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Who Gets What in Education: Can School Matching Improve Student Achievement?

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Who Gets What in Education: Can School Matching Improve Student Achievement?*

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Abstract

We examine two approaches to improving urban school systems: changing who gets to go to existing schools (reallocation) and restructuring school portfolios through closures and reconstitution (resource augmentation). Using data from New York City high schools, we estimate models of school effects allowing for both vertical school quality differences and horizontal student-specific match effects. While sophisticated reallocation 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 replacement of 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 targeting closure decisions. Our findings indicate that capacity constraints, rather than poor school matching, primarily drive educational inequality.

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1 Introduction

Urban school districts use two main groups of policies as part of reform efforts: changing who goes where, and reinvesting in schools by reconstituting them or closing them. We'll call these **reallocation** and **resource augmentation policies**, respectively.[1] Reallocation policies are about matchmaking: these reforms reshuffle students across a fixed set of schools. Examples include changes to school assignment processes, informational interventions, and moves by districts to drop test scores for selective school admissions. 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 schools will close and which will remain open.[2] Following Roth (2016), reallocation policies affect "who gets what," while resource augmentation policies determine "the what."

This paper contrasts the prospects of reallocation and resource augmentation policies for improving student outcomes. Specifically, we compare the performance of best-case reallocation policies with equally-effective resource augmentation policies. Our analysis uses 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). Reallocation 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 admission criteria in 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 student/school match effects. Without match effects, reallocating students among existing schools is zero-sum: some students gain what others lose. With match effects, reallocation policies can result in placing students in schools where they stand to benefit the most. We therefore begin our analysis

[1]The terminology "resource augmentation" 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 Akbarpour et al. (2023).

[2]Dee (2023) analyzes enrollment declines following COVID-19. Several districts, including Boston, Chicago, and Rochester, 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.

by estimating models of school quality 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 variance of school effects on the vertical dimension is roughly ten times the variance on the horizontal dimension.

These estimates allow us to simulate a set of reallocation 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 and 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 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 reallocation can generate modest improvements in student outcomes, but these gains 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 reallocation 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 2008 and 2017, New York closed 42 high schools from our analysis sample. The schools chosen for closure overlap considerably with those that would have been selected based on our estimates of school effectiveness. Schools with lower graduation rates in New York were more likely to face closure. We find that graduation rate levels are strongly related to causal effects on graduation among New York City high schools, so decisions based on graduation rates select many of the same schools as our more complex measures of school effects. This suggests that sophisticated value-added models may not be necessary to identify the worst-performing schools for closure decisions. Finally, we show that closure decisions and educational effectiveness are weakly correlated with school popularity, suggesting central planners with access to graduation data may be better equipped than parents to identify failing schools. Taken together, our findings indicate that resource augmentation is a more promising means of improving student outcomes than reallocation both in principle – because variation in school quality is primarily vertical rather than horizontal – and in practice – as evidenced by NYC's effective targeting of low-quality schools for closure.

The rest of the paper is organized as follows. Section 2 provides additional details on reallocation 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 simulated effects of reallocation and resource augmentation policies. Section 6 presents a case study of New York City's closure decisions to assess the feasibility of effective resource augmentation in practice. Section 7 concludes and offers some directions for future research.

2 Examples of Reallocation and Resource Augmentation

2.1 Reallocation Policies

Reallocation policies are pervasive in large US school districts. This can be seen in Table 1, which describes three categories of reallocation 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 place-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 switched to a lottery system 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. Such information campaigns aim to direct students to higher-quality or better-matched schools, and therefore fall under the reallocation umbrella. 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

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

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 Policies

Resource augmentation policies are also common in large urban US school districts. In the early 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 closures, restarts, or phase-outs.

More recently, closures have often been motivated by declining enrollment rather than federal policy efforts. 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. Declining public school enrollment has been particularly pronounced in urban areas as seen in Figure 2, with cities like Los Angeles, Chicago, and Philadelphia experiencing over 40% declines relative to their peak enrollment numbers. 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 pressure has led to a moratorium on further closures in Chicago until 2027 (Mohamad, 2024; Vevea, 2024).

Table 2 presents statistics on school closures and enrollment declines in the largest 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.

Other examples of resource augmentation can be seen in the growth of new school models. Between 2010 and 2021, the share of US public school students attending charter schools grew from four percent to seven percent. A large literature studies the effects of charter schools (see, e.g., Abdulkadiroğlu et al. 2011; Angrist, Pathak and Walters 2013; 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 efforts 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; Cohodes, Setren and Walters, 2021). In 2016, voters in Massachusetts rejected a referendum proposing to increase this cap (Scharfenberg, 2016; Walters, 2018).

3 Conceptual Framework

We compare effects of reallocation and resource augmentation policies with a potential outcomes model building on the framework of Abdulkadiroğlu et al. (2020*a*). Consider a district with N students, each of whom will enroll 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 $S_i \in \{1, ..., J\}$, and her observed outcome is $Y_i = \sum_j S_{ij}Y_{ij}$, where the indicator variable $S_{ij} = 1\{S_i = j\}$ 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

$$\bar{Y} = \frac{1}{N}\sum_i Y_i$$

$$= \frac{1}{N}\sum_i\sum_j S_{ij}\left[A_i + ATE_j + M_{ij}\right]$$
$$= \frac{1}{N}\sum_i A_i + \sum_j w_j ATE_j + \frac{1}{N}\sum_i\sum_j S_{ij}M_{ij}, \tag{3}$$

where $w_j = N^{-1}\sum_i S_{ij}$ is the share of students attending school j and the last equality uses the fact that $\sum_j S_{ij} = 1$ 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, $\sum_j w_j ATE_j$, 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 reallocation and resource-augmentation policies affect aggregate achievement. Reallocation policies seek to improve outcomes by re-sorting 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 $\sum_j S_{ij}M_{ij}$. Reallocation policies target realized match effects, as reflected in the third term of equation (3). Can we change the configuration of S_{ij}'s to increase average match quality, holding fixed enrollment shares w_j? In contrast, resource-augmentation policies seek to increase educational outcomes by changing enrollment shares or school-specific parameters. For example, a school closure policy can be viewed as setting the enrollment share w_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, following methods developed in Abdulkadiroğlu et al. (2020*a*). The first is an OLS *value-added model* (VAM) approach based on the following selection-on-observables assumption:

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

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 S_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, linear terms in 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

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

where $E[e_i|X_{i1}, X_{i2}, S_{i1}, ..., S_{iJ}] = 0$. We estimate equation (5) by OLS, which delivers unbiased estimates of causal value-added parameters if condition (4) 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 (and preferred) strategy leverages information on student preferences to relax selection-on-observables and allow for match effects on unobserved dimensions. Specifically, we estimate two-step *rank-ordered control function* models that use students' rank-ordered preference lists submitted to the NYC match (described in more detail below) 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 the same list of observed characteristics included in the OLS VAM model, 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. We add distance to the model for student preferences because previous work shows it is a powerful predictor of school choices (Hastings, Kane and Staiger, 2009; Laverde, 2024). Student i's rank-ordered list is $R_i = (R_{i1}, ..., R_{i\ell(i)})'$, where $R_{i1} = \arg\max_j U_{ij}$, $R_{ik} = \arg\max_{j \notin \{R_{i1},...,R_{i(k-1)}\}} U_{ij}$ for $k > 1$, and $\ell(i)$ is the length of this student's list. 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:

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

where vectors $D_i = (D_{i1}, ..., D_{iJ})'$ and $\eta_i = (\eta_{i1}, ..., \eta_{iJ})'$ collect distances and unobserved preferences for all schools. 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 the 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:

$$Y_i = \sum_j \{S_{ij}[\alpha_j + X'_{1i}\beta_j] + \lambda_j(X_i, D_i, R_i)[\psi_j + S_{ij}\varphi]\} + X'_{2i}\gamma + D'_i\kappa + u_i, \tag{8}$$

with $E[u_i|X_i, D_i, R_i, S_{i1}, ..., S_{iJ}] = 0$. 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 function approach proposed by Dubin and McFadden (1984). 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 (Kline and Santos, 2012).

To understand the intuition underlying this control function procedure, note that variation in the control functions conditional on (X_i, D_i, S_i) comes from the fact that not all students attend their most-preferred schools. As a result of differences in priorities, random tie-breaking, and non-compliance with school assignments, rankings R_i will differ for observably-similar students attending the same school. Comparisons of such students reveal the relationship between unobserved school preferences and outcomes. The assumption that motivates this strategy is similar to the "self-revelation" model proposed by Dale and Krueger (2002; 2011). Dale and Krueger assume that students' college application portfolios capture all selection into college enrollment, so conditioning on these portfolios reveals causal effects of college attendance. Similarly, we assume that students reveal their unobserved types through their rank-ordered preference rankings, so that controlling for these rankings eliminates any remaining selection bias in OLS VAM estimates.

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 vector 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 the 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.[5] 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 these hyperparameters as follows:

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

where $\hat{\Omega}_j$ is an estimate of Ω_j computed via bootstrap. 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, $\theta_j^* = (\alpha_j^*, \beta_j^{*'}, \psi_j^*)'$ 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 describe a scenario 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:

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

where $S_{ij}(p)$ is 1 if i attends school j under policy p and zero otherwise. We will focus on $\bar{Y}(p) -$

[5]See Walters (2024) for further discussion of empirical Bayes methods for estimating school quality.

$\bar{Y}(p_0)$ where p_0 is the policy status quo. In this case, mean student ability A_i differences out:[6]

$$\bar{Y}(p) - \bar{Y}(p_0) = \frac{1}{N} \sum_i \sum_j \left(S_{ij}(p) - S_{ij}(p_0)\right)\left(ATE_j + M_{ij}\right). \tag{11}$$

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

As noted above, students are assigned New York City high schools with a centralized matching process based on the student-proposing deferred acceptance (DA) algorithm (Gale and Shapley, 1962; Abdulkadiroğlu, Pathak and Roth, 2009). We build a simulation of this system to clear the market under counterfactual assumptions about preferences and priorities. Every year in New York, about 80,000 eighth graders submit rank-ordered lists of up to 12 academic programs at roughly 400 high schools. 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 and 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, including those they did not rank in the status quo. 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 3.1. Appendix A provides additional details on the simulation procedure and underlying assumptions.

[6]Student ability A_i includes the average intercept μ_α and average slope μ_β. We adopt the convention that these averages equal means of school-specific intercepts α_j and slopes β_j under the status quo, so that A_i is invariant to policy changes that may change school-specific parameters.

3.3 Threats to Validity of Counterfactuals

Our framework treats value-added parameters θ_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, our counterfactual results are robust to certain forms of such forces. Under a standard linear-in-means peer effect specification in which peer effects are proportional to mean peer characteristics, peer effects are zero-sum and wash out in aggregate, so have no impact on the change in overall mean achievement in equation (11). 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 reallocation reforms.

4 Estimates of School Effects

4.1 Data and Samples

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

Our analysis focuses on four cohorts of students enrolled in New York City public schools in eighth 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 and observed high school outcomes, enrolled for ninth grade at one of 294 schools with at least 50 students with outcomes. The analysis sample restricts attention to the 2006 cohort and is used for simulating 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 thirds 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. 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. Some programs require auditions or portfolios, showcasing artistic or specialized talents. The bottom rows of Table 3 show that roughly one-third of schools employ unscreened admissions, one-third employ Educational Option admissions, one-quarter are screened, and ten percent admit students based on auditions.

4.2 The Distribution of School Quality

The first step of our empirical analysis estimates the parameters governing schools' causal effects on student outcomes. OLS VAM and control function estimates of the distributions of these school value-added parameters appear in Table 4. Specifically, this table reports the mean and variance parameters from equation (9), which characterize the distribution of school quality while accounting for the noise in school-specific estimates. The estimated standard deviation of Regents math average treatment effects across New York high schools equals 0.26σ in the control function model and 0.27σ 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.[7] 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 varies with student characteristics. In the control function model, the estimated cross-school standard deviation of 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 0.06σ). These match effect estimates are consistent with recent research on teacher value-added, which also shows evidence of heterogeneity in teacher effects across students (Aucejo et al., 2022; Graham et al., 2023; Bates et al., 2024; Laverde et al., 2025; Umosen, 2025; Eastmond et al., 2026). While our estimates show statistically significant match effects, 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). This finding will turn out to be a key driver of counterfactual results in the sections to follow.

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 for

[7] 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 SAT math.

girls. 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 is modest for Regents math scores. In the control function model, our estimate of the unobserved match effect parameter φ is statistically significant but small (estimate $= 0.008\sigma$, standard error $= 0.001\sigma$). This indicates that there is positive selection of students into schools based on unobserved test score gains, but the magnitude implies a modest effect on the observed distribution of student achievement.[8]

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), 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 quality by comparing estimates of school effectiveness for the observed allocation of students to schools vs. the broader list of potential assignments. The first two columns of Table 5 report Regents math treatment effect parameters from the control function model for the observed allocation, with raw unbiased estimates in column (1) and shrunk empirical Bayes posterior means in column (2). These estimates come from predicting treatment effects for the schools actually attended by the 61,879 students in the counterfactual analysis sample. Corresponding estimates from the roughly 18 million potential student/school combinations – including schools where students do not attend in the status quo – appear in columns (3) and (4).

