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Teacher Labor Market Equilibrium and Student Achievement*

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Abstract

We study whether reallocating existing teachers across schools within a district can increase student achievement, and what policies would help achieve these gains. Using a model of multi-dimensional value-added, we find meaningful achievement gains from reallocating teachers within a district. Using an estimated equilibrium model of the teacher labor market, we find that achieving most of these gains requires directly affecting teachers' preferences over schools. In contrast, directly affecting principals' selection of teachers can lower student achievement. Our analysis highlights the importance of equilibrium and second-best reasoning in analyzing teacher labor market policies.

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Outcomes in matching markets depend on which agents match and to whom. In teacher labor markets, the literature has focused on the former, as large cross-sectional dispersion in estimated productivity naturally leads to proposals for replacing low-performing teachers (Chetty, Friedman and Rockoff (2014*b*), see Jackson, Rockoff and Staiger (2014) for a review).[1] Additional gains are possible on two margins through which within-district reassignments can produce aggregate gains: match effects and differential class sizes. If these two margins are quantitatively large, then changing where a teachers work could also be an important policy tool.

Yet reassigning teachers to achieve allocative gains is not easy because, unlike other inputs, teachers have preferences over assignments and may choose not to supply labor (Rothstein, 2015). Moreover, in most public school districts, salaries are set by rigid schedules (Biasi, 2021). Observed allocations therefore reflect an equilibrium of teacher preferences over amenities like student composition (Antos and Rosen, 1975), principal preferences, and market institutional features, including timing and equilibrium selection.

This paper explores the potential student achievement gains from within-district teacher reassignment and the effectiveness of combinations of different policy levers in achieving these gains. We have two central findings. First, because teachers vary in their absolute and comparative advantage, there is scope for meaningful gains from reallocating teachers within a district—the reallocation that maximizes achievement raises student test scores by 0.05 student standard deviations (σ) per student. These gains are significant relative to several benchmarks. Second, the most effective policies directly affect teachers' preferences over schools (using teacher bonuses). In contrast, directly affecting principals' selection of teachers (Ballou, 1996) can lower student achievement by giving highly sought-after teachers the ability to choose positions where they have a comparative disadvantage. This asymmetry in the effect of seemingly symmetric policies highlights the importance of equilibrium reasoning in policy analysis in this setting.

We arrive at these findings using an equilibrium model of the teacher labor market combined with novel data on job vacancies and applications. In our model, teachers and schools meet and form matches. Reflecting the rolling timing of the labor market, teachers may only match with school openings that are active at the same time. Teachers have non-wage preferences over the set of positions, and principals serve as hiring intermediaries who use their preferences to rank teachers on behalf of the district. Each match generates student achievement based on the absolute and comparative advantage of teachers. To predict the equilibrium matches, we use the concept of pair-wise stability (Roth and Sotomayor, 1992; Hitsch, Hortaçsu and Ariely, 2010; Banerjee et al., 2013; Boyd et al., 2013).

The model allows us to study several proposed or implemented policy interventions to increase student achievement. Policies that change the market's timing enter the model as changes to the

[1] For cross-sectional dispersion see, e.g., Hanushek, Kain and Rivkin (2004); Rockoff (2004); Chetty, Friedman and Rockoff (2014*a*).

subset of teachers and schools that meet, and we model changes to the market's institutions as selecting among the pair-wise stable equilibria.[2] Teacher and principal bonuses change teacher and principal preferences over matches.[3] Bonuses for match effects can be made large enough so that allocative efficiency perfectly shapes teacher and principal choice.

Even in the "best case" version of these policies, where all matches are available and teachers and principals only rank on output, there may still be unrealized gains from prices not being completely flexible. In a decentralized equilibrium, teachers are often assigned to schools based on absolute advantage, but the first-best allocation leans more heavily on comparative advantage.[4] Hence, we also consider unrestricted wages in each match that implement the first-best allocation by giving teachers incentives to seek out positions that make the most of their comparative advantage.

Estimating an equilibrium model allows us to characterize a variety of policy effects, especially in cases that do not lend themselves easily to program evaluation because the policies tend to be implemented in isolation, on a small scale, and with limited variation in policy parameters.[5] The complex equilibrium in the teacher labor market makes us cautious in extrapolating policy effects. In some cases, the sign of a simple policy's effect may be ambiguous, depending on the other policies in place.

To make these counterfactual predictions, the model highlights the four empirical objects we need to estimate: teachers' output from each potential assignment, teachers' non-wage preferences over positions, principals' non-wage preferences over teachers, and the timing of when teachers and positions are active. Inferring these objects from observed equilibrium allocations requires strong assumptions.

We therefore supplement data on actual assignments with novel data from the job application system of a school district in North Carolina. These data include the timing of all teacher appli-

[2]For example, some districts have focused on changing the algorithm that clears the market (Davis, 2021). Other districts allow some schools to hire first (Levin and Quinn, 2003; Kraft et al., 2020). Many districts, like New York City, have moved from teacher transfer priority based on experience to mutual consent where teacher and principal must each agree to the assignment (Daly et al., 2008).

[3]Examples of teacher-level output bonuses include Indiana (Marcotte, 2015) and the ProComp policy in Denver (Atteberry and LaCour, 2020). North Carolina implemented bonuses for teaching in hard-to-staff schools from 2001-2004 (Clotfelter et al., 2008) while South Carolina provides high poverty districts with funding for teacher bonuses (Fox, 2017). Examples of principal-level bonuses include North Carolina, which instituted principal bonuses as a function of the growth in student test scores in 2017-2018 (Pridemore, 2017), and New York City, which piloted a program giving principals information about their teachers' performance in 2007-2008 (Rockoff et al., 2012).

[4] To see why the stable and first best allocations can be different, suppose that teacher 1 has output $\{10,9\}$ at schools 1 and 2, respectively, and teacher 2 has output $\{8,0\}$ at schools 1 and 2. Then in any stable equilibrium where both teachers and principals only value output, teacher 1 is assigned to school 1 and teacher 2 is assigned to school 2. In contrast, in the first best, teacher 1 is assigned to school 2 and teacher 2 to school 1. This assignment reflects teachers' comparative advantage. If the comparative advantage of teacher 2 is strong enough, say, her output is $\{11,0\}$, then the decentralized and first-best allocations coincide.

[5]While a growing empirical literature evaluates the responsiveness of teacher labor supply to bonus or incentive schemes (Clotfelter et al., 2008; Falch, 2010; Steele, Murnane and Willett, 2010; Falch, 2011; Glazerman et al., 2013; Protik et al., 2015; Springer, Swain and Rodriguez, 2016; Cowan and Goldhaber, 2018; Feng and Sass, 2018), even in this literature, it is hard to find analogues to the types of targeted policies we consider.

cations to open vacancies and the outcome of each application (including whether the teacher was hired and whether the hiring principal rated the application positively).

Importantly, we also link the applicant data to the classroom assignment and student achievement data in North Carolina. These data allow us to characterize each teacher's (possibly multi-dimensional) value-added, and to estimate the joint distribution of preferences and value-added.

We start with the first empirical object: teachers' output from each potential assignment. We specify a multi-dimensional value-added model where teachers may have absolute advantage and comparative advantage in teaching specific student types (Condie, Lefgren and Sims, 2014; Delgado, 2021; Bau, Forthcoming; Biasi, Fu and Stromme, 2021). We divide students based on whether they are economically disadvantaged, and we find robustness to other forms of heterogeneity. We estimate that teachers' value-added across student types is highly dispersed across teachers and highly correlated within teacher. Nonetheless, we can reject a homogeneous value-added model.

We use these estimates to solve for the student achievement-maximizing allocation of teachers to positions and find gains of 0.054σ per student. Just under half of these gains are from sorting teachers based on comparative advantage (Delgado, 2021), while the rest comes from assigning high absolute advantage teachers to larger classes. These gains are the equivalent of 39% of a standard deviation in teacher forecasted value-added, or an additional year of experience for a novice teacher, and larger than the 0.012σ gains we estimate from replacing the bottom 5% of teachers with the median teacher (Staiger and Rockoff, 2010; Neal, 2011).

Despite a negative correlation between teacher experience and student disadvantage (Lankford, Loeb and Wyckoff, 2002; Clotfelter, Ladd and Vigdor, 2005), the actual allocation is close to equitable in value-added (similar to Mansfield (2015) and Angrist et al. (2021)). The allocation slightly favors economically disadvantaged students, with the potential gains for reallocation concentrated among advantaged students. The distributional consequences of gains from reallocation reflect two competing forces. First, high absolute advantage teachers tend to have comparative advantage in teaching economically disadvantaged students; and, second, advantaged students are in larger classes on average. The latter force dominates such that maximizing achievement sends more of the best teachers toward advantaged students.

We then turn to the second empirical object: teachers' non-wage preferences over positions (Barbieri, Rossetti and Sestito, 2011; Engel, Jacob and Curran, 2014; Bonhomme, Jolivet and Leuven, 2016; Fox, 2016; Johnston, 2021). Based on institutional features and analysis of application behavior, we argue that teachers apply non-strategically to positions they prefer relative to their outside option. We specify a rich characteristics-based model of teacher utility with observed and unobserved preference heterogeneity. Teachers prefer schools where they have comparative advantage (higher value-added), which might lead teachers to sort optimally if allowed to choose their positions. This pattern, however, is overwhelmed by teachers' preferences against schools with more economically disadvantaged students. High absolute advantage teachers have the lowest preferences for schools with more economically disadvantaged students, but we estimate a large random component to such preferences. In general, we find significant preference heterogeneity across teachers.

The third empirical object is principals' preferences over teachers (Ballou, 1996; Boyd et al., 2011; Jacob et al., 2018; Jatusripitak, 2018). Using the observed set of applications to each position, we model whether principals give an application a positive outcome: a positive rating without an interview, an interview without a hire, or a hire. We combine these outcomes because principals might interview or offer strategically by passing on preferred teachers who are unlikely to accept an offer. As long as these teachers receive at least a positive rating, then we can model positive outcomes as non-strategic choices. We estimate that principals prefer teachers who would produce high value-added in the position. Principals consider a variety of other observed and unobserved factors such that the highest value-added teacher does not always receive a positive outcome.

Finally, we estimate market timing based on applicants' and vacancies' periods of activity in our administrative records.

With these four empirical objects, we use our model to evaluate how far various policies could move the teacher allocation from the status quo toward the first-best. To characterize potential gains, we start with the maximal form of each policy. There is little role for equilibrium selection since there is nearly always a unique stable equilibrium. Complete market coordination such that teachers can apply to any position in a cycle—not just those concurrently active—has a somewhat larger effect, moving student achievement 15% of the way toward the first-best.

Principal output bonuses, when added to the status quo, actually lead to lower value-added, while teacher output bonuses yield about 75% of the total potential achievement gains. The remaining gains come from completely flexible prices that vary with potential output at all matches, not just at the assigned ones.

The surprising negative effect of principal output bonuses reflects the theory of the second-best (Lipsey and Lancaster, 1956). We have documented that both principal and teacher preferences are not aligned with the planner. By standard intuition, aligning either one in isolation should improve outcomes. Instead, aligning principal preferences alone lowers student achievement. The reason is that principal bonuses lead to more homogeneous rankings of teachers so that the highest absolute advantage teachers have many options to choose from. When teachers rank schools according to their estimated preferences, in which output plays a small role relative to a school's student body composition, increased choice leads to misallocation.

Of the policies we consider, only teacher bonuses have the potential to close most of the gap between the status quo and output-maximizing allocations. We end by considering realistic bonus schemes that pay teachers a piece rate bonus and a participation subsidy which guarantees that all teachers are weakly better off. We find that when teacher bonuses are added to the status quo, targeting value-added is, unsurprisingly, the most cost-effective way to produce value-added. When principals *also* have output bonuses, teacher bonuses that target teaching economically disadvantaged students are more cost-effective in producing value-added because they more quickly counteract teachers' preferences toward advantaged students. More generally, we find that teacher bonuses are considerably more costly than flexible prices that implicitly only pay more to teachers with the weakest preferences for their optimal assignment.

We show that our main results are robust to sensible permutations in the value-added model (what student characteristics matter for match effects, whether our model of match effects understates the total match component), the teacher preference model (smaller or larger empirical choice sets, the form of heterogeneity), and the principal preference model (modeling hires instead of positive outcomes, smaller or larger empirical choice sets).

Our paper fits in an emerging literature that uses teacher labor market equilibrium models to assess the gains in student achievement from various policies. These papers range in the allocation problem they consider from national (Combe, Tercieux and Terrier, Forthcoming; Bobba et al., 2021; Combe et al., 2021) to state cross-district (Biasi, Fu and Stromme, 2021) to local within-district (Boyd et al., 2013; Laverde et al., 2021) to sectoral (Tincani, 2021).[6] Our unique combination of detailed data on teacher applications, principal ratings, and student-teacher classroom assignments allows us to identify a two-sided heterogeneous preference and multi-dimensional production model with straightforward assumptions on behavior.

1 An equilibrium model of the teacher labor market

We begin with an equilibrium model of the labor market for teachers. The model allows us to define the school district's (first-best) allocation problem and the decentralized equilibrium. We then discuss how we use the model to consider the impact of various policies to affect the matching process. The model highlights the empirical objects that we estimate in Sections 3, 5, and 6.

We defer a full description of the empirical setting to Section 4, but we highlight a few features that inform the model. First, the teacher labor market operates on a rolling basis from April to August each year. This segments the market in time. Second, the market is decentralized such that teachers choose which positions to apply to, and principals choose whom to interview and then whom to offer jobs. Finally, a teacher's wage does not vary based on the offer she accepts.

1.1 Set-up

We begin with specialized notation that we generalize in the next section. Denote teachers by j, and schools and principals by k (for notational compactness, we consider single-position schools).

[6] Bau (Forthcoming) studies an equilibrium model of school competition with school-student match effects. A broader literature considers non-education allocation problems with non-choice outcomes (Agarwal, Hodgson and Somaini, 2020; Ba et al., 2021; Cowgill et al., 2021; Dahlstrand, 2021).

Teachers and principals both receive quasi-linear utility from an assignment. Teacher j derives utility u_{jk} from teaching at school k:

$$u_{jk} = \tilde{u}_{jk} + w_{jk}, \tag{1}$$

where w_{jk} is the wage, and $\tilde{u}_{jk}$ is a match-specific amenity. School k (or the principal who runs it) derives utility, v_{jk}, from hiring teacher j. This utility is linear in a non-wage component less the wage paid to teacher j:

$$v_{jk} = \tilde{v}_{jk} - w_{jk}. \tag{2}$$

A teacher-school assignment produces student value-added VA_{jk}. Below, we specify a functional form for VA_{jk}.

Finally, let $\mathcal{J}$ be the set of teachers, $\mathcal{K}$ be the set of schools, and assume for simplicity that the number of teachers and schools is the same. An assignment of teachers to classrooms is a one-to-one and onto function (bijection): $\phi : \mathcal{J} \to \mathcal{K}$ so that $\phi(j) = k$, the school k to which teacher j is assigned. Denote by Φ the set of all possible assignments.

1.2 First-best problems

We consider a school district's first-best assignment problem, where the district values students' outcomes and teachers' preferences over assignment (non-wage utility):

$$\max_{\phi \in \Phi} \{\omega \sum_{j \in \mathcal{J}} VA_{j\phi(j)} + \sum_{j \in \mathcal{J}} \tilde{u}_{j\phi(j)}\}. \tag{3}$$

To understand this allocation problem, note that the the first term ($\sum_{j \in \mathcal{J}} VA_{j\phi(j)}$) is the output achieved given an assignment ϕ. The second term ($\sum_{j \in \mathcal{J}} \tilde{u}_{j\phi(j)}$) is the total amenity value that teachers gain from this allocation. Finally, ω is the weight that the district places on student achievement relative to teacher preferences.

We exclude principal preferences from the district's value of an allocation to focus on the essential elements of the problem. Specifically, the district could plausibly bypass the intermediary of the principal and direct schools on whom to hire. In this sense, we do not commit to a utility interpretation of principals' preferences, and could instead interpret them simply as a hiring rule.

We consider a range of district first-best problems where the relative weight on students varies. We refer to the resulting set of optimal allocations as the production possibilities frontier. The slope of the frontier captures the trade-off between student achievement and teacher utility.

1.3 Decentralized equilibrium

We have three agent types (teachers, principals, and students) with asymmetric roles. Teachers and principals/schools determine the allocation, and the district values the allocation based on teacher and student payoffs.

To characterize which allocations are implementable with different policy tools, we use a decentralized matching model. Schools meet with all teachers who are in the market at the same time. The equilibrium concept is pair-wise stability. Under a stable allocation, no teacher and school pair would prefer to jointly deviate and match (Roth and Sotomayor (1992), Definition 2.3).

To model the empirical status quo, we assume (1) teachers and principals have the preferences we estimate for them and (2) the timing of the market follows that which we observed in the administrative records, where not all matches are feasible. There is not necessarily a unique stable equilibrium. We model the status quo using the teacher-proposing deferred-acceptance algorithm (DA).

1.4 Policies

We are interested in the effects of five policies, both by themselves as well as their combination. The first two policies affect market institutions. The first policy is equilibrium selection. The teacher-proposing DA is the teacher-optimal stable allocation (Roth and Sotomayor (1992), Corollary 2.14) meaning every teacher would (weakly) prefer their assignment to that in every other stable allocation. In contrast, the school-proposing DA (where schools only value output) is the best stable allocation in terms of student achievement. Thus, a policymaker might wish to shift the effective equilibrium. The second policy is market coordination. We model this policy by expanding each teacher's (school's) choice set to include all openings (candidates) available at any time during that cycle.

The next two policies affect the choices of agents over matches. The third policy provides teachers with output bonuses. We simulate the effect of an extreme version of this policy in which teachers' only preference is to go where they maximize student achievement (in Section 8 we consider intermediate versions of this policy). Parallel to teacher bonuses, the fourth policy provides principals with output bonuses.

Finally, all four of these policies—even in combination—are not necessarily sufficient to achieve the first-best allocations described in the previous section. The reason is that even when teachers and principals only value output, the allocation sorts teachers based in part on absolute rather than comparative advantage. To achieve the first-best allocations, it is sufficient to have the following combination of policies: (1) districts compensate principals for output (so that principals rank teachers by match-specific value-added, VA_{jk}), (2) all teachers and schools are in the market simultaneously, and (3) wages may vary with each teacher-school pair. This last policy makes utility transferable. Whereas output bonuses only let wages vary depending on the output in the assigned position, flexible wages would let wages depend on a teacher's output in other assignments, teacher preferences, and the distribution of other teachers' potential output and preferences. Having this flexibility guarantees that the district can implement any first-best allocation (Shapley and Shubik, 1971).

Other than for equilibrium selection, there is no theorem that the other policies in isolation necessarily improve output. The theory of the second-best states that when we are away from the first-best allocation because of multiple factors, then fixing any one factor can worsen outcomes.

1.5 Empirical plan

The model highlights the empirical objects we need to estimate to be able to simulate the impact of the above policies. We start by estimating the potential outcomes of teachers across schools, VA_{jk}. We then estimate teachers' amenity value across all assignments, $\tilde{u}_{jk}$, and principals' non-wage utility from hiring each teacher, $\tilde{v}_{jk}$. Finally, we model which positions were available to each teacher in the observed equilibrium, which we read directly from administrative records.

