Education*, 127(4): 563–595.

Idoux, Clemence. 2024. "Integrating New York City Schools: The Role of Admission Criteria and Family Preferences." Working Paper, UCSD.

IES. 2018. "State Education Practices (SEP). Table 4.4. States with charter school caps, automatic exemptions, required teacher certification, and identification of special education responsibilities for charter schools, by state: 2017–18." Available at: https://nces.ed.gov/programs/statereform/tab4_4.asp.

Kemple, James. 2015. "High School Closures in New York City: Impacts on Students' Academic Outcomes, Attendance, and Mobility." The Research Alliance for New York City Schools.

Kowal, Jule, and Bryan Hassel. 2008. "Closing Troubled Schools." University of Washington-Bothell, NCRSP Working Paper 2008-8.

Mohamad, Ikram. 2024. "Chicago Board of Education Votes to Halt School Closures Until January 2027." Available at: https://www.chicagotribune.com/2024/09/26/chicago-board-of-education-votes-to-halt-school-closures-until-january-2027/.

Mohammad Akbarpour, Yeganeh Alimohammadi, Shengwu Li, and Amin Saberi. 2023. "The Value of Excess Supply in Spatial Matching Markets." Working paper.

Mojadad, Ida. 2022. "Selective admissions are back at Lowell High after a split vote reinstates the controversial policy." The San Francisco Standard, June 22. Available at: https://sfstandard.com/2022/06/22/selective-admissions-are-back-at-lowell-high-after-a-split-vote-reinstates-the-controversial-policy/.

NCES. 2023. "Public Charter School Enrollment. Condition of Education." U.S. Department of Education, Institute of Education Sciences, Retrieved June 21, 2023, from https://nces.ed.gov/programs/coe/indicator/cgb.

NYC Charter Center. 2024. "25 Years of NYC Charter Schools." NYC Charter School Center Fact Sheet, Available at: https://nyccharterschools.org/wp-content/uploads/2023/10/NYC-CS-25ANN-FS-2023-24-1.pdf.

Pathak, Parag A. 2017. "What Really Matters in Designing School Choice Mechanisms." In *Advances in Economics and Econometrics, 11th World Congress of the Econometric Society.* eds. Bo Honore, Monika Piazessi, Ariel Pakes, and Larry Samuelson. Cambridge University Press.

Pathak, Parag A., and Jay Sethuraman. 2010. "Lotteries in Student Assignment: An Equivalence Result." *NBER Working paper 16140.*

Pathak, Parag A., and Tayfun Sönmez. 2008. "Leveling the Playing Field: Sincere and Sophisticated Players in the Boston Mechanism." *American Economic Review*, 98(4): 1636–1652.

Roper, Glenn E., and Joshua P. Thompson. 2024. "Stopping New York's attempt to discriminate against Asian-American students." Pacific Legal Foundation, Available at: https://pacificlegal.org/case/christa-mcauliffe-pto-v-de-blasio/.

Roth, Alvin E. 2016. *Who Gets What – and Why: The New Economics of Matchmaking and Market Design.* Eamon Dolan/Mariner Books.

Rothstein, Jesse. 2010. "Teacher Quality in Educational Production: Tracking, Decay, and Student Achievement." *Quarterly Journal of Economics*, 125(1): 175–214.

Rothstein, Jesse. 2017. "Measuring the Impacts of Teachers: Comment." *American Economic Review*, 107(6): 1656–1684.

Roughgarden, Tim. 2020. "Resource Augmentation." In *Beyond the Worst-Case Analysis of Algorithms*. eds. Tim Roughgarden. Cambridge University Press.

Sacerdote, Bruce. 2011. "Peer Effects in Education: How Might They Work, How Big Are They and How Much Do We Know Thus Far?" In *Handbook of Economics of Education*. eds. Eric A Hanushek, Stephen Machin, and Ludger Woessmann, 249–277. Elsevier.

Saul, Stephanie. 2023. "High School Did Not Discriminate Against Asian American Students, Court Rules." New York Times, May 23.

Saul, Stephanie, and Adam Liptak. 2023. "Supreme Court Is Asked to Hear a New Admissions Case on Race." New York Times, August 23.

Scharfenberg, David. 2016. "Mass. Voters Reject Ballot Question on Charter Schools." Boston Globe, November 8.

Schueler, Beth, Joshua S. Goodman, and David J. Deming. 2017. "Can states take over and turn around school districts? Evidence from Lawrence, Massachusetts." *Educational Evaluation and Policy Analysis*, 39(2): 311–332.

Shapiro, Eliza. 2019. "Segregation Has Been the Story of New York City's Schools for 50 Years." New York Times, March 26. Available at: https://www.nytimes.com/2019/03/26/nyregion/school-segregation-new-york.html.

Shapley, Lloyd, and Martin Shubik. 1971. "The assignment game I: The core." *International Journal of Game Theory*, 1: 111–130.

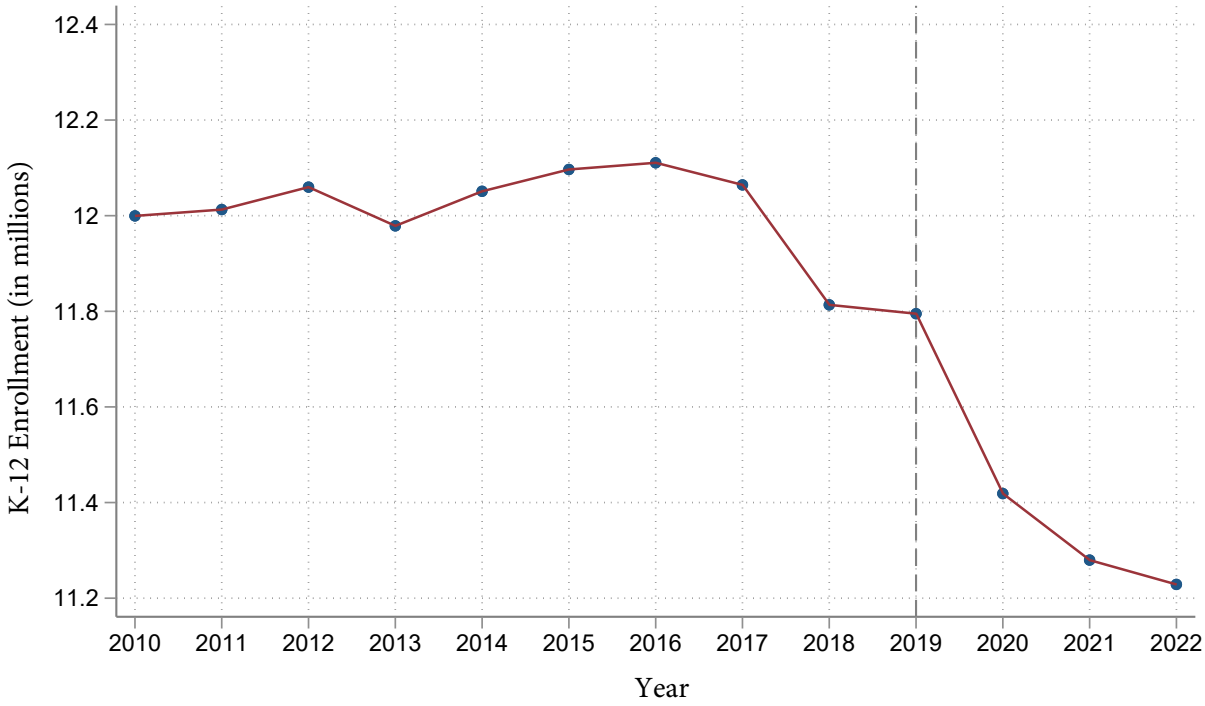
Shi, Peng. 2015. "Guiding School-Choice Reform through Novel Applications of Operations Research." *Interfaces*, 45(2).

Valant, Jon. 2022. "What Happens When Families Whose Schools Close Receive EdNavigator Support and OneApp Priority?" NOLA Education Research Alliance Policy Brief, June 14.

Vevea, Becky. 2024. "Illinois lawmakers propose extending Chicago's moratorium on school closures to 2027." Chalkbeat Chicago, Available at: https://www.chalkbeat.org/chicago/2024/04/17/chicago-school-closings-moratorium-could-last-until-2027/.

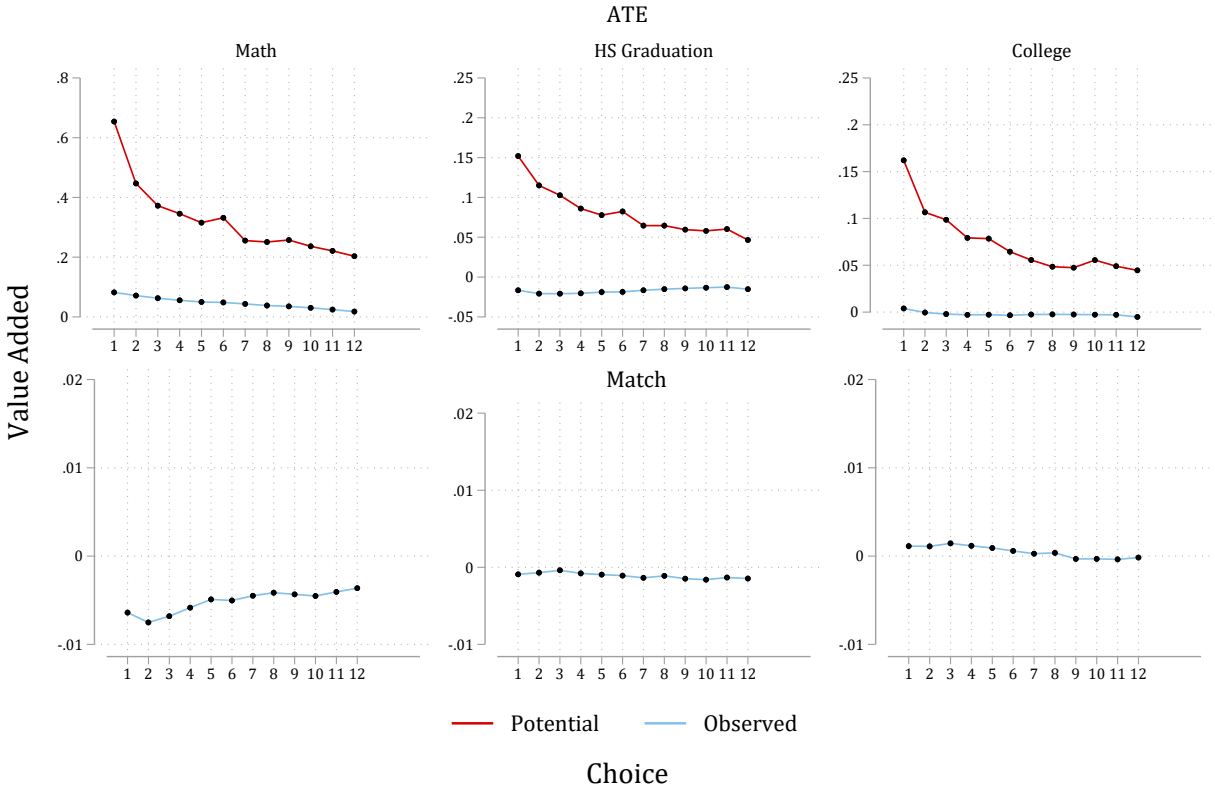
Walters, Christopher R. 2018. "The Demand for Effective Charter Schools." *Journal of Political Economy*, 126(6): 2179–2223.