These calculations highlight that dispersion of the vertical component of school effects is substantially larger than the horizontal component. Column (1) of Table 5 shows that the standard deviation of the estimated ATE is 0.24σ, while the standard deviation of the estimated match effect is 0.12σ. This ratio is 0.27σ to 0.15σ when considering all possible student-school matches in column (3). Empirical Bayes 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. Column (2) of Table 5 shows that the standard deviation of shrunk math ATEs for observed assignments equals 0.23σ, compared to the raw estimate of 0.24σ. The shrunk estimate of the standard deviation of the match effect is 0.07σ, compared to 0.12σ for

[8]To understand the magnitude of this estimate, note that our logit choice model implies that the average student enrolls at a school where her (de-meaned) unobserved taste η_{ij} is roughly 2.0. This implies that selection-on-gains increases scores by about $2 \times 0.008\sigma = 0.016\sigma$ on average.

the raw estimate. These estimates imply that the variance in posterior mean ATEs is more than ten times the variance of posterior mean match effects ($(0.232/0.071)^2 = 10.67$). A comparison of panels A and B shows similar patterns for 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 potential educational outcomes compare to the status quo? Both exercises tell us what the average student stands to gain from changing preferences or priorities to target school effectiveness, but these exercises do not account for congestion that would occur if all students or schools changed behavior simultaneously.

Figure 3 contrasts 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 0.08σ to about 0.64σ, indicating that many parents leave substantial school value-added on the table when choosing schools.[9] 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 0.014σ, 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 4 reports estimated effects of optimal school admissions which select students based on potential outcomes. For each school, we rank applicants in order of predicted match effects, 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 0.14σ for Regents math scores and roughly 7 to 8 percentage points for high school graduation and college attendance.

The analyses shown in Figure 3 and Figure 4 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

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

next analysis.

5 Reallocation vs. Resource Augmentation

5.1 Reallocation Policies

We consider several idealized reallocation policies that focus on how families choose schools (demand-side), how schools select students (supply-side), and the combination, taking capacity constraints into account. These three counterfactuals only change the ordinal information submitted to the school assignment mechanism, either student rankings, school rankings, or both. We also explore a benchmark using cardinal information about the magnitude of school effects. Throughout, school capacities are held fixed.

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.[10]

3. **Aligned Demand and Supply**: Both student preferences and school priorities are based on educational effectiveness. The market clears with student-proposing DA.

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

Each counterfactual imputes school average treatment effects and match effects using the empirical Bayes posterior mean predictions discussed in the previous section. 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 be computed by solving the Shapley-Shubik assignment problem with the objective of maximizing overall educational outcomes (Shapley and Shubik, 1971). This problem is solvable as a linear program, which allows TEMA to easily scale to settings with many students and schools.

The counterfactual results reveal that capacity constraints limit the aggregate gains that can be achieved with reallocation policies. Table 6 shows the predicted impact of reallocation 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 0.56σ 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.

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

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 achievement. Aligned Supply is predicted to increase mean Regents scores by 0.05σ, just over a third of the average effect from optimal school admissions for each school in isolation. Combining optimized demand and supply 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. As a comparison, this first-best reallocation effect is only about one-third of the standard deviation of school average treatment effects, as reported in Table 4.

The effects of reallocation policies on long-term outcomes parallel the math score patterns. Aligned Demand improves high school graduation and college attendance by 1.5 and 2.1 percentage points. Aligned Supply shows similar effects, at 1.6 and 1.5 percentage points for graduation and college attendance, and aligning both sides of the market leads to gains of 2.1 and 2.6 percentage points for these outcomes. Treatment effect maximization achieves the largest gain at 3.8 percentage points for both outcomes. Appendix Table B5 shows that counterfactuals based on OLS VAM rather than control function estimates lead to similar results for all outcomes.

The debate over reallocation policies often centers on equity and access for specific groups. This is particularly true in arguments about scaling back test-based admissions at highly-selective schools. 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).[11] We investigate this issue by reporting reallocation gains separately for students with low (bottom-quartile) and high (top three quartiles) baseline math scores.

Columns (1)-(3) of Table 7 demonstrate that treatment effect 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 0.18σ 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 reallocation policies focusing on lower-achieving students could potentially yield greater improvements for this subgroup than approaches across all students. At the same time, such policies necessarily require displacing higher-achieving students from the best schools.

The remaining columns of Table 7 explore this tradeoff with hypothetical reallocation policies prioritizing achievement for lower-achieving students. Columns (4)-(6) implement a modified

[11] 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).

TEMA approach that first chooses seats to maximize average achievement for bottom-quartile math students, then allocates the remaining seats to maximize achievement for higher-scoring students. While producing substantial gains for low-achieving students (0.41σ in Regents Math and 13 to 14 percentage points in graduation and college attendance), this algorithm *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 the seats they occupy in the status quo. Columns (7)-(9) of Table 7 show this constrained algorithm 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 schools. When reallocation across these two groups is prohibited, treatment effect maximization loses almost all impact for the low-performing group. This indicates that obtaining improvements for disadvantaged students through reallocation requires taking away high-quality seats from other groups.

5.2 Resource Augmentation

Our investigation of the potential impacts of resource augmentation focuses on closure policies that shut down low-performing schools and shift capacity to elsewhere in the school system. Specifically, we simulate a policy regime that closes schools in order of their posterior mean predictions of ATE, starting with the lowest estimate and moving up the list. Next, we reallocate capacity from closed schools to the remaining open schools, holding total capacity fixed. Finally, DA is used to clear the market. Due to equilibrium effects that operate through the centralized assignment system and the presence of match effects, this "greedy" closure rule is not necessarily optimal – an optimal policy would consider all possible portfolios of closures and select the portfolio yielding the largest aggregate gain. We study the greedy closure policy to avoid the combinatoric complexity of a portfolio-based rule and to mimic the behavior of policymakers who are likely to select closures based on simpler heuristics.

We simulate three closure 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 closures.

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

3) **Neighborhood Replacement**: re-allocate seats to the five closest schools, as measured by distance between school addresses.

In each scenario, school capacity expands proportionally, rounded so that remainders go to largest schools first.[12]

To compare the efficacy of these resource augmentation policies to the reallocation policies considered earlier, we determine the number of school closures that would be needed to match the achievement gains from Aligned Demand or TEMA. We focus on these two reallocation policies as benchmarks since other reallocation policies produce intermediate results between these two extremes.

Figure 5 reports simulated closure impacts, with markers indicating the number of closures necessary to match achievement gains generated by reallocation policies.[13] To match the same 0.04σ improvement in Regents Math produced by Aligned Demand, 7% of schools must close. Matching Aligned Supply requires 8% of schools to close, while matching Aligned Demand and Supply requires 10%. The most aggressive policy, TEMA, is matched by a closure policy that shuts down 13% of schools. To benchmark this finding, note that New York City closed 42 out of the 294 schools in our sample (14%) in the decade following our analysis period. This implies that a closure policy at a realistic scale is predicted to match the impact of even the most extreme reallocation policy (TEMA).

Matching reallocation policy effects on long-term outcomes generally requires fewer school closures than for math achievement. For high school graduation, for example, matching Aligned Demand requires closing just 2% of schools, while matching TEMA requires closing 7% of schools. For college enrollment, Aligned Demand and Aligned Supply effects can be matched by closing 5% of schools respectively, while matching TEMA's college enrollment gains requires closing 15% of schools.

Figure 6 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 more schools to achieve the same gains, but the results suggest that closure policies at a realistic scale can continue to match reallocation policies. For instance, matching Aligned Demand's effects for Regents Math requires closing 11% of schools under Typical Replacement compared to 7% under Effective Replacement.

Table 8 summarizes 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

[12] 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.

[13] The occasional non-monotonic pattern in the results stems from the fact that the greedy closure rule is not optimal. In particular, each closure triggers a new round of DA matching with updated capacities, creating potentially complex ripple effects that may reduce achievement even when an ineffective school is removed from the system.

Replacement create comparable levels of disruption. For example, to match Aligned Demand's effects on high school graduation, Typical Replacement and Neighborhood Replacement both require closing 23 schools.

When aiming to match TEMA's more ambitious outcomes, Effective Replacement remains relatively efficient, requiring reallocation of less than 22% of school seats across all outcomes (math, graduation, and college). However, Typical and Neighborhood Replacement approaches become more disruptive, with over 100 schools needing closure to match the TEMA college effects. This stark difference highlights that matching extreme reallocation policy gains is more feasible when it's possible to add capacity at higher-performing schools.

5.3 Implementation Considerations

The reallocation policies we've discussed represent idealized scenarios that would face significant implementation challenges in practice. Our analysis in Figure 3 reveals only a weak relationship between applicants' school preferences and school value-added. This is consistent with broader evidence that educational choices often fail to align with impacts on student outcomes (Rothstein, 2006; Bates et al., 2024; Laverde et al., 2025). As a result, 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.[14] 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. 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 families to maximize value-added in their choices. These interventions may also have unintended effects on educational equity.[15]

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 metrics as proxies for diversity often introduces substantial inefficiencies and statistical noise in the admissions process (Ellison and Pathak, 2021).

[14] 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).

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

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 context: 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 reallocation policies considered here 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 reallocation 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 48%, while Aligned Supply further increases it to 50%. 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 75%.

Most reallocation policies would also require significant changes to how students choose schools, though Aligned Supply is an exception since it maintains current student preferences. As Figure 3 demonstrates, shifting students to rank schools according to effectiveness would require substantial changes in application behavior. The magnitude of these changes is striking: Under Aligned Demand, students would typically be assigned to their 352nd choice school-program (out of a total of 849 school-programs). Since New York's Deferred Acceptance system only allows students to rank up to 12 school-programs, these statistics are based on forecasted preferences from the rank-ordered logit model. 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 their status quo schools. TEMA produces similarly dramatic changes: students would on average receive their 117th choice school-program, with even fewer students assigned to their status quo schools.

Reallocation policies would also significantly increase the distance students must travel to attend school. Under the Status Quo policy, 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. The Aligned Demand policy would also increase average travel distance by 2.8 miles, to an average of 5.9 miles. TEMA would create even more dramatic changes to student travel patterns. Under this policy, three out of four students (77%) would attend schools outside their home borough, and average travel distance would increase to 9.4 miles. More than 40%

of students would face commutes exceeding 10 miles under the achievement-maximizing school assignment regime.

Resource augmentation policies have more modest effects on assignment patterns than reallocation approaches, primarily because they affect fewer schools. Both Effective Replacement and Typical Replacement policies (calibrated to match the Regents math effect of Aligned Demand) maintain current levels of racial segregation. Students are also more likely to be assigned to schools they prefer compared to equally-effective reallocation policies, and average travel distances remain similar compared to the Status Quo.

6 New York City School Closures: A Case Study

6.1 Background on School Closures

The statistics above suggest that school closure policies may be more realistic than equally-effective reallocation policies. We next delve into the feasibility of effective school closures by studying the actual closure decisions made by New York policymakers around our sample period. 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. These 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.

Motivated by this background, we examine 42 high school closure decisions made in New York City between 2008-2017.[16] Figure 7 shows the geographical distribution of these closed schools. Our analysis compares New York's actual closures with schools that would have been selected

[16] We use the full list of school closures provided by New York City Public Schools, and then examine closures within our analysis school sample for which we observe more than 50 student outcomes and can thus estimate value-added measures.

by a posterior mean math ATE rule that matches the impact of Aligned Demand (under Typical Replacement). As shown in Table 10, schools identified by the posterior mean ATE rule share several characteristics with observed closure decisions: both rules select schools with low achievement-related treatment effects (ATEs), poor levels of academic performance, higher proportions of Black and Hispanic students, and locations in lower-income neighborhoods. Of the 33 schools closed by our simulated policy, 8 overlap with the 42 high schools that were actually closed. We do observe some differences in the spatial location of closures, with actual closures more concentrated in Brooklyn (28%) and the Bronx (41%) and simulated closures more concentrated in Queens (46%). This suggests that geographic considerations may have influenced closure decisions in a manner not captured by our simulation.

6.2 Who Decides?

The statistics in Table 10 indicate that the schools actually closed by New York policymakers were similar to schools that would have been selected based on educational effectiveness. Expanding on this finding, Figure 8 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 an office in charge of school portfolios using a simple rule based on graduation rates would identify many of the same schools as a more sophisticated posterior mean ATE approach.

Could targeting of closures be further improved by leveraging signals of market demand? We investigate this question in Figure 9, which relates school popularity (measured by the share of students ranking a school first) to a school's educational effectiveness (measured as the posterior mean ATE for high school graduation). The figure shows only a weak relationship between household demand and school effectiveness, and a corresponding lack of correlation between popularity and closure decisions. This suggests that household demand adds little useful information that could be used to target ineffective schools.

We investigate these patterns further in Table 11, which provides a multivariate analysis of school closure decisions, examining the relationships between closures and various school characteristics. The estimate in column (1) confirms that schools with lower graduation rates were more likely to be closed. However, the results in column (2) reveal that New York City successfully targeted ineffective schools for closure, not just schools with low outcome levels. In particular, schools with lower graduation treatment effects (ATEs) and average realized match effects were more likely to be closed. In contrast, we see no relationship between closure decisions and school popularity, as measured by the school mean utility from a rank-ordered logit model predicting student preferences (column (3)).[17] When including all these factors simultaneously in column (4), we find that both low graduation rates and low ATEs independently predict school closures, revealing that ineffective schools were targeted for closure even holding the graduation rate fixed. In contrast, the coefficient

[17]The mean utility estimate in column (3) is the student-weighted average of the estimated $\delta_j(X_i)$ parameters from the rank-ordered logit model discussed in Section 3.1.

on school popularity remains small and statistically insignificant.

Figure 10 summarizes the efficacy of NYC closure efforts by contrasting predicted treatment effects for actual closure decisions and the posterior mean ATE closure rule discussed earlier. NYC's actual closure decisions yield treatment effects only slightly inside the frontiers generated by the posterior mean ATE rule. This suggests that, for the purpose of boosting student achievement, the observed closures were almost as effective as a policy directly targeting schools based on estimated treatment effects. In a scenario with effective replacement, the actual closure decisions yield a predicted math effect of 0.08σ when closing 14% of schools, an outcome comparable to the simulated impact of implementing TEMA. Estimates of actual closure effects for graduation and college outcomes are also close to effects of TEMA. These results indicate that NYC policymakers implemented a feasible resource augmentation program that nearly matched the hypothetical impact of the most extreme possible reallocation reform.