2 Data

To estimate the objects of interest from the previous section, we use rich data on the labor market for teachers. The first type of data comes from the platform used to hire teachers in our focal district. We use this data to estimate teacher and principal preferences. The second type of data comes from staffing and achievement records from state accountability records. This data provides us with student-level test score data that we link to teachers and use to estimate value-added models. In addition, these records provide information about a variety of demographic characteristics of teachers and students as well as teachers' education and experience in the district. In this section, we briefly describe the data. See Appendix A for further details and Appendix Table A1 for summary statistics across samples.

2.1 Job application and vacancy data

We obtained application records from our focal district's system, which spans 2010 through 2019 and records 346,663 job applications. In the system, schools post job vacancies, and applicants apply for jobs. The system also records various actions that principals take.

For every posted position, the vacancy files indicate the school, position title, and whether the position is full-time or part-time. We use the detail on the position title to isolate non-specialized elementary school teacher jobs (i.e., we omit elementary school jobs such as "literary facilitator elementary").

We use two features of the teacher file. First, the file records which vacancies the candidate applied to, and when she submitted the application. The timing information allows us to construct choice sets, which we detail in Section 4. Second, the file records the city, zip code, and, in some cases, exact address where the teacher lives. This feature allows us to construct the commute time for each teacher-position combination.

We also have data in which principals record their assessments of teachers. Principals record their interest in different applicants, the equivalent of a "good" and a "bad" pile. Principals also often record which candidates they invited to interview, which candidates were offered the position, and which candidates were hired.

2.2 Administrative data

We link the platform data to state administrative records on teachers and students. For teachers, we have their experience, salary, licensing, certification scores, class assignments, and the school where they work. For students, we have scores on standardized exams, grades, race, sex, and whether they qualify as disadvantaged based on Federal programs. Records on class assignments allow us to link teachers to students.

The North Carolina Education Research Data Center (NCERDC) matched the data from the job-market platforms to the state's administrative data. They performed an interactive fuzzy match using names and birth year. For teachers who had a sufficiently good match (that is, a unique name-birth-year combination), we have a de-identified ID that allows us to connect their platform data to their staffing records and students' achievement.

3 Production of student achievement

In this section, we first specify the production model, which generates a low-dimensional form of match effects between teachers and schools. Second, we describe our three-step estimation procedure and discuss parameter estimates. Third, we present a range of validation checks. Finally, we use our estimates to show that there are meaningful efficiency gains from reallocating teachers across schools.

3.1 Model

Our model needs to predict how teacher output would change depending on the teacher-school match. Given the heterogeneity of student compositions across schools and the quickly expanding literature documenting match effects, we allow for comparative advantage (Dee, 2004, 2005; Jackson, 2013; Aucejo et al., 2021; Delgado, 2021; Graham et al., 2020; Biasi, Fu and Stromme, 2021).

We specify a model of match effects that is identified with our data and allows us to make output predictions in unobserved matches. Since a teacher typically works in just a few schools during her career, we cannot identify fully flexible match effects. Instead, we use low-dimensional match effects where teachers have different value-added with students of different observable types; here, we focus on a single student characteristic—economic disadvantage. Thus, a teacher's school-level match effect depends on the observable demographic composition of the school and the teacher's comparative advantage with each type of student.

We use notation that follows Chetty, Friedman and Rockoff (2014*a*) and Delgado (2021). Let i index students and t index years, where t refers to the spring of the academic year, e.g., 2016 refers to 2015-2016. Each student i has an exogenous type $m(i,t) \in \{0,1\}$ in year t (whether the student is economically disadvantaged or not). Student i attends school $k = k(i,t)$ in year t and is assigned to classroom $c = c(i,t)$. Each classroom has a single teacher $j = j(c(i,t))$, though teachers may have multiple classrooms.

Student achievement depends on observed student characteristics, teacher value-added, school effects, time effects, classroom-student-type effects, and an error term. Formally, we model student achievement A^*_{it} as:

$$A^*_{it} = \beta_s X_{it} + \nu_{it} \tag{4}$$

where X_{it} is a set of observable determinants of student achievement and

$$\nu_{it} = f(Z_{jt};\alpha) + \mu_{jmt} + \mu_k + \mu_t + \theta_{cmt} + \tilde{\varepsilon}_{it}. \tag{5}$$

Here, Z_{jt} is teacher experience (and f maps experience into output) and μ_{jmt} is teacher j's value-added in year t for student type m, excluding the return to experience. As in Chetty, Friedman and Rockoff (2014*a*), we allow a teacher's effectiveness to "drift" over time. μ_k captures school factors, such as an enthusiastic principal, while μ_t are time shocks. θ_{cmt} are classroom shocks specific to a student type, and $\tilde{\varepsilon}_{it}$ is idiosyncratic student-level variation.

We make three assumptions, which are standard in the literature (see Appendix B for formal statements of these assumptions). The first assumption is that classroom-student type shocks and idiosyncratic student-level variation are orthogonal to teacher and school assignments and follow a stationary process. We allow classroom shocks to be correlated across student types in the same classroom, but restrict all cross-classroom or cross-year correlations in shocks to be zero.

The second assumption is that the non-experience part of teacher value-added for each student type follows a stationary process that does not depend on the teacher's school. We also assume that the covariances between the teacher's value-added across student types depend only on the number of years elapsed.

The third assumption is that drift and school effects are independent. This assumption rules out teacher mobility (or initial assignments) related to the drift of the teacher's effect. We still permit teacher-school assignments to be non-random, and quite possibly related to a teacher's comparative advantage in teaching different student types.

Our object of interest is a forecast of teacher j's value-added from a hypothetical assignment to a new classroom (or set of classrooms) in school k. Define p_{kmt} as the proportion of type-m students in school k in year t. Given our low-dimensional model of match effects, a teacher's predicted mean value-added at school k in year t is:

$$VA^{p}_{jkt} = p_{k0t}\mu_{j0t} + p_{k1t}\mu_{j1t} + f(Z_{jt};\alpha), \tag{6}$$

such that a teacher's total value-added for n_{jkt} students is $VA_{jkt} = n_{jkt}VA^{p}_{jkt}$. We use data through $t-1$ from the whole state to forecast VA^{p}_{jkt} for assignments we see in the data and for counterfactual assignments. For the observed assignments, we forecast the teacher's value-added were she to receive a new draw of students and classrooms at that school. For the counterfactual assignments, we predict a teacher's value-added for schools at which she did not teach.

3.2 Estimation

We estimate our model in three steps using math scores.[7] In the first step, we estimate the coefficient on characteristics by regressing test scores (standardized to have mean 0 and standard deviation 1 in each grade-year) on a set of student characteristics and classroom-student-type fixed effects. In the second step, we project the residuals (A_{it}) onto teacher fixed effects, school fixed effects, and the teacher experience return function. In the final step, we form our estimate of teacher j's value-added (net of experience effects) in year t for type m (μ_{jmt}) as the best linear predictor based on the prior data in our sample. Since in this final step we shrink the estimates, we understate the dispersion in match effects relative to the true dispersion. That said, using shrunken estimates and prior data means that we use the information available to policy-makers. See Appendix B.2 for estimation details and a discussion of what variation pins down parameters.

The first key parameter estimate is the significant dispersion in value-added for both student types of about 0.24σ. The second key parameter estimate is the strong correlation of 0.86 between the teacher's value added with the two types of students (Appendix Table A2). We find large returns to experience in the first year, and then a profile that flattens out after about four years of experience (Appendix Table A3). Appendix Figure A1 plots the drift parameters.

[7] Focusing on a single subject allows us to rank all possible levels of output. We follow Biasi, Fu and Stromme (2021) in choosing math because it is typically more responsive to treatment (e.g., Rivkin, Hanushek and Kain (2005), Kane and Staiger (2008), and Chetty, Friedman and Rockoff (2014*a*) for evidence).

3.3 Validation of the match effects model

To test whether our estimates of teacher comparative advantage with different types of students simply reflect statistical noise, we perform three tests of our multi-dimensional value-added model versus a single-dimensional model. First, we estimate confidence intervals for the structural parameters in our production model. The 95 percent confidence interval allows us to reject a correlation coefficient of 1. Second, we perform a likelihood-ratio test comparing our model to a model with one-dimensional teacher value-added. We reject the homogeneous value-added model in favor of the heterogeneous model with a high degree of significance. Third, we test whether teachers who have previously been stronger with disadvantaged students see increases in estimated value-added when transferring to schools with greater shares of disadvantaged students. Similarly, we test the reverse relationship. If our comparative advantage estimates only reflected spurious relationships, then they would not predict changes in output upon transfer. In both cases, we find statistically strong evidence that this relationship indeed holds. See Appendix B.3 for further details on all three tests.

To validate our value-added model, we slightly modify Chetty, Friedman and Rockoff (2014*a*)'s test for mean forecast unbiasedness. We predict a teacher j's value-added in school k in year t (μ_{jkt}) using data from all years prior to t. We then regress the realized mean student residuals in year t ($\bar{A}_{jt}$) and test whether the coefficient on our prediction equals 1. Column (1) of Table 1 shows that the math value-added estimate is an unbiased predictor of residualized output, with a tight confidence interval around 1.05. Figure 1 shows that the forecast unbiasedness holds throughout the distribution of teacher value-added.

We conduct a similar test for the comparative advantage component of value-added, which will be important for the potential reallocation gains. If teachers' heterogeneous effects by student type vary with the environment—for instance, teachers might target instruction toward the median student in the class—then our model may poorly forecast a teacher's comparative advantage. In column (2) we compare our forecast of the difference in a teacher's value-added across (economically) disadvantaged and advantaged students with the realized test score difference. Again, we find that our estimates are nearly forecast unbiased. Appendix Figure A2 shows that the forecast unbiasedness holds throughout the distribution.

We perform three tests of whether our measure of teacher value-added also forecasts output across the types of teacher moves that we consider in counterfactuals. Our motivation in specifying a low-dimensional model of match effects is that we do not observe a teacher's potential outcomes at all schools, and so we cannot directly assess the quality of our model across all potential outcomes. What we can do, however, is look at the types of changes in the data that are closest to those that we contemplate in counterfactuals. First, we consider moving teachers across schools. Second, we consider moving teachers across classrooms (schools) with large changes in classroom composition in terms of advantaged and disadvantaged students. Third, we consider moving teachers across classrooms (schools) with different numbers of students.

How well do our measures predict output when teachers switch schools? In column (3) of Table 1 we show how output changes when teachers change schools and find no systematic change in value-added after transferring.[8] We also test how well our value-added estimates predict transfer effects in column (4). We find that our value-added measure is mean forecast unbiased, as we estimate a prediction coefficient of 0.97 ($1.060-0.885$), which is not statistically different from 1.

How well do our measures predict output when there are large changes in student composition? We split the data into three groups based on the size of the change between the estimation sample (before year t) and the prediction sample (year t) in the share of disadvantaged students. To examine the validity of our prediction in extreme reassignments, we look at changes below the 10th percentile, above the 90th percentile, and between the 10th to the 90th percentiles. For large negative changes (in Column (5) of Table 1), we find that our measure is forecast unbiased while, for large positive changes, we find a small forecast bias of 7%.

How well do our measures predict output when there are large changes in class size? We perform a parallel analysis for class size as we did for student composition and find similar answers. Specifically, in Column (6) of Table 1 we find that for large negative changes our measure is forecast unbiased, while for large positive changes we find slight evidence of forecast bias.[9]

Our parsimonious model's ability to predict value-added across settings with minimal bias instills confidence that we can predict the production effects from counterfactual allocations of teachers to schools.

3.4 Gains from alternate allocations

We solve the district's problem in Equation 3 where the district only values student output. Table 2 shows that there are sizeable output gains from hypothetical re-allocations of all teachers in our district in 2016. To focus solely on gains from reallocating teachers across schools, we give each classroom within a school the same composition and number of students. The top panel shows per-student gains or losses (column 1) from movements to the output-maximizing ("best") or output-minimizing ("worst") allocations. The per-student gain of 0.054σ in the output-maximizing allocation reflects improved sorting of teachers to schools without changing the set of available teachers. The actual allocation is only slightly better than randomly assigning teachers to schools (row 2 of Table 2), and the range of annual output between the best and worst allocations is 0.11σ

[8] In the same setting but in an earlier sample, Jackson (2013) estimates increases in output following transfers. In our later sample, we see zero or negative effects when estimating Jackson (2013)'s event study specification (Appendix Figure A3).

[9] The regressions assess forecast unbiasedness with a linear model. We show how our predictions perform nonparametrically in Appendix Figures A4a and A4b for large decreases and increases in class size, respectively. We see that our predictions are forecast unbiased throughout the quality distribution such that quality differences across teachers are likely to remain across schools with different class sizes. Note that we are not ruling out class size effects, but rather that these enter the school fixed effects, and we assume that class size effects do not interact with the teacher's identity.

(row 1 minus row 3 of Table 2).

Are these gains large or small? One way of contextualizing these gains is to compare them with the cross-sectional standard deviation of predicted teacher value added in our district, which is about 0.14σ (see Appendix Figure A5 for the distribution). The effect of the first-best reallocation is to increase teacher output by about a third of a standard deviation.

Another way of contextualizing the size of the gains is to compare them to the impacts of two commonly-proposed policies or allocation rules. Re-allocations within schools—i.e., matching teachers based on within-school variation in classroom composition—would only achieve 28% of the gains from the cross-school gains (row 4). Replacing the bottom 5% of teachers with teachers of median quality (as in Hanushek (2009) and Chetty, Friedman and Rockoff (2014*a*)), would achieve 22% of the gains from better sorting of the existing teacher pool (row 5), where we rank teachers based on their forecasted value added in their actual assignment.

In our reallocation analysis, we move more than 5% of teachers, so this comparison to teacher replacement policies might seem unbalanced. In Appendix Figure A6, we show that even replacing all below median teachers would achieve per-student gains of less than 0.054σ. Going the other way, in Appendix Figure A7 we show that reassigning just 10% of teachers delivers gains of over 0.02σ per student and full gains are nearly realized once 80% of teachers are reassigned.

In terms of distributional consequences, the first-best allocation entails larger gains for "advantaged" students than for disadvantaged students. The top panel of Figure 2 shows that schools with more disadvantaged students have smaller class sizes, and thus the first-best allocation favors advantaged students who tend to be assigned larger classes. At the same time, the bottom panel shows that teachers with high absolute advantage also tend to have a comparative advantage for disadvantaged students, so there is also a reason to send the best teachers to disadvantaged students. Here, we find that the first data pattern (the relationship between advantaged students having larger classes) dominates so that non-disadvantaged students have larger gains in the first-best allocation.

These distributional consequences come from a policy where the district weighs student types equally. If the district cared only about a single student type, then it could achieve large gains for that type. The bottom panel of Table 2 shows that targeting non-disadvantaged students would yield a 0.137σ per-student gain. But the non-targeted group sustains significant losses such that overall per-student gains are 30-45% of the potential efficiency gains.

The overall gains come (1) from sorting teachers to schools based on comparative advantage and (2) from placing high absolute advantage teachers in schools with larger class sizes (see Appendix Figure A8). These assignments may be very different from the ones in the data, which introduces two concerns. First, if our "reassignments" are farther away than the in-sample variation we use to validate our value-added model, then we may be less confident in our output predictions. We find that while some teachers end up in classrooms with different composition or sizes from the ones where we observe them, this variation is still within the support of our data (see Appendix Figure

A9). Second, by moving teachers across different student types, we are relying on the cardinality in the test score measures. As an alternative way to scale test scores, we express them in percentiles and find that the predicted gains from reallocation are nearly identical (see Appendix Table A4).

We include sorting based on class size for three reasons. First, it is a feature of the environment: there is class size dispersion and we show in Appendix C that this variation is persistent over time.[10] Second, teachers differ in their absolute advantage such that reassignments based on class size have the potential to matter for student achievement. Third, our validation exercise found that our output measures were mean forecast unbiased across class size changes. But because some readers may prefer reassignments based solely on comparative advantage, in row (4) we present the potential gains where we impose constant class sizes across all schools (see Appendix Table A5 for the full set of constant class size results). We estimate gains of 0.021σ, which are nearly identical to Delgado (2021)'s estimates using race-based match effects. These gains are 38% as large as the gains that incorporate class size variation. Thus, the potential to sort solely on comparative advantage remains economically meaningful.

Our specification likely misses some match effects.[11] We find similar results when we allow for match effects to vary with different student observable characteristics. In Appendix Table A7 we show the structural parameters we estimate from a model where student types are summarized by race (White or non-White) or lagged math achievement (above or below median). In Section 7.4 we show that our allocation conclusions are similar for these other forms of heterogeneity. Further, we conduct a simulation exercise where we allow our modeled form of match effects to be incomplete. Specifically, we add i.i.d. match effects and assess how the potential gains vary with the size of the unmodelled match effect. We present the result in the top panel of Appendix Figure A10 and find that the potential gains only increase, such that our results may be a lower bound. We also find that our results from Section 7 are qualitatively unchanged.

4 The vacancy posting, application, and hiring process

We focus on the market for elementary-school classroom teachers for two reasons. First, teachers in these positions are typically responsible for instruction in the tested subjects and thus we can infer their quality from systematic gains in their students' test scores. Second, because these positions also have common certification requirements, we can reliably classify which teachers are eligible for the position.

[10] In that Appendix, we also show that there are not systematic patterns of teachers "bargaining" over assignments within schools: i.e., we show that newly hired and more experienced teachers are not assigned smaller classes or fewer disadvantaged students within a school.

[11] A form of potential match effects we have not included are same-race (between teacher and student) match effects (Dee, 2004, 2005; Gershenson et al., 2018), and same-gender match effects (Dee, 2005; Lim and Meer, 2017). In our data, we find minimal evidence of same-race or same-gender effects (see Appendix Table A6). A form of match effects we cannot test for in our data is that of teaching practices discussed in Aucejo et al. (2021) and Graham et al. (2020).

4.1 Market overview

Our district organizes a decentralized hiring and transfer process in which teachers choose where to apply and principals choose whom to hire. External and internal (transfer) applicants are pooled into one market. Here we describe the basic market structure.

Market organization: The school district runs a centralized online hiring platform. Each school posts its openings on the platform, and teachers choose whether to apply to each posting.

Timing: We examine the "on-cycle" part of the market, which dictates hiring and transfers between school years. It begins in the winter, when the district notifies each school of known and expected attrition among the school's work force and of how many positions that school may hire. It ideally ends with filled positions by late August before the new school year. Similar to Papay and Kraft (2016), some schools are unable to fill all positions by the start of the new school year.

Postings: The number of postings at a school reflects a combination of enrollment, budget, and the number of teachers who leave. All three pieces of information are not necessarily known before the main hiring season starts. This information delay generates variation within and across schools in the timing of postings. For example, late information about enrollment or budget fluctuations often necessitate late posting. Or if there is mid-year attrition, then the school would know long before hiring season started that there would be a vacancy, which allows for early posting.

Applications: An application consists of a variety of documents including a teacher certification and a brief diversity statement. The same set of documents applies to all positions. Thus, a prospective teacher faces a fixed cost of applying.