Figure 1: Total K-12 Enrollment for 100 Largest Districts



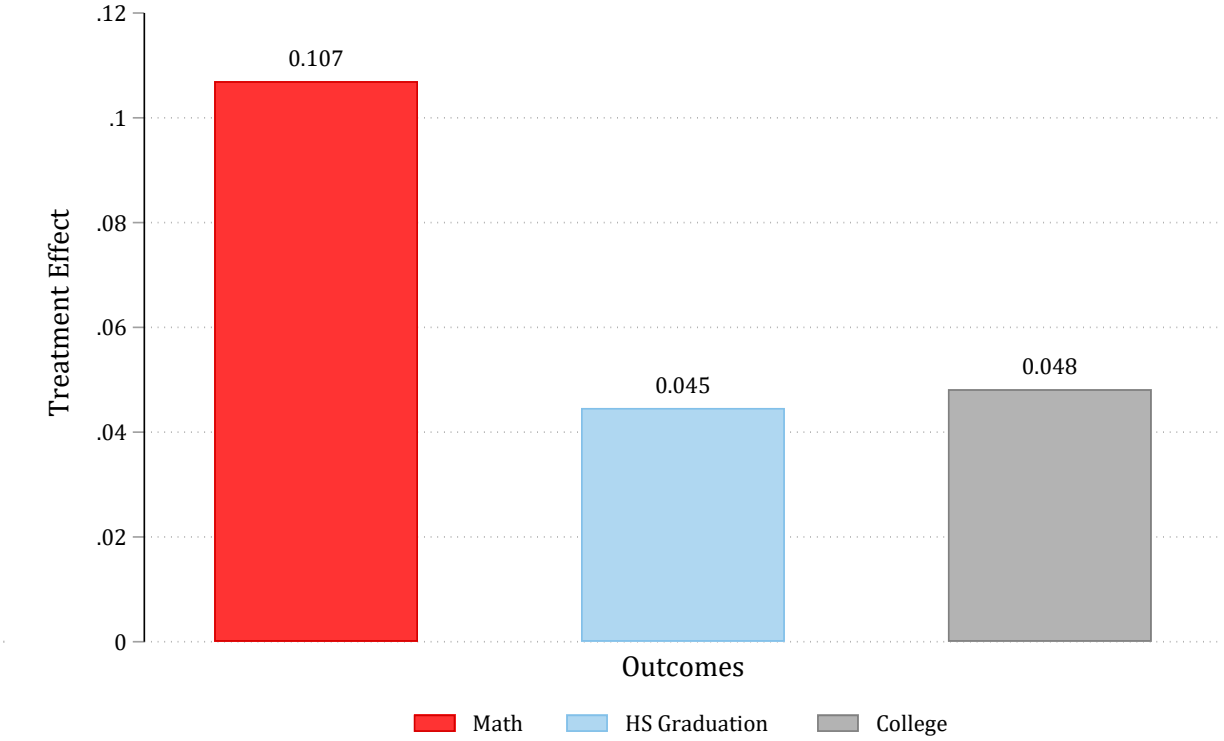
Notes: This figure reports the total K-12 enrollment in the 100 largest school districts in 2023 using data from the National Center for Education Statistics.

Figure 2: Potential Achievement Gains from Rankings Schools by Regents Math Effectiveness



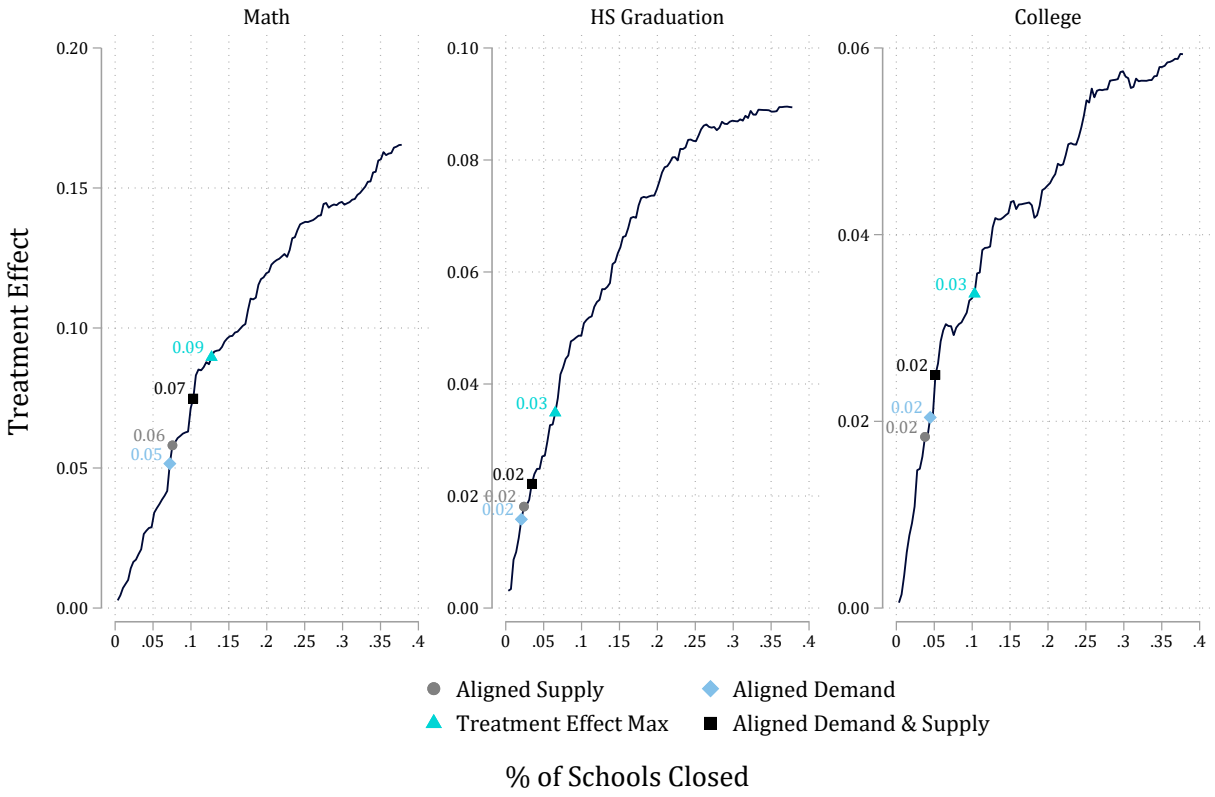
Notes: This figure compares the the observed student rankings to potential school rankings on the basis of treatment effects, estimated using the control function model.

Figure 3: Potential Achievement Gains from Optimal School Admissions



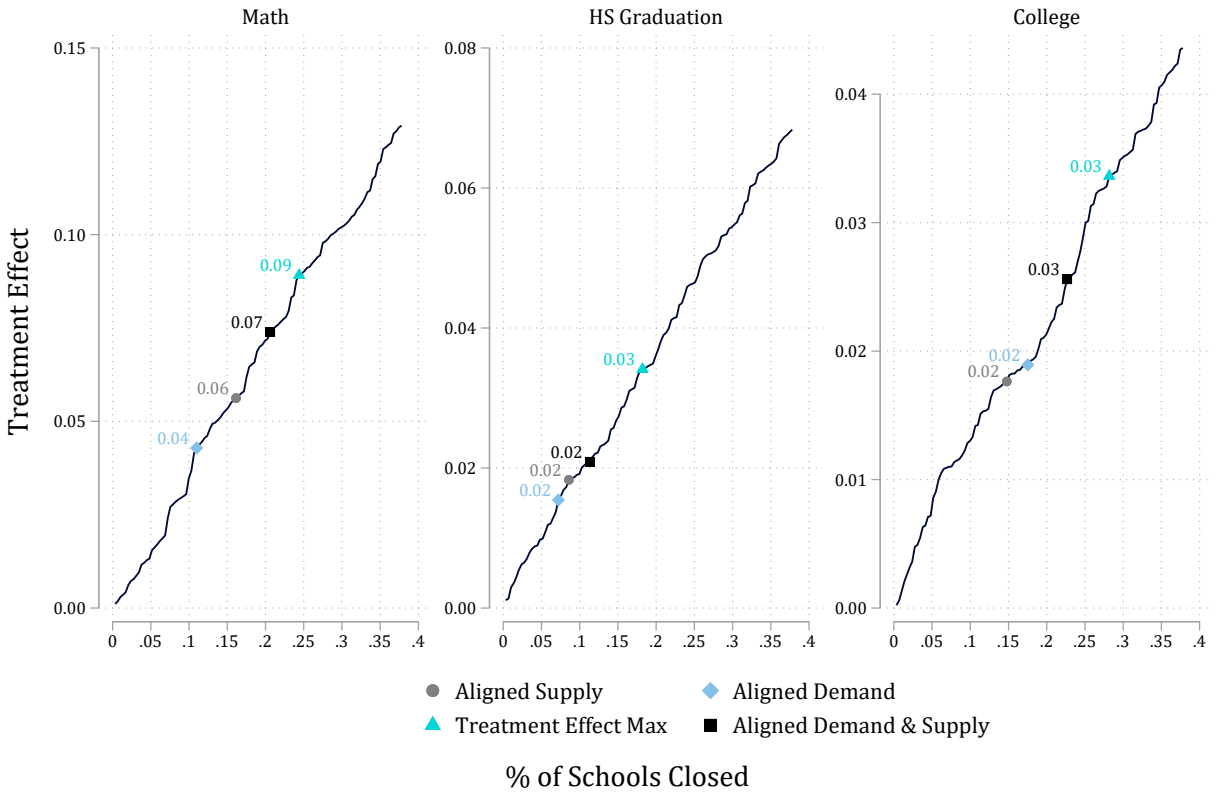
Notes: This figure reports the effect of assigning priorities at each school in order of students' match effects, holding student preferences fixed. Estimates are effects relative to the status quo.