While New York's closure efforts appear to have successfully targeted ineffective schools, the weak relationship between closure decisions and preferences reported in Table 11 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. Schools selected for closure are reasonably popular despite being ineffective. Second, to improve student outcomes, our results suggest that using parental demand as a guide to closure decisions would be less effective than the district's approach of focusing on graduation rates and other achievement measures. A demand-based strategy would likely miss many popular but ineffective schools, while the central office's closure rule appears more successful at identifying schools that need intervention to improve student outcomes.

7 Conclusion

Policies pursued by America's urban school reformers can be divided into those focused on shifting students across schools (reallocation) and those focused on improving school quality (resource augmentation). We've shown that ambitious reallocation policies are likely to yield only modest gains, due to 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 clearly underperforming schools, prove similarly 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 reallocation 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 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 enrolled in closed schools instead 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.

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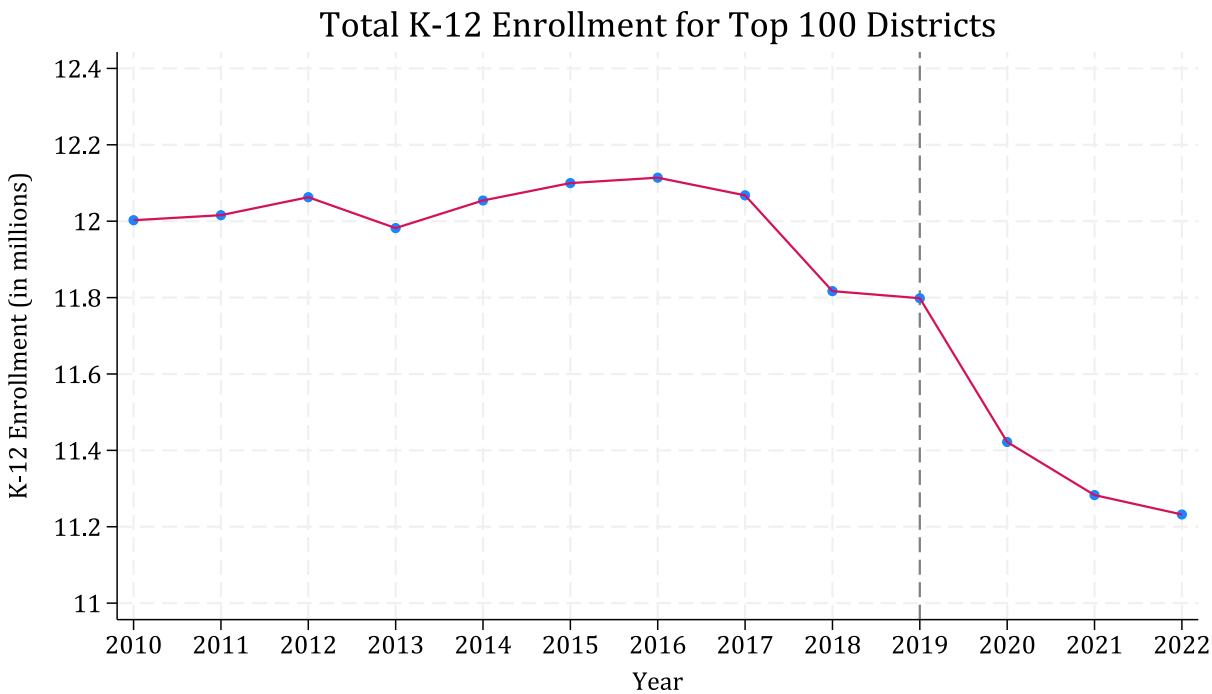
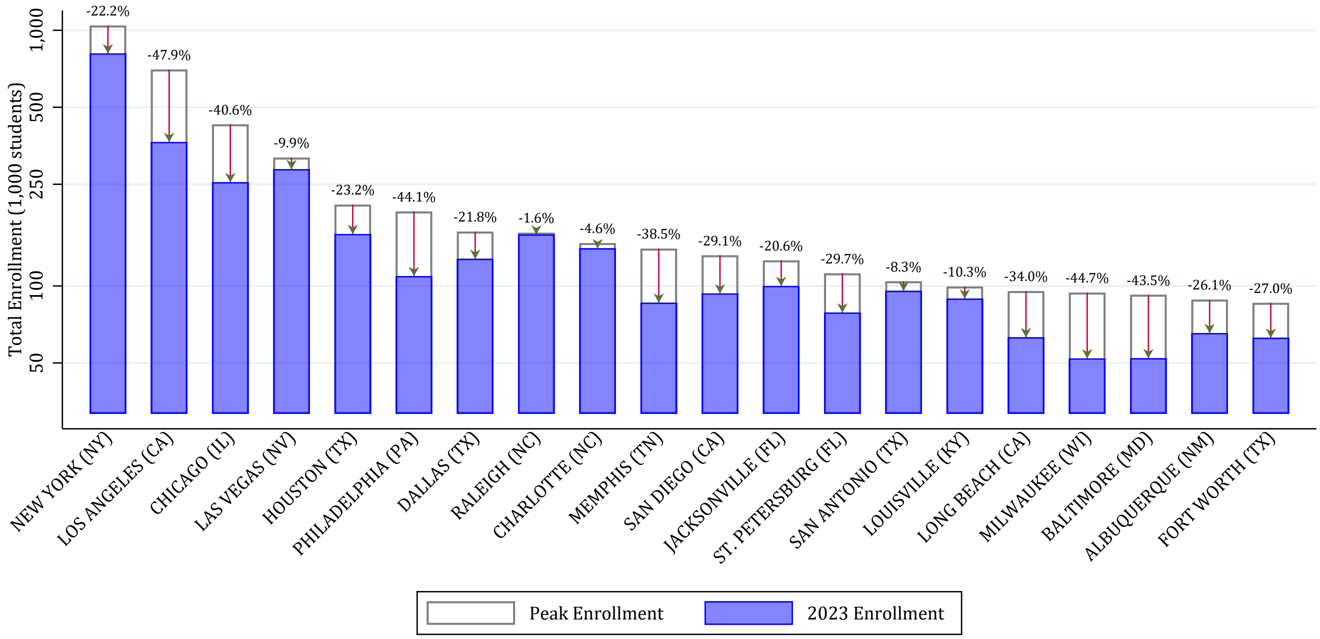
Figure 1: Total K-12 Enrollment for 100 Largest Districts



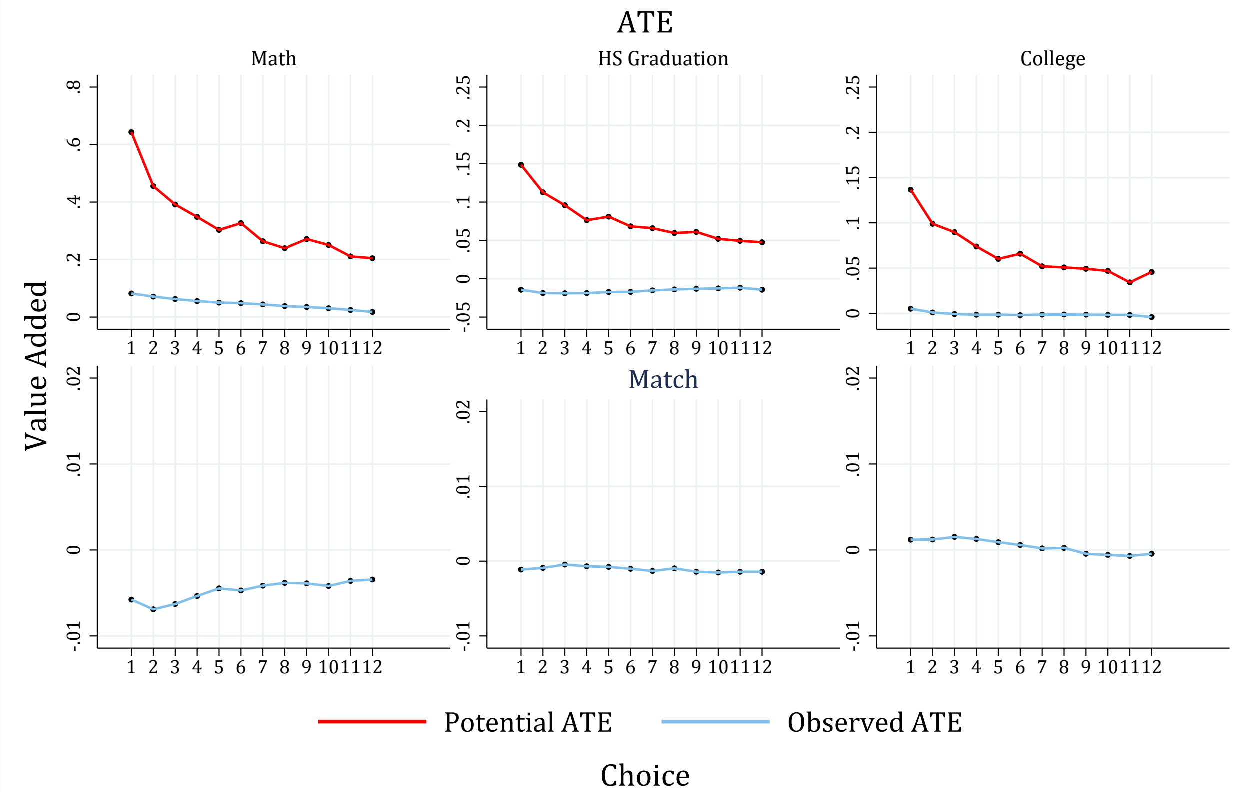
Notes: This figure reports the total K-12 enrollment in the 100 largest school districts in 2023 using data from the National Center for Education Statistics.

Figure 2: Enrollment Decline in the 20 Largest Urban School Districts



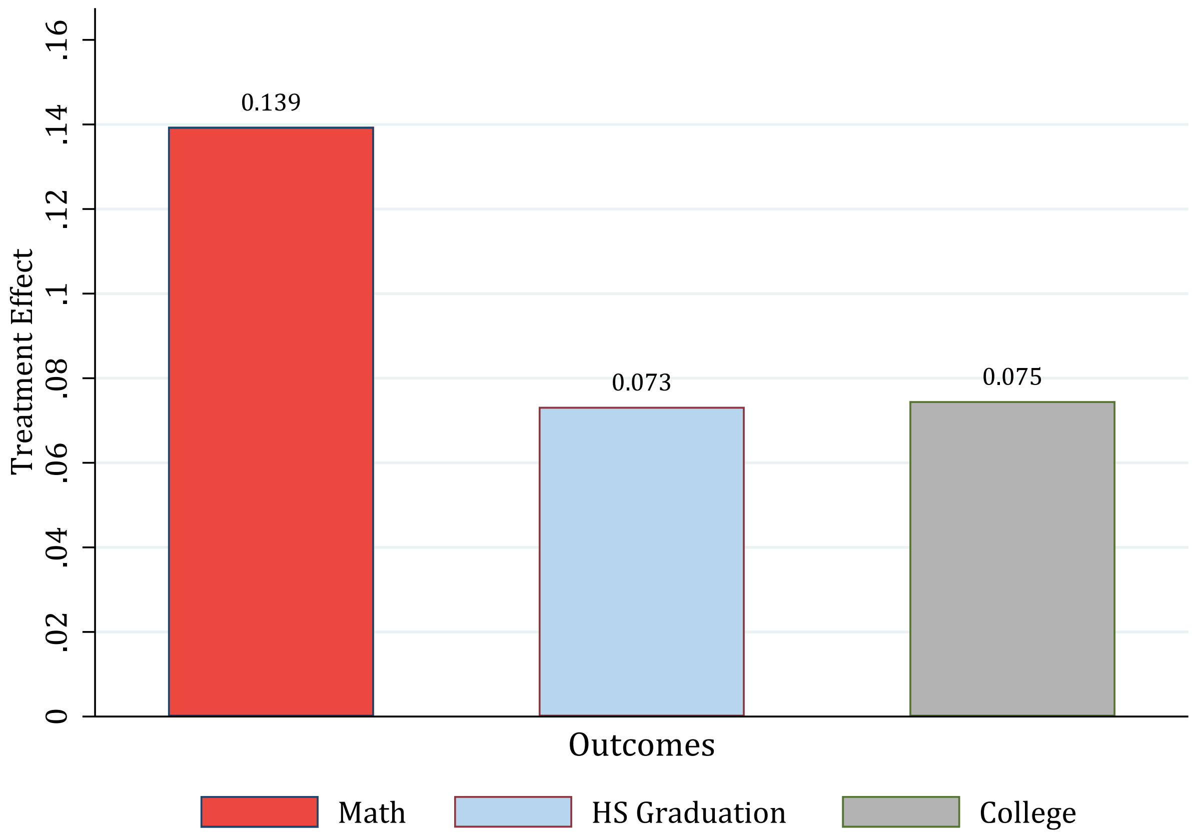
Note: This figure reports the decline in K-12 enrollment in the 20 largest urban school districts using data from the National Center for Education Statistics. Peak enrollment was calculated from the years 2000-2023. School district names were simplified to the largest city within the jurisdiction.