Evaluation and hiring: When a teacher applies to a position, the hiring school receives her application materials through the platform. The school's principal may then rate the applications and choose to interview applicants on a rolling basis. For known positions at the beginning of the hiring period, there is a short window during which only transfers from within the district are able to apply. Schools can either hire from this pool or wait and consider more applicants.

If the principal wants to hire the candidate, she extends a job offer. The candidate has 24 hours to accept the offer, and if the teacher accepts, she commits to not accepting an alternate offer in the same cycle.

4.2 Empirical features and implications for modeling teacher applications

We document eight empirical patterns (or features of the market) that inform how we model the labor market.

The first set of features leads us to treat teacher applications as non-strategic. The natural alternatives to non-strategic applications would be a portfolio choice problem (Chade, Lewis and Smith, 2014), possibly involving a waiting strategy. A portfolio choice problem would arise through positive marginal costs of each application or other interactions across applications. The following two features are inconsistent with these rationales for modeling applications as a portfolio choice problem:

Feature #1: The marginal cost of applications is essentially zero. Applying amounts to clicking a button that sends the standardized materials to the particular position. Indeed, a teacher certifies that she will not dis-intermediate the process. The website asks a teacher to sign the following statement: "I understand that I should not send materials to individual hiring managers or principals."

Feature #2: Principals do not see what other applications a teacher submits.

Some versions of waiting strategies amount to dynamic portfolio management, and so the previous two institutional features push against these being empirically relevant. More generally, a waiting strategy would be sub-optimal in the sense that a teacher could miss many potentially desirable vacancies because of the following feature:

Feature #3: Posting, applications, and hiring happen on a rolling basis throughout the hiring season. From April to August, both sides of the market operate on a rolling basis (Appendix Figure A11). The left columns in Table 3a show that the modal month for posting is June, with only 16% posting in April. The middle columns show that applications lag postings. The right columns show that hiring occurs on a rolling basis and tends to lag posting by about a month. Over half of hires are made by the end of June, even though over a quarter of positions have yet to be posted.

In practice, teacher application behavior appears inconsistent with a waiting strategy as the following feature shows:

Feature #4: Teachers who are on the platform apply to vacancies very soon after they are posted. To characterize the timing of applications, we construct a measure of a teacher's wait time to apply to a vacancy. The wait time is the time elapsed between the first day a teacher could have applied to a vacancy and the day the teacher actually applied to the vacancy, where we assume that the teacher only learns that a vacancy is available on days she logs into the system and applies.[12]

[12] More formally, let $\mathcal{A}_{jt}$ denote the set of days where teacher j applied to at least one vacancy in year t, with $a_{jt} \in \mathcal{A}_{jt}$ measured in calendar days. Let b_{kt} be the (calendar) day that position k's vacancy is posted, and let c_{jkt} be the day that teacher j applies to position k. For every application j sent in year t, we define wait time w_{jkt} as:

$$w_{jkt} \equiv c_{jkt} - \min_{a_{jt} \in \mathcal{A}_{jt} : a_{jt} \geq b_{kt}} a_{jt}.$$

Figure 3 shows that the wait time to apply for vacancies is typically very short. The top panel of Figure 3 shows that the median wait time to apply to vacancies that were already posted on the first day the teacher logged into the system (the "stock" of vacancies) is 0 days. Thus, the applicant's first day likely includes searching for older vacancies. Indeed, the mean vacancy an applicant applies to on day one has been posted for 23 days (Appendix Table A8). The bottom panel shows that the median wait time to apply to vacancies that were posted after the first day the teacher applies (the "flow" of vacancies) is also 0 days.

This feature leads us to treat applications as non-strategic and teachers' choice sets as all positions with postings active between a teacher's first and last application. In a full information environment, we would interpret the applications as revealing that these vacancies were preferred to the vacancies that the teacher did not apply to. But if teachers were inattentive, then this inference would be mistaken. One empirical implication of inattention would be that teachers wait to apply to vacancies because they only notice the vacancy on the second or third (or nth) time that they use the platform. The absence of waiting is inconsistent with this implication of inattentiveness.

These assumptions imply very large applicant choice sets (Panel A of Figure 4), from which applicants apply to many positions (Panel B of Figure 4). These large choice sets and application sets allow us to precisely estimate heterogeneous preferences. For a case study of this heterogeneity, in Appendix D we present descriptive evidence of significant amounts of cross-teacher heterogeneity in application rates to Title I (high-poverty schools), which our model interprets as preference heterogeneity.

We now turn to principals' choice sets, which we define as all of the applications they receive. Natural alternative assumptions include (1) due to rolling nature of the market, the position receives a meaningful number of applications after the principal has made a decision, or (2) because those teachers might still be in the market, the principal pays attention to the most recent applications. The following feature is inconsistent with both of these alternatives (and is evidence against another strategic motive for teachers to time their applications):

Feature #5: The timing of applications that principals rate and do not rate is similar. We view all applications to each position. Table 4 shows that we see a hire in 80% of the postings. In 12% of postings, we see a declined offer. In 18% of postings, we see further principal evaluations and outcomes. We classify these into five groups: (1) interviews, (2) positive assessments, (3) neutral assessments, (4) negative assessments, and (5) application withdrawals.

From the data on the subset of vacancies for which we have multiple outcomes, the applications that receive comments have similar timing to those that principals do not rate. Table 4 shows the day of application relative to the application date of the eventual hire. Applications with principals' comments are received on average only 2.2 days earlier than applications without comments.

Our construction of choice sets implies that teachers and positions are not active for the whole cycle. With respect to identifying preferences, the concern would be that there is some systematic correlation between teacher and position characteristics and the timing of when they are active. Naturally, we cannot rule out all forms of sorting. We can, however, explore various forms of sorting based on observables. The following features show that there is little evidence for such sorting:

Feature #6: The timing of postings is hard to predict based on school observables. Institutionally, we have already discussed why posting—even within a school—is likely spread out: the arrival of relevant information is spread out.

One key source of heterogeneity in the timing of vacancies is that the the district has traditionally allocated replacement positions only once it is aware that a teacher is leaving, rather than "in expectation" of the number of vacancies. Since most teacher attrition occurs over the summer, this policy necessarily generates spread out posting. There are many reasons why teachers would not notify the schools they are leaving early enough for the school to post the job in April. For example, teachers may not know that they will leave a given school until they have secured another position, setting up a vacancy chain in which schools that lose a transferring teacher must search later in the market. Or, teachers may withhold the information, particularly if they fear their leaving could negatively affect them.

While some of these factors suggest that there could be a systematic relationship between posting date and school type, we do not observe such patterns in the data. First, Table 3a shows that the months with highest shares of Title I postings occur early in the cycle (in April (62%) and May (52%)). This finding runs counter to a vacancy chain in which teachers systematically flee Title I schools as jobs in non-Title I schools become available. Second, there is vast variation in the timing of job postings within the same school. Table 3c pools posting dates across the years in our data and shows that 89% of schools that post jobs in July also post jobs in April. A similar pattern holds for schools with April postings.

Feature #7: The timing of applications is hard to predict based on teacher observables. We focus on one key observable: the value-added score of a teacher. In Table 3b we show that teachers with above-median value-added scores apply slightly earlier in the cycle, but that these differences are small.

Feature #8: Teachers' application stopping behavior is hard to predict. In terms of applicants ending their search, many applicants' final applications come early enough in the cycle (9% in April or before, 16% in May, 22% in June) that they are potentially forgoing many yet-to-be-posted vacancies (Appendix Table A8, panel C). While some teachers who stop searching may have accepted a job, we see similar patterns among teachers who do not transfer that cycle. Thus, the end of search might be driven by shocks unrelated to accepting a job.[13]

4.3 Modeling principals

Principals take three actions: rating applicants, interviewing applicants, and hiring applicants. While it is conceivable that the offer decision might reflect strategic considerations (e.g., *is this teacher likely to accept the offer?*), such considerations are not relevant in the principal rating. We therefore use the principal rating as our primary indicator of principal preferences. (In our data, there is only a single field that records the principals' actions. If a positive assessment turns into an interview, then the field records an interview. Hence, we interpret the entry "interview" or "hired" as being an application that received a positive rating.)

In terms of the principal choice set, we view Feature #5 (the timing of applications that principals do and do not rate is similar) as suggestive that principals consider all applications. But we do not have additional evidence on the timing of principals' actions that would allow a more precise characterization of the process. In Section 7.4 we pursue a variety of robustness checks around this assumption.

5 Teacher preferences

5.1 Applications Model

We specify a model that formalizes the discussion of how to infer teacher utilities from application choices. The district's labor market consists of a finite set of potential teachers, indexed by j, and a finite set of positions, indexed by p. Each position is associated with a specific school, $k = k(p)$, and may be assigned to at most one teacher. The exception is the outside option ($p = 0$), which is not part of any school and may be filled by an unlimited number of teachers. This outside option includes leaving the district or leaving teaching.

At the beginning of year t, each teacher may have an initial assignment, denoted by c. For teachers new to the district, this assignment is the outside option ($c = 0$), while for incumbent teachers, the assignment is j's position in the prior year, $c = p(j,t-1)$. Teachers may always keep their initial assignment as their final assignment. On an exogenous date $d = d(j,t)$, teacher j enters the transfer system.[14] If she enters, then she is active in the transfer system until an exogenous end date, $d' = d'(j,t)$.

If the teacher enters the transfer system, then she may apply to any position p that is active at some point between d and d'. There is no marginal cost to applying and there is no limit on

[13] Teachers may continue searching after their final application day. The frequency of applications after the first application day is low enough that statistically we cannot rule out long periods of search without making an application. In Section 7.4, we report a robustness check of adding a seven day buffer to the end of the window.

[14] We treat the decision to enter the system as exogenous. We discuss selection into the system in Appendix E.

the number of applications she can submit. Let a_{jpt} be an indicator for whether teacher j applied to position p in year t. Teachers' application decisions are private information known only to the position p and the teacher j.

These assumptions lead teachers to treat the application process non-strategically by applying to any position with utility higher than her current position and the outside option.[15] Slightly abusing notation (since $c = 0$ for teachers outside the district), a teacher submits an application to position p if:

$$a_{jpt} = \mathbf{1}\{u_{jpt} > \max\{u_{jct}, u_{j0t}\}\}, \tag{7}$$

where u_{jpt} is teacher j's utility from working at position p in time t.

5.2 Parameterization

We adopt a characteristics-based representation of teacher utilities over positions. By summarizing the position in terms of a lower-dimensional set of characteristics, we allow teachers to vary in their valuations of these characteristics.

We specify teacher utilities over positions as:

$$u_{jpt} = -\gamma d_{jpt} + \pi_j VA_{jpt} + \beta_j X_{pt} + \eta_{jt} + \varepsilon_{jpt}. \tag{8}$$

d_{jpt} is the one-way commute time (in minutes) between the teacher and the position and will serve as a numeraire for exposition (Appendix Figure A12 shows a binscatter of application probabilities against distance, revealing a strong downward slope until about 40 minutes). VA_{jpt} is teacher j's total value added at position p in year t.

Value-added, VA_{jpt}, combines absolute and comparative advantage. We define a teacher's absolute advantage to be her predicted value-added at a representative school: $AA_{jt} = n_{1t}\hat{\mu}_{j1t} + n_{2t}\hat{\mu}_{j2t}$, where n_{mt} is the average number of type m students in a classroom in the district. Comparative advantage, CA_{jpt}, at a specific position is then the difference between predicted value-added at school $k(p)$ and absolute advantage: $CA_{jpt} = VA_{jpt} - AA_{jt}$. Because we control for absolute advantage in the person-time effects, η_{jt}, the coefficient on VA_{jpt}, π_j, captures the strength of teachers' preferences for schools where their comparative advantage is high, reflecting the alignment between teachers' preferences and student output. We allow for preference heterogeneity by including a random coefficient in π_j:

$$\pi_j = \bar{\pi} + \sigma^{VA}\nu_j^{VA}, \tag{9}$$

[15] We assume that any post-application steps necessary to be assigned to a position – e.g., interviews – are costless. In our data, teachers with multiple interviews are so rare that even if interviews are costly, they are rare enough that it is unlikely teachers consider dependence across applications.

where $\nu_j^{VA} \sim^{iid} N(0,1)$. Since π_j varies across teachers but we do not have random coefficients on absolute advantage, π_j includes both the preference over comparative advantage and any cross-teacher heterogeneity in preference over output.

X_{pt} is a vector of observed characteristics of positions: the fraction of a school's students that are economically disadvantaged (e), the fraction that are Black (b), the fraction that are Hispanic (h), and the fraction with an above median prior year math test score (s). We allow teachers to have heterogeneous preferences over these school characteristics. Specifically,

$$\begin{aligned}
\beta_j^e &= \beta_{j0}^e + \beta_{j1}^e AA_{jt} + \sigma^e \nu_{jt}^e \\
\beta_j^b &= \beta_{j0}^b + \beta_{j1}^b AA_{jt} + \beta_{j2}^b Black_j + \sigma^b \nu_{jt}^b \\
\beta_j^h &= \beta_{j0}^h + \beta_{j1}^h AA_{jt} + \beta_{j2}^h Hispanic_j + \sigma^h \nu_{jt}^h \\
\beta_j^s &= \beta_{j0}^s + \beta_{j1}^s AA_{jt} + \sigma^s \nu_{jt}^s
\end{aligned} \tag{10}$$

where $Black_j$ and $Hispanic_j$ are indicators for teacher race categories and ν_{jt} is a vector of independent, standard normal random coefficients. Thus, the σ parameters capture the standard deviation of idiosyncratic preferences over each school characteristic.

We follow Mundlak (1978) and Chamberlain (1982) and model η_{jt} using correlated random effects. We model teacher-year unobserved heterogeneity in preferences for teaching in the district as the sum of several components:

$$\eta_{jt} = \lambda Z_{jt} + \rho CM_{jt} + \sigma^\eta \nu_{jt}^\eta. \tag{11}$$

Z_{jt} are teacher-year characteristics – whether the teacher is in the district, whether the teacher is Black, whether the teacher is Hispanic, whether the teacher is female, the teacher's predicted value-added for economically disadvantaged students, the teacher's predicted value-added for non-economically disadvantaged students, and dummy variables for whether the teacher has 2-3 years of prior experience, 4-6 years of prior experience, or more than 6 years of prior experience. CM_{jt} is a set of teacher-year averages of the variables that vary across the job postings within teacher-year (value-added, commute time, interactions of teacher and school characteristics). Thus, through CM_{jt}, we allow unobserved heterogeneity to be correlated with CA_{jpt} and X_{pt}. Finally, ν_{jt}^η is an independent standard normal random effect.

ε_{jpt} is an iid Type I extreme value error. Let $V_{jpt} = u_{jpt} - \varepsilon_{jpt}$ be j's representative value for position p in year t. Then the distributional assumption on ε_{jpt} implies that:

$$Pr(a_{jpt} = 1) = \frac{exp(V_{jpt})}{1 + exp(V_{jct}) + exp(V_{jpt})} \text{ and } Pr(a_{jpt} = 1) = \frac{exp(V_{jpt})}{1 + exp(V_{jpt})}, \tag{12}$$

for teachers already in the district and teachers new to the district, respectively.

5.3 Estimation and Identification

We estimate the teacher preference parameters using the teachers' applications to positions. We define a teacher's choice set, $\mathcal{P}_{jt}$, to be the set of vacancies that were active at the same time as the teacher. We estimate a teacher's start and end (search) date as the dates of her first and last application. Similarly, we estimate a vacancy's start and end (active) date as the dates it receives its first and last application.

We estimate teacher preferences via maximum simulated likelihood, where we simulate from the normal distributions of the random coefficients. Let n index each simulation iteration and let $A_{jptn}(\theta)$ be the model-predicted probability that j applies to position p in year t in simulation iteration n at parameter vector θ. For each teacher j in year t, we construct the simulated likelihood as:

$$L_{jt} = \frac{1}{100}\sum_{n=1}^{100}\prod_{p\in\mathcal{P}_{jt}}(a_{jpt}A_{jptn}(\theta)+(1-a_{jpt})(1-A_{jptn}(\theta))), \tag{13}$$

where a_{jpt} is an indicator for whether j applied to p in the data. Our full simulated log likelihood function is:

$$l = \frac{1}{J}\sum_{j} logL_{jt}. \tag{14}$$

Variation within choice sets help us identify the model's parameters. For mean coefficients, the relevant features of the data are the mean application rates to schools with certain characteristics. We use the variation in these application rates across teacher characteristics to identify observable teacher preference heterogeneity. Finally, if individual teachers have high correlations in the characteristics of the positions they apply to (relative to those they do not) beyond what we would predict based on observables, then we would infer unobservable preference heterogeneity for these position characteristics.

We seek to predict teachers' valuations over positions rather than causal effects of changes in position characteristics on choices. As a convenient way to interpret magnitudes, we will sometimes convert utility to minutes of commute time, which requires the stronger assumption that commute time is exogenous. But because we primarily make relative comparisons of the costs of various policies, we do not rely on having consistently estimated the causal effect of commute time, unless noted.

5.4 Teacher Preference Estimates

Table 5 presents the teacher preference model estimates. First, teachers prefer positions with greater shares of advantaged students. Second, teachers dislike positions with longer commutes. Finally, teachers have only slight preference toward positions where they have higher value-added.

Responsiveness to school and match characteristics varies with observable and unobservable heterogeneity. For example, teachers with higher absolute advantage have more negative preferences over the school's fraction of students that are disadvantaged. We also find a large positive same-race premium for Black teachers and schools with large fractions of Black students. In terms of unobservables, we typically find substantial dispersion in the random coefficients. For example, a standard deviation of the random coefficients on comparative advantage and fraction disadvantaged are each about 1.5 times the mean valuation.

To help interpret the strength of some of these relationships, Panels (a) through (c) of Figure 5 show how the average rank of positions in teachers' preferences change as single characteristics change. We do not hold fixed other characteristics so that, for example, when we study commute time, other characteristics of schools are potentially changing. The figure emphasizes that commute time is a powerful predictor of rankings: changing commute time from 5 minutes to 25 minutes decreases the average rank of a position (for the average teacher) from about the 80th percentile to the 50th percentile. Similarly, the fraction of students that are disadvantaged is a powerful predictor of ranking: across the support, the mean ranking moves by about 20 percentiles, and if teachers were given their top choice there would be oversupply toward economically advantaged students (Appendix Figure A13). In contrast, while teachers do pursue comparative advantage, this relationship is quite weak: across the support of the data, varying teachers' comparative advantage only increases the rank of a position by a couple percentiles.

Teachers' preferences are not particularly aligned with the first-best allocation that maximizes student achievement. Panel (d) of Figure 5 shows that the mean ranking of the first-best position in teachers' preferences is the 48th percentile (we use the same sample as in Section 7). Even if on average teacher preferences do not align with the planner, stronger teachers having more aligned preferences could limit the misallocation resulting from teachers' preferences. The figure shows that this possibility does not occur: for the average strong teacher, the first-best position remains below her 50th percentile ranking. Thus, giving teachers more choice might not produce achievement gains.

6 Principal preferences

6.1 Model and parameterization

Each position p is associated with a principal with the same index. Principal p derives non-wage utility $\tilde{v}_{jpt}$ from teacher j holding position in year t. Because principals in our empirical context do not have to pay teacher wages out of a school budget, we model a principal as giving teacher j a positive rating ($b_{jpt} = 1$) if the non-wage utility is positive: $\tilde{v}_{jpt} > 0$.