Figure 4: Treatment Effect (CF) Following Closure under Effective Replacement



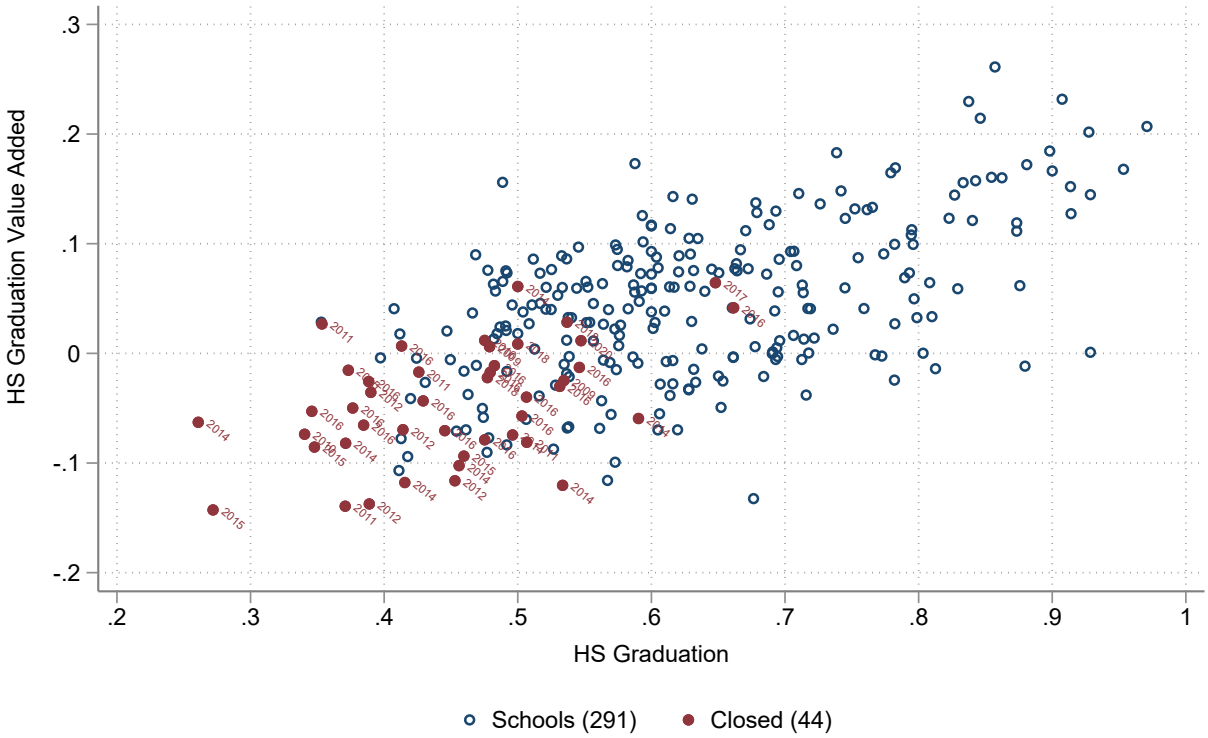
Notes: This figure shows the fraction of schools that need to be closed to equal to aggregate treatment effect under market-clearing counterfactuals using the control function estimates of school value-added. 1% of schools is approximately equal to three schools.

Figure 5: Treatment Effect (CF) Following Closure under Typical Replacement



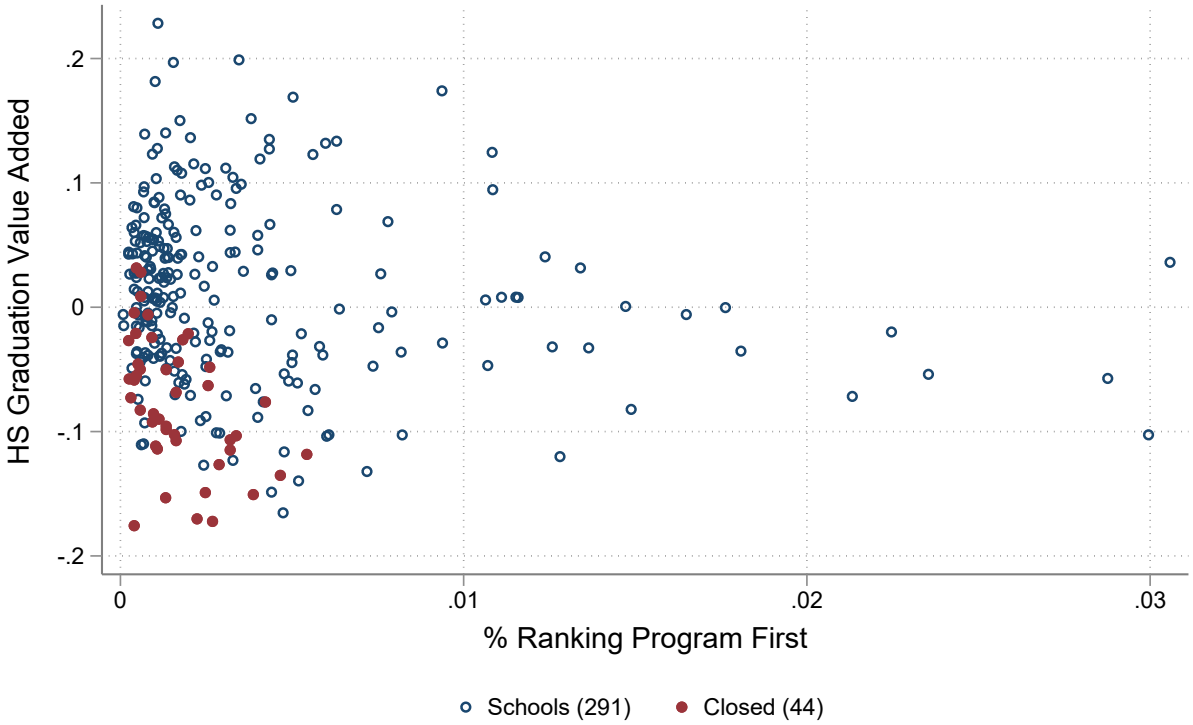
Notes: This figure shows the fraction of schools that need to be closed to equal to aggregate treatment effect under market-clearing counterfactuals using the control function estimates of school value-added. 1% of schools is approximately equal to three schools.

Figure 6: HS Graduation Levels and Value Added for Open and Closed Schools



Notes: This figure reports the high school graduation levels and value added for open and closed schools between 2007-2017.

Figure 7: Top Choice Market Share of Programs for Open and Closed Schools



Notes: This figure reports the top choice market share of schools and schools that are closed between 2007-2017.

Table 1—Changes to Market Clearing

Location	Previous Policy	Year	New Policy
(1)	(2)	(3)	(4)
	A. New Admissions Criteria		
Chicago Exam Schools	Admissions Test with Racial Quotas	2009	Admissions Test with Census-Tract Based Affirmative Action
Traditional Boston PS	Three Zone Choice Menu with Walk Zone Reserve	2013	Home-Based Assignment without Walk Zone
New York City (MS/HS)	Screened Admissions Criteria	2018-2022	Diversity in Admissions (Income, Special Ed, and Other Status)
Northern Virginia (TJ)	Selective Admission with State Test	2020	Holistic Review with Top 1.5% MS Feeder System
San Francisco Lowell	GPA and State Test	2020	Random Lottery
Philadelphia	Selective Admissions Based on School Criteria	2021	Lottery with 6 Priority Zip Codes
Boston Exam Schools	Admissions Test	2021	Grade and State Test with Census Tract-Based Affirmative Action
	B. Changes to Matching Process		
Denver	Decentralized Lotteries	2012	Unified Enrollment with Matching Algorithm
Washington DC	First-come First Serve; Decentralized Lotteries	2013	Unified Enrollment with Matching Algorithm
Newark	First-come First Serve; Decentralized Lotteries	2014	Unified Enrollment with Matching Algorithm
Camden	First-come First Serve; Decentralized Lotteries	2015	Unified Enrollment with Matching Algorithm
Indianapolis	Decentralized Partial Choice	2016	Unified Enrollment with Matching Algorithm
Atlanta, Baton Rouge, Boston, Buffalo, Kansas City, Houston, Los Angeles, Oakland, Rhode Island, Rochester	Decentralized Lotteries	2017-2021	Common Charter Application
Newark	Unified Enrollment with Matching Algorithm	2021	Separate Charter School Match
	C. Decision Aids		
Charlotte-Mecklenburg	School Choice Mechanism	2006	Informational Intervention (School Test Score Levels)
New Haven	School Choice Mechanism	2016	Informational Intervention (School-specific Demand, Priority Groups, Overall Ranking System)
New York City	School Choice Mechanism	2017	Informational and Technological Intervention (HS Graduation Rate)
Romania	School Choice with Test-Based Screening	2019	Informational Intervention to Households (School Value-Added)

Notes: This table summarizes large-scale admission policy changes of public schools with periods of when those admission policies were implemented. Panel A outlines admission policy changes that are major changes to the systems, while Panel B summarizes examples of sites where single-best offer matching systems was implemented. Panel C describes implementation of decision aids in school matching systems.

Table 2—Summary of Large-scale Closures in Urban School Districts

Site	Number of Schools Closed	Time Period	Selection Criteria Summary
Detroit	195	2000 – 2015	▪ Academic performance (levels) ▪ Utilization ▪ School building condition and age
New York City	157	2002 – 2020	▪ Trends in graduation rate ▪ Trends in attendance rate or credit accumulation ▪ Ability to turn around with additional supports ▪ School letter grades
Chicago	50	2000 – 2013	▪ Enrollment / demand ▪ Academic performance ▪ School building condition ▪ Geographic isolation ▪ Paired each closed school with a higher performing "welcoming school" within 1 miles of the closed school
Milwaukee	80	2001 – 2012	▪ Enrollment ▪ Academic performance (including trends) ▪ Building condition
Baltimore City	75	2004 – 2018	▪ Enrollment trends ▪ School building condition and age ▪ Student commutes ▪ Educational programming ▪ Cost-savings potential ▪ Community impact
Denver	60	2002 – 2022	▪ School performance ratings and trends ▪ School quality review ▪ Enrollment/ demand
Houston	56	2003 – 2019	▪ Enrollment/ demand ▪ Low academic achievement
Kansas City	32	2010 – 2023	▪ Academic performance ▪ Building condition ▪ Enrollment / demand ▪ Geographic isolation
Philadelphia	28	2013 – 2023	▪ Building condition ▪ Enrollment / demand ▪ Academic record ▪ Cost savings potential ▪ Geographic isolation
Pittsburgh	29	2006 – 2023	▪ VAM ▪ Ability to reassign relocated students to higher-VA school
St. Louis	70	2003 – 2023	▪ Enrollment / demand ▪ Academic achievement ▪ Building condition and age
Newark	11	2011 – 2014	▪ Enrollment / demand ▪ Academic performance
Oakland	7	2022	▪ Enrollment / demand
Indianapolis	7	2008 – 2015	▪ Enrollment / demand ▪ Relocated students attend nearby schools with excess capacity

Notes: This table describes large-scale closures across big urban school districts. The time period indicates the years that the district closed schools, and the criteria used to close the schools is summarized.