Figure 3: Potential Achievement Gains from Rankings Schools by Effectiveness



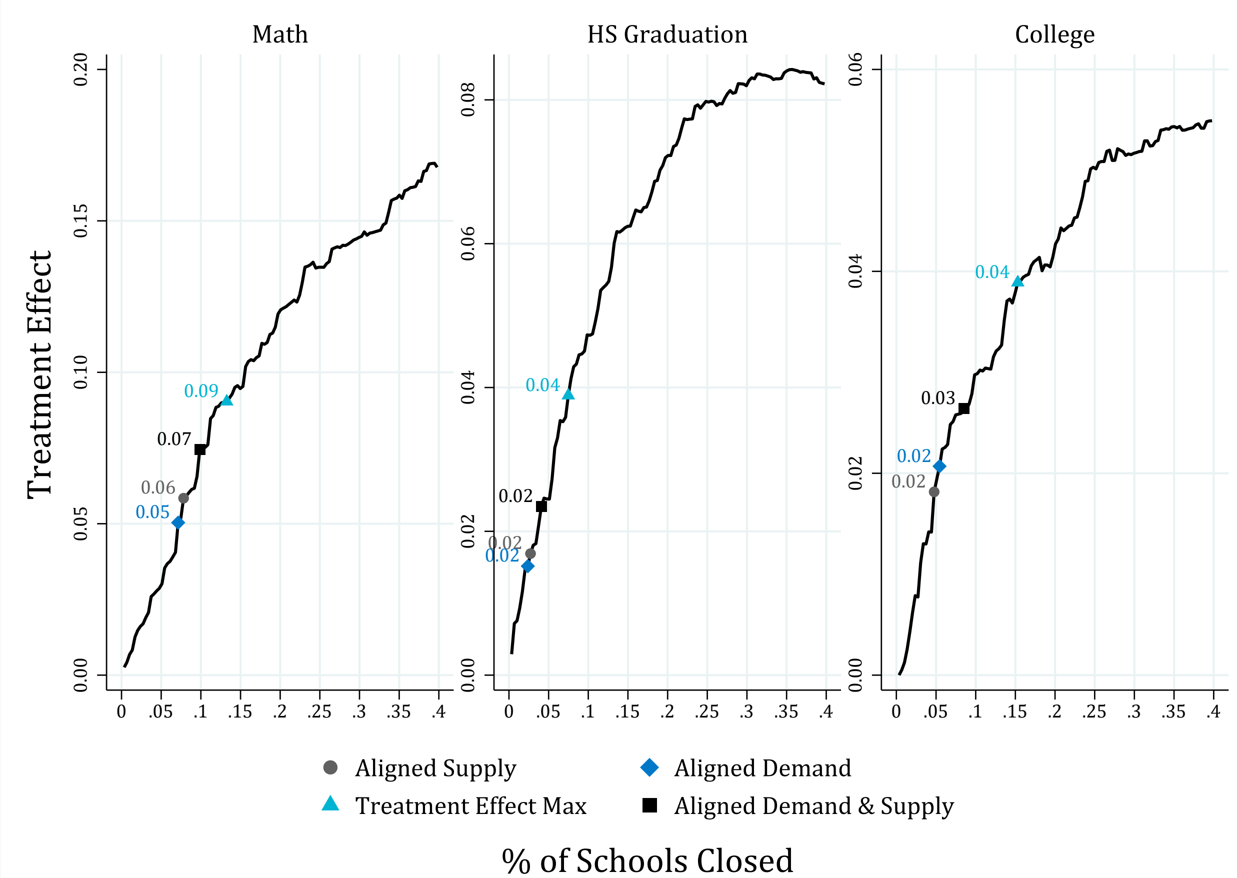
Notes: This figure compares Regents Math effects for observed student rankings and hypothetical rankings based on school effectiveness. Regents Math estimates come from control function models. The upper panel shows estimates of Average Treatment Effects at each rank position, and the lower panel shows estimates of student/school match effects.

Figure 4: Potential Achievement Gains from Admitting Students by Effectiveness



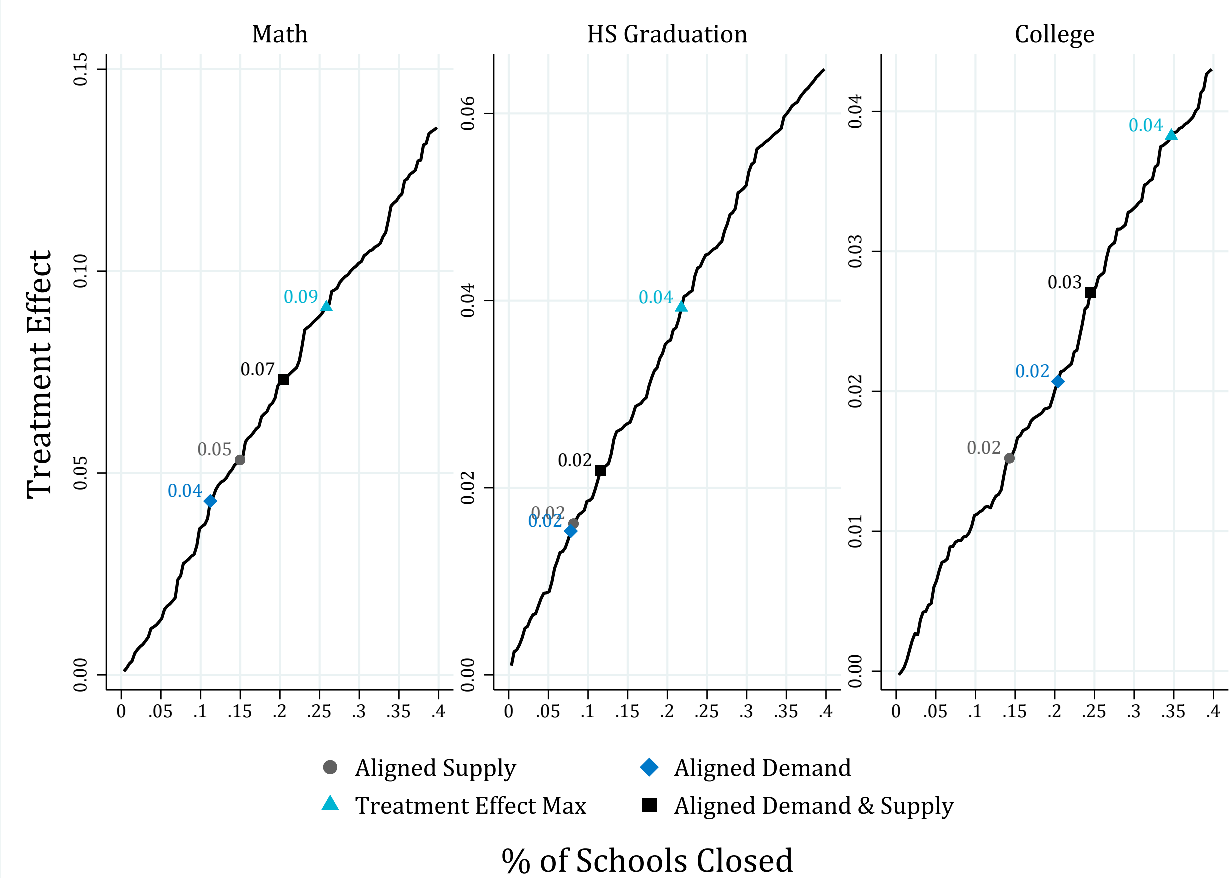
Notes: This figure reports the estimated effect of assigning priorities at each school in order of students' Regents Math match effects, holding student preferences fixed. Estimates are computed by altering admissions policies at each school separately, computing the predicted improvement in the school's average treatment effect on its students relative to the status quo, then averaging across schools.

Figure 5: Simulated Effects of School Closure Policies with Effective Replacement



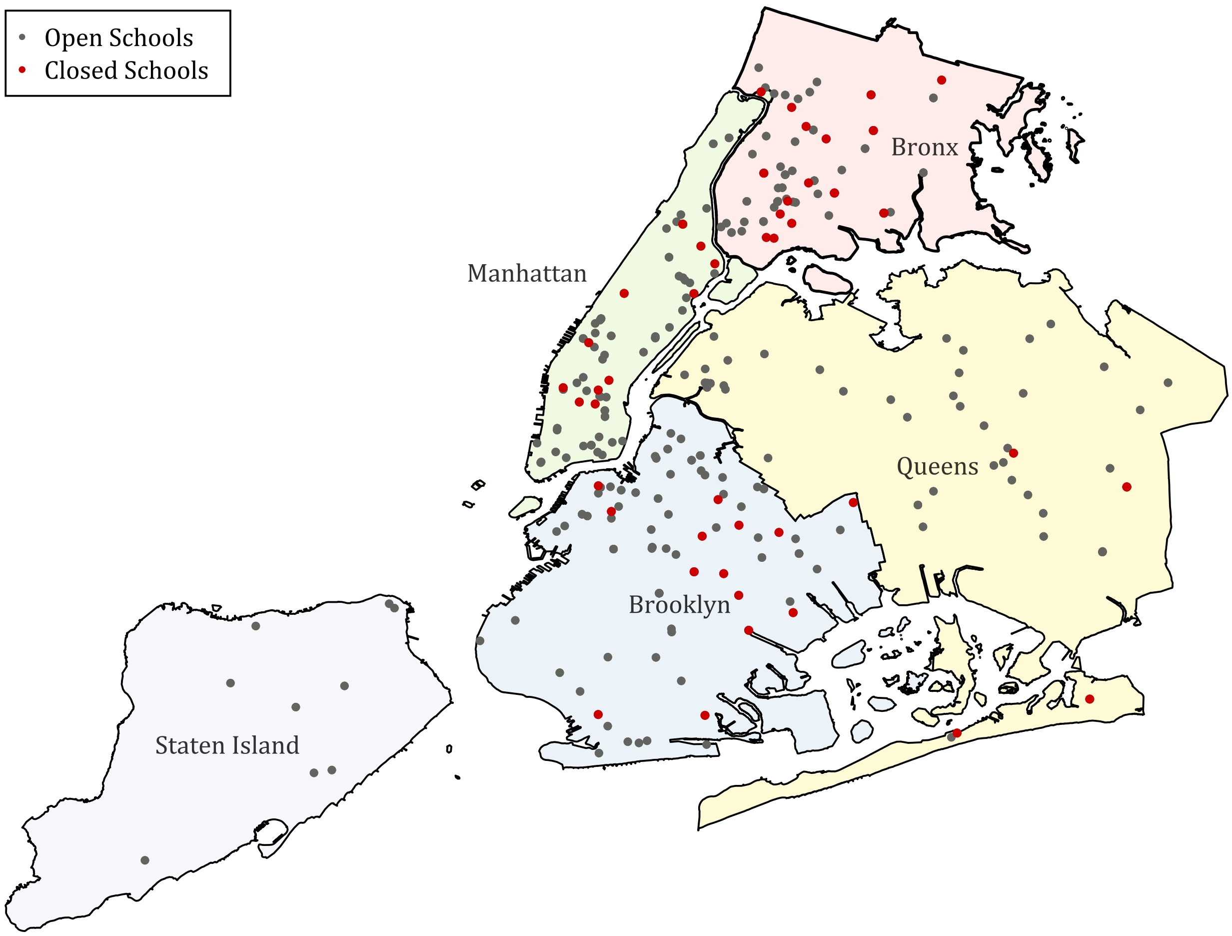
Notes: This figure shows the fraction of schools that need to be closed to match the predicted treatment effects of reallocation policies. The simulated closure rule selects schools in order of posterior mean Average Treatment Effects (from lowest to highest). Capacity from the closed schools is distributed with an Effective Replacement rule that selects schools with highest posterior mean effectiveness, with capacity going to the same number of schools selected for closure (and allocated in proportion to baseline enrollment). Treatment effect estimates come from control function models.

Figure 6: Simulated Effects of School Closure Policies with Typical Replacement



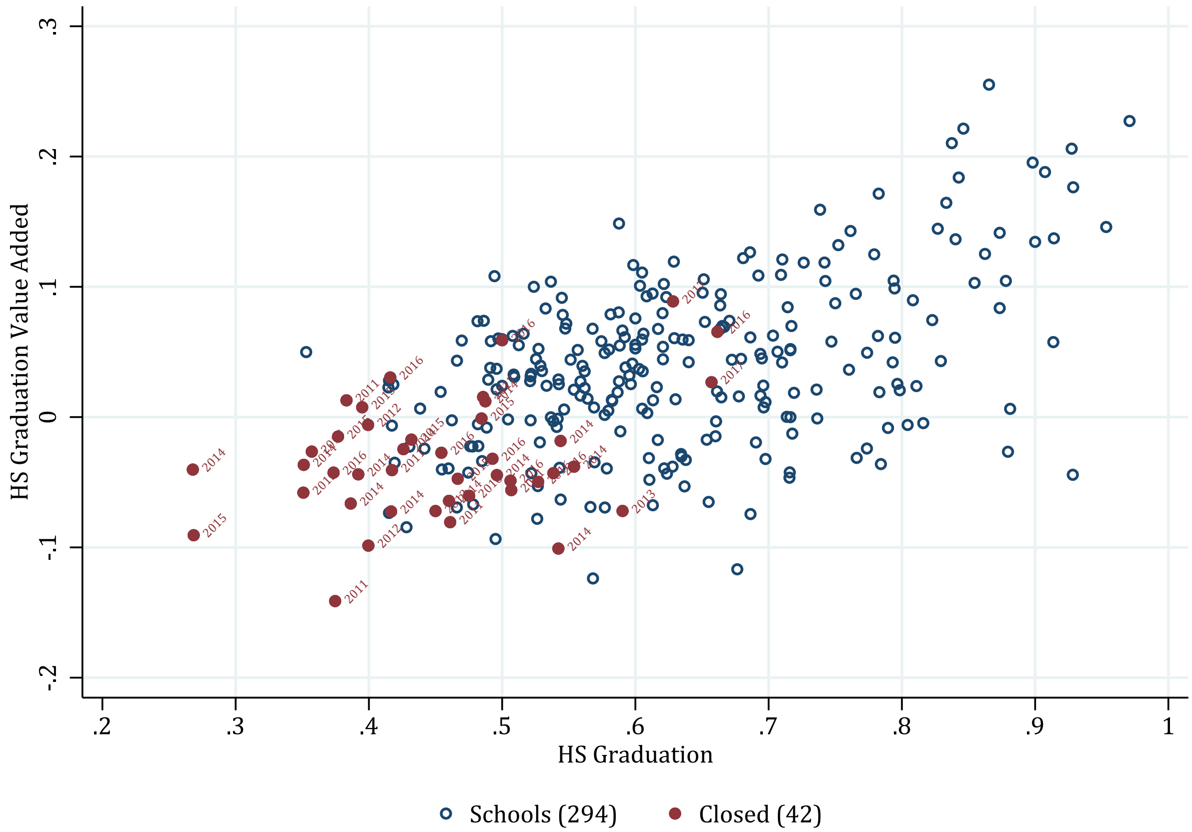
Notes: This figure shows the fraction of schools that need to be closed to match the predicted treatment effects of reallocation policies. The simulated closure rule selects schools in order of posterior mean Average Treatment Effects (from lowest to highest). Capacity from the closed schools is distributed with a Typical Replacement rule that allocates seats to all schools in proportion to baseline enrollment. Treatment effect estimates come from control function models.