We adopt a characteristics-based model and parameterize $\tilde{v}_{jpt}$ to be a linear function of position and teacher characteristics, a random effect, and an idiosyncratic teacher-position error:

$$\tilde{v}_{jpt} = \alpha_p W_{jpt} + \sigma_\kappa \kappa_{pt} + \upsilon_{jpt}. \tag{15}$$

To allow principal preferences to possibly align with output, W_{jpt} includes j's total value-added at school $k(p)$. We further include common teacher characteristics: teacher prior experience (in bins of 2-3 years, 4-6 years, and 7+ years), whether the teacher has a Masters degree, whether the teacher is Black, whether the teacher is Hispanic, and whether the teacher is female.[16] Finally, we include a constant and interact whether the teacher is Black with the fraction of the school's students that are Black and whether the teacher is Hispanic with the fraction of the school's students that are Hispanic. We allow principals to have heterogeneous valuations over W_{jpt} by letting α_p vary with whether the school has Title I status.

To capture principals' heterogeneous outside options and variation in a principal's propensity to assign ratings, we include a normally distributed random effect (κ_{pt}). Finally, υ_{jpt} is i.i.d. Type I extreme value.

6.2 Estimates

Table 6 shows that principals favor teachers with higher value-added.[17] Principals in general rate non-novice teachers and those with Masters degrees more highly. Title I school principals rate Black and Hispanic teachers more positively than non-Title I teachers. Because Title I schools have a larger share of Black students, schools with higher fractions of Black students assign higher total valuations to Black teachers.

To help interpret the strength of the value-added relationship, Panel (e) of Figure 5, shows how the average percentile of teachers in principals' preferences changes as the teacher's projected value added in the position changes. The figure shows that projected value-added meaningfully changes the principal's ranking of a teacher: across the support, the mean percentile goes from about the 25th percentile to the 60th percentile. That said, there is clearly substantial noise in principal's rankings since even the best teachers are on average only in the 60th percentile.

How aligned are principals' preferences with the first-best allocation that maximizes student achievement? From the difference in the slope between Panels (c) and (e) in Figure 5, we might expect that the principals' preferences are more aligned with the first-best ranking than teachers'. This increased strength is quantitatively small: Panel (f) of Figure 5 shows that the average percentile of the first-best teacher for a principal is the 52nd (compared to the 48th for teachers). Thus, schools' preferences are not very aligned with the planner's preferred allocation.

[16] We also include indicators for whether each demographic covariate is missing.
[17] See Appendix F for the likelihood, which closely parallels the one for teachers.

7 Main results

7.1 Simulation details

We make several choices in how we simulate allocations.

Sample: We restrict attention to the teachers for whom we can compute value-added. This restriction drops a large number of teachers: in the labor market for the 2015-2016 school year, we end up with 178 teachers and 296 positions. Because we do not want artificial imbalance in the number of agents on each side of the market to play a role in our estimates (as highlighted by Ashlagi, Kanoria and Leshno (2017)), in each simulation run we randomly drop positions so that there are the same number of teachers and positions.

Randomness: While we estimate a distribution of random coefficients, in simulations we use a single draw of the random coefficients per teacher. This draw is the one used in estimation that maximizes the likelihood for the teacher. We draw the errors in the teacher and principal preferences in an i.i.d. fashion.

Preferences: In using DA to find stable allocations, we have teachers and principals submit rankings according to their true preferences. If there are multiple equilibria, then for one side of the market it is not a dominant strategy to report truthfully. Below we show, however, that the equilibrium is essentially always unique.

To average over the randomness in both the errors and the random dropping of vacancies, we average over 200 simulation runs.

7.2 Model fit

We begin by considering the model's fit under status quo policies. We model the status quo as the teacher-propose equilibrium with restricted timing, and estimated teacher and school preferences. Figure 6 shows that the model matches the basic qualitative patterns in the data: schools with a larger share of disadvantaged students have teachers (a) with stronger absolute advantage in teaching, (b) with comparative advantage in teaching economically disadvantaged students, (c) less likely to be experienced, and (d) more likely to be Black. Quantitatively, the model almost exactly matches the slope for teacher experience and whether teachers are Black. The model slightly underpredicts the slope in absolute advantage and misses the intercept on absolute and comparative advantage and experience. The difference in intercept comes from the data and model samples differing (see Appendix E).

To assess whether our model fits better than alternate equilibrium assumptions, we examine the fit of models where schools and teachers match according to serial dictatorships, as opposed to our pairwise stability assumption. We find that a teacher serial dictatorship ordered by absolute advantage (Appendix Figure A14) and experience (Appendix Figure A15) and a principal serial dictatorship ordered by fraction of students that are economically disadvantaged (Appendix Figure A16) each produce a much worse fit than our model.

7.3 Trade-offs and effects of idealized policies

Now that we have preference estimates and the estimates of student achievement for any teacher-position combination, we can carry out the exercises outlined in Section 1.

7.3.1 Trade-offs

Figure 7 presents our main results. The figure's production possibilities frontier (PPF) comes from solving a set of first-best problems (equation (3)) where we place different relative weight on students' achievement and teachers' utility. The top-left point reflects the allocation of teachers to schools that maximizes student achievement. The bottom-right point reflects the allocation of teachers to schools that maximizes teacher utility. There are two notable features of these points. First, there is a large gap in student achievement between the teacher and school first-best: the difference is 0.03 standard deviations of test scores. Second, there is a large gap in teacher utility between these allocations: the difference is about 35 minutes of one-way commuting time a day. A sensible valuation of an hour of commute time is about half of the hourly wage (Johnston, 2021). Hence, this finding, plus a causal interpretation of the commute time coefficient, implies that the gap between the teacher and school first-best is worth about one-sixteenth of a teachers' annual earnings.

The third feature of the PPF is the very favorable trade-offs available between teacher utility and student achievement implied by the PPF. Concretely, if we start from the teacher-preferred allocation, then there are large gains in student achievement that barely affect teacher utility. For example, starting from the teacher preferred allocation, we can achieve three-quarters of the gains in student achievement (0.03 standard deviation units) from moving to the student achievement maximizing output at about 8 percent of the cost in terms of teachers' utility.

7.3.2 Effects of policies

Turning to the the set of stable allocations, we have several findings about the effects of various policies, which we further summarize in Figure 8. First, we find essentially no role for equilibrium selection. Relative to the status quo, changing from teacher-proposing to school-proposing DA has essentially no effect on the allocation. Indeed, with combinations of preferences and choice sets other than those in the status quo, we almost always find that the allocations from the teacher-propose and school-propose DA are identical. Second, policies that complete choice sets achieve about 15% of the total allocative gains.

Third, we find that simply making principals only value output slightly **reduces** student achievement. This finding might appear counterintuitive as we are aligning principals' preferences with those of the planner and in Panel (e) of Figure 5 we showed that estimated preferences left considerable room for alignment. Instead, the result reflects natural "theory of the second-best" reasoning and thus highlights important interactions between teacher and principal preferences. We elaborate on these reasons below, after discussing the remaining points.

Fourth, making teachers also only value output (for instance with large output bonuses—which would be a large change in preferences compared to Panel (c) of Figure 5) has large effects on student achievement. If teachers only value output, then we achieve 74% of the total allocative gains available in this sample.

Finally, once we complete choice sets and make both teachers and principals only value output, the remaining 26% gap in allocative gains is due exclusively to the absence of transferable utility. Prices play two possible roles in improving allocations. First, prices let the district change the agents' value from a match to align with that of the planner. Second, a quarter of the potential gains from reallocation are achievable only by making utility transferable; that is, principals who only value output will rank teachers largely based on absolute advantage rather than comparative advantage, and flexible prices ("transferable utility") allows the district to take into account comparative advantage. In this context, this amounts to flexible, position-teacher specific prices, where this specificity means that two teachers in the same position could potentially earn different wages because of the combination of their different preferences and different productivity (at all positions, not just the matched one).

Teacher utility in various allocations: Teacher utility increases as we move from the status quo and first expand choice sets and then make principals only value output. Each of these steps increases teacher utility on average by about 5 minutes of one way commute time. In contrast, if we make teachers only value output, but still evaluate the utility of the assignment using our estimated preferences, then we find that this change reduces teacher utility by about 20 minutes of one way commute time relative to the status quo.

Teacher ability to choose: Making principals only value output does not achieve better student outcomes because of second-best reasoning: when principals only value output, stronger teachers achieve more preferred assignments, and—as we saw in Panels (c) and (d) of Figure 5—the preferences of (stronger) teachers are not solely about maximizing output. Figure 9 illustrates this result. We sort teachers by their absolute advantage and plot the rank in the teacher's own preferences of the position to which she is assigned. The top panel shows the results with principals having their estimated preferences, and the bottom panel shows results with principals instead only valuing output. The key similarity between these figures is the upward slope: under both sets of preferences, principals value output, and so teachers who on average produce more output are assigned to schools that the teachers prefer more.

The key difference between these figures is the steepness of the slopes: the slope is much steeper when principals only value output. Intuitively, more weight on output means that higher-ability teachers get more choice because (a) all principals agree on the value of this characteristic and (b) the dispersion in absolute advantage is high enough that teachers who are effective at one school are likely effective at another. These strong teachers then go to their preferred schools, which are likely to have more economically advantaged students (Figure 5, Panel (b)), even though stronger teachers tend to have comparative advantage with economically disadvantaged students (Figure 2). Hence, the strong teachers are not likely to choose the assignments that maximize student achievement.

This emphasizes the importance of second-best reasoning: in the presence of teacher preferences that are not aligned with the achievement-maximizing planner's objectives, aligning only principals' preferences may lead to allocations that are further from the planner's objectives.[18] Indeed, Bates (2020) studies policies in the same state that increased principals' information about teacher effectiveness and finds changes in teacher sorting consistent with this reasoning.

Distributional considerations: The bottom panel of Figure 7 shows the mean achievement of economically advantaged and disadvantaged students in the allocations depicted in the top panel. There are a few notable features. First, in the status quo allocation, we find that disadvantaged students have better teachers (the gap is slightly larger than 0.01 standard deviations). Second, in the student achievement maximizing allocation there is no gap in value-added between advantaged and disadvantaged students. Like in the full sample, in the transfer sample class size is negatively correlated with the fraction of economically disadvantaged students and teachers with absolute advantage tend to have comparative advantage with economically disadvantaged students (Appendix Figure A17). But in the transfer sample, these factors balance out such that the first-best allocation splits the strongest teachers (Appendix Figure A18) and produces equal value-added across student types.

Third, and most strikingly, when we give principals output maximizing preferences the distributional implications reverse: now, advantaged students receive (quite dramatically) higher value-added than disadvantaged students. The reversal reflects the same consequences, discussed above, from giving the strongest teachers more choice.

[18] To see a simple example of this phenomenon, let us continue with the example in Footnote 4. We maintain the output structure: teacher 1 has output $\{10,9\}$ and 2 has output $\{8,0\}$ at schools 1 and 2, respectively. We now assume that teachers have identical preferences $1 \succ 2$. If principals maximize output, then they both rank teacher 1 above 2, and we end up with teacher 1 at school 1 and teacher 2 at school 2. Suppose principals place weight on other characteristics and instead both rank teacher 2 above 1. Then the decentralized equilibrium corresponds to the first-best and teacher 2 ends up at school 1 and teacher 1 at school 2.

7.4 Robustness

In Table 7, we present a wide variety of robustness checks to our main analysis. First, we show that are results are quantitatively very similar when we use the other years in our data. Second, we show that our main results are quantitatively similar when we instead split students by race (white and non-white) and on lagged achievement (at the median), rather than on economic advantage.

We consider two alternative definitions of teachers' choice sets. First, we add a seven day buffer on the end, and assume that a teacher also considered the vacancies that were active seven days after her last application. Second, we narrow the choice set to focus only on vacancies that were available on the first day the teacher applied. In both cases, we find that our results are quantitatively unchanged.

A non-trivial share of teachers in our analysis sample only submit one application. There are a variety of reasons why these teachers might be different, or their applications might reflect different considerations. The table shows that dropping such single-application teachers leaves our results quantitatively unchanged.

We also consider a wide variety of alternative preference models. First, we explore various combinations of teacher and school fixed and random effects. Second, we allow for correlation in random coefficients on a constant, value-added, and fraction of students that are economically disadvantaged. Our results are quantitatively unchanged across these alternatives.

Turning to principal preferences, we consider two alternative definitions of principal choice sets. First, we restrict to applications that were submitted within plus or minus two weeks of the application that was hired. Second, we look at the window in which teachers were submitting applications to the vacancy. We split the window into halves, and estimate principal preferences on each half separately. The table shows that our results are quantitatively unchanged.

We also consider alternative ways of using the information in the data. First, we estimate a rank order logit model where we allow, e.g., a "hire" outcome to be better than a "positive assessment" outcome. Second, we retain the rank order logit specification, but use only the applications where the principal actively supplied information; that is, we drop the applications that are not rated. Finally, we use a binary logit but use as the only positive outcome an indicator for whether the individual was hired. The table shows that our results are quantitatively unchanged across these three alternatives.

Finally, we explore two alternative set-ups. First, we redo all our analysis using constant class size. As with the full sample, we find smaller gains from reallocation with constant class size in the transfer sample. Here, though, the attenuation is stronger. Qualitatively, the conclusions about the types of policies that would be effective remain unchanged. Second, we show the subset of outcomes that we can compute on the full sample. A larger share of the gains from reallocation can be achieved with output bonuses in the full sample than in the transfer sample (see Appendix E for further discussion of selection into the transfer sample).

8 Teacher bonus counterfactuals

In the last section, we showed the effects of idealized policies on output. The most effective such policy—and the only one that attained more than 15% of the potential achievement gains—was aligning teacher preferences over schools with the output they would produce. Complete alignment may be difficult to implement. Therefore, we now consider the effect of more realistic teacher bonus policies, similar to those that some districts have piloted. The upper bound on the effects of policies that subsidize output is to reach the point where teachers and principals only value output.

8.1 Implementation details

The district offers a two-part bonus on the basis of a teacher-position characteristic, z_{jpt}, where each teacher receives b_0, a lump-sum amount, and $b_1 z_{jpt}$, a bonus b_1 per unit of z_{jpt}. Teacher j's utility for teaching at position p in year t thus becomes:

$$u_{jpt} = \tilde{u}_{jpt} + \gamma(b_0 + b_1 z_{jpt}), \tag{16}$$

where we multiply by the commute time coefficient (γ) to express bonus spending in minutes of commute time. We consider a range of b_1 for each z_{jpt}, which allows us to trace out the effects of different bonus sizes. For each b_1, we solve for the teacher-optimal stable equilibrium assignments, where $p^*(j)$ is j's assigned position, given the bonus size and the object that generates the bonus.

To focus on policies that are likely to receive teachers' support, we hold teachers harmless by making each teacher weakly better off than in the status quo equilibrium. Let $\Delta u^{b_1}_{jpt} = (\tilde{u}_{jp^*(j)t} - \tilde{u}_{jpt}) + \gamma b_1 z_{jp^*(j)t}$ be the change in teacher j's utility (excluding the transfer) between the zero-bonus and the b_1 bonus equilibria. We set the transfer such that the teacher with the worst change is indifferent:

$$b_0 = -\min_j \Delta u^{b_1}_{jpt}. \tag{17}$$

This lump-sum transfer can be either positive or negative, and so the district can pay teachers to enter this policy. Thus, the district's total cost to the bonus scheme is $b_0 + b_1 z_{jp^*(j)t}$, which depends on both the choice of b_1 and how it changes the allocation.

We examine bonus schemes over three objects (z_{jpt}). We start with bonuses for output ($\sum_m n_{k(p)m}\hat{\mu}_{jm}$). Then we look at bonuses based on the fraction of disadvantaged students the teacher has ($p_{k(p)1t}$). These bonuses mimic the hard-to-staff school bonuses that some districts have piloted. Finally, we interact school and teacher characteristics by considering bonuses based on a teacher's absolute advantage times the fraction of disadvantaged students ($(p_{0t}\hat{\mu}_{j0t} + (1 - p_{0t})\hat{\mu}_{j1t})p_{k(p)1t}$).

8.2 Results

The top panel of Figure 10 shows the effect of these three bonus schemes on overall achievement relative to the status quo. For reference, the top dashed horizontal line shows the level of achievement in the the first-best allocation (in the transfer sample), the middle dashed line shows the "best case" for bonuses when teachers and schools only value output, and the lower dashed line shows the gains from an institutional policy of simply changing market timing. To allow for comparisons across bonus schemes, the horizontal axis is the total realized spending (normalized to be in minutes of commute time per teacher).

The first notable aspect of this figure is that we can see that bonuses are more costly than the first-best policies depicted in Figure 7, where the difference in teacher utility between the non-price stable allocation and the student achievement maximizing allocation was about 20 minutes of commute time per teacher. Here, even at 100 minutes of (one way) commute time per teacher, bonuses still do not achieve the maximal student achievement. This large difference in cost is driven by the uniformity of the bonus scheme. Prices that implement the first best allocations take into account preference variation in a way that keeps costs down. For example, if a school is trying to convince a close-to-indifferent teacher to take a position, then the school only needs to increase the wage offer slightly for the teacher to accept the offer. We demonstrate the savings from flexible prices by allowing for separate lump sum payments to each teacher and plotting the gains in the dashed black line. We find that at the spending level where the full potential gains are realized, the uniform bonus schemes have barely increased achievement.

Second, paying directly for achievement is the most efficient bonus scheme. In the status quo, disadvantaged students already have slightly better (matched) teachers and so paying teachers to be at schools with more disadvantaged students hardly increases output.

Third, changing market timing to complete choice sets only achieves 15% of the overall gains from achievement. Interpreting the commute coefficient causally, Figure 10 shows that such a policy would be cheaper than bonuses if it cost less than about 40 minutes of one way commute time per teacher to implement.

The bottom panel shows that there are important interactions between principal preferences and the effectiveness of teacher bonuses. We conduct an identical exercise except that we pair the teacher bonuses with a bonus to principals such that principals only value output. As we have seen, when we pay principals for output, the distributional consequences change dramatically. Now, the fact that teachers have strong preferences against teaching at schools with larger shares of disadvantaged students means that there is a large range of spending where policies that target this issue directly are the most cost effective. As we saw in Section 7, achievement is higher when there is some force pushing back on teachers' preferences toward advantaged schools. In the absence of principals' heterogeneous valuations, bonuses targeted toward disadvantaged students serve this purpose.

9 Discussion

This paper studies the allocation of teachers to schools and its implications for student outcomes. We start by estimating the potential gains in student achievement from within-district teacher reassignment and find that they are large (0.05σ). In the achievement-maximizing allocation, the strongest teachers are split between economically advantaged students (because they are in larger classes) and the economically disadvantaged students (because stronger teachers tend to have comparative advantage with them). We consider how to achieve these gains, recognizing that the allocation of teachers across schools represents a labor market equilibrium.

To study equilibrium in the labor market, we estimate teacher and principal preferences over matches. We find that teachers prefer positions described by homogeneous characteristics (e.g., fraction of advantaged students) and heterogeneous characteristics (e.g., commute time), with only slight preference toward positions where they have higher value-added. Giving teachers the ability to choose their position leads to excess supply at schools with advantaged students and sorting based on non-output heterogeneity. Thus, if teachers have some degree of choice in their assignment, then the district may want to counteract the sorting by changing how teachers value positions (e.g., with bonuses).