Table 3—Characteristics of Samples

	Population (1)	VAM Sample (2)	Analysis Sample (3)
		A. Students	
Total	275,225	222,202	62,231
2003-2004	67,644	54,402	
2004-2005	67,331	54,482	
2005-2006	68,053	55,954	
2006-2007	72,197	57,364	62,231
Bronx	0.23	0.23	0.24
Brooklyn	0.32	0.34	0.32
Manhattan	0.12	0.12	0.12
Queens	0.26	0.25	0.26
Staten Island	0.06	0.06	0.06
Black/Hispanic	0.73	0.77	0.77
Subsidized Lunch	0.65	0.68	0.71
Female	0.50	0.50	0.49
SPED	0.07	0.08	0.08
Baseline Math	0.00	-0.10	-0.10
		B. Programs	
Number of Schools	391	291	291
Number of Programs	1,054	845	845
Audition	0.10	0.10	0.10
Educational Option	0.36	0.34	0.34
Screened	0.24	0.24	0.24
Unscreened	0.35	0.32	0.32

Notes: This table displays summary statistics from three samples. The sample consists of 8th graders with demographic information applying to the match. VAM sample is a restricted version of the sample that contains NYC public school students attending schools for which we compute VAM estimates. Analysis sample is the sample of students and schools used for counterfactual analysis. Baseline math is from 8th grade citywide standardized math test.

Table 4—Distributions of School Value-Added Parameters for Regents Math

	Intercept (1)	Above-median Math (2)	Black/Hispanic (3)	Female (4)	Preference coefficient (ψ_j) (5)
			A. Control function		
Mean	0	0.278	-0.109	-0.032	-0.001
	—	(0.007)	(0.017)	(0.005)	(0.001)
SD	0.262	0.128	0.143	0.054	0.006
	(0.004)	(0.007)	(0.035)	(0.013)	(0.001)
Correlation: Above-median Math	-0.196				
	(0.049)				
Black/Hispanic	-0.008	-0.242			
	(0.111)	(0.132)			
Female	0.284	0.136	-0.201		
	(0.118)	(0.162)	(0.247)		
Preference coefficient (ψ_j)	0.225	-0.178	-0.069	0.176	—
	(0.080)	(0.106)	(0.176)	(0.198)	
			B. OLS VAM		
Mean	0	0.282	-0.095	-0.040	—
	—	(0.007)	(0.011)	(0.005)	
SD	0.275	0.129	0.144	0.052	—
	(0.004)	(0.007)	(0.038)	(0.014)	
Correlation: Above-median Math	-0.191				
	(0.048)				
Black/Hispanic	-0.041	-0.258			
	(0.107)	(0.127)			
Female	0.245	0.212	-0.271	—	—
	(0.120)	(0.170)	(0.267)		

Notes: This table reports estimates of features of the distributions of school value-added parameters for Regents math scores. School-specific estimates come from a regression of school indicators interacted with a constant (intercept), an indicator for an above-median baseline math score, a Black/Hispanic indicator, and a female indicator. The regression also controls for main effects of baseline math and reading scores, borough indicators, a subsidized lunch indicator, and log median income in a student's census tract. Estimated standard deviations and correlations are derived from bias-corrected estimates of the variances and covariances of the interaction coefficients across schools, with standard errors computed by the delta method.

Table 5—Descriptive Statistics for Regents Math Value Added

		Realized		Potential	
		Raw	Shrunk	Raw	Shrunk
		(1)	(2)	(3)	(4)
		A. Control function			
Treatment Effect					
	Mean	0.00	0.00	0.00	0.00
	SD	0.25	0.24	0.30	0.27
	Range	[-1.14,1.14]	[-0.96,1.07]	[-1.64,1.17]	[-1.01,1.07]
Average Treatment Effect					
	Mean	0.01	0.01	0.00	0.00
	SD	0.24	0.23	0.27	0.26
	Range	[-0.72,1.02]	[-0.64,0.99]	[-0.72,1.02]	[-0.64,0.99]
Match Effect					
	Mean	0.00	0.00	0.00	0.00
	SD	0.13	0.08	0.13	0.08
	Range	[0.88,-1.11]	[-0.58,0.36]	[-1.11,0.88]	[-0.58,0.36]
		B. OLS VAM			
Treatment Effect					
	Mean	0.01	0.01	0.00	0.00
	SD	0.26	0.25	0.31	0.28
	Range	[-1.27,1.15]	[-0.86,1.07]	[-1.78,1.15]	[-0.86,1.07]
Average Treatment Effect					
	Mean	0.01	0.01	0.00	0.00
	SD	0.26	0.25	0.28	0.27
	Range	[-0.71,1.01]	[-0.66,0.99]	[-0.71,1.01]	[-0.66,0.99]
Match Effect					
	Mean	0.00	0.00	0.00	0.00
	SD	0.10	0.07	0.14	0.07
	Range	[-0.74,0.77]	[-0.32,0.32]	[-1.18,0.77]	[-0.36,0.32]

Notes: The table displays the mean, standard deviation, and range of treatment effects, average treatment effects, and match effects for Regents Math from models with three interacted covariates (Black/Hispanic indicator, female indicator, and above-median baseline math). Shrunk estimates are Empirical Bayes estimates described in the text. Estimates are generated using student data from 2003-06, and statistics are displayed for 2006 applicants. The number of observations for realized is 62,231, and the number for potential is 18,109,220.

Table 6—Potential Gains from Counterfactual Assignments Relative to Status Quo

	Aligned Demand	Aligned Supply	Aligned Demand and Supply	Treatment Effect Maximization
	(1)	(2)	(3)	(4)
		A. Control function		
Math	0.042	0.056	0.073	0.089
HS Graduation	0.015	0.018	0.021	0.034
College	0.019	0.018	0.025	0.034
		B. OLS VAM		
Math	0.037	0.049	0.065	0.082
HS Graduation	0.013	0.018	0.022	0.035
College	0.017	0.018	0.026	0.034

Notes: This table compares treatment effects in the counterfactual models compared to status quo. Aligned Demand has students rank schools based on effectiveness and leaves school-side rankings unchanged. Aligned Supply has schools rank students based on effectiveness and leaves student-side rankings unchanged. Aligned Demand and Supply is the outcome when both students and schools rank each other based on maximizing treatment effects. Treatment Effect Maximization is a linear program that maximizes treatment effects. The total number of students is 62,231.

Table 7—Potential Gains from Counterfactual Assignments Relative to Status Quo

	TEMA			Max TE for Bottom Quartile using all Seats			Max TE for Bottom Quartile using Status Quo Seats
	All	Bottom Math Quartile	Top 3 Math Quartiles	All	Bottom Math Quartile	Top 3 Math Quartiles	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Math	0.089	0.165	0.060	0.086	0.405	-0.032	0.031
HS Graduation	0.034	0.067	0.022	0.033	0.134	-0.004	0.013
College	0.034	0.054	0.026	0.033	0.125	-0.002	0.017

Notes: This table compares treatment effects in counterfactual assignments relative to the status quo based on control function estimates of school effects. All represents an average predicted achievement for all students. The bottom math quartile represents the average predicted achievement for the lowest 25% of students on baseline math.

Table 8—Closures Needed to Match Counterfactual Market-Clearing Policies

	Effective Replacement (1)	Typical Replacement (2)	Neighborhood Replacement (3)
		A. Aligned Demand	
Math			
% Students in Same School	0.72	0.67	0.79
Schools Closed	21	32	31
% Seats Reallocated	0.05	0.10	0.09
Seats Reallocated	3,319	6,304	5,663
HS Graduation			
% Students in Same School	0.73	0.61	0.69
Schools Closed	6	21	32
% Seats Reallocated	0.04	0.15	0.17
Seats Reallocated	2,779	9,398	10,815
College			
% Students in Same School	0.68	0.55	0.67
Schools Closed	13	51	41
% Seats Reallocated	0.07	0.23	0.19
Seats Reallocated	4,625	14,298	11,583
		B. TEMA	
Math			
% Students in Same School	0.63	0.58	0.65
Schools Closed	37	71	72
% Seats Reallocated	0.11	0.23	0.22
Seats Reallocated	7,130	14,123	13,884
HS Graduation			
% Students in Same School	0.58	0.46	0.47
Schools Closed	19	53	66
% Seats Reallocated	0.12	0.34	0.38
Seats Reallocated	7,449	21,092	23,738
College			
% Students in Same School	0.58	0.40	0.51
Schools Closed	30	82	87
% Seats Reallocated	0.15	0.40	0.34
Seats Reallocated	9,299	24,832	21,411

Notes: This table displays school closure DA estimates after closing a number of schools. Each measure displayed represents the equivalent closures needed to match the model's treatment effects using the control function estimates. Typical Replacement represents a closure model where seats are reallocated from closed schools to all remaining schools. Effective Replacement represents a closure model where seats are reallocated from closed schools to top-performing schools. Neighborhood Replacement represents a closure model where seats are reallocated from closed schools to the five nearest open schools.