Figure 7: Map of High School Closures in New York City (2008-2017)



Note: Each dot represents a high school. Boroughs are indicated by color.

Figure 8: High School Graduation Levels and Value-Added for Closed and Open Schools

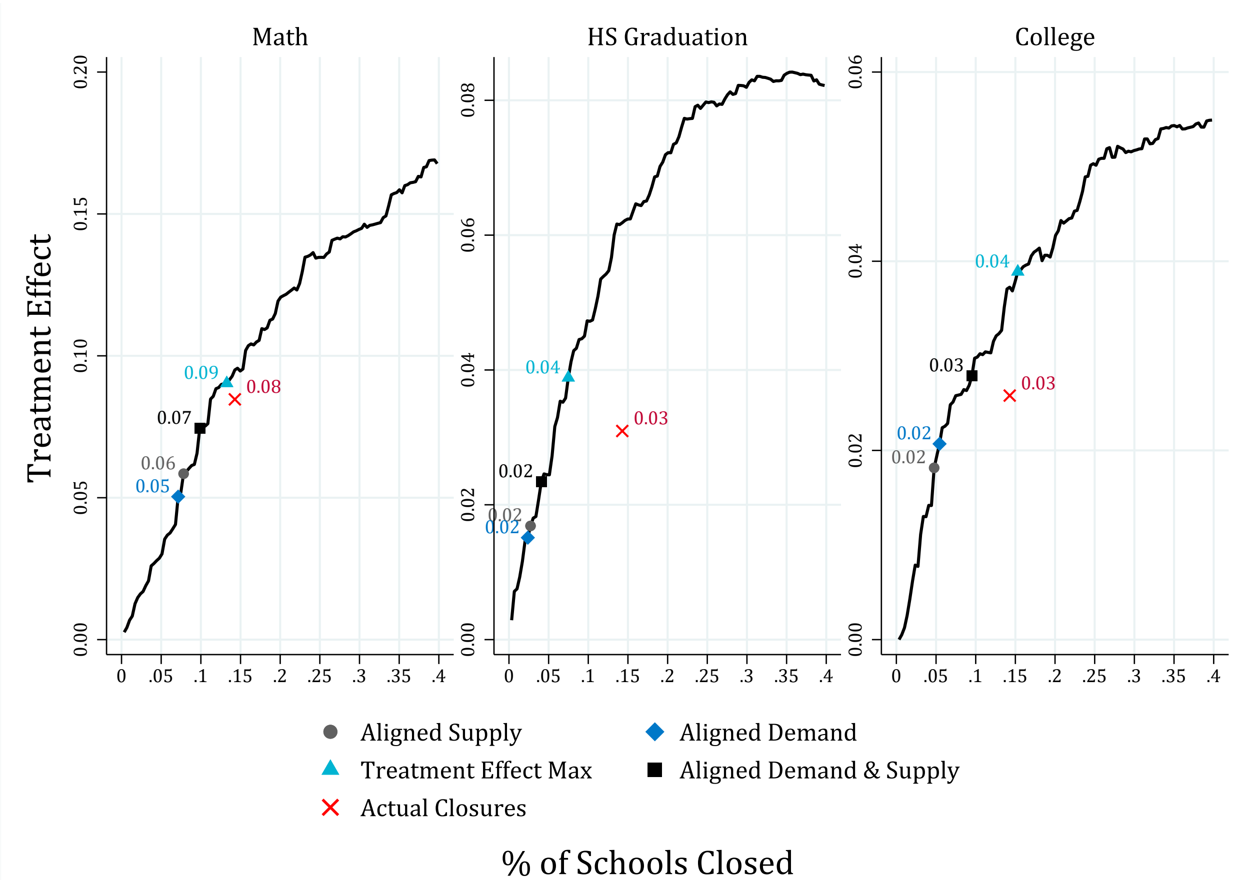


Notes: This figure reports high school graduation levels and estimated high school graduation value added for 42 schools that closed in the ten years after our sample period (closed dots) and schools that remained open (open dots). Closed schools are marked by year of closure. Value-added estimates on the vertical axis are posterior mean high school graduation Average Treatment Effects from control function models.

Figure 9: Top Rank Market Shares and High School Graduation Value-Added for Closed and Open Schools

Notes: This figure reports top rank market shares – given by the share of students ranking a program at the school first – and estimated high school graduation value added for 42 schools that closed in the ten years after our sample period (closed dots) and schools that remained open (open dots). Closed schools are marked by year of closure. Value-added estimates on the vertical axis are posterior mean high school graduation Average Treatment Effects from control function models.

Figure 10: Actual NYC School Closures (2008-2017) Relative to Simulated School Closure Policies with Effective Replacement



Notes: This figure reproduces Figure 5, which plots the fraction of schools that need to be closed to match the predicted treatment effects of reallocation policies. An additional simulation is added using the actual list of high schools closed by New York City Public Schools from 2008-2017, with capacity from these schools distributed with an Effective Replacement rule that selects schools with highest posterior mean effectiveness. Treatment effect estimates come from control function models.

Table 1—Reallocation Policies

Location (1)	Previous Policy (2)	Year (3)	New Policy (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 reallocation policy changes in large US public school districts. Panel A outlines changes to admission criteria, panel B lists changes to school assignment and matching processes, and panel C describes implementation of information campaigns and decision aids.

Table 2—Urban School Districts with Most Closures between 2000 and 2023

City	Schools in 2000	Closures	Openings	Most Yearly Closures	Enrollment in 2000	Enrollment in 2023	Change in Enrollment (2000-2023)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
New York, NY	1196	344	729	31 (2016)	1,050,095	812,872	-237,223
Chicago, IL	579	205	159	57 (2013)	427,799	254,660	-173,139
Milwaukee, WI	192	113	47	16 (2007)	94,260	52,113	-42,147
Philadelphia, PA	253	94	57	34 (2013)	195,741	109,333	-86,408
Washington, DC	155	88	32	27 (2008)	67,979	44,915	-23,064
Memphis, TN	204	78	92	15 (2016)	161,080	86,076	-75,004
Baltimore, MD	163	76	34	9 (2009)	92,456	52,203	-40,253
Houston, TX	268	61	63	12 (2011)	205,061	159,784	-45,277
Dallas, TX	215	58	81	11 (2018)	160,856	127,735	-33,121
St. Petersburg, FL	147	52	28	16 (2009)	110,599	78,781	-31,818
Denver, CO	114	48	61	9 (2008)	67,586	61,131	-6,455
Nashville, TN	121	48	52	7 (2009)	67,491	63,602	-3,889
Columbus, OH	142	48	18	12 (2006)	69,340	43,275	-26,065
Los Angeles, CA	550	42	180	6 (2013)	679,562	366,542	-313,020
San Diego, CA	158	38	33	9 (2007)	131,902	93,567	-38,335

Notes: This table displays the cities within the top 100 school districts by enrollement in 2023 with the largest number of district-run school closures (excluding charter and special education schools) between 2000 and 2023. Data is retrieved from the National Center for Education Statisitics's Common Core of Data. School district names are simplified as the largest city within their jurisdiction.

Table 3—Descriptive Statistics

	Population (1)	VAM Sample (2)	Analysis Sample (3)
		A. Students	
Total	275,225	222,891	61,879
2003-2004	67,644	54,477	
2004-2005	67,331	54,679	
2005-2006	68,053	56,196	
2006-2007	72,197	57,539	61,879
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	-0.09	-0.09
		B. Programs	
Number of Schools	391	294	294
Number of Programs	1,054	849	849
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. Column (1) describes characteristics of New York City 8th graders with baseline (8th grade) demographic information and test scores applying for NYC high schools. The VAM sample in column (2) is restricted to students attending schools for which we can compute VAM estimates. The analysis sample in column (3) includes students and schools used for counterfactual analysis.

Table 4—Distributions of School Treatment Effect Parameters for Regents Math

		Intercept (1)	Above-median Math (2)	Black/Hisp. (3)	Female (4)	Preference coefficient (ψ_j) (5)
				A. Control function		
Mean		0	0.279	-0.100	-0.034	-0.001
		—	(0.007)	(0.018)	(0.005)	(0.001)
SD		0.261	0.128	0.143	0.055	0.006
		(0.004)	(0.007)	(0.035)	(0.013)	(0.001)
Correlation:	Above-median Math	-0.197				
		(0.049)				
	Black/Hispanic	-0.024	-0.246			
		(0.106)	(0.130)			
	Female	0.272	0.185	-0.233		
		(0.116)	(0.162)	(0.244)		
	Preference coefficient (ψ_j)	0.179	-0.193	-0.082	0.155	
		(0.072)	(0.099)	(0.160)	(0.183)	
				B. OLS VAM		
Mean		0	0.282	-0.093	-0.040	
		—	(0.007)	(0.010)	(0.005)	
SD		0.274	0.129	0.145	0.053	
		(0.004)	(0.007)	(0.037)	(0.013)	
Correlation:	Above-median Math	-0.199				
		(0.048)				
	Black/Hispanic	-0.050	-0.249			
		(0.104)	(0.125)			
	Female	0.229	0.260	-0.296		
		(0.116)	(0.170)	(0.264)		

Notes: This table reports estimated distributions of school treatment effect 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 VAM regression in panel B 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. Control function models in panel A add controls for distance and predicted tastes for each school. Estimated standard deviations and correlations come from bias-corrected estimates of the variances and covariances of the interaction coefficients across schools. Standard errors are computed by the delta method. Selection on Gains Parameter (φ) Estimate under Control Function: 0.008 (Standard Error: 0.001)

Table 5—Distributions of Regents Math Effect Estimates

		Realized		Potential	
		Raw	Shrunk	Raw	Shrunk
		(1)	(2)	(3)	(4)
		A. Control function			
Treatment Effect					
	Mean	0.004	0.002	0.000	0.000
	SD	0.256	0.238	0.305	0.267
	Range	[-1.20, 1.18]	[-0.83, 1.10]	[-1.68, 1.24]	[-0.90, 1.14]
Average Treatment Effect					
	Mean	0.009	0.007	0.000	0.000
	SD	0.239	0.232	0.269	0.256
	Range	[-0.70, 1.02]	[-0.65, 1.00]	[-0.70, 1.02]	[-0.65, 1.00]
Match Effect					
	Mean	-0.005	-0.005	0.000	0.000
	SD	0.119	0.071	0.145	0.075
	Range	[-1.09, 1.16]	[-0.39, 0.38]	[-1.17, 1.16]	[-0.48, 0.41]
		B. OLS VAM			
Treatment Effect					
	Mean	0.010	0.008	0.000	0.000
	SD	0.272	0.255	0.316	0.279
	Range	[-1.32, 1.16]	[-0.86, 1.08]	[-1.83, 1.16]	[-0.86, 1.12]
Average Treatment Effect					
	Mean	0.013	0.012	0.000	0.000
	SD	0.256	0.249	0.281	0.269
	Range	[-0.71, 1.01]	[-0.66, 0.99]	[-0.71, 1.01]	[-0.66, 0.99]
Match Effect					
	Mean	-0.003	-0.004	0.000	0.000
	SD	0.117	0.072	0.145	0.076
	Range	[-0.97, 1.17]	[-0.37, 0.36]	[-1.23, 1.17]	[-0.39, 0.40]

Notes: The table displays the mean, standard deviation, and range of estimated total school effects, average treatment effects, and match effects for Regents Math scores. Raw estimates are based on coefficients from regressions of math scores on school indicators and their interactions with an above-median baseline math score indicator, a Black/Hispanic indicator, and a female indicator. The VAM regression in panel B 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. Control function models in panel A add controls for distance and predicted tastes for each school. Shrunk estimates are empirical Bayes posterior means that shrink school-specific parameters toward the mean. Estimates are generated using student data from 2003-06, and treatment effect statistics are displayed for 2006 applicants. The number of observations for the realized treatment effect estimates in columns (1) and (2) is 61,879, and the number of student/school pairs for the potential treatment effects in columns (3) and (4) is 18,192,426.