On the principal side, we find preferences for teachers who produce more student achievement, but that differences in output only explains some of the variation in preferences. Thus, the district might consider changing how principals value teachers.

When we put teacher and principal preferences together in an equilibrium model, however, we find more complicated policy implications. When teachers receive bonuses for output, they sort toward positions closer to the first-best. When principals receive bonuses for output, they seek the best teachers. But because absolute advantage dispersion is large, a second consequence of principal bonuses is that it grants the strongest teachers more choice. More choice among the best teachers does not necessarily lead toward higher achievement.

Our analysis concludes that in the absence of flexible prices, teacher bonuses are the primary policy tool for realizing achievement gains because they align teachers' preferences with the district's. But the optimal form of bonuses depends on how principals value teachers. Flexible prices, however, would produce achievement gains at a much lower cost.

While we find that in our district teacher value-added is relatively balanced across student types, our data and framework could be useful in designing policies that go beyond equalizing achievement gains to try to close baseline gaps.

We have abstracted from various margins that are relevant to teacher allocation. By considering the within-district assignment to schools, we have abstracted from selection at higher levels—the extensive margin of which district to teach in (Biasi, Fu and Stromme, 2021) or whether to become a teacher (Tincani, 2021)—and at lower levels–within-school assignments. We have also held fixed the assignment of students to schools (e.g., Abdulkadiroğlu, Agarwal and Pathak (2017)) and the distribution of class sizes (e.g., Angrist and Lavy (1999); Hoxby (2000); Leuven, Oosterbeek and Rønning (2008)). Finally, we have held teacher and principal non-wage utility fixed in counterfactuals. But changes in malleable school characteristics, either under direct policy control (e.g., principal's support of teachers (Dizon-Ross, 2020; Johnston, 2021)) or that change in equilibrium (e.g., teacher peer effects (Jackson and Bruegmann, 2009)) may be a substitute or complement to the policies we consider.

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Table 1: Forecast Unbiasedness Tests for Value-Added Predictions

	Mean Res	Mean Diff	Mean Res	Mean Res	Mean Res	Mean Res
VA (Heterog)	1.052			1.060		
	(0.00650)			(0.00681)		
VA Diff		0.879				
		(0.0243)				
Post Transfer			-0.00243	0.00576		
			(0.00367)	(0.00280)		
VA * Post Transfer				-0.0885		
				(0.0212)		
VA – below 10th (disadv)					0.990	
					(0.0223)	
VA – 10th-90th (disadv)					1.058	
					(0.00698)	
VA – above 90th (disadv)					1.066	
					(0.0228)	
VA – below 10th (size)						1.011
						(0.0224)
VA – 10th-90th (size)						1.066
						(0.00713)
VA – above 90th (size)						0.961
						(0.0188)
Constant	0.00810	0.0477	0.00779	0.00745	0.00810	0.00800
	(0.000835)	(0.00101)	(0.00174)	(0.000883)	(0.000835)	(0.000843)
Subject	Math	Math	Math	Math	Math	Math
Mean DV	0.00764	0.0527	0.00754	0.00764	0.00764	0.00764
Clusters	21514	21514	21834	21514	21514	21514
N	74552	74552	75459	74552	74552	74552

The table includes tests of whether a value-added estimate is forecast unbiased. In the first and third through sixth columns, the outcome ("Mean Res") is the mean student math test score, residualized by student demographics including lagged scores, school fixed effects, and teacher experience measures. The mean is taken over all students for a given teacher-year. In the second column, the outcome ("Mean Diff") is the difference in the mean residualized math scores between a teacher's economically disadvantaged and advantaged students. The "VA" measures allow for match effects ("Heterog"). The measures predict mean student residuals using data from all prior years a teacher taught. "VA Diff" is the difference in predicted value-added between a teacher's economically disadvantaged and advantaged students (i.e., the predicted comparative advantage). "Post Transfer" refers to years after a teacher switched schools. The interaction with "VA" multiplies the post-transfer indicator with the heterogeneous value-added measure. Column (4) splits the year t observations into bins as a function of the change in share of disadvantaged students relative to the data observed for the teacher before year t. The split is based on percentiles of the change. Column (5) splits the year t observations into bins as a function of the change in classroom size relative to the data observed for the teacher before year t. The split is based on percentiles of the change. For columns (4) and (5) the p-value comes from F-test that the three coefficients are equal. Standard errors are clustered at the teacher level.

Table 2: Potential Gains from Reassignment

	Per-Student Gains (σ)	As a Fraction of (Best-Actual)	Non-Disadvantaged	Disadvantaged
Alternate Allocations				
Best	0.054		0.095	0.018
Random	-0.003	-0.05	0.019	-0.023
Worst	-0.057	-1.06	-0.053	-0.062
Alternate Policies				
Best w/i School	0.013	0.28	0.015	0.010
Replace Bottom 5% of Teachers	0.012	0.22	0.015	0.009
Targeting Student Types				
Max Non-Disadvantaged VA	0.025	0.45	0.137	-0.077
Max Disadvantaged VA	0.016	0.30	-0.049	0.075
Constant Class Size				
Best	0.021	0.38	0.018	0.023

The table shows the potential gains from reassignments of teachers to different schools. The sample is all teachers with non-missing value-added forecasts in 2016 (based on prior data), along with their corresponding 2016 assignments. Gains come from better matching of teachers to students, as teachers' effectiveness may differ across student types, and placing better teachers in schools with larger class sizes. The first column shows the per-student gains from various allocations relative to the actual allocation. Gains are measured in student standard deviations (σ). The second column shows the gain as a fraction of the full difference between the best (output-maximizing) and actual allocations. The third and fourth columns show the per-student gains, relative to the actual allocation, for non-disadvantaged and disadvantaged students. The best within school allocation only changes the teacher-classroom assignments within a school. "Replacing Bottom 5% of Teachers" refers to replacing the bottom 5% of teachers according to realized per-student output with teachers with median value-added for each student type. The allocations that target particular student types maximize per-student output for students of one type only. "Constant Class Size" imposes an equal number of students (but possibly different composition) across all classes, in both the best and actual allocations. We assign classrooms the mean student composition and class sizes in that school in 2016 in all allocations except the "Best w/i School" and "Constant Class Size" allocations.

Table 3: Timing of posting, applying, and hiring

(a) Monthly shares by position

	Posting			***Applying***			***Hiring***		
	Vacs	Share	Share TI	Apps	Share	Share TI	Apps	Share	Share TI
April	295	16.24	0.62	24799	7.13	0.50	393	13.23	0.69
May	392	21.57	0.52	70248	20.21	0.50	585	19.70	0.63
June	502	27.63	0.52	108776	31.29	0.51	827	27.85	0.60
July	451	24.82	0.42	94171	27.09	0.50	755	25.42	0.50
August	167	9.19	0.46	44673	12.85	0.51	358	12.05	0.57
Total	1807	100		342667	100		2918	2918	

(b) Monthly shares by teacher value-added

	Has VA			***Above median VA***			***Top decile VA***		
	Apps	Share	Share TI	Apps	Share	Share TI	Apps	Share	Share TI
April	3050	6.23	0.44	1552	7.16	0.42	373	9.15	0.41
May	9662	19.75	0.44	4218	19.46	0.44	918	22.53	0.45
June	16832	34.40	0.46	8035	37.08	0.45	1396	34.26	0.47
July	13673	27.95	0.47	5600	25.84	0.46	944	23.17	0.46
August	5522	11.29	0.48	2189	10.10	0.47	434	10.65	0.52
Total	48739	100		21594	100		4065	100	

(c) Early vs. late posting times by school

	Posts in July		
Posts in April	No	Yes	Total
No	8	15	23
Yes	10	88	98
Total	18	103	121

This table shows the timing of posting, applying, and hiring during a cycle. Panel (a) shows the distribution of vacancy postings, applications, and hires by month, where hires correspond to the timing of the applicant who was hired to the position. For each type of action, we show the share that corresponds to Title I positions. Some of the vacancies produce multiple hires. In Panel (b) we show the distribution of applications by month, where we split the sample of applicants into those with a value-added forecast (i.e., had taught in tested grades and subjects in North Carolina prior to applying), those with above median value-added, and those in the top decile. Panel (c) shows the cross-tabulation of whether a school posts a vacancy in April and whether that school posts a vacancy in July (in the same cycle).

Table 4: Application evaluations, outcomes, and timing

	Hired successfully	Hired but taught elsewhere	Hired but not in district	Declined offer	Interview	Positive	Middle	Negative	Withdrew	No comment
mean	0.00051	0.00003	0.00017	0.00006	0.00000	0.00064	0.00029	0.00037	0.00002	0.07367
count	2,291	122	750	292	7	2,887	1,300	1,655	74	333,780

(a) Outcomes at the application level

	Hired	Declined offer	Interview	Positive	Middle	Negative	Withdrew	No comment	Any Non-Hire Action
mean	0.799	0.117	0.001	0.101	0.023	0.075	0.037	0.985	0.179
count	1,457	213	2	184	42	136	67	1,797	327

(b) Outcomes at the position level

	Obs	Mean	10th	25th	50th	75th	90th	Std. dev.
All applications	343,161	-0.0	-15.6	-5.8	-0.8	4.6	16.4	14.74
No notes	333,780	0.1	-15.2	-5.6	-0.7	4.5	16.1	14.38
Evaluated with notes	9,381	-2.0	-32.1	-15.0	-4.1	7.9	31.7	24.26

(c) Timing relative to hired applicant

This table shows the frequency and timing of application outcomes. The data record a single outcome per application; as an example, "Interview" implies not hired as otherwise the "Interview" outcome would be replaced by "Hired." The data record "Hired," which we split into "Hired successfully" for teachers who taught in the position's school the following year, "Hired but taught elsewhere" for teachers hired who taught in district but not at that position's school, and "Hired but not in district" for teachers hired who did not appear in the district the following year. "Positive," "Middle," and "Negative" reflect the authors' coding of different text categories. "No comment" includes applications without an updated status. Panel (a) shows frequencies at the application level and panel (b) shows frequencies at the position level for at least one outcome across all applications to that position (i.e., "Hired" indicates at least one application led to a hire). "Any Non-Hire Action" is a positive, middle, or negative assessment or an application withdrawal. In panel (c) we calculate the difference in timing (in days) between when an application was made and when the application that led to a hire was made. A value of 1 would indicate an application made 1 day after the one that led to a hire. In the last two rows, we split the sample into those with no notes ("No comment") and those with an outcome.

Table 5: Teacher preference estimates

	Estimate	Standard Error
Constant	2.032	4.453
Commute Time	-0.073	0.001
Commute Time Missing	-1.660	0.223
Value Added	0.081	0.008
St Dev Value Added RC	0.128	0.007
School Characteristics and Interactions		
Fraction Disadvantaged	-1.188	0.136
Fraction Black	-0.452	0.132
Fraction Hispanic	0.441	0.144
Fraction Above Median Achievement	0.163	0.149
Abs Adv x Fraction Disadvantaged	-0.797	1.029
Abs Adv x Fraction Black	-1.635	1.025
Abs Adv x Fraction Hispanic	2.487	1.074
Abs Adv x Fraction Above Median Achievement	-1.997	1.185
Black x Fraction Black	1.072	0.130
Hispanic x Fraction Hispanic	0.491	0.771
St Dev Fraction Disadvantaged RC	1.591	0.034
St Dev Fraction Black RC	1.296	0.054
St Dev Fraction Hispanic RC	0.637	0.065
St Dev Fraction Above Median Achievement RC	1.397	0.045
Teacher Characteristics		
VA Non-Disadvantaged Students	0.746	0.307
VA Disadvantaged Students	0.937	0.331
In District	-0.509	0.061
Black	-0.095	1.043
Hispanic	6.017	3.762
Female	0.284	0.064
Experience 2-3	0.070	0.083
Experience 4-6	-0.268	0.082
Experience 7+	-0.141	0.074
St Dev Random Effect	1.687	0.030
Chamberlain-Mundlak Device		
Fraction Disadvantaged Mean	-1.903	3.182
Commute Time Mean	0.032	0.004
Commute Time Missing Mean	1.231	0.249
Value Added Mean	-0.489	0.295
Fraction Black Mean	-2.786	2.707
Fraction Hispanic Mean	0.041	2.457
Fraction Above Median Achievement Mean	-0.986	4.718
Abs Adv x Fraction Disadvantaged Mean	-37.628	19.086
Abs Adv x Fraction Black Mean	36.183	18.362
Abs Adv x Fraction Hispanic Mean	15.838	19.942
Abs Adv x Fraction Above Median Achievement Mean	-16.346	6.488
Black x Fraction Black Mean	-2.200	2.412
Hispanic x Fraction Hispanic Mean	-20.462	14.686
Number of Students Mean	0.009	0.023

The table shows teacher preference coefficients, estimated using maximum simulated likelihood. We model the probability that a teacher applies to a position where the alternate options are not teaching in the district or keeping the current position. Random coefficients ("RC") are independent and simulated from the standard normal distribution. We model unobserved teacher-year heterogeneity using a Mundlak (1978) and Chamberlain (1982) device, taking the mean of each covariate across an applicant's choices. Commute time is measured in minutes, value added is total predicted output. Experience below 2 years is the omitted category.

Table 6: Principal preference estimates

	Estimate	Standard Error
Constant	-4.363	0.127
St Dev Random Effect	1.531	0.022
Title I	0.521	0.156
Value-Added	0.092	0.026
Value-Added x Title I	0.038	0.034
Experience 2-3	0.351	0.128
Experience 2-3 x Title I	-0.005	0.163
Experience 4-6	0.271	0.117
Experience 4-6 x Title I	0.035	0.160
Experience 7+	0.097	0.089
Experience 7+ x Title I	-0.344	0.120
Experience Missing	-0.342	0.060
Experience Missing x Title I	0.371	0.086
Masters	0.188	0.098
Masters x Title I	0.124	0.125
Black	-1.035	0.227
Black x Title I	1.722	0.453
Black x Fraction Black	0.396	0.267
Black x Fraction Black x Title I	-0.253	0.511
Hispanic	-0.690	0.454
Hispanic x Title I	0.450	0.561
Hispanic x Fraction Hispanic	2.259	2.219
Hispanic x Fraction Hispanic x Title I	-1.833	2.345
Female	0.053	0.106
Female x Title I	0.031	0.129
Gender Missing	-0.327	0.230
Gender Missing x Title I	-0.197	0.277
Race Missing	-0.530	0.210
Race Missing x Title I	0.374	0.247
VA Missing	0.490	0.089
VA Missing x Title I	-0.230	0.124

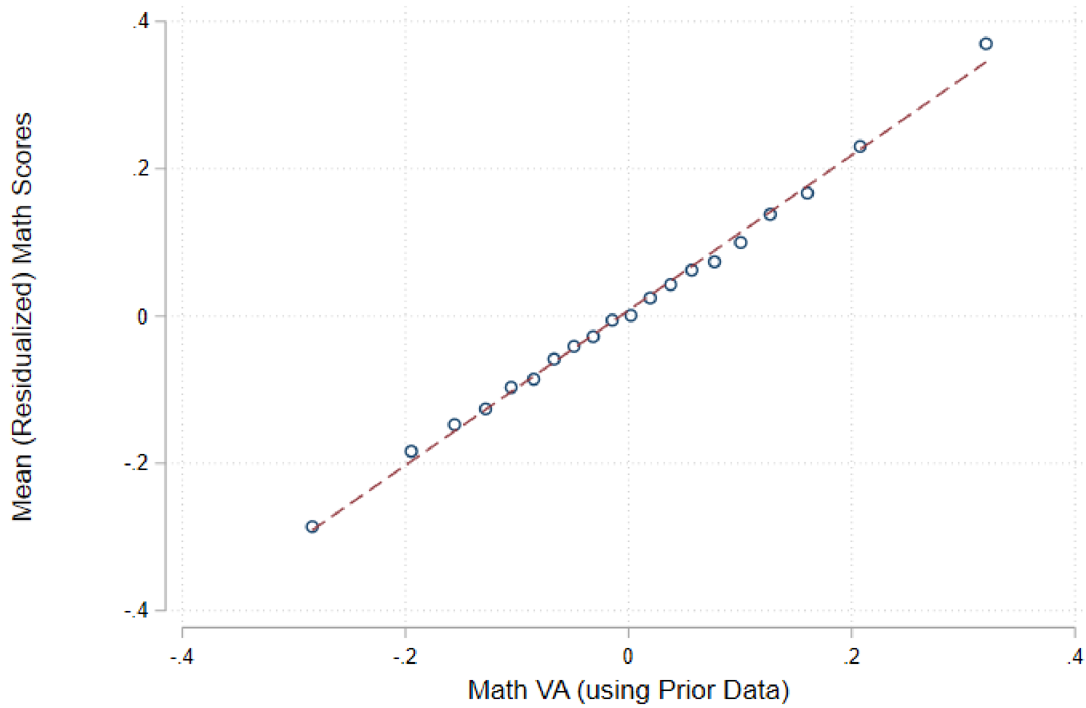
The table shows principal preference coefficients, estimated using maximum simulated likelihood. We model the probability that a principal submits a positive outcome (hire, interview, positive rating) for an application. Random effects are simulated from the normal distribution. Experience below 2 years is the omitted category. Value-added is total predicted output.