Table 9—Characteristics of Assignments for Regents Math

	Status Quo (1)	Aligned Demand (2)	Aligned Supply (3)	Aligned Demand and Supply (4)	Treatment Effect Maximization (5)	School Closure: Effective Replacement (6)	School Closure: Typical Replacement (7)
				A. School Segregation			
Share in School with 90% Black/Hispanic Peers	0.46	0.47	0.54	0.76	0.76	0.45	0.45
Share in School with 90% Low Baseline Peers	0.00	0.08	0.06	0.09	0.08	0.00	0.00
				B. Choice and Assignment			
Average Rank	2.65	350.60	3.44	333.59	116.92	2.65	2.58
Attend School in Original Choice List	0.88	0.20	0.75	0.03	0.03	0.86	0.86
Share Assigned to Same School as in Status Quo	1.00	0.12	0.24	0.01	0.01	0.72	0.67
				C. Distance and Travel			
Travel Outside Home Borough	0.12	0.41	0.14	0.78	0.77	0.13	0.13
Share Travel to Borough Different than Status Quo	0.00	0.45	0.12	0.78	0.78	0.04	0.05
Average Distance (Miles)	3.08	5.88	3.45	9.46	9.36	3.21	3.26
Travel 1+ Miles	0.79	0.86	0.85	0.99	0.99	0.79	0.80
Travel 5+ Miles	0.18	0.47	0.21	0.78	0.77	0.19	0.19
Travel 10+ Miles	0.02	0.20	0.03	0.42	0.42	0.03	0.04

Notes: This table displays characteristics of assignments on segregation, choice and assignment, and distance and travel. Average rank in columns (1) and (3) are from choice lists. Average rank in columns (2), (4), and (5) are estimated via rank-order logit choice models.

Table 10—Characteristics of Students and Schools in Open and Closed Schools (Aligned Demand)

	Actual Closure		Typical Replacement	
	Open	Closed	Open	Closed
	(1)	(2)	(3)	(4)
		A. Student Characteristics		
% Minority	0.74	0.94	0.76	0.88
% Black	0.34	0.45	0.35	0.37
% Hispanic	0.41	0.49	0.41	0.51
% Subsidized Lunch	0.69	0.78	0.70	0.77
8th Grade Math	-0.03	-0.47	-0.07	-0.31
8th Grade ELA	-0.02	-0.40	-0.06	-0.30
% Above Median Math	0.47	0.24	0.45	0.32
Log(Census Tract Income)	10.72	10.53	10.69	10.66
% Brooklyn	0.33	0.28	0.35	0.14
% Manhattan	0.11	0.16	0.11	0.16
% Queens	0.28	0.15	0.24	0.46
% Bronx	0.21	0.40	0.24	0.24
% Staten Island	0.07	0.00	0.06	0.00
		B. School Characteristics		
9th Grade Enrollment	212	227	218	185
Survey Score (1-40)	26.51	26.35	26.53	26.18
Crimes Per Student	0.08	0.09	0.08	0.09
% of Teachers with Masters+	0.32	0.31	0.32	0.32
		C. Performance Levels		
Regents Math	0.02	-0.55	0.02	-0.60
HS Graduation Rate	0.64	0.44	0.62	0.55
College Attendance	0.48	0.30	0.46	0.39
		D. ATE		
Regents Math	0.03	-0.15	0.05	-0.36
HS Graduation Rate	0.01	-0.04	0.00	-0.01
College Attendance	0.00	-0.02	0.00	-0.01

Notes: This table contains information on students and schools following closures to match Aligned Demand and TEMA outcomes from the control function model. Columns (1) and (2) reports the same characteristics for schools closed between 2007-2017 in New York as of the 2006-07 school year.

Table 11—Correlates of Market Characteristics to Actual NYC Closures

	(1)	(2)	(3)	(4)
HS Graduation	-2.44			-1.80
	(0.38)			(0.42)
ATE		-1.56		-1.36
		(0.25)		(0.39)
Match		-0.53		-0.37
		(0.22)		(0.26)
Mean Utility			-0.18	0.10
			(0.17)	(0.35)
Capacity				-0.64
				(0.34)

Notes: This table displays coefficients from logit models predicting school closure. The explanatory variable in column (1) is the school's high school graduation rate. Column (2) uses posterior mean estimates of the schools average treatment effect and average realized match component for high school graduation. Column (3) uses the mean utility from a rank-ordered logit model predicting students' school rankings. Column (4) includes all predictors from columns (1)-(3). The sample includes 291 schools.

A Market Equilibration

Programs in NYC may assign admissions priorities based on applicant characteristics and/or rank applicants by applying a program specific set of criteria. This appendix explains how we predict priorities and ranks.

A.1 Predicting School Priorities

To predict priorities for each student-program combination, we analyze two data sources from New York's matching system:

1. Program Priority Files: Documentation showing how each program determines their priority levels for applicants

2. Applicant Data Files: Records containing characteristics of each applicant

Using these two inputs, we can calculate priority predictions for every possible pairing of applicants and programs in our sample.

The first file records five criteria:

1. District: Applicant lives in the same district as program,

2. Feeder: Applicant attends a school that is designated as feeder for the program,

3. Borough: Applicant lives in the same borough as program,

4. Language: Applicant speaks a certain language at home,

5. HS Zone: Applicant lives in the zone of the program.

The priority rules vary by program, with each school using its own set of criteria. For instance, Program X gives top priority to students who speak Spanish at home, while Program Y reserves its highest priority for students coming from School Z. These rules are documented in a program-level file where each row specifies the program, a criterion, its assigned priority level, and the specific condition that meets that criterion. Programs can list between one and five different criteria, and a single criterion may have multiple qualifying conditions. As an example, Program X might assign Priority Level 2 to students from any of three feeder schools: W, Y, and Z, with all three schools listed as separate conditions under the same criterion.

The applicant data file contains information about the students' characteristics corresponding to the fields used in program priority rules. For each program-applicant pair, we cross-reference these two files to identify which of the five possible criteria the applicant satisfies at that program. When an applicant meets the conditions for multiple priority levels at a program, they are assigned their highest qualifying priority level.

We evaluate our priority predictions by comparing them to the actual priorities that programs assigned to applicants. Our model demonstrates high accuracy across all years studied (2003-2004

to 2006-2007), correctly predicting 95% of priority assignments (shown in Table A1). For 2006-2007, the specific year used in our counterfactual analysis, the accuracy rate is even higher at 96.7%.

A.2 Predicting Ranks

For Screened and Educational Option programs, an applicant may list the program as one of their choices but still not receive a ranking from that program. This occurs frequently. 62.6% of all applicant-program pairs where the student ranked the program have no corresponding program ranking for that student. Among these cases of missing rankings, only 4.4% of applicants end up being assigned to the program, likely due to unfilled seats at undersubscribed programs. Given this pattern, we interpret missing rankings as indicating either student ineligibility or very low standing in the program's evaluation process.

We use two input files to predict rankings: First, a file containing applicants' program preferences and any rankings they received from Screened or Educational Option programs; second, a file containing applicant characteristics.

We predict applicant-program rankings in two steps. In the first step, we determine eligibility at each program. For applicants who included a Screened or Educational Option program on their choice list, eligibility is straightforward: they are eligible if the program assigned them a rank, and ineligible if not. For all other applicant-program pairs, we run a logit model using academic and demographic characteristics, home language, and missing data indicators. We classify an applicant as eligible if their predicted probability is ≥ 0.5. We default to marking all applicants eligible if the logit model fails to converge (due to small sample size, no variation in outcomes, etc.). In the second step, we predict ranks for eligible applicants. We first categorize programs as either "batched" (≤ 5 distinct ranks used) or "unbatched" (> 5 distinct ranks). For batched programs that use only two ranks, we run a logit model to predict the probability of receiving the top rank, using the same characteristics as the first step. We assign the top rank to applicants with predicted probability ≥ 0.5, and default to assigning the top rank to all applicants if the model fails to run.

For batched programs using 3-5 distinct ranks, we run an ordered logit model using academic and demographic characteristics, home language, and missing data indicators to predict the probability of each possible rank. Each applicant is then assigned the rank for which they have the highest predicted probability. If the ordered logit fails to run, all applicants receive the highest rank. For unbatched programs (which may assign the same rank to multiple students), we first create a strict ranking by breaking ties randomly: we assign each applicant a random number from a uniform distribution, then sort applicants first by their observed rank and then by this random number. We use these strict rankings to create quintiles, placing each applicant into one of five rank bins. We then follow the same ordered logit process as with batched programs, assigning each applicant to the rank quintile where they have the highest predicted probability. If the model fails to run, all applicants are assigned to the top quintile. Finally, if an applicant has an observed rank at a program, use the observed rank to place them into their observed rank quintile.

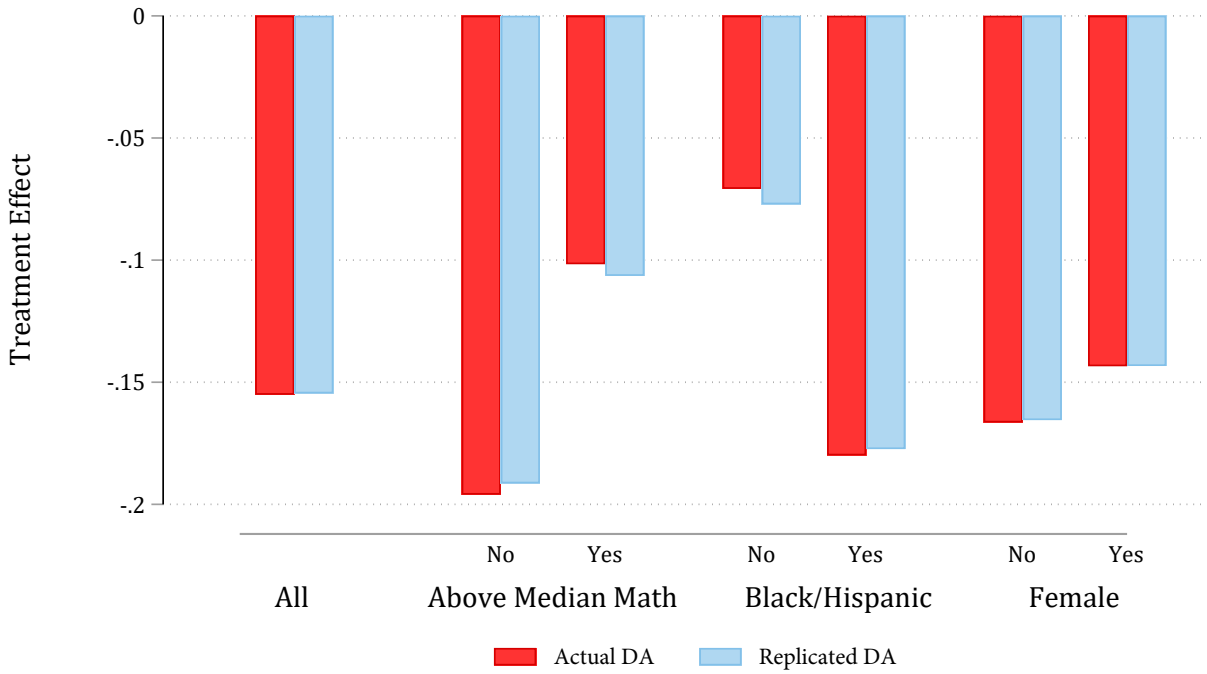
To evaluate our eligibility prediction accuracy, we focus on the subset of applicants who actually

applied to programs. We rerun our eligibility logit model on this group and generate predictions following our established method. We then construct a confusion matrix comparing predicted versus actual eligibility, which shows the proportions of correct predictions (true eligibles and true ineligibles) on the diagonal and incorrect predictions (false positives and false negatives) on the off-diagonal.