Table 6—Potential Gains from Reallocation Policies Relative to Status Quo

	Aligned Demand	Aligned Supply	Aligned Demand and Supply	Treatment Effect Maximization
	(1)	(2)	(3)	(4)
Math	0.041	0.053	0.073	0.090
HS Graduation	0.015	0.016	0.021	0.038
College Attendance	0.021	0.015	0.026	0.038

Notes: This table compares predicted changes in average student outcomes based on simulating reallocation policies. Estimates are predicted average outcomes minus average status quo outcomes. 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 has both students and schools rank each other based on treatment effects. Treatment Effect Maximization assigns students to schools to maximize the overall average outcome. Estimates come from the paper's preferred specification, control function models controlling for student covariates and predicted unobserved tastes.

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 their Assigned Seats		
	All	Bottom Math Quartile	Top 3 Math Quartiles	All	Bottom Math Quartile	Top 3 Math Quartiles	All	Bottom Math Quartile	Top 3 Math Quartiles
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Math	0.090	0.181	0.057	0.088	0.410	-0.030	0.077	0.038	0.092
HS Graduation	0.038	0.073	0.026	0.037	0.136	0.001	0.034	0.019	0.039
College Attendance	0.038	0.066	0.028	0.038	0.130	0.004	0.035	0.024	0.039

Notes: This table compares treatment effects in counterfactual assignments relative to the status quo. 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. Top 3 math quartiles represents the average predicted achievement for the top 75% of students on baseline math. TEMA computes the treatment effects maximization outcome. Max TE for Bottom Quartile using all Seats computes the treatment effect maximization outcome for the bottom math quartile students, using all available seats. Then, it maximizes treatment effects for the remaining top three quartile students using the remaining seats. Max TE for Bottom Quartile using their Assigned Seats computes the treatment effect maximization outcome for the bottom math quartile students, using only the seats they initially occupied. Then, it maximizes treatment effects for the remaining top three quartile students using the seats that they initially occupied.

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	0.77	0.70	0.79
Schools Closed	21	33	32
% Seats Reallocated	0.05	0.10	0.09
Seats Reallocated	3,215	6,292	5,541
HS Graduation			
% Students in Same School	0.75	0.62	0.69
Schools Closed	7	23	23
% Seats Reallocated	0.05	0.16	0.16
Seats Reallocated	2,842	9,969	9,969
College Attendance			
% Students in Same School	0.69	0.51	0.58
Schools Closed	16	60	60
% Seats Reallocated	0.09	0.28	0.28
Seats Reallocated	5,477	17,623	17,623
		B. TEMA	
Math			
% Students in Same School	0.65	0.60	0.63
Schools Closed	39	76	78
% Seats Reallocated	0.12	0.23	0.24
Seats Reallocated	7,205	14,296	14,991
HS Graduation			
% Students in Same School	0.58	0.40	0.44
Schools Closed	22	64	65
% Seats Reallocated	0.15	0.41	0.42
Seats Reallocated	9,317	25,126	25,707
College Attendance			
% Students in Same School	0.50	0.36	0.40
Schools Closed	45	102	112
% Seats Reallocated	0.22	0.47	0.49
Seats Reallocated	13,859	29,249	30,517

Notes: This table displays school closure DA estimates after closing a number of schools. 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	0.48	0.50	0.75	0.75	0.45	0.45
Share in School with 90% Low Baseline Peers	0.00	0.07	0.05	0.08	0.10	0.00	0.00
				B. Choice and Assignment			
Average Rank	2.66	352.00	2.96	336.77	117.13	2.65	2.59
Attend School in Original Choice List	0.87	0.20	0.80	0.03	0.03	0.86	0.86
Share Assigned to Same School as in Status Quo	1.00	0.12	0.30	0.01	0.01	0.77	0.70
				C. Distance and Travel			
Travel Outside Home Borough	0.12	0.41	0.12	0.77	0.77	0.13	0.13
Share Travel to Borough Different than Status Quo	0.00	0.44	0.12	0.78	0.77	0.04	0.05
Average Distance (Miles)	3.07	5.86	3.36	9.29	9.38	3.20	3.26
Travel 1+ Miles	0.79	0.86	0.84	0.98	0.98	0.79	0.80
Travel 5+ Miles	0.18	0.47	0.20	0.77	0.77	0.19	0.19
Travel 10+ Miles	0.02	0.20	0.03	0.41	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	0.87
% Black	0.34	0.45	0.35	0.36
% Hispanic	0.41	0.48	0.41	0.52
% Subsidized Lunch	0.69	0.78	0.70	0.76
8th Grade Math	-0.03	-0.46	-0.07	-0.31
8th Grade ELA	-0.02	-0.39	-0.05	-0.31
% Above Median Math	0.47	0.25	0.45	0.32
Log(Census Tract Income)	10.72	10.54	10.69	10.65
% Brooklyn	0.33	0.28	0.35	0.11
% Manhattan	0.11	0.15	0.11	0.15
% Queens	0.28	0.16	0.24	0.46
% Bronx	0.21	0.41	0.24	0.27
% Staten Island	0.07	0.00	0.06	0.00
B. School Characteristics				
9th Grade Enrollment	208.35	223.19	212.98	190.67
Survey Score (1-40)	26.77	24.56	26.43	26.63
Crimes Per Student	0.09	0.10	0.09	0.11
% of Teachers with Masters+	0.31	0.36	0.32	0.31
C. Performance Levels				
Regents Math	0.01	-0.55	0.01	-0.62
HS Graduation Rate	0.64	0.44	0.62	0.54
College Attendance	0.48	0.30	0.46	0.38
D. ATE				
Regents Math	0.03	-0.16	0.04	-0.39
HS Graduation Rate	0.01	-0.05	0.00	-0.01
College Attendance	0.01	-0.04	0.00	-0.01

Notes: This table contains information on students and schools following closures to match Aligned Demand outcomes for Regents math scores 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 New York City School Closures

	(1)	(2)	(3)	(4)
HS Graduation	-2.45			-1.90
	(0.38)			(0.41)
ATE		-1.67		-1.51
		(0.26)		(0.38)
Match		-0.91		-0.59
		(0.23)		(0.35)
Mean Utility			-0.23	0.13
			(0.17)	(0.38)
Capacity				-0.60
				(0.33)

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) along with the number of students enrolled in 9th grade in the 2003-2004 school year as a proxy for school capacity. The sample includes 294 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 95-96% of priority assignments (shown in Table A2). For 2006-2007, the specific year used in our counterfactual analysis, the accuracy rate is even higher at 97%.

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.

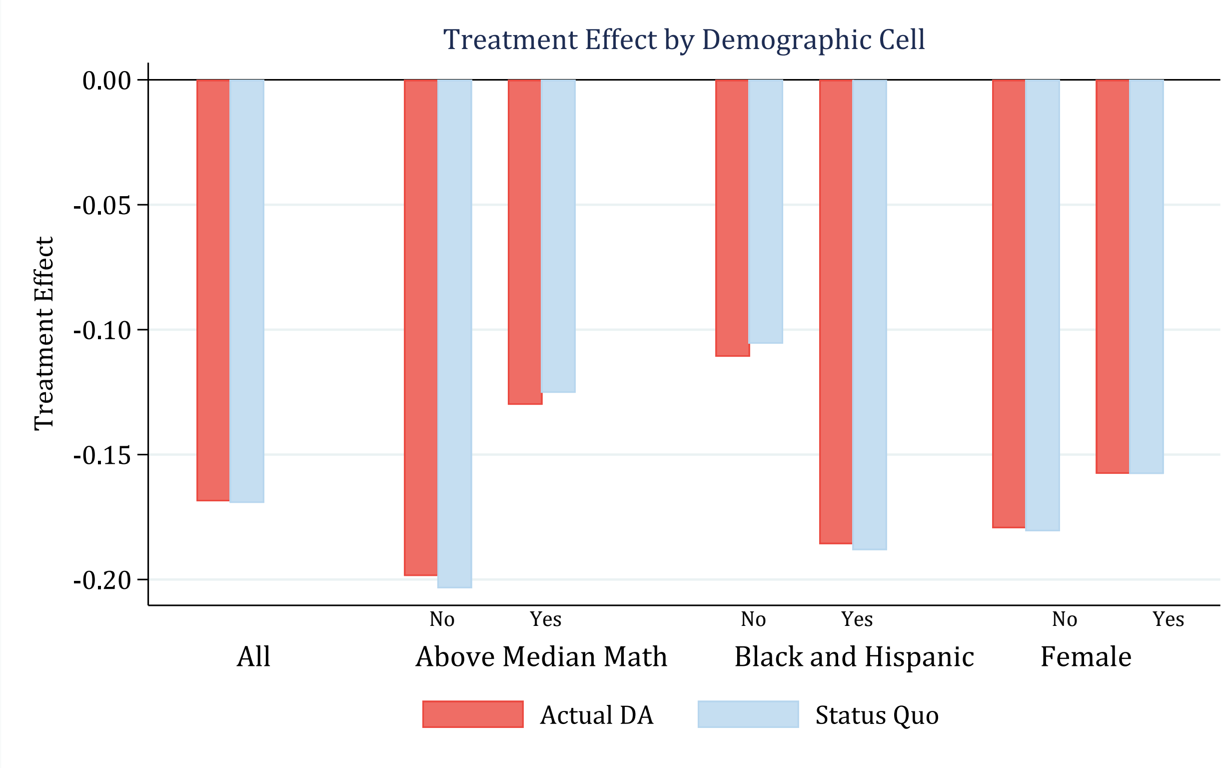
For batched programs using 3-5 distinct ranks, we run an ordered logit model using academic and demographic characteristics, home language, and missing data indicators to predict the probability of each possible rank. Each applicant is then assigned the rank for which they have the highest predicted 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 reallocation 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	176,129
	Yes	0.16	0.84	120,475
	Total	177,005	119,599	296,604

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. Row and column totals respectively refer to the number of students in each bin ("No" or "Yes") of actual and predicted eligibility. Cell entries refer to the proportion of correct and incorrect predictions by actual eligibility status.

Table A2—Accuracy of Predicted Priorities

	All	2004	2005	2006
Percent Correct	0.96	0.96	0.95	0.97
Number of Applications	1,398,754	484,134	464,722	449,898

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.

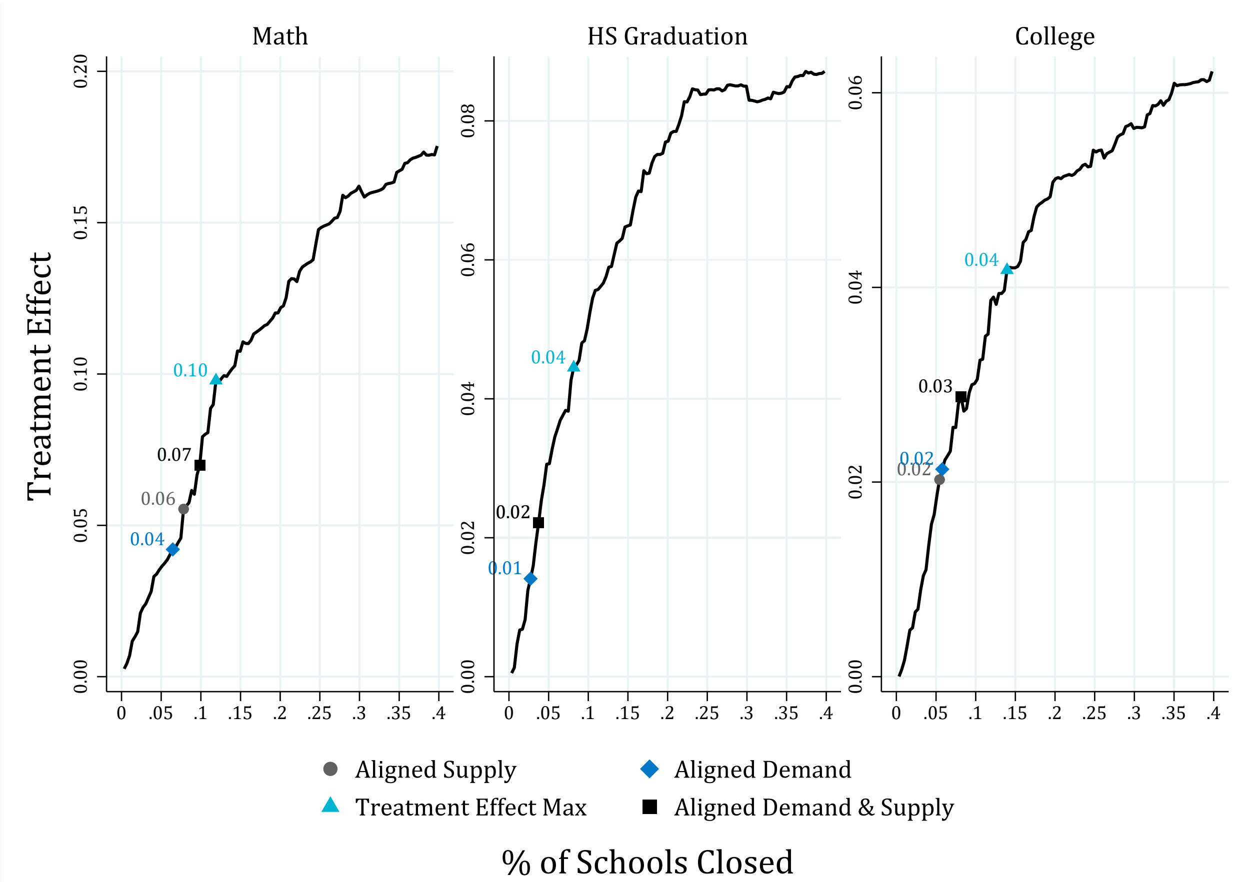
Table A3—Differences between DA and Status Quo

	Actual DA	Status Quo
	(1)	(2)
% Identical Program Offered		0.53
% Offered Program in Original Choice List		0.86
Average Rank of Program Offered	2.45	2.61
N	61,879	

Notes: This table displays summary statistics on actual DA results versus the status quo model. Column (1) corresponds to the actual DA procedure that was implemented by NYCDOE. Column (2) corresponds to our own simulation that 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, as detailed in Appendix A.