Table 7: Robustness: output relative to status quo

	All Options	Principal Max VA	Teach Max VA	Both Max VA	First Best
Baseline	0.0044	-0.0002	0.0216	0.0228	0.0309
1. Vary year: baseline is 2016					
2012	0.0025	0.0031	0.0238	0.0228	0.0328
2013	0.0028	0.0017	0.0234	0.0225	0.0313
2014	0.0015	0.0031	0.0217	0.0225	0.0312
2015	0.0031	-0.0091	0.0228	0.0243	0.0342
2017	0.0058	-0.0053	0.0244	0.0276	0.0363
2. Vary student type split: baseline is economic disadvantage					
Achievement	0.0052	-0.0003	0.0243	0.0277	0.0359
Race	0.0032	0.0002	0.0232	0.0275	0.0356
3. Vary choice set construction for teachers					
7 day buffer	0.0046	-0.0021	0.0215	0.0227	0.0309
First day choice sets only	0.0064	0.0011	0.0251	0.0263	0.0345
Drop single app. teachers	0.0032	-0.0026	0.0205	0.0221	0.0303
4. Vary teacher preference specification to use binary logit					
No REs or FEs	0.0034	-0.0011	0.0219	0.0232	0.0315
School FEs	0.0031	-0.0009	0.0231	0.0245	0.0328
School REs	0.0031	-0.0019	0.0215	0.0232	0.0312
Teacher FEs	0.0045	-0.0001	0.0213	0.0225	0.0307
Teacher REs	0.0037	-0.0004	0.0209	0.0225	0.0307
Teacher REs, School FEs	0.0019	-0.0091	0.0239	0.0256	0.0337
Teacher FEs, School FEs	0.0014	-0.0110	0.0248	0.0261	0.0344
5. Allow for correlated random coefficients in teacher preferences					
Corr. R.C.	0.0038	-0.0018	0.0230	0.0243	0.0324
6. Vary window in which we estimate principal preferences: baseline is all applications					
W/in 2 weeks of hire	0.0046	0.0001	0.0219	0.0231	0.0312
First half	0.0041	-0.0002	0.0215	0.0233	0.0314
Second half	0.0044	0.0001	0.0214	0.0236	0.0316
7. Estimate principal preferences using rank order logit: baseline is binary logit					
All data	0.0041	0.0004	0.0210	0.0233	0.0315
Active choices	0.0027	-0.0003	0.0194	0.0231	0.0313
Hire outcome only	0.0048	0.0006	0.0220	0.0241	0.0322
8. Hold class sizes constant: baseline uses class size					
Constant class size	-0.0005	-0.0029	0.0047	0.0045	0.0064
9. Some outcomes on full sample: baseline uses transfer sample					
Full sample				0.0473	0.0543

The table shows robustness checks for our main results. The columns correspond to the change in mean student achievement (in student standard deviation units) between the considered counterfactual and the estimated status quo. “All Options” expands teachers’ choice sets to all positions, “Max VA” corresponds to ranking positions (or teachers) by predicted value-added, and “First Best” is the output-maximizing allocation. In the first section, we vary the year in which we implement our main exercise. In the second section, we show results where teacher-school match effects depend on different student observable characteristics. In the third section we vary the assumptions around teachers’ choice sets or drop teachers who make single applications. In the fourth section, we vary the level of random or fixed effects in the teacher preference model, while in the fifth section we allow for correlated random coefficients on a constant, total value-added, and fraction of students who are economically disadvantaged. In the sixth section we vary principals’ choice sets while in the seventh we vary how we treat an application’s outcome in the principal preference model. In the eighth section we show results where preference estimation and counterfactual analysis use constant class sizes across all positions in the district. In the final section, we show outcomes for the full sample of teachers with value-added forecasts, not just the teachers who apply to transfer. Because we lack preferences for these teachers, we only estimate a few equilibria.

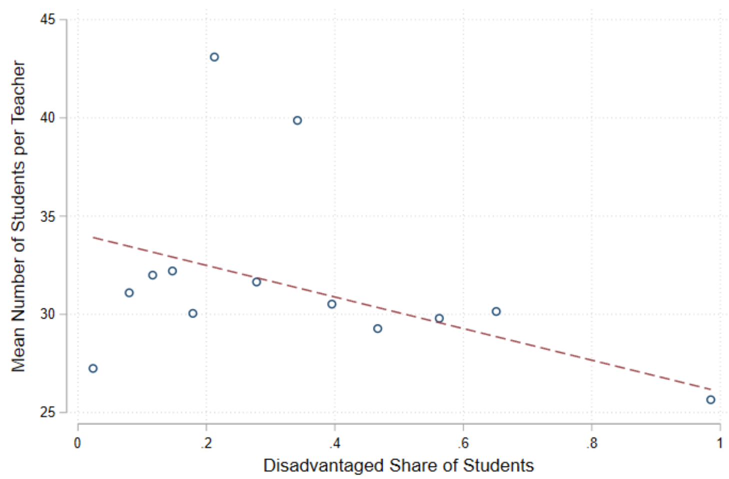
Figure 1: Math Value-Added Forecast Unbiasedness



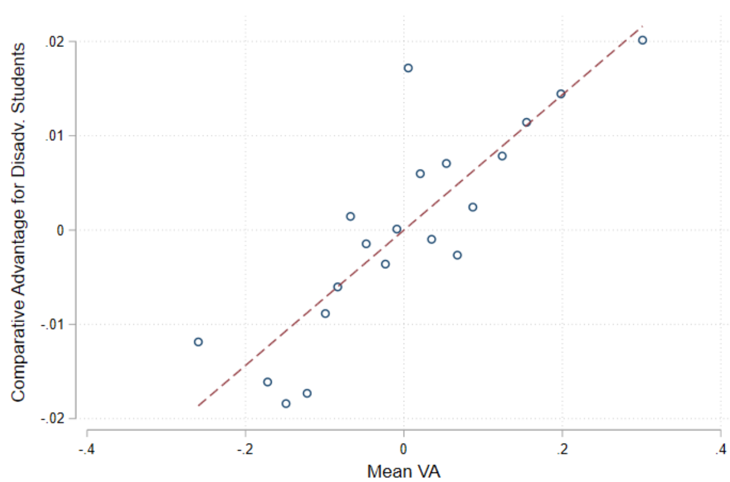
The figure is a binscatter, where an observation is a teacher-year and math value-added estimates are predictions using data from prior years. Units are student standard deviations. The y-axis is the mean student math test score, residualized by student demographics including lagged scores, school fixed effects, and teacher experience measures. The mean is taken over all students for a given teacher-year.

Figure 2: Features of classes and teachers

(a) Class size and fraction disadvantaged



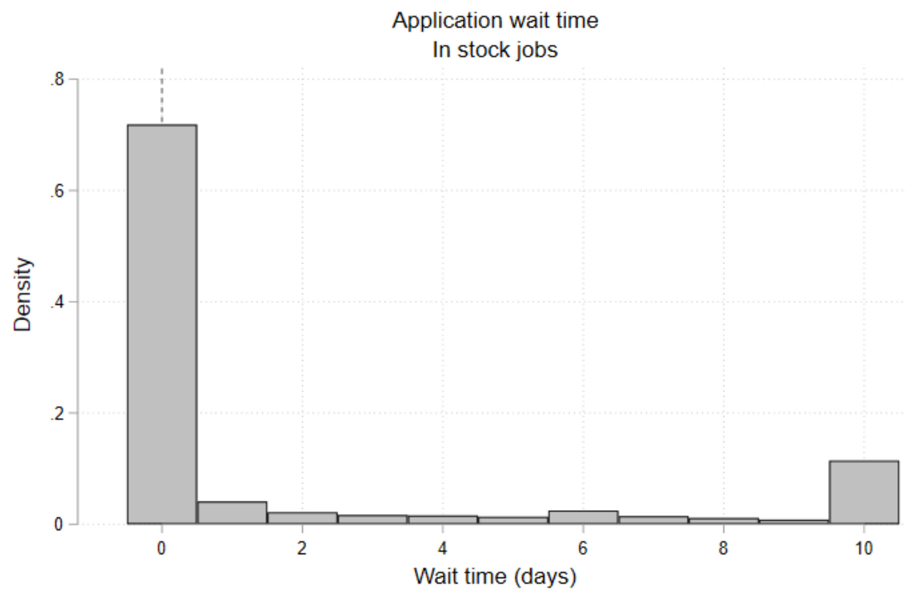
(b) Comparative advantage for disadvantaged students, and absolute advantage



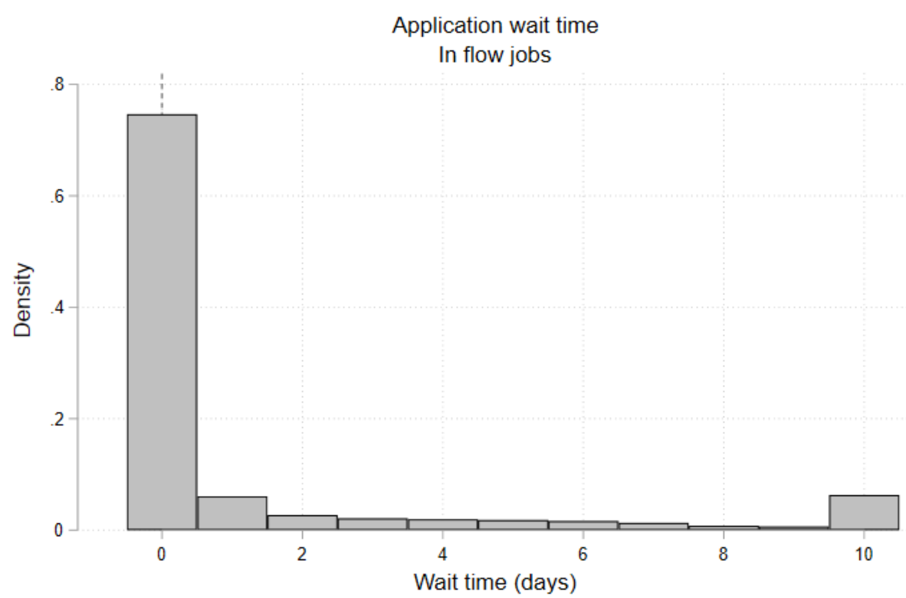
The figures show binscatters related to classroom characteristics and teacher characteristics. The top panel shows the relationship between a school's (mean) disadvantaged share of students and a school's (mean) number of students per teacher. The right-most point of the binscatter, with 100% of a school's students economically disdavantaged, accounts for 36% of the sample. The bottom panel shows the relationship between a teacher's absolute advantage (x-axis) and comparative advantage in teaching economically disadvantaged students (y-axis). For this figure, absolute advantage is the average value-added across students types (rather than the value-added at a representative school) to avoid mechanical correlations between absolute and comparative advantage.

Figure 3: Wait time to apply to vacancies

(a) Stock of vacancies



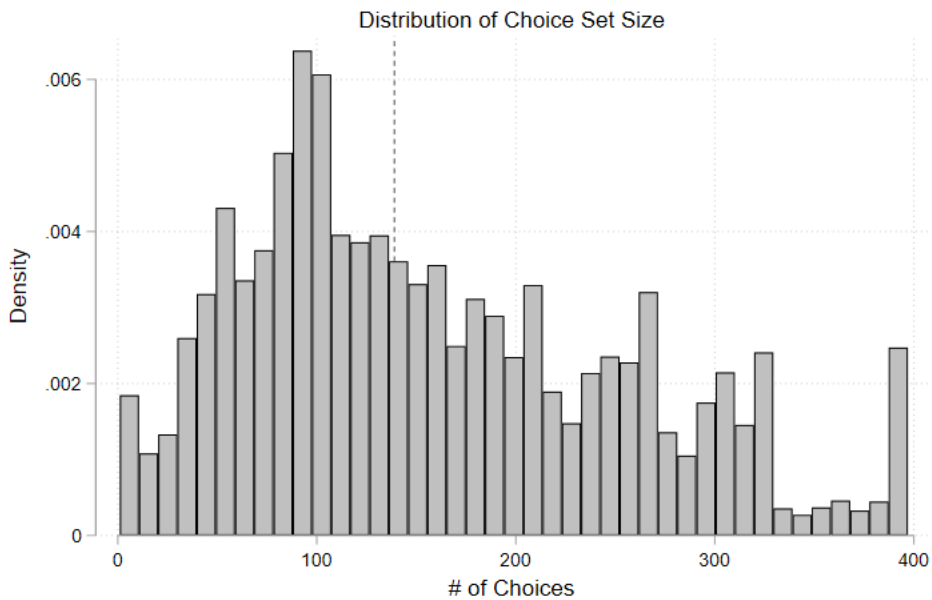
(b) Flow of vacancies



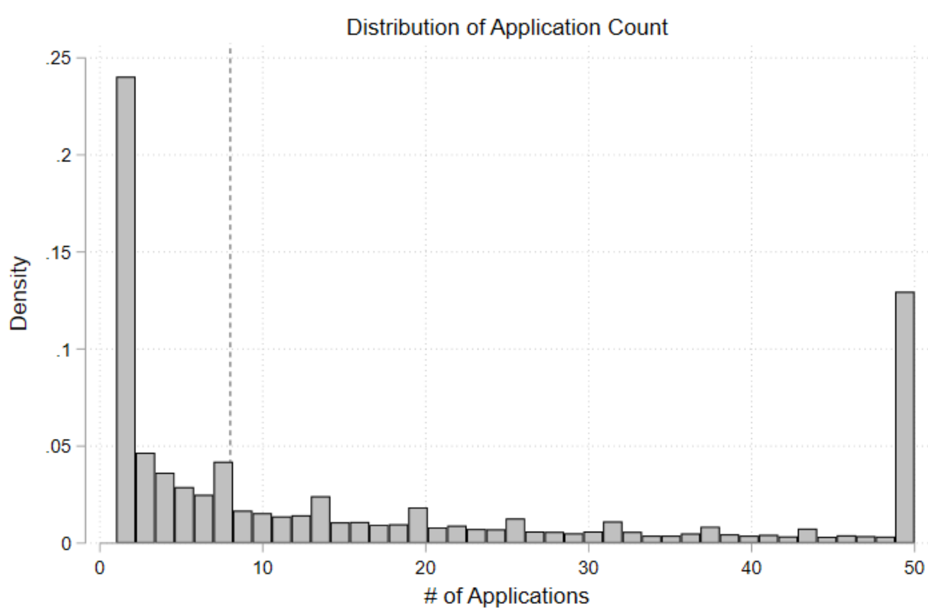
The figures show the wait time for applicants to apply to vacancies. In Panel A, we look at vacancies that were "in stock" (already posted) on the day the teacher first applied on the platform. We plot the "leave one out" wait time, where we omit one job the teacher applied to on the first day. In Panel B we look at the wait time to apply to vacancies that were posted after the teacher first applied on the platform. We measure wait time as the time from when the teacher first applied to another job (once the focal position is posted) until they apply to the posted job. We place vertical dashed lines at the median wait time.

Figure 4: Choice and application set sizes

(a) Choice sets



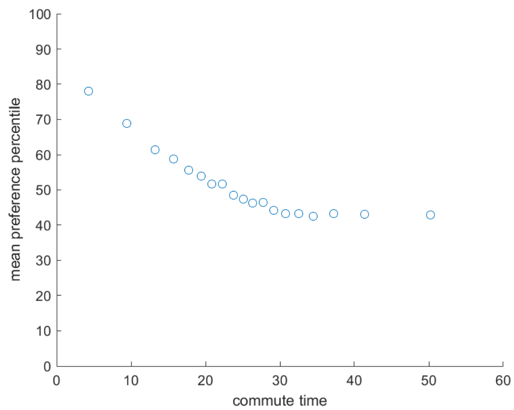
(b) Application sets



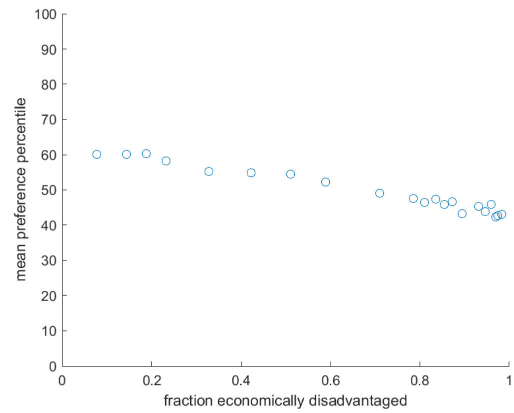
The figures are histograms of the number of positions a teacher has in her choice set (Panel A) and the number of positions a teacher applies to (Panel B). An observation is an applicant-year. Choice sets comprise the set of vacancies that are active while at some point between the teacher's first and last application in a given cycle.

Figure 5: Bivariate preference relationships

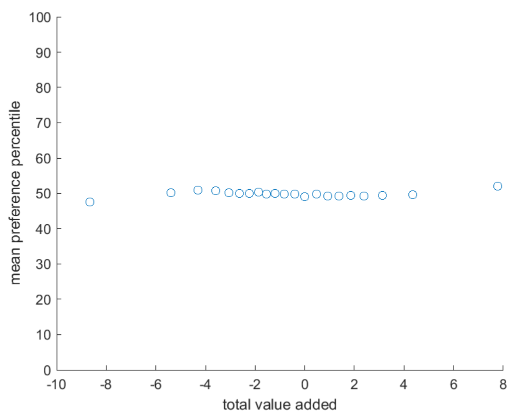
(a) Teachers: commute time



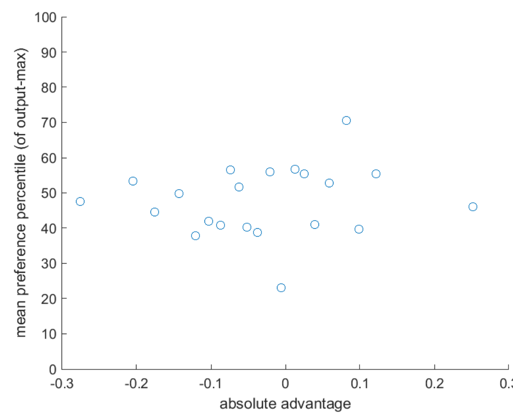
(b) Teachers: fraction disadvantaged



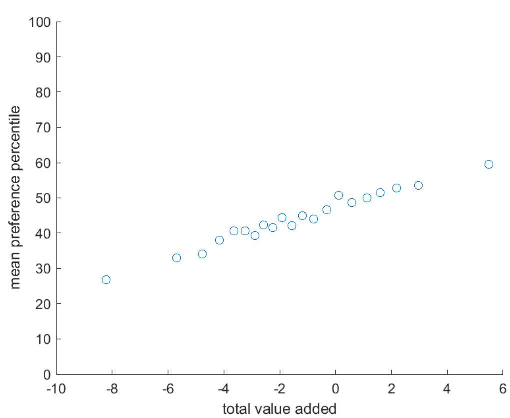
(c) Teachers: output



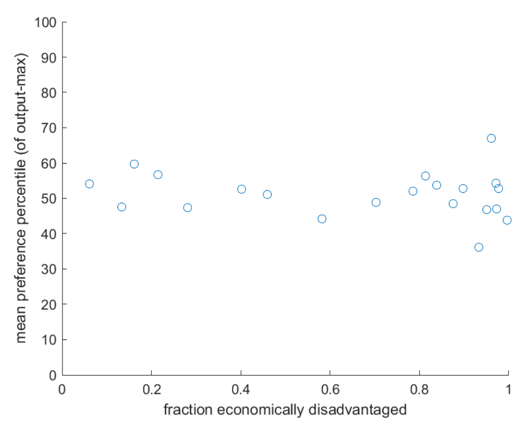
(d) Teachers: rank of first best in preferences



(e) Principals: output



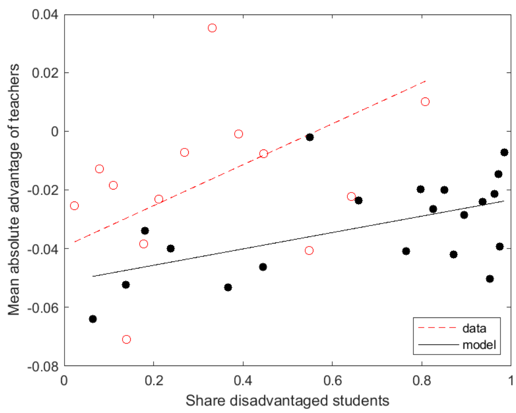
(f) Principals: rank of first best in preferences



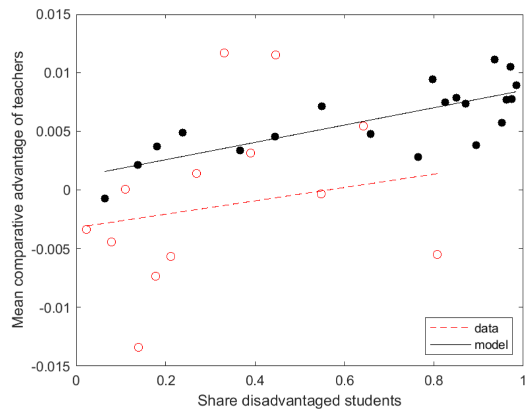
This figure shows binscatters of bivariate relationships between characteristics and preferences. In Panels (a)-(d), we estimate each teacher's ranking over positions and order positions from a teacher's most preferred (100) to least preferred (0). In Panels (e)-(f), we estimate each principal's ranking over teachers and order teachers from a principal's most preferred (100) to least preferred (0). Panels (a)-(c) show of bivariate relationships between characteristics in our teacher preference model and the position's mean preference percentile from our teacher preference mode. Panel (e) shows the bivariate relationship between the teacher's total value-added in the position and the mean preference percentile of the principal for the teacher in the principal preference model. For these bivariate relationships, we do not hold fixed other characteristics; for example, commute time may covary with other characteristics. For panels (d) and (f) we estimate the output-maximizing allocation of teachers to positions and then calculate the preference percentile for each assignment in this allocation.