Finally, the 2006-07 version of New York's mechanism used a Supplementary Round to assign students who did not receive an assignment from the main round (see, e.g., These students were asked to submit new rankings over programs with remaining capacity (Pathak and Sethuraman (2010) describes more details on this round.) Rather than simulating this two-step assignment process, we extend the rank order list of all applicants using the rank-order logit choice models in equation (6). This extrapolation allows us to simulate the matching process with a single run of the student-proposing deferred acceptance algorithm.

Our replication analysis successfully matches 54% of student assignments made under the Status Quo system, as shown in Table A3. Figure A1 compares the predicted educational outcomes between actual assignments and our replicated assignments. This comparison examines outcomes for all students, as well as breakdowns by race, initial academic performance, and gender. The strong similarity between actual and replicated outcomes suggests that any discrepancies between our replication and the Status Quo system are unlikely to affect our broader comparison of market clearing versus resource augmentation policies.

Figure A.1: Regents Math Effects for Actual and Replicated DA



Notes: This figure compares the estimated treatment effect from actual DA in 2006 to the replicated version of DA. The steps involved in the replication are described in Appendix A.

Table A1—Predicting Student Eligbility Levels

		Predicted Eligiblity		
		No	Yes	Total
Actual Eligibility	No	0.90	0.10	177,296
	Yes	0.16	0.84	120,586
	Total	178,213	119,669	297,882

Notes: The table above measures the frequency in which a logit assigns students as eligible to a screened program or the screened component of an ed-opt program. The logit regresses a dummy of a populated rank variable on demographic and baseline characteristics. Logits are fit on 2006 students and programs, running one logit per program. Cell entries represent the average number of students correctly predicted.

Table A2—Accuracy of Predicted Priorities

	All	2004	2005	2006
Percent Correct	0.96	0.96	0.95	0.97
Number of Applications	1,400,342	484,134	464,722	451,486

Notes: The table displays the accuracy of predicting priorities of students who applied to programs across 2004-2006.To test accuracy we used administrative data from NYCDOE that listed each program's criteria for priority levels. Next we matched student demographic information to the appropriate priority they would be imputed if applied. Lastly, we compared actual priority level vs. predicted priority level. Accuracy is the average percent of students predicted correctly. Observations represent total student/application observations that we tested accuracy with.

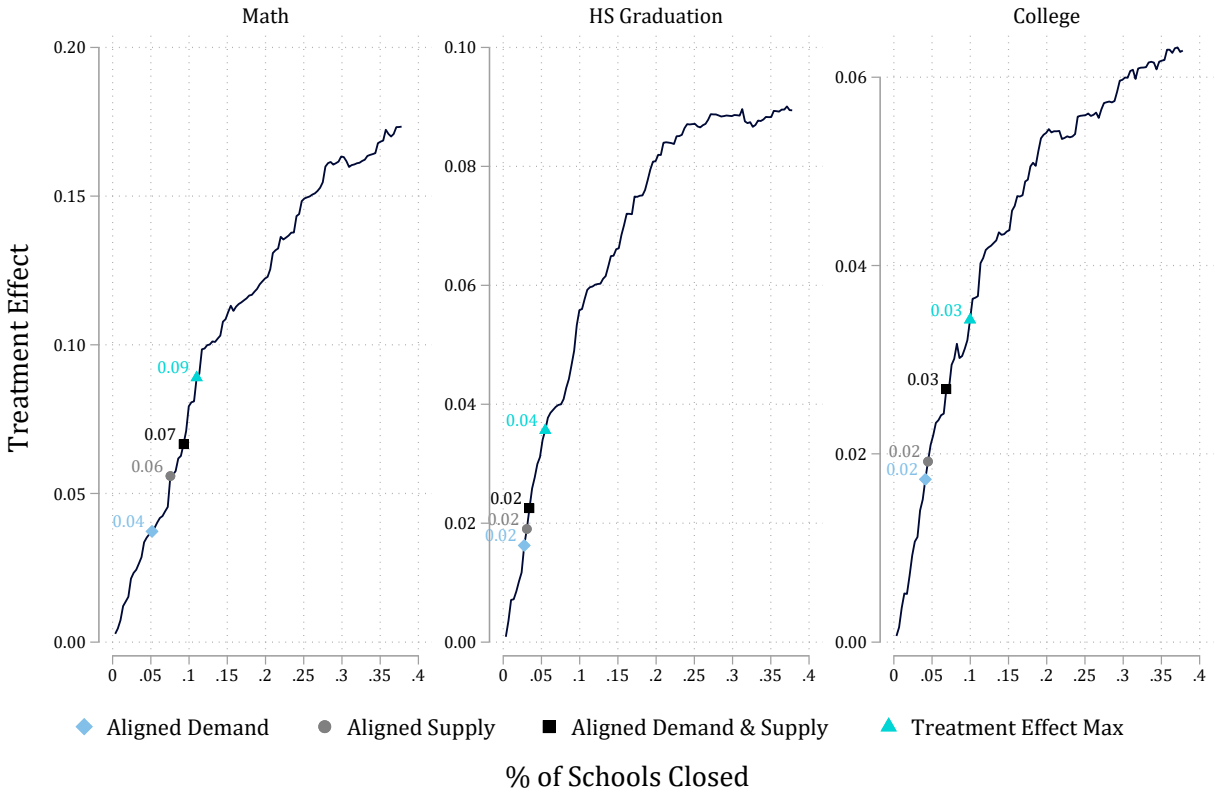
Table A3—Differences between DA and Status Quo

	DA	Status Quo
	(1)	(2)
% Identical Program Offered		0.54
% Offered Program in Original Choice List		0.86
Average Rank of Program Offered	2.45	2.61
N	62,231	

Notes: This table displays summary statisics on actual DA results versus status quo model. Status quo takes a student's original choice list and appends expected choices to create an extended choice list. Status quo also uses predicted student priority levels and ranks.

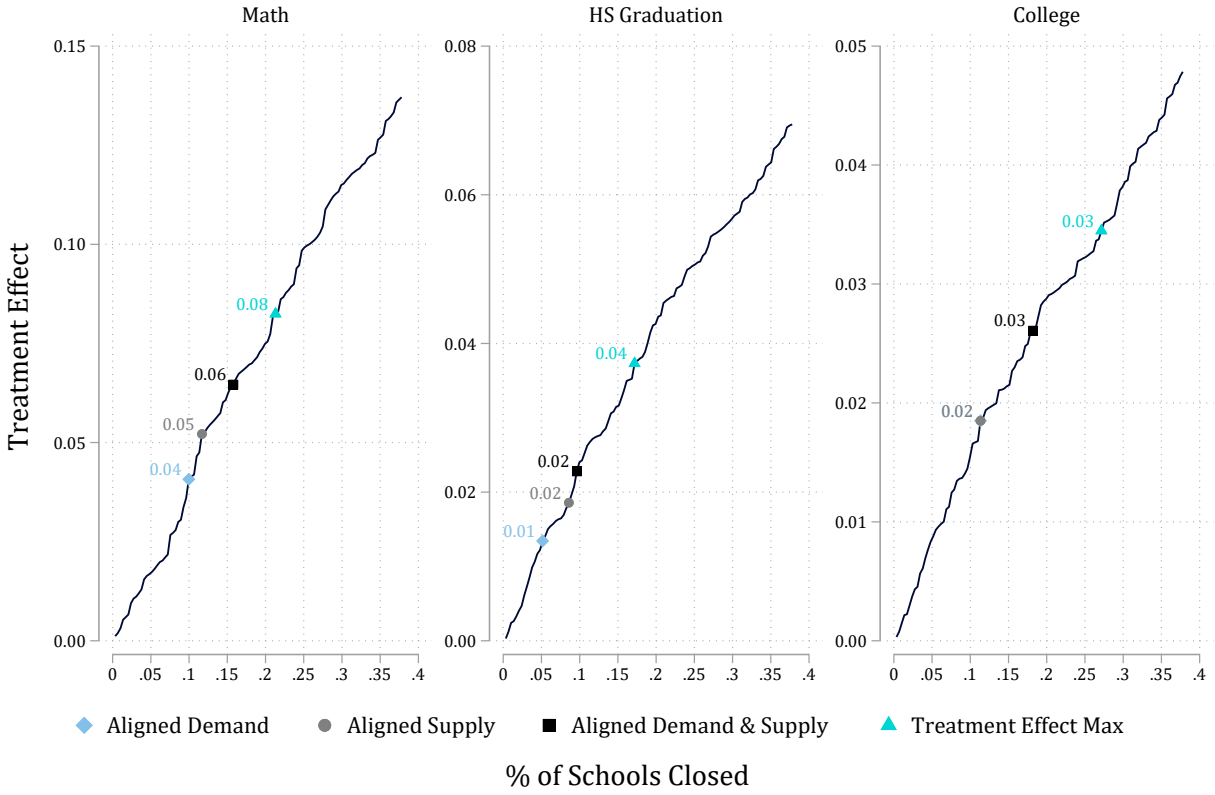
B Key Exhibits with OLS VAM Estimates

Figure B.1: Treatment Effects (OLS VAM) Following Closure Under Effective Replacement



Notes: This figure shows the fraction of schools that need to be closed to equal to aggregate treatment effect under market-clearing counterfactuals using the OLS VAM estimates of school-value added. 1% of schools is approximately equal to three schools.

Figure B.2: Treatment Effects (OLS VAM) Following Closure Under Typical Replacement



Notes: This figure shows the fraction of schools that need to be closed to equal to aggregate treatment effect under market-clearing counterfactuals using the OLS VAM estimates of school-value added. 1% of schools is approximately equal to three schools.