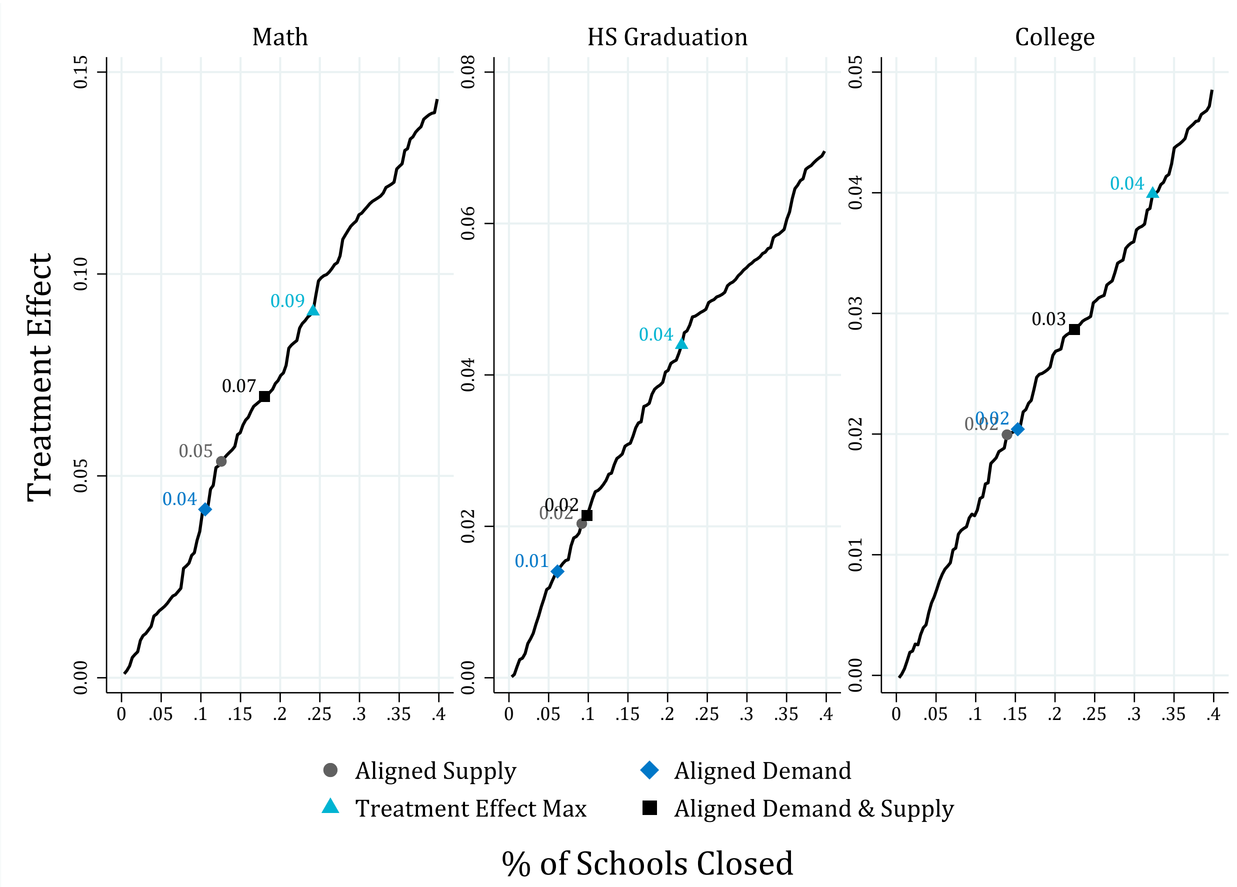
B Additional Exhibits for Control Function and OLS VAM Models

Figure B.1: Simulated Effects of School Closure Policies with Effective Replacement, OLS VAM



Notes: This figure shows the fraction of schools that need to be closed to match the predicted treatment effects of reallocation policies. The simulated closure rule selects schools in order of posterior mean Average Treatment Effects (from lowest to highest). Capacity from the closed schools is distributed with an Effective Replacement rule that selects schools with highest posterior mean effectiveness, with capacity going to the same number of schools selected for closure (and allocated in proportion to baseline enrollment). Treatment effect estimates come from OLS VAM models.

Figure B.2: Simulated Effects of School Closure Policies with Typical Replacement, OLS VAM



Notes: This figure shows the fraction of schools that need to be closed to match the predicted treatment effects of reallocation policies. The simulated closure rule selects schools in order of posterior mean Average Treatment Effects (from lowest to highest). Capacity from the closed schools is distributed with a Typical Replacement rule that allocates seats to all schools in proportion to baseline enrollment. Treatment effect estimates come from OLS VAM models.

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

		Intercept (1)	Above-median Math (2)	Black/Hispanic (3)	Female (4)	Preference coefficient (ψ_j) (5)
				A. Control function		
Mean		0	0.060	-0.001	0.059	-0.001
		—	(0.003)	(0.010)	(0.003)	(0.000)
SD		0.064	0.054	0.066	0.027	0.006
		(0.003)	(0.004)	(0.017)	(0.006)	(0.000)
Correlation:	Above-median Math	-0.851				
		(0.087)				
	Black/Hispanic	0.081	-0.220			
		(0.162)	(0.180)			
	Female	-0.605	0.366	-0.297		
		(0.182)	(0.204)	(0.341)		
	Preference coefficient (ψ_j)	0.353	-0.430	0.024	-0.146	
		(0.075)	(0.082)	(0.126)	(0.145)	
				B. OLS VAM		
Mean		0	0.055	-0.007	0.056	
		—	(0.003)	(0.006)	(0.003)	
SD		0.070	0.056	0.058	0.029	
		(0.002)	(0.004)	(0.017)	(0.006)	
Correlation:	Above-median Math	-0.896				
		(0.075)				
	Black/Hispanic	0.122	-0.361			
		(0.175)	(0.207)			
	Female	-0.621	0.394	-0.274		
		(0.157)	(0.189)	(0.362)		

Notes: This table reports estimated distributions of school treatment effect parameters for high school graduation rates. 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 VAM regression in panel B 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. Control function models in panel A add controls for distance and predicted tastes for each school. Estimated standard deviations and correlations come from bias-corrected estimates of the variances and covariances of the interaction coefficients across schools. Standard errors are computed by the delta method. Selection on Gains Parameter (φ) Estimate under Control Function: 0.006 (Standard Error: 0.001)

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

		Intercept (1)	Above-median Math (2)	Black/Hispanic (3)	Female (4)	Preference coefficient (ψ_j) (5)
				A. Control function		
Mean		0	0.086	-0.005	0.072	-0.002
		—	(0.004)	(0.012)	(0.003)	(0.000)
SD		0.064	0.047	0.099	0.031	0.005
		(0.003)	(0.007)	(0.014)	(0.008)	(0.000)
Correlation:	Above-median Math	-0.654				
		(0.141)				
	Black/Hispanic	-0.077	-0.106			
		(0.122)	(0.194)			
	Female	-0.301	-0.054	-0.218		
		(0.167)	(0.255)	(0.285)		
	Preference coefficient (ψ_j)	0.370	-0.191	0.203	0.168	
		(0.098)	(0.130)	(0.123)	(0.160)	
				B. OLS VAM		
Mean		0	0.082	-0.020	0.068	
		—	(0.004)	(0.007)	(0.003)	
SD		0.073	0.049	0.088	0.032	
		(0.003)	(0.007)	(0.015)	(0.008)	
Correlation:	Above-median Math	-0.574				
		(0.119)				
	Black/Hispanic	0.071	-0.168			
		(0.126)	(0.204)			
	Female	-0.254	0.017	-0.142		
		(0.147)	(0.245)	(0.311)		

Notes: This table reports estimated distributions of school treatment effect parameters for college attendance rates. 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 VAM regression in panel B 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. Control function models in panel A add controls for distance and predicted tastes for each school. Estimated standard deviations and correlations come from bias-corrected estimates of the variances and covariances of the interaction coefficients across schools. Standard errors are computed by the delta method. Selection on Gains Parameter (φ) Estimate under Control Function: 0.005 (Standard Error: 0.001)

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	-0.029	-0.030	0.000	0.000
	SD	0.085	0.070	0.112	0.076
	Range	[-0.47, 0.44]	[-0.28, 0.37]	[-0.60, 0.53]	[-0.31, 0.37]
Average Treatment Effect					
	Mean	-0.032	-0.030	0.000	0.000
	SD	0.074	0.065	0.082	0.067
	Range	[-0.22, 0.29]	[-0.17, 0.23]	[-0.22, 0.29]	[-0.17, 0.23]
Match Effect					
	Mean	0.003	-0.001	0.000	0.000
	SD	0.058	0.035	0.076	0.036
	Range	[-0.42, 0.52]	[-0.24, 0.20]	[-0.48, 0.54]	[-0.26, 0.25]
			B. OLS VAM		
Treatment Effect					
	Mean	-0.034	-0.034	0.000	0.000
	SD	0.089	0.076	0.114	0.082
	Range	[-0.49, 0.44]	[-0.48, 0.33]	[-0.58, 0.46]	[-0.48, 0.33]
Average Treatment Effect					
	Mean	-0.033	-0.031	0.000	0.000
	SD	0.079	0.070	0.086	0.074
	Range	[-0.20, 0.27]	[-0.18, 0.23]	[-0.20, 0.27]	[-0.18, 0.23]
Match Effect					
	Mean	-0.001	-0.004	0.000	0.000
	SD	0.056	0.036	0.075	0.037
	Range	[-0.42, 0.53]	[-0.30, 0.29]	[-0.45, 0.55]	[-0.30, 0.37]

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 61,879, and the number for potential is 18,192,426.

Table B4—Descriptive Statistics for College Attendance Value Added

		Realized		Potential	
		Raw	Shrunk	Raw	Shrunk
		(1)	(2)	(3)	(4)
		A. Control function			
Treatment Effect					
	Mean	-0.012	-0.012	0.000	0.000
	SD	0.087	0.065	0.122	0.075
	Range	[-0.51, 0.69]	[-0.31, 0.40]	[-0.93, 0.76]	[-0.63, 0.43]
Average Treatment Effect					
	Mean	-0.017	-0.015	0.000	0.000
	SD	0.071	0.057	0.081	0.061
	Range	[-0.18, 0.33]	[-0.15, 0.28]	[-0.18, 0.33]	[-0.15, 0.28]
Match Effect					
	Mean	0.005	0.003	0.000	0.000
	SD	0.072	0.042	0.091	0.044
	Range	[-0.59, 0.87]	[-0.35, 0.51]	[-0.75, 0.87]	[-0.47, 0.51]
		B. OLS VAM			
Treatment Effect					
	Mean	-0.013	-0.013	0.000	0.000
	SD	0.093	0.074	0.125	0.083
	Range	[-0.53, 0.59]	[-0.35, 0.44]	[-0.75, 0.82]	[-0.64, 0.44]
Average Treatment Effect					
	Mean	-0.015	-0.013	0.000	0.000
	SD	0.079	0.067	0.088	0.072
	Range	[-0.22, 0.35]	[-0.17, 0.28]	[-0.22, 0.35]	[-0.17, 0.28]
Match Effect					
	Mean	0.001	0.000	0.000	0.000
	SD	0.069	0.042	0.089	0.042
	Range	[-0.61, 0.73]	[-0.38, 0.53]	[-0.69, 0.73]	[-0.47, 0.53]

Notes: The table displays the mean, standard deviation, and range of treatment effects, average treatment effects, and match effects for college attendance 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 61,879, and the number for potential is 18,192,426.