Figure 6: Model fit

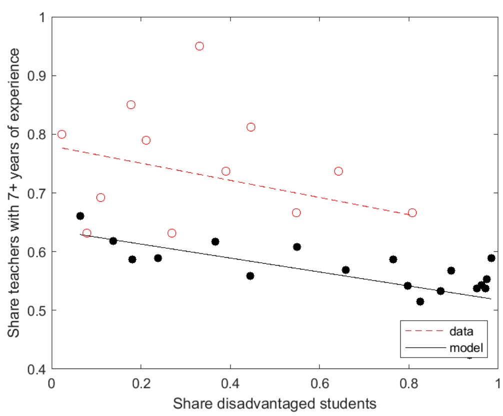
(a) Teacher absolute advantage



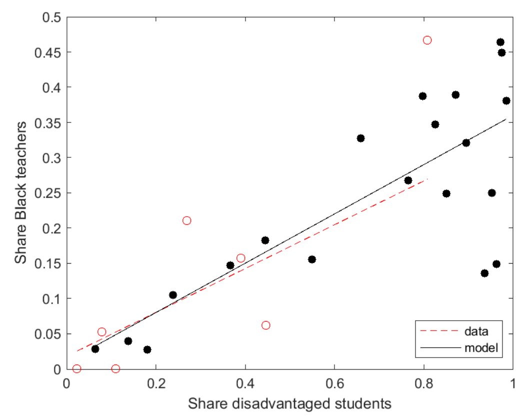
(b) Teacher comparative advantage



(c) Teachers with 7+ years of experience



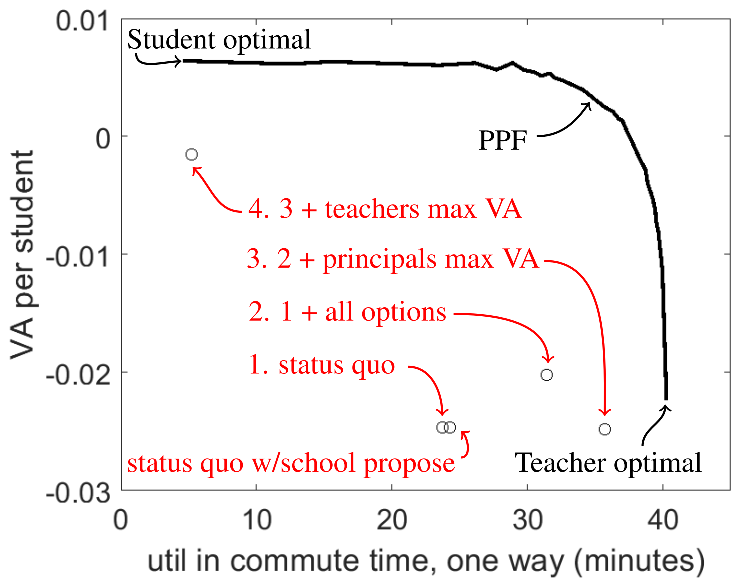
(d) Teachers that are Black



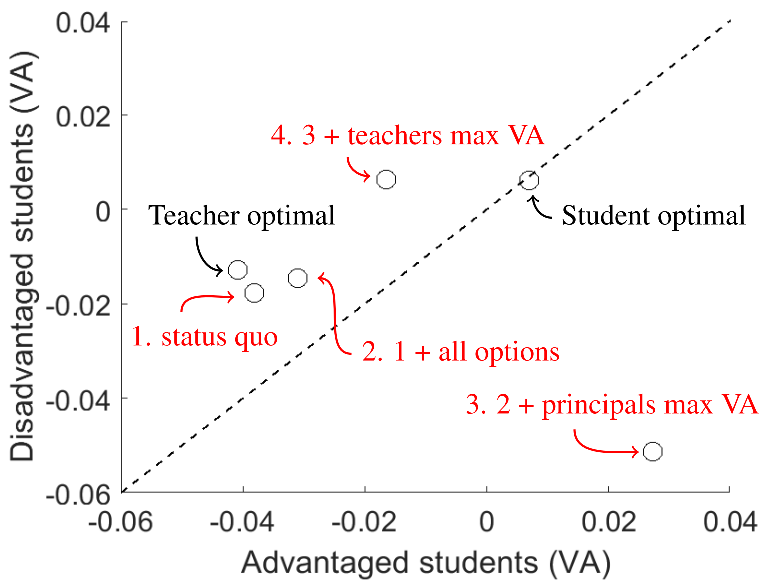
This figure compares the allocations implied by the model to the allocations we observe in the data. The data refers to all teachers in the district. The model refers to the teachers who apply in the transfer system for whom we have value-added scores. Positions are sorted on the x-axis by share of disadvantaged students.

Figure 7: Production possibilities frontier

(a) Production possibilities frontier

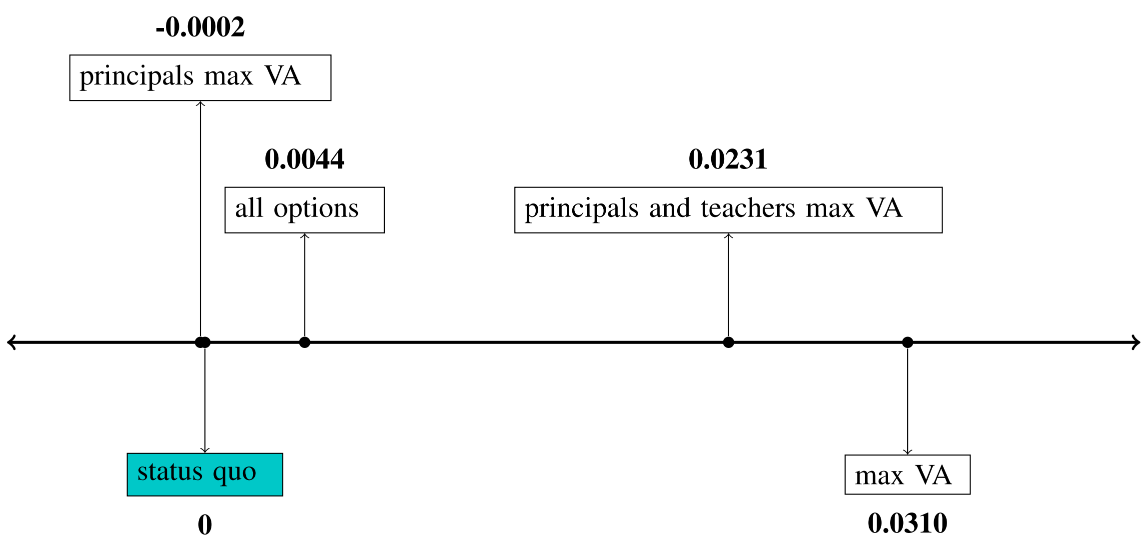


(b) Distributional consequences



This figure simulates the trade-off between teacher preferences and student achievement (Panel A) and between student achievement for economically advantaged and disadvantaged students (Panel B). The "PPF" represents the solution to the social planner's problem from placing different relative weights on teacher preferences and student achievement. The student-optimal point maximizes student achievement and is when the planner only weights students. The teacher-optimal point maximizes teacher preferences and is when the planner only weights teachers. The status quo (point 1) uses teacher and principal estimated preferences, restricted choice sets, and solves for the teacher proposing stable allocation. The status quo with school proposing allocation is the same as the status quo except it is the school-proposing solution. Point 2 takes the status quo and gives teachers and principals all options. Point 3 takes point 2 and gives principals preferences to maximize value-added. Point 4 takes point 3 and also gives teachers preferences to maximize value-added. The Figure plots averages over 200 simulations.

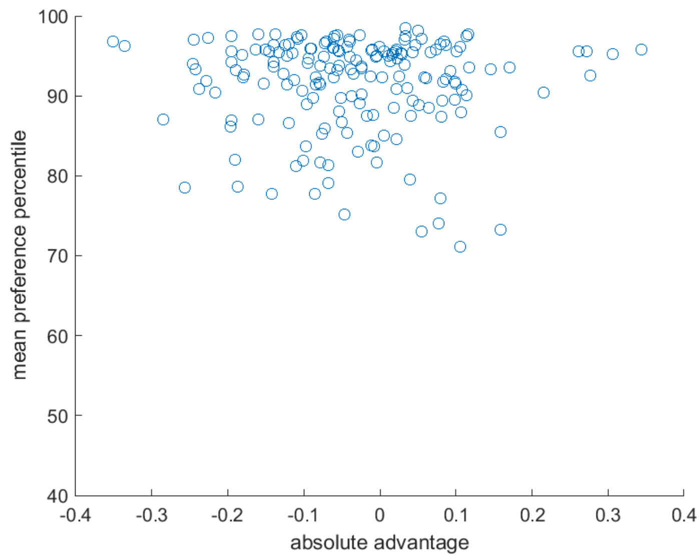
Figure 8: Summary of changes in output relative to status quo



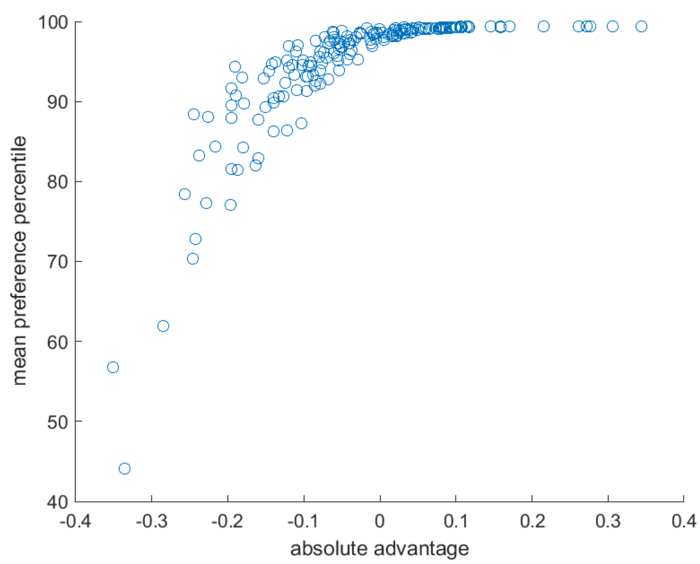
This figure summarizes the mean student achievement from allocations presented in Figure 7. The status quo output has been normalized to 0. The status quo corresponds to point 1, all options to point 2, principals maximize VA to point 3, and principals and teachers maximize VA corresponds to point 4. The "max VA" point corresponds to the "student-optimal" point in Figure 7.

Figure 9: Teacher choice rankings

(a) Estimated principal preferences



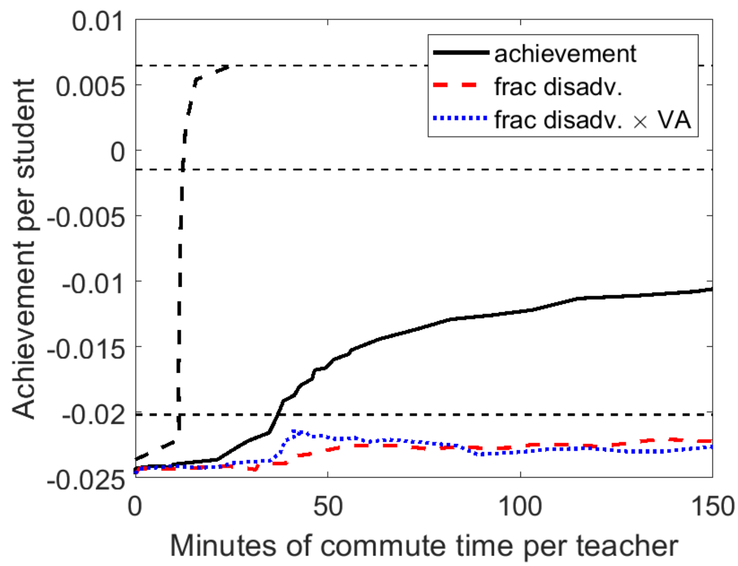
(b) Principals maximize value-added



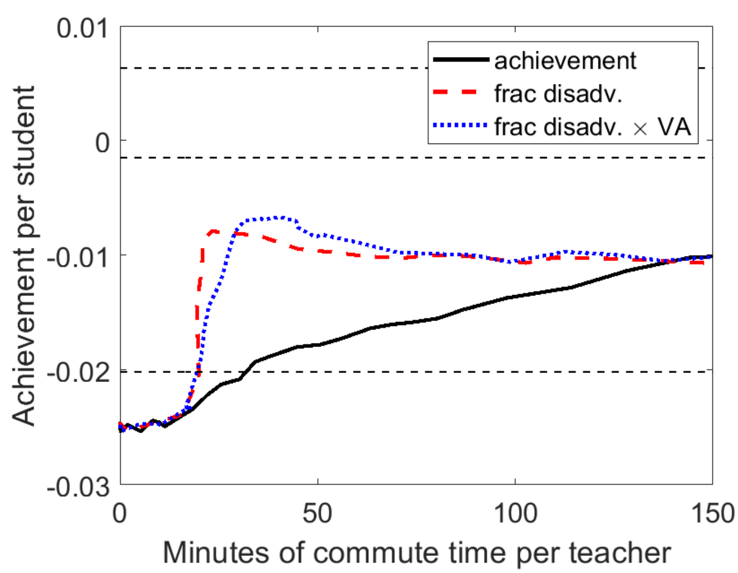
This figure presents the preference percentile of the position to which the teacher is assigned in two equilibria. We estimate a teacher's ranking of all positions and express it in percentiles, where 100 is the teacher's most preferred position. The top panel shows the status quo (point 1 in Figure 7). The bottom panel shows the same outcomes in point 3 in Figure 7: the status quo with the complete choice set, and principals maximize value-added. Teachers are ordered on the x-axis by their absolute advantage (predicted value-added at the district's representative school).

Figure 10: Bonus schemes

(a) Relative to the status quo



(b) Relative to a status quo with principal bonuses



This figure shows the effect of bonus schemes on achievement per student. In both panels, the x-axis shows the cost of the policy per teacher, which we express in minutes of commute time per teacher. The y-axis shows the benefits in terms of achievement per student. We consider three policies: subsidizing achievement directly, subsidizing the position based on the fraction of disadvantaged students in the position, and subsidizing the position based on fraction disadvantaged interacted with the teacher's absolute advantage. In the top panel, we take as the baseline allocation the status quo, and the constant part of the bonus is chosen to make teachers weakly better off relative to this allocation. In the bottom panel we replace estimated principal preferences with preferences that maximize output. The dashed line in the top panel is the cost of the first-best policy and represents movements along the PPF. The three horizontal dashed lines correspond to the output in the first-best (top), the output in the allocation where teachers and principals each maximize value-added and choice sets are complete (point 4 in Figure 7) (middle), and the output in the allocation where teachers and principals each maximize estimated preferences and choice sets are complete (point 2 in Figure 7 (bottom)).

A Data Appendix

A.1 Student-level data

We use student records from the NCERDC over the years of 2006-2007 through 2017-2018 to measure multi-dimensional teacher productivity in raising math test scores. This provides 8,177,312 student-year observations. We focus on math teachers in grades 4 through 8 to capture the majority of teachers with prior performance data who enter the applicant pool. We use third to seventh grade math and reading scores as lagged achievement. Test score data as well as student demographics such as ethnicity, gender, gifted designation, disability designation, whether the student is a migrant, whether the student is learning English, whether the student is economically disadvantaged, test accommodations, age, and grade come from the NCERDC master-build files. We use only data from standard end-of-grade exams. This leaves us with 5,322,896 student-year observations.

Beginning in the 2006-2007 school year, the state began recording course membership files linking students directly to courses and instructors. Prior to this change, teachers were linked to students through data on the proctors of the end-of-course exams. The new course membership files provide stronger teacher–subject-student links than the previous system, in which teachers were more frequently linked to the wrong subject (Harris and Sass, 2011).

With the course membership files, we still must determine which teacher is most responsible for teaching math. We use a tiered system. We use course codes (starting with "20") and course names (including text "math," "alg," "geom," and "calc") to do so. We also want to prioritize standard classes as opposed to temporary or supplemental instruction (course names including text such as "study," "special," "resource," "pullout," "remed," "enrich," "indiv," and "except"). We assign students to the teacher most likely to be the math teacher according to the following rules: (1) Students are assigned first to a high-certainty math teacher (the course code and title indicate a standard math class without mention of supplemental instruction). (2) Students with self-contained teachers are assigned to that teacher if there is no high-certainty math teacher present. (3) Students with course codes and course titles indicating math teachers but no self-contained teachers or high-certainty math teachers are assigned to those middle-certainty math teachers. (4) Students with a teacher of a course that either has a math code or a math course title but no other math course or self-contained teacher are assigned to those low-certainty math teachers. (5) Students with a science course code but no math course or self-contained courses are assigned to their science teachers to accommodate recent trends of math and science block scheduling. We exclude classes in which more than half the class requires special accommodations. Ultimately, our sample for constructing teacher value-added measures is composed of 5,159,337 student-year observations providing measures for 38,566 teachers.

A.2 Application and vacancy data

Our application and vacancy data cover the 2010-2019 cycles. We restrict our sample to applications and vacancies for on-cycle, standard elementary school positions. We show how these restrictions change the sample in Appendix Table A1.

We define on-cycle as positions that receive their first applications of a cycle between April 1 and August 15.

We select standard elementary school positions by filtering on the vacancy type (”instructional”) and the vacancy title. Seventy percent of posted vacancies are for instructional positions. We require that the position indicate elementary school grades by having at least one of the following text strings in the title: “k-”, “3rd”, “4th”, “5th”, “-5”, “-6”, “4-6”, or ”elem”. 39% of vacancies include at least one of these strings in the title.

We then exclude positions with specific subjects mentioned in the title or indications that the position is non-standard (“specialized”, “end of year”, “interim”, “assistant”, “virtual”, “resource”, “itinerant”, “exchange”, “extensions”, “immersion”, “academic support”, “temporary”, “continuous”, “early end”, “interventionist”, or “substitute”). With all of the restrictions above, our final sample consists of 20% of the full set of applications, 25% of the full set of applicants, and 7% of the full set of vacancies.