Table B1—Distributions of School Value-Added Parameters for HS Graduation

	Intercept	Above-median Math	Black/Hispanic	Female	Preference coefficient (ψ_j)
	(1)	(2)	(3)	(4)	(5)
			A. Control function		
Mean	0	0.059	0.006	0.057	-0.001
	—	(0.003)	(0.009)	(0.003)	(0.000)
SD	0.075	0.054	0.055	0.027	0.005
	(0.002)	(0.004)	(0.020)	(0.006)	(0.000)
Correlation: Above-median Math	-0.721				
	(0.072)				
Black/Hispanic	0.146	-0.225			
	(0.187)	(0.213)			
Female	-0.433	0.406	-0.475		
	(0.149)	(0.208)	(0.410)		
Preference coefficient (ψ_j)	0.308	-0.469	0.025	-0.116	—
	(0.068)	(0.086)	(0.158)	(0.153)	
			B. OLS VAM		
Mean	0	0.055	-0.008	0.055	—
	—	(0.003)	(0.006)	(0.003)	
SD	0.079	0.056	0.050	0.029	—
	(0.002)	(0.004)	(0.020)	(0.006)	
Correlation: Above-median Math	-0.776				
	(0.063)				
Black/Hispanic	0.176	-0.387			
	(0.203)	(0.253)			
Female	-0.468	0.411	-0.419	—	—
	(0.129)	(0.188)	(0.425)		

Notes: This table reports estimates of features of the distributions of school value-added parameters for HS graduation outcome. School-specific estimates come from a regression of school indicators interacted with a constant (intercept), an indicator for an above-median baseline math score, a Black/Hispanic indicator and a female indicator. The regression also controls for main effects of baseline math and reading scores, borough indicators, a subsidized lunch indicator, and log median income in a student's census tract. Estimated standard deviations and correlations are derived from bias-corrected estimates of the variances and covariances of the interaction coefficients across schools, with standard errors computed by the delta method.

Table B2—Distributions of School Value-Added Parameters for College

	Intercept	Above-median Math	Black/Hispanic	Female	Preference coefficient (ψ_j)
	(1)	(2)	(3)	(4)	(5)
			A. Control function		
Mean	0	0.084	-0.001	0.070	-0.002
	—	(0.004)	(0.011)	(0.003)	(0.001)
SD	0.069	0.048	0.084	0.031	0.005
	(0.003)	(0.007)	(0.016)	(0.008)	(0.000)
Correlation: Above-median Math	-0.527				
	(0.121)				
Black/Hispanic	0.030	-0.180			
	(0.141)	(0.217)			
Female	-0.200	-0.031	-0.247		
	(0.153)	(0.256)	(0.334)		
Preference coefficient (ψ_j)	0.337	-0.223	0.238	0.215	—
	(0.094)	(0.132)	(0.154)	(0.173)	
			B. OLS VAM		
Mean	0	0.006	-0.023	0.067	—
	—	(0.000)	(0.007)	(0.000)	
SD	0.078	0.050	0.078	0.032	—
	(0.003)	(0.007)	(0.017)	(0.008)	
Correlation: Above-median Math	-0.483				
	(0.107)				
Black/Hispanic	0.150	-0.254			
	(0.148)	(0.226)			
Female	-0.162	0.018	-0.221	—	—
	(0.133)	(0.240)	(0.347)		

Notes: This table reports estimates of features of the distributions of school value-added parameters for college outcome. School-specific estimates come from a regression of school indicators interacted with a constant (intercept), an indicator for an above-median baseline math score, a Black/Hispanic indicator and a female indicator. The regression also controls for main effects of baseline math and reading scores, borough indicators, a subsidized lunch indicator, and log median income in a student's census tract. Estimated standard deviations and correlations are derived from bias-corrected estimates of the variances and covariances of the interaction coefficients across schools, with standard errors computed by the delta method.

Table B3—Descriptive Statistics for HS Graduation Value Added

		Realized		Potential	
		Raw	Shrunk	Raw	Shrunk
		(1)	(2)	(3)	(4)
		A. Control function			
Treatment Effect					
	Mean	-0.03	-0.04	0.00	0.00
	SD	0.08	0.08	0.11	0.08
	Range	[-0.42,0.43]	[-0.26,0.32]	[-0.46,0.48]	[-0.26,0.34]
Average Treatment Effect					
	Mean	-0.03	-0.03	0.00	0.00
	SD	0.07	0.07	0.08	0.07
	Range	[-0.22,0.29]	[-0.20,0.23]	[-0.22,0.29]	[-0.20,0.23]
Match Effect					
	Mean	0.00	0.00	0.00	0.00
	SD	0.05	0.04	0.07	0.03
	Range	[-0.36,0.32]	[-0.23,0.20]	[-0.41,0.32]	[-0.26,0.20]
		B. OLS VAM			
Treatment Effect					
	Mean	-0.04	-0.04	0.00	0.00
	SD	0.09	0.08	0.11	0.08
	Range	[-0.46,0.42]	[-0.25,0.30]	[-0.46,0.45]	[-0.25,0.30]
Average Treatment Effect					
	Mean	-0.03	-0.03	0.00	0.00
	SD	0.08	0.07	0.09	0.08
	Range	[-0.20,0.27]	[-0.18,0.23]	[-0.20,0.27]	[-0.18,0.23]
Match Effect					
	Mean	0.00	0.00	0.00	0.00
	SD	0.05	0.03	0.07	0.03
	Range	[-0.40,0.32]	[-0.15,0.11]	[-0.40,0.34]	[-0.15,0.14]

Notes: The table displays the mean, standard deviation, and range of treatment effects, average treatment effects, and match effects for HS Graduation from models with three interacted covariates (Black/Hispanic indicator, female indicator, and above-median baseline math). Shrunk estimates are Empirical Bayes estimates described in the text. Estimates are generated using student data from 2003-06, and statistics are displayed for 2006 applicants. The number of observations for realized is 62,231, and the number for potential is 18,109,220.

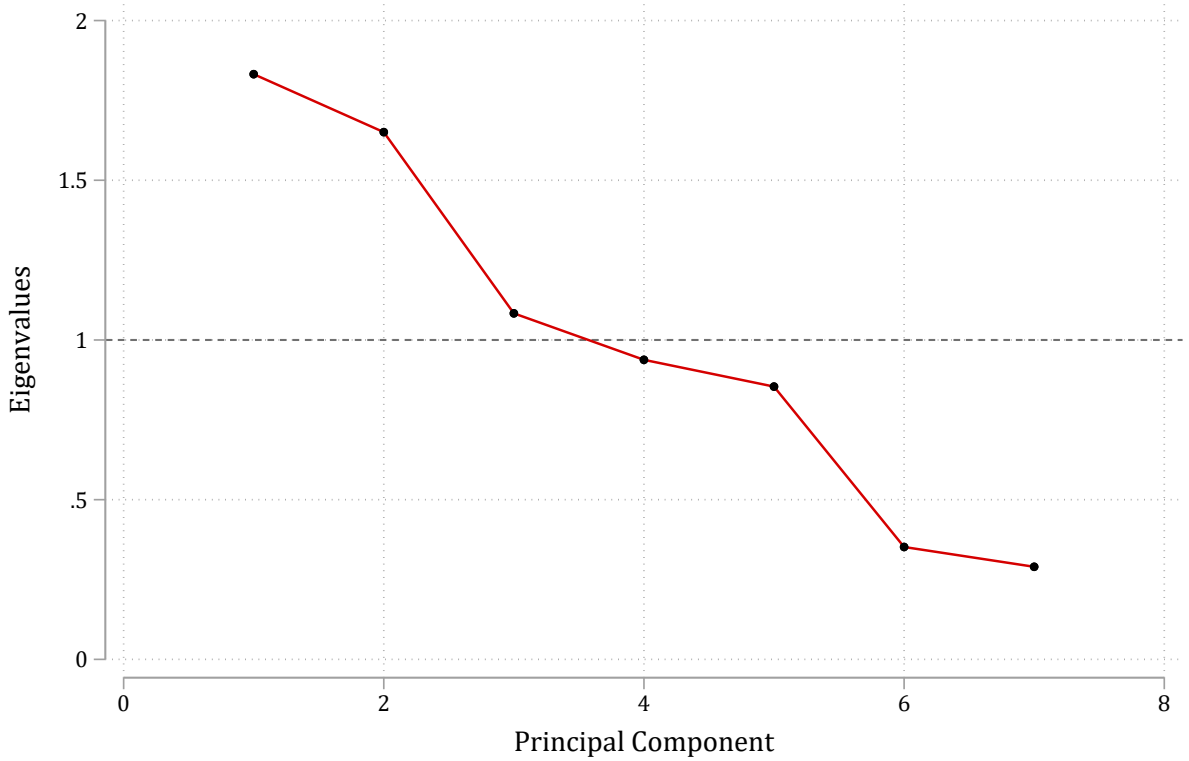
Table B4—Descriptive Statistics for College Value Added

		Realized		Potential	
		Raw	Shrunk	Raw	Shrunk
		(1)	(2)	(3)	(4)
		A. Control function			
Treatment Effect					
	Mean	-0.02	-0.02	0.00	0.00
	SD	0.08	0.07	0.11	0.08
	Range	[-0.50,0.50]	[-0.25,0.40]	[-0.61,0.53]	[-0.40,0.44]
Average Treatment Effect					
	Mean	-0.02	-0.02	0.00	0.00
	SD	0.07	0.06	0.08	0.07
	Range	[-0.18,0.33]	[-0.14,0.28]	[-0.18,0.33]	[-0.14,0.28]
Match Effect					
	Mean	0.00	0.00	0.00	0.00
	SD	0.06	0.04	0.08	0.04
	Range	[-0.58,0.40]	[-0.21,0.21]	[-0.69,0.42]	[-0.32,0.30]
		B. OLS VAM			
Treatment Effect					
	Mean	-0.02	-0.02	0.00	0.00
	SD	0.08	0.07	0.12	0.08
	Range	[-0.45,0.55]	[-0.31,0.40]	[-0.54,0.55]	[-0.46,0.40]
Average Treatment Effect					
	Mean	-0.02	-0.01	0.00	0.00
	SD	0.08	0.07	0.09	0.07
	Range	[-0.22,0.35]	[-0.18,0.28]	[-0.22,0.35]	[-0.18,0.28]
Match Effect					
	Mean	0.00	0.00	0.00	0.00
	SD	0.05	0.03	0.08	0.03
	Range	[-0.58,0.42]	[-0.22,0.26]	[-0.67,0.43]	[-0.31,0.32]

Notes: The table displays the mean, standard deviation, and range of treatment effects, average treatment effects, and match effects for College from models with three interacted covariates (Black/Hispanic indicator, female indicator, and above-median baseline math). Shrunk estimates are Empirical Bayes estimates described in the text. Estimates are generated using student data from 2003-06, and statistics are displayed for 2006 applicants. The number of observations for realized is 62,231, and the number for potential is 18,109,220.