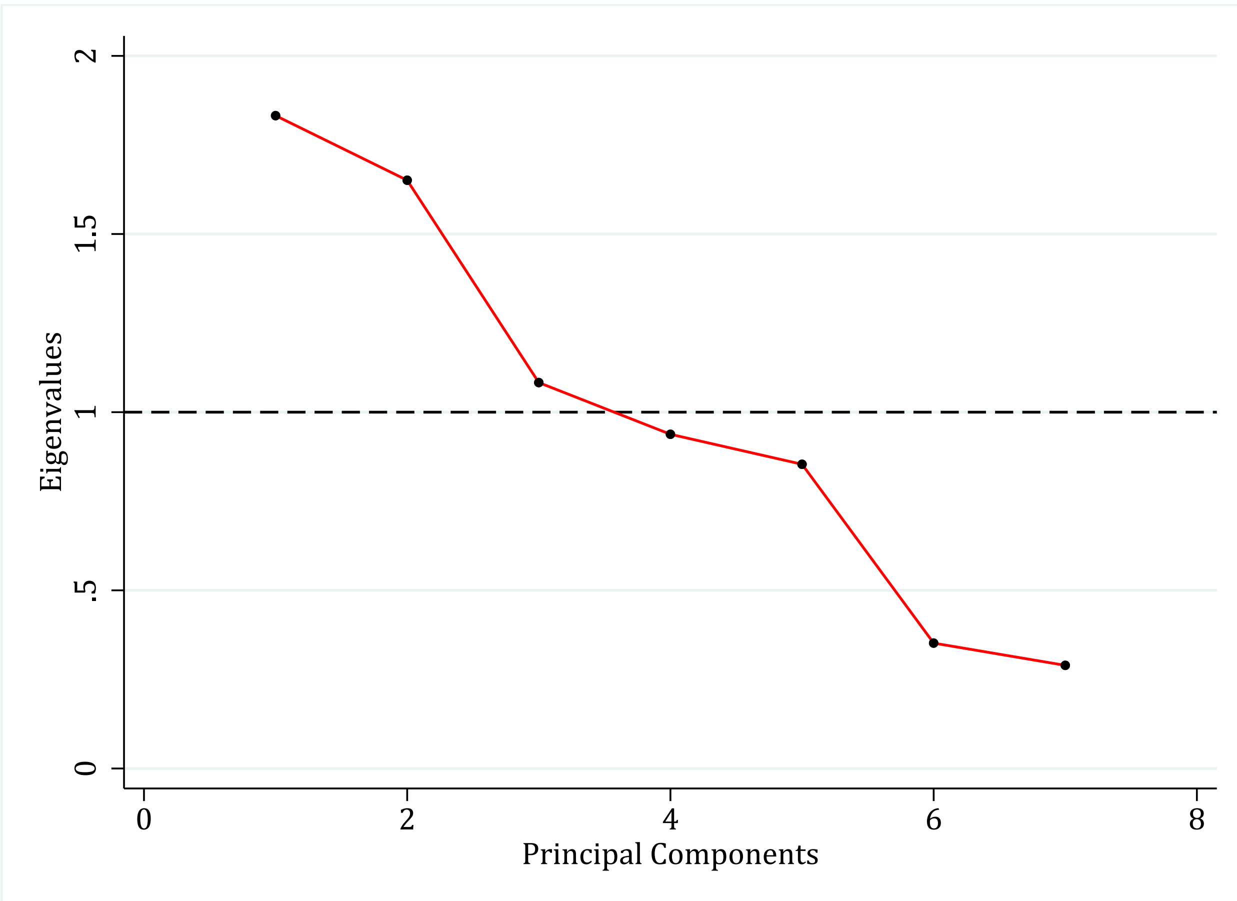
Table B5—Potential Gains from Reallocation Policies Relative to Status Quo (OLS VAM)

	Aligned Demand	Aligned Supply	Aligned Demand and Supply	Treatment Effect Maximization
	(1)	(2)	(3)	(4)
Math	0.041	0.053	0.069	0.091
HS Graduation	0.014	0.020	0.021	0.044
College Attendance	0.020	0.019	0.029	0.040

Notes: This table compares predicted changes in average student outcomes based on simulating reallocation policies. Estimates are predicted average outcomes minus average status quo outcomes. 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 has both students and schools rank each other based on treatment effects. Treatment Effect Maximization assigns students to schools to maximize the overall average outcome. Estimates come from value-added models omitting the unobserved taste controls.

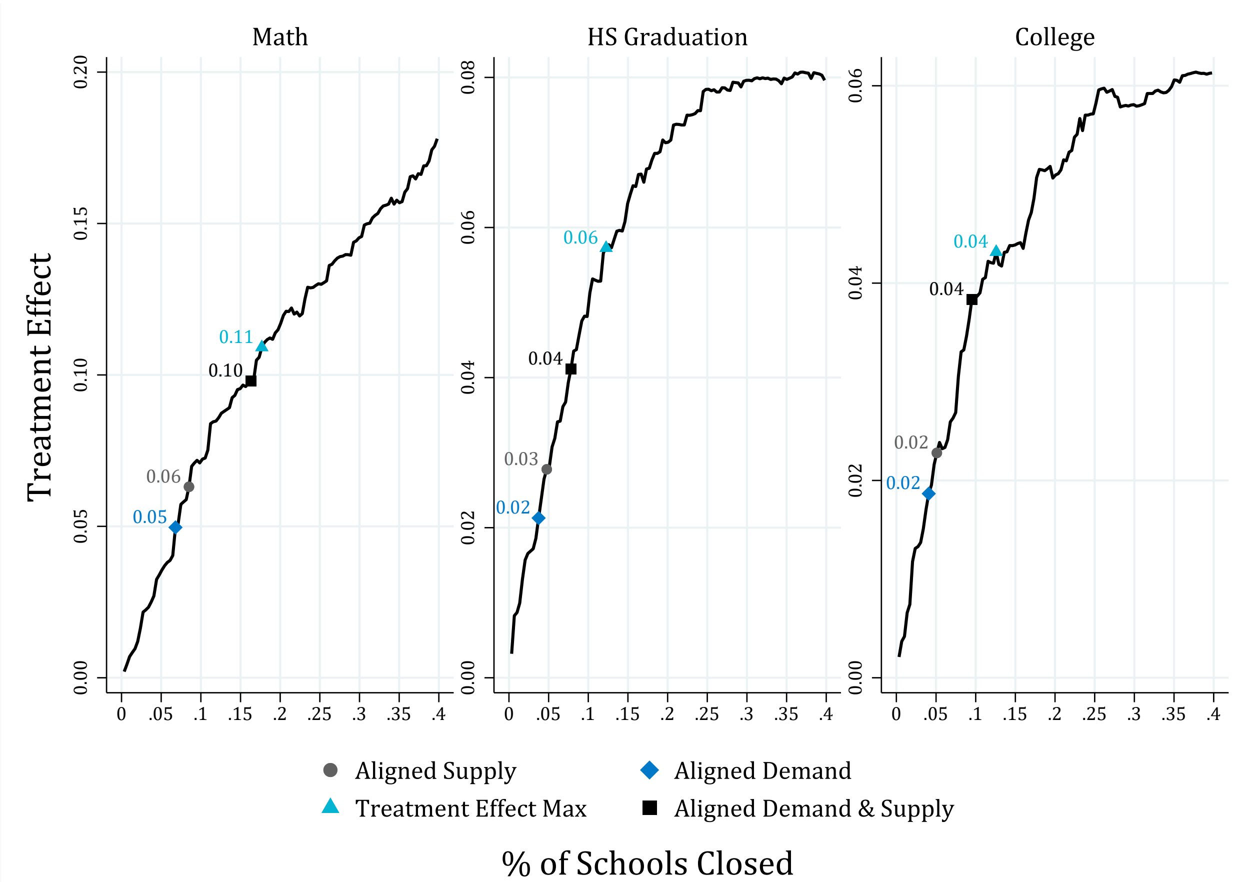
C Exhibits Based on Principal Components Analysis (PCA) Estimates

Figure C.1: Scree Plot of Eigenvalues



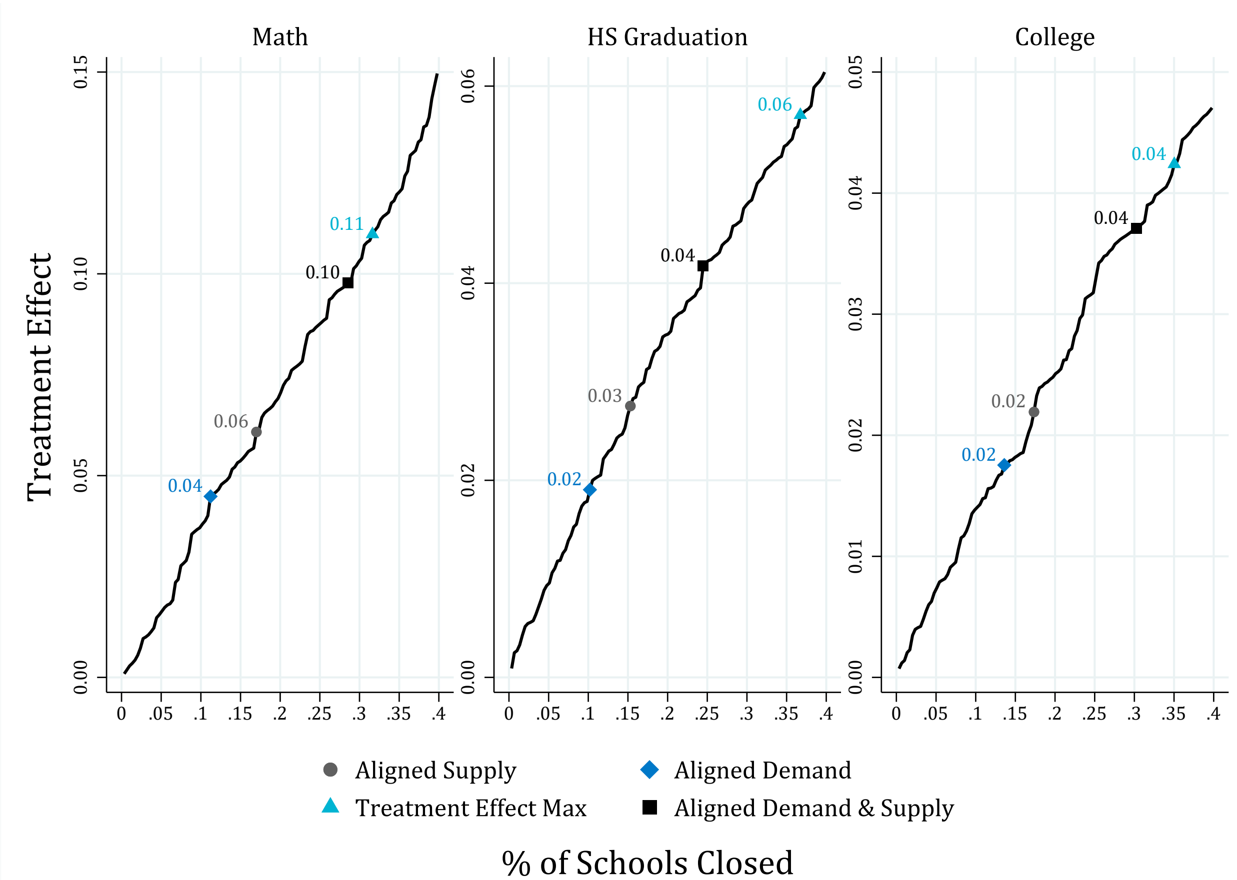
Notes: This table reports the scree plot of eigenvalues, used to choose PCA factors.

Figure C.2: Simulated Effects of School Closure Policies with Effective Replacement, PCA Estimates



Notes: This figure shows the fraction of schools that need to be closed to match the predicted treatment effects of reallocation policies. The simulated closure rule selects schools in order of posterior mean Average Treatment Effects (from lowest to highest). Capacity from the closed schools is distributed with an Effective Replacement rule that selects schools with highest posterior mean effectiveness, with capacity going to the same number of schools selected for closure (and allocated in proportion to baseline enrollment). Treatment effect estimates come from models with match effects parameterized by interactions with three PCA factors. For the High School Graduation outcome, the PCA specification produced a between-school covariance matrix that was not strictly positive definite. Because the empirical Bayes shrinkage formula requires inversion of this matrix, we applied a standard regularization step: the matrix was symmetrized and any eigenvalue below a small tolerance (1×10^{-8}) was replaced with that tolerance. In practice, this adjustment replaces one eigenvalue.

Figure C.3: Simulated Effects of School Closure Policies with Typical Replacement, PCA Estimates



Notes: This figure shows the fraction of schools that need to be closed to match the predicted treatment effects of reallocation policies. The simulated closure rule selects schools in order of posterior mean Average Treatment Effects (from lowest to highest). Capacity from the closed schools is distributed with a Typical Replacement rule that allocates seats to all schools in proportion to baseline enrollment. Treatment effect estimates come from models with match effects parameterized by interactions with three PCA factors. For the High School Graduation outcome, the PCA specification produced a between-school covariance matrix that was not strictly positive definite. Because the empirical Bayes shrinkage formula requires inversion of this matrix, we applied a standard regularization step: the matrix was symmetrized and any eigenvalue below a small tolerance (1×10^{-8}) was replaced with that tolerance. In practice, this adjustment replaces one eigenvalue.

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

		Intercept (1)	P1 (2)	P2 (3)	P3 (4)	Preference coefficient (ψ_j) (5)
Mean		0	0.668	0.124	0.069	-0.001
		—	(0.013)	(0.031)	(0.020)	(0.001)
SD		0.272	0.091	0.029	0.024	0.006
		(0.004)	(0.006)	(0.006)	(0.006)	(0.001)
Correlation:	P1	-0.062				
		(0.054)				
	P2	-0.032	0.742			
		(0.097)	(0.166)			
	P3	0.122	0.482	0.307		
		(0.123)	(0.217)	(0.324)		
	Preference coefficient (ψ_j)	0.118	-0.195	-0.164	-0.311	
		(0.071)	(0.116)	(0.185)	(0.237)	

Notes: This table reports estimated distributions of school treatment effect parameters for college attendance rates. School-specific estimates come from a regression of school indicators interacted with a constant (intercept) and the first three principal components. The VAM regression in panel B 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. Control function models in panel A add controls for distance and predicted tastes for each school. Estimated standard deviations and correlations come from bias-corrected estimates of the variances and covariances of the interaction coefficients across schools. Standard errors are computed by the delta method. Selection on Gains Parameter (φ) Estimate under Control Function: 0.006 (Standard Error: 0.002)

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	-0.002	-0.004	0.000	0.000
SD	0.253	0.240	0.307	0.273
Range	[-1.32, 1.16]	[-0.90, 1.08]	[-2.35, 2.25]	[-1.32, 1.61]
Average Treatment Effect				
Mean	0.015	0.011	0.000	0.000
SD	0.250	0.241	0.279	0.262
Range	[-0.77, 1.04]	[-0.67, 0.99]	[-0.77, 1.04]	[-0.67, 0.99]
Match Effect				
Mean	-0.017	-0.015	0.000	0.000
SD	0.117	0.084	0.129	0.076
Range	[-1.10, 0.90]	[-0.97, 0.65]	[-1.66, 1.82]	[-1.20, 1.10]

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 61,879, and the number for potential is 18,192,426.

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	0.044	0.061	0.097	0.109
HS Graduation	0.019	0.027	0.040	0.057
College Attendance	0.017	0.022	0.037	0.042

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 61,879.