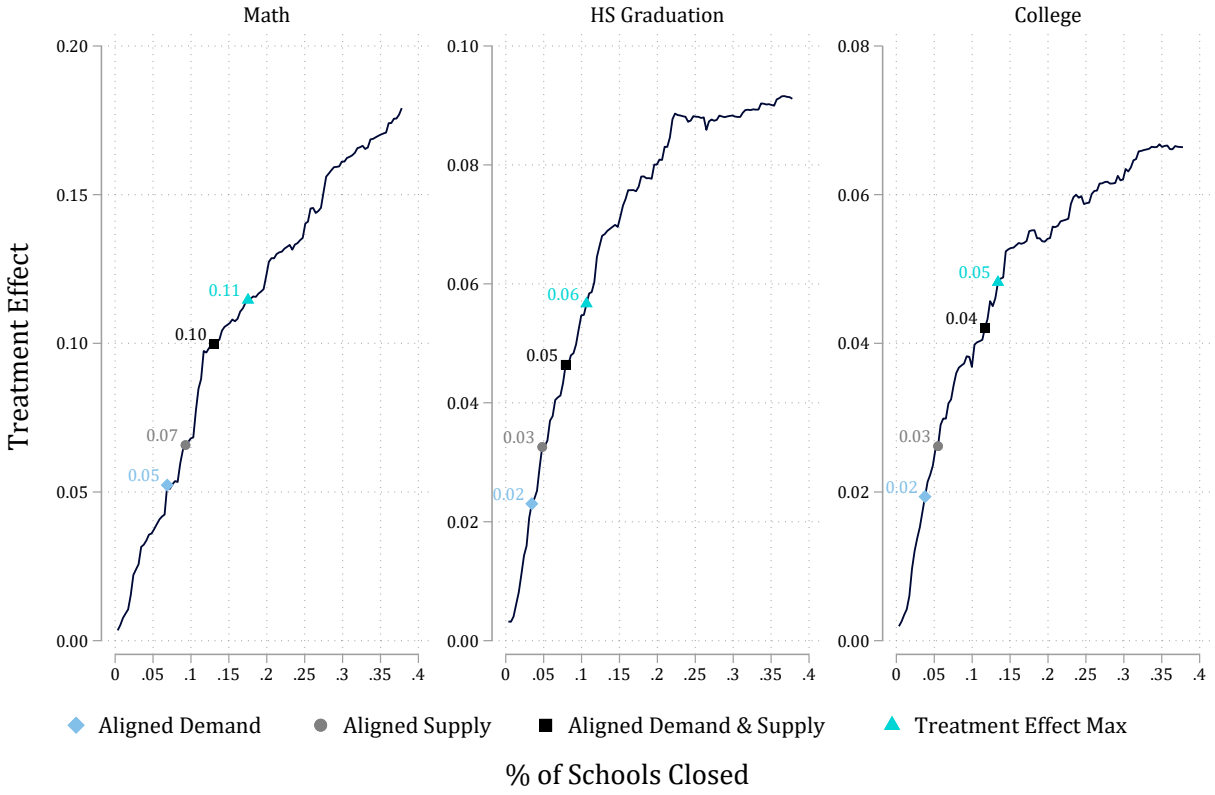
C Key Exhibits with PCA Estimates

Figure C.1: Scree Plot of Eigenvalues



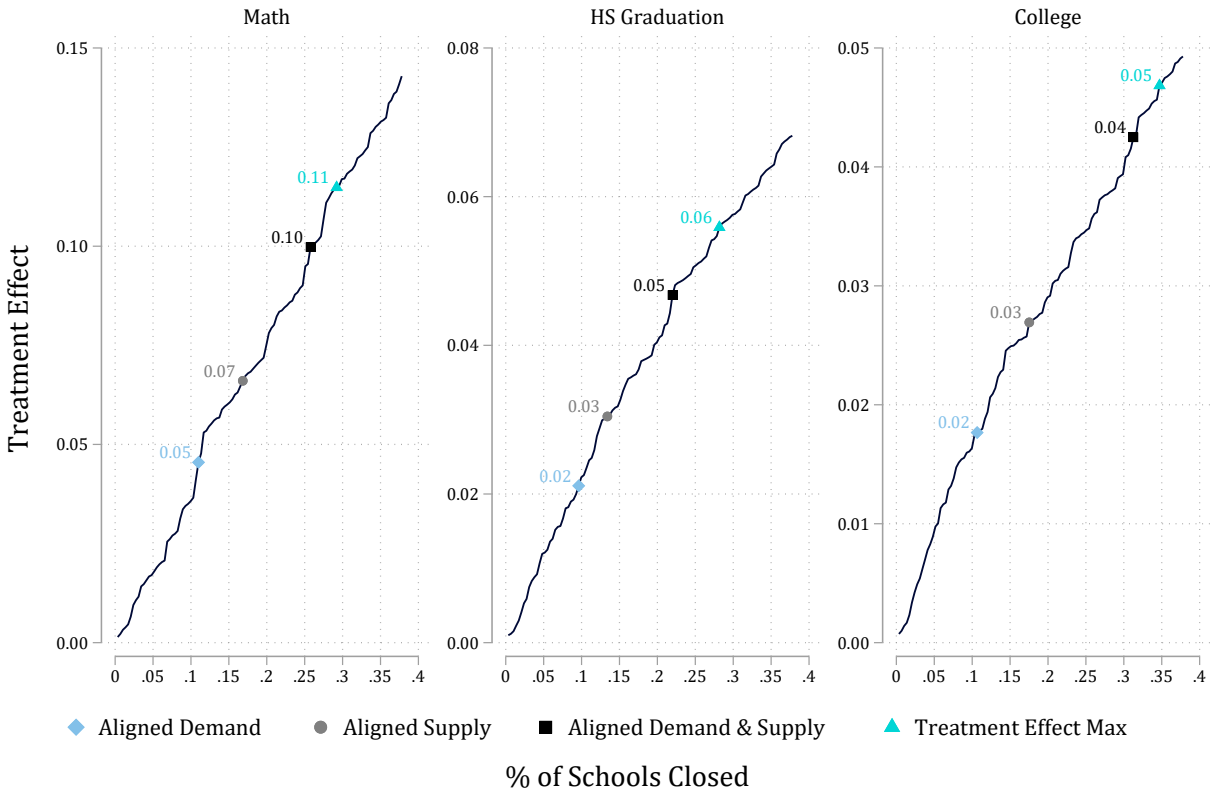
Notes: This table reports the scree plot of eigenvalues, used to choose PCA factors.

Figure C.2: Treatment Effects (PCA) Following Closure Effective Replacement



Notes: This figure shows the fraction of schools that need to be closed to equal to aggregate treatment effect under market-clearing counterfactuals using the PCA estimates of school-value added. 1% of schools is approximately equal to three schools.

Figure C.3: Treatment Effects (PCA) Following Closure Typical Replacement



Notes: This figure shows the fraction of schools that need to be closed to equal to aggregate treatment effect under market-clearing counterfactuals using the PCA estimates of school-value added. 1% of schools is approximately equal to three schools.

Table C1—Distributions of School Value-Added Parameters for Regents Math for Principal Component Factors

		Intercept	P1	P2	P3
		(1)	(2)	(3)	(4)
Mean		0	0.681	0.137	0.095
		—	(0.004)	(0.003)	(0.003)
SD		0.287	0.099	0.032	0.026
		(0.003)	(0.007)	(0.005)	(0.008)
Correlation:	P1	-0.073			
		(0.073)			
	P2	-0.008	0.693		
		(0.093)	(0.203)		
	P3	0.077	0.480	0.349	—
		(0.143)	(0.305)	(0.366)	

Notes: This table reports estimates of features of the distributions of school value-added parameters for Regents Math outcome. School-specific estimates come from a regression of school indicators interacted with a constant (intercept), and three principal component factors. The regression also controls for main effects of baseline math and reading scores, borough indicators, a subsidized lunch indicator, and log median income in a student's census tract. Estimated standard deviations and correlations are derived from bias-corrected estimates of the variances and covariances of the interaction coefficients across schools, with standard errors computed by the delta method.

Table C2—Descriptive Statistics for Regents Math Value Added for Principal Component Factors

		Realized		Potential	
		Raw	Shrunk	Raw	Shrunk
		(1)	(2)	(3)	(4)
Treatment Effect					
	Mean	0.00	0.00	0.00	0.00
	SD	0.27	0.26	0.32	0.29
	Range	[-1.32,1.41]	[-0.9,1.1]	[-2.36,2.35]	[-1.3,1.76]
Average Treatment Effect					
	Mean	0.02	0.02	0.00	0.00
	SD	0.27	0.26	0.29	0.28
	Range	[-0.75,1.02]	[-0.68,0.99]	[-0.75,1.02]	[-0.68,0.99]
Match Effect					
	Mean	-0.02	-0.02	0.00	0.00
	SD	0.12	0.09	0.13	0.08
	Range	[-1.13,1.04]	[-0.95,0.67]	[-1.76,1.98]	[-1.25,1.19]

Notes: The table displays the mean, standard deviation, and range of treatment effects, average treatment effects, and match effects for Regents Math from models with three interacted principal component factors. Shrunk estimates are Empirical Bayes estimates described in the text. Estimates are generated using student data from 2003-06, and statistics are displayed for 2006 applicants. The number of observations for realized is 62,231, and the number for potential is 18,109,220.

Table C3—Potential Gains from Counterfactual Assignments Relative to Status Quo for Principal Component Factors

	Aligned Demand	Aligned Supply	Aligned Demand and Supply	Treatment Effect Maximization
	(1)	(2)	(3)	(4)
Math	0.045	0.065	0.100	0.114
HS Graduation	0.021	0.030	0.045	0.056
College	0.018	0.026	0.042	0.046

Notes: This table compares treatment effects in the counterfactual models compared to status quo. Aligned Demand has students rank schools based on effectiveness and leaves school-side rankings unchanged. Aligned Supply has schools rank students based on effectiveness and leaves student-side rankings unchanged. Aligned Demand and Supply is the outcome when both students and schools rank each other based on maximizing treatment effects. Treatment Effect Maximization is a linear program that maximizes treatment effects. The total number of students is 62,231.