We code the application’s outcome into whether the candidate is hired (“Accepted-Pending Licensure”, “Hired”, “Hiring Request in Process”, “Offer Accepted”), declines an offer (“Offer Declined”), offered an interview (“Completed BEI Interview”, “Contact for Interview”, “Interview Scheduled”, “Invited to Complete Virtual Interview”, “Invited to Interview”, “Recommended for Interview (By Request)”), or given a positive rating (“1st Choice”, “2nd Choice”, “Highly Recommend for Interview”, “Recommend”, “Recommend for Interview”, “Recommendation Accepted”, “Strong Candidate”). These categories are encodings of a single variable, so they are mutually exclusive (i.e., if a candidate is hired, the prior outcome may be overwritten). For robustness analysis, we also split up the remaining applications into middle ratings (“Attended Info Session/Class”, “Hold for Later Consideration”, “Invited to Info Session/Class”, “Possible recommend for interview”, “Recommend with Hesitation”), negative ratings (“Failed Job Questionnaire”, “Incomplete Application”, “Ineligible Selection”, “Not Good Fit”, “Not Qualified”, “Pool - Ineligible”, “SS - INELIGIBLE”, “Screened - Not Selected”), withdrawals (“Candidate Withdrew Interest”), or no evaluation (“Eligible Selection”, “New”, “Pool - Eligible”, “Pool Candidate”).

A.3 Matching across datasets

For this project the North Carolina Education Research Data Center (NCERDC) combined records held there on teacher work histories, school characteristics, and student achievement with data provided by a large urban school district containing further personnel files, open positions within the school district, and applications for those positions. They performed an interactive fuzzy match using names and birth year. For teachers who had a sufficiently good match (that is, a unique name-birth-year combination), we have a de-identified ID that allows us to connect their platform data to their staffing records and students' achievement.

The NCERDC reports that of the 74,395 applicants to positions, 29,008 are matched to NCERDC records. Many of these applicants never teach in the state and thus would not be expected to match. Of the 26,983 employees listed within the district, 20,966 are matched to NCERDC records. However, the match rate is much better among personnel who teach tested subjects. Of the 13,982 teachers with EVAAS scores in the district, 13,865 are matched to the NCERDC data.

A.4 Sample characteristics

Returning to Appendix Table A1, we see how the sample's characteristics varies with sample restrictions. The "Elementary Sample" restricts to on-cycle elementary school instructional positions without specialization, the "Value-Added Sample" further restricts to teachers with value-added forecasts based on prior years, and the "2015 Sample" further restricts to the 2015 application cycle. We use the "Elementary Sample" for estimating principal preferences, the "Value-Added Sample" for estimating teacher preferences, and the "2015 Sample" for estimating counterfactual allocations.

We see a few expected patterns based on the sample restrictions. For the last two columns, we require teachers to have value-added forecasts based on data from prior years. This restrictions leads us to a more experienced sample of teachers. These teachers are more likely both to already be in the district and to transfer to a new school (from a prior school or from out of district). We also see these teachers have lower application rates, perhaps because many already have in-district placements. We see little change in the teacher sample's mean value-added (by student type or at a representative school) or choice set size. The mean characteristics in the positions sample also change minimally with the sample restrictions.

B Omitted details on value-added model: assumptions, results, and validation

B.1 Formal statement of assumptions for value-added model

Here we formally state the assumptions that were informally discussed in Section 3.

Assumption 1 (Exogeneity and stationarity of classroom and student-level shocks)**.** *Classroom-student-type shocks (θ_{cmt}) are independent across classrooms and independent from teachers and schools. Classroom-student-type shocks follow a stationary process:*

$$\mathbb{E}[\theta_{c0t}|t] = \mathbb{E}[\theta_{c1t}|t] = 0 \tag{A1}$$

$$Var(\theta_{c0t}) = \sigma^2_{\theta_0},\ Var(\theta_{c1t}) = \sigma^2_{\theta_1},\ Cov(\theta_{c0t}, \theta_{c1t}) = \sigma_{\theta_0\theta_1} \tag{A2}$$

for all t.

Student-level idiosyncratic variation is independent across students and independent from teachers and schools. Student-level shocks follow a stationary process depending on the student's type:

$$\mathbb{E}[\tilde{\varepsilon}_{it}|t] = 0 \tag{A3}$$

$$Var(\tilde{\varepsilon}_{it}) = \sigma^2_{\varepsilon m}\ for\ m = 0,1 \tag{A4}$$

for all t.

Assumption 2 (Joint stationarity of teacher effects)**.** *The non-experience part of teacher value-added for each student type follows a stationary process that does not depend on the teacher's school. The covariances between the teacher's value-added across student types depend only on the number of years elapsed:*

$$\mathbb{E}[\mu_{j0t}|t] = \mathbb{E}[\mu_{j1s}|t] = 0 \tag{A5}$$

$$Var(\mu_{j0t}) = \sigma^2_{\mu_0},\ Var(\mu_{j1t}) = \sigma^2_{\mu_1},\ Cov(\mu_{j0t}, \mu_{j1t}) = \sigma_{\mu_0\mu_1} \tag{A6}$$

$$Cov(\mu_{j0t}, \mu_{j0,t+s}) = \sigma_{\mu_0 s},\ Cov(\mu_{j1t}, \mu_{j1,t+s}) = \sigma_{\mu_1 s} \tag{A7}$$

$$Cov(\mu_{j0t}, \mu_{j1,t+s}) = \sigma_{\mu_0\mu_1 s} \tag{A8}$$

for all t.

Assumption 3 (Independence of drift and school effects)**.** *Let $\bar{\mu}_{jm}$ be teacher j's mean value-added for student type m. Let k be j's assigned school in year t. Then:*

$$(\mu_{jmt} - \bar{\mu}_{jm}) \perp \mu_k\ for\ m = 0,1. \tag{A9}$$

B.2 Additional details on estimation

In the first step, we estimate β_l by regressing test scores (standardized to have mean 0 and standard deviation 1 in each grade-year) on a set of student characteristics (X_{it}) and classroom-student-type fixed effects:

$$A^*_{it} = \beta_s X_{it} + \lambda_{cmt} + \upsilon_{it}. \tag{A10}$$

For characteristics, we include ethnicity, gender, gifted designation, disability designation, whether the student is a migrant, whether the student is learning English, whether the student is economically disadvantaged, test accommodations, age, and grade-specific cubic polynomials in lagged math and lagged reading scores. We subtract the estimated effects of the student characteristics to form the first set of residuals, $\hat{\nu}_{it}$:[19]

$$\hat{\nu}_{it} = A^*_{it} - \hat{\beta}_s X_{it}. \tag{A11}$$

These student-level residuals include teacher, school, and classroom components, as well as idiosyncratic student-level variation.

In the second step, we project the residuals onto teacher fixed effects, school fixed effects, and the teacher experience return function. Following the literature, we specify the experience return function as separate returns for every level of experience up to 6 years, and then a single category of experience of at least 7 years:

$$\hat{\nu}_{it} = \sum_{e=1}^{6} \alpha^e \mathbb{1}\{Z_{jt} = e\} + \alpha^7 \mathbb{1}\{Z_{jt} \geq 7\} + \mu_{jm} + \mu_k + \mu_t + \varepsilon_{it}, \tag{A12}$$

where $\varepsilon_{it} = (\mu_{jmt} - \mu_{jm}) + \theta_{cmt} + \tilde{\varepsilon}_{it}$. We then form a second set of student-level residuals by subtracting off the estimated school and experience effects:

$$A_{it} = \hat{\nu}_{it} - \left(\sum_{e=1}^{6} \hat{\alpha}^e \mathbb{1}\{Z_{jt} = e\} + \hat{\alpha}^7 \mathbb{1}\{Z_{jt} \geq 7\} + \hat{\mu}_k + \hat{\mu}_t \right). \tag{A13}$$

We aggregate these student-level residuals into teacher-year mean residuals for each student type: $\bar{A}_{jmt}$. Let $\mathbf{A}_j^{-t}$ be a vector of mean residuals for each student type-year that j teaches in the data, prior to year t.

In the final step, we form our estimate of teacher j's value-added (net of experience effects) in year t for type m as the best linear predictor based on the prior data in our sample:

$$\hat{\mu}_{jmt} \equiv \mathbb{E}^* \left[\mu_{jmt} | \mathbf{A}_j^{-t} \right] = \psi_m' \mathbf{A}_j^{-t}, \tag{A14}$$

where ψ_m is a vector of reliability weights. We estimate ψ_m following Delgado (2021). Our estimate of teacher j's composite value-added at school k in year t is:

$$\widehat{VA}_{jkt} = p_{k0t}\hat{\mu}_{j0t} + p_{k1t}\hat{\mu}_{j1t} + f(Z_{jt}; \hat{\alpha}). \tag{A15}$$

Variation in the data: We now discuss the variation in the data that pins down key parameters. The coefficient on student characteristics uses how test scores vary with within-classroom-student type variation in student characteristics.[20] The school effects use the change in (student) output

[19] Here we deviate from the standard notation, by introducing $\hat{\nu}_{it}$. Our procedure has two residualization steps because we include classroom-student type fixed effects in the first step, which would subsume the teacher and school fixed effects. We thus decompose student residuals into teacher and school components in a second step.

[20] Because we include classroom-student-type fixed effects, our model allows for an arbitrary correlation between students' characteristics and the quality of their assigned teachers. Allowing such correlation is important in a context where teachers have some control over where they work.

when teachers switch schools, beyond what would be predicted by drift and by the change in student type composition. Heuristically, if teachers' output regularly increases when teachers transfer to a certain school, then we would estimate a high school effect. The teacher mean effects for each student type are pinned down by relative increases in students' (residualized) test scores across different teachers. We are able to rank teachers both within and across schools, provided teachers and schools are in a set connected by transfers so that we can identify the school effects.

Finally, we identify the parameters of the teacher value-added distribution and the drift process based on the stationarity assumptions and the observations of teachers across years, classrooms, and student types. As an example, the variance of the teacher effects for student type m is identified by the covariance between a teacher's mean student residuals for student type m in two different classrooms in the same year.[21] With our assumptions that classroom and student shocks are uncorrelated across classrooms, the only reason a teacher's students would have similar (residualized) outcomes is the teacher's value-added.

B.3 Testing for comparative advantage

Our measures forecast teachers' future value-added without bias. Our high estimated correlation between a teacher's effectiveness with the two student types raises the question of whether our estimates of comparative advantage simply reflect statistical noise. We perform three exercises to test our multi-dimensional value-added model versus a single-dimensional model.

First, we estimate standard errors and confidence intervals for the structural parameters in our production model. The estimated correlation in teacher value-added across student types is 0.86. We can, however, decisively reject a correlation of 1 as the bootstrap standard error is 0.035, with a 95% confidence interval of $(0.73, 0.87)$ (Appendix Table A2).

Second, we perform a likelihood-ratio test comparing our model with a model with one-dimensional teacher value-added. We take the mean residuals at the level of the teacher-classroom-student type, $\bar{A}_{jcmt}$, and collect a teacher's mean residuals across classrooms and student types, which come from a normal distribution:

$$\begin{pmatrix} \bar{A}_{jc1t} \\ \bar{A}_{jc'2t} \end{pmatrix} \sim \mathcal{N}\left(\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} \sigma^2_{\mu_1} + \sigma^2_{\theta_1} + \frac{\sigma^2_{\varepsilon_1}}{N_{jc0t}} & \sigma_{\mu_1\mu_2} \\ \sigma_{\mu_1\mu_2} & \sigma^2_{\mu_2} + \sigma^2_{\theta_2} + \frac{\sigma^2_{\varepsilon_2}}{N_{jc2t}} \end{pmatrix} \right). \tag{A16}$$

We compare the likelihoods across our baseline model and an alternate model of homogeneous value-added where $\sigma^2_{\mu_1} = \sigma^2_{\mu_2}$, $\sigma^2_{\theta_1} = \sigma^2_{\theta_2}$, $\sigma^2_{\varepsilon_1} = \sigma^2_{\varepsilon_2}$, and $\sigma_{\mu_1\mu_2} = 0$. Our likelihood-ratio test has 4 degrees of freedom, and we reject the homogeneous value-added model in favor of the heterogeneous model, with a test statistic of 610, so the p-value is arbitrarily small ($p < 0.0001$).[22]

[21] In our setting many elementary school teachers have students from multiple classes. The prevalence of multiple classrooms is increasing over time (Appendix Table A9).

[22] We restrict the sample to one randomly-chosen vector of mean residuals per teacher so that the observations in our

Third, we fix a teacher's type according to whether she is above or below the median in comparative advantage in teaching economically disadvantaged students in pre-transfer schools. We then test whether changes in the share of economically disadvantaged students differentially predict changes in student test score residuals ($\hat{v}_{it}$ from equation A13) in post-transfer schools by teacher-type. The logic of the test is as follows. Under a homogeneous value-added model, changes in the share of economically disadvantaged students should have no bearing on changes in teacher productivity across schools. If our estimated comparative advantage is meaningful, however, then as the share of disadvantaged students rises, teachers with a comparative advantage in teaching disadvantaged students should see gains in average productivity relative to teachers with a comparative advantage in teaching economically advantaged students. Accordingly, we regress across-transfer changes in teacher-by-school average student residuals on across-transfer changes in the share of disadvantaged students interacted with teachers' type. The results appear in Appendix Table A10. For teachers with a comparative advantage in teaching advantaged students in pre-transfer schools, productivity falls as the share of disadvantaged students rises (p-value=0.043). In contrast, for teachers with a comparative advantage in teaching disadvantaged students, productivity rises as the share of disadvantaged students rises (p-value=0.014). These findings indicates that comparative advantage is persistent across settings and predictive of match-specific productivity.

C Within-school assignments

Our analysis focuses on the allocation of teachers across schools in a district. Another margin of allocation could be within-school assignment of teachers based on class size or composition. Ignoring this margin could affect our results in two ways. First, we could understate the potential allocation gains (or even focus on the less important margin). In Table 2 we show that the gains to within-school reallocation are much smaller than the gains from reallocation across schools.

Second, if within-school position characteristics are endogenous, our preference model might be misspecified. For example, suppose that an experienced teacher can negotiate for the Honors class at a school but the inexperienced teacher cannot. We assess this possibility in two ways.

C.1 Persistence of classroom characteristics

If within-school assignment characteristics were endogenous and a function of the teacher's type, we would expect persistence in these characteristics over time. In Appendix Tables A11 and A12, we show the autocorrelations in number of students taught by a teacher and the fraction of students that are economically disadvantaged. In each table's top panel, we show the school-level autocorrelation. We find that differences across schools – which we leverage in our analysis – are fairly persistent.

likelihood are independent. We also find a similar test statistic when we use mean residuals, $\bar{A}_{jcmt}$, from a model where the fixed effects in the residualizing steps are not separated by student type.

In each table's bottom panel, we show the teacher-level autocorrelation where we residualize by school-year fixed effects to isolate the within-school deviation. These within-school differences across teachers – which we do not leverage in our analysis – are not persistent at all.

C.2 Do teachers bargain over student assignment on the job market?

Second, we examine how students are assigned to teachers within and across schools. This question is of particular interest since we would like to know whether teachers bargain with principals over their student assignments. Are sought-after teachers assigned "preferable" class compositions? The primary teacher characteristic we use is *experience*, which principals value and is reliably measured in our data. We first explore the relationship visually. Student attributes have a linear relationship with log(experience), so we estimate models in which the outcome variables are student attributes and the primary explanatory variable is a teacher's log(experience). In regressions, standard errors are clustered by teacher and by year.

Without controlling for school setting, there is a strong relationship between experience and student attributes (see Appendix Table A13). More experienced teachers are assigned higher-scoring students, fewer economically disdavantaged students, more students designated as gifted, and fewer Black students.

Much sorting takes place across schools—as teachers gain experience, they sort to more suburban schools where students are less economically disadvantaged and higher achieving. In the basic cross section, we find that a 100 percent increase in experience reduces poverty shares by 0.037 percentage points (significant at the 0.001 level). When we control for year and school fixed effects, the coefficient on (log) experience falls by over 80 percent to 0.006 (significant at the 0.001 level). We examine the gradient among newly hired teachers. This group is of particular relevance because applicants (as opposed to teachers not applying to new jobs) are the teachers we consider in our counterfactual exercises. When looking at this group, we find no significant relationship between experience and disadvantaged-student assignment, conditional on school-year fixed effects. This suggests that principals do not sort students to teachers based on experience within a school, and indicates that bargaining over student characteristics is unlikely.

The pattern of sorting Black students to teachers is quite similar. We find that doubling teacher experience reduces the Black share of a teacher's class by 0.033 percentage points (significant at the 0.001 level). When looking within a school, the experience gradient falls by 97 percent—the sorting of Black students to teachers is almost exclusively across schools. When we examine the relationship among new hires, the relationship is even smaller and statistically insignificant. The gradient between student test scores and teacher experience attenuates by 90 percent when accounting for school-year fixed effects. There still is a small, systematic difference in test scores which appears to arise from hiring more experienced teachers to serve in gifted-and-talented classrooms. We see very experienced teachers assigned somewhat less desirable class assignments than would be predicted by the rest of the support. It may be that schools encourage older teachers to leave by giving them more difficult class compositions.

In summary, among new teachers, experience does not significantly predict assignment to disadvantaged students or Black students within schools. There is a small experience gradient for higher achieving students among new teachers. It seems teachers of gifted-and-talented classrooms tend to be senior.

D Heterogeneity in application rate gap between Title I and non-Title schools

To showcase unobservable preference heterogeneity, we focus on teacher preferences over a binary characteristic: whether the school has Title I designation. Appendix Table A16 shows that on average teachers are less likely to apply to Title I schools. The application rate to non-Title I schools is almost 16% and to Title I schools is about 14%, and leaving an application gap of close to 2 percentage points (or 10%).

The second and third columns of Appendix Table A16 show why we are able to estimate heterogeneity precisely: the median number of applications choices that each teacher makes is over 65 for both Title I positions and non-Title I positions. Thus, teachers' application sets have the potential to include many or few Title I positions.

Appendix Figure A20 shows that the mean gap in application rates across school types masks substantial heterogeneity. For each teacher, we calculate the gap in application rates (for positions in the teacher's choice set) between Title I and non-Title I schools, and then we plot the distribution of the gap. Visually, the distribution almost appears centered on zero (the median is 0.003). But there is substantial dispersion: the standard deviation of the raw gaps is 0.134.

Naturally, the standard deviation of the raw gap overstates the extent of dispersion because it incorporates noise. We now describe and implement a simple minimum distance estimator for the true standard deviation of the applicant gap. For each teacher j we observe a_{j1} applications sent to a Title I school and c_{j1} is the number of Title I vacancies in the teacher's choice set. We can then estimate

$$\hat{p}_{j1} = \frac{a_{j1}}{c_{j1}}$$

or teacher j's application probability to a Title I school.

Using the natural notation for a "not-Title I" school, we also have:

$$\hat{p}_{j0} = \frac{a_{j0}}{c_{j0}}$$

We can then compute the "gap", or Title I penalty, as

$$\hat{g}_j = \hat{p}_{j1} - \hat{p}_{j0}.$$

We are then interested in the distribution of these gaps – e.g., the standard deviation (sd) of g_j. Naturally, taking $sd(\hat{g}_j)$ will result in an over-estimate of the amount of dispersion.

We fit the following model.

$$p_{j0} = \hat{\bar{p}}_0$$

$$p_{j1} = N(\hat{\bar{p}}_1, \sigma)$$

where $\hat{\bar{p}}_.$ are the population average application probabilities and σ is a parameter to estimate. We estimate σ by simulated method of moments where the moment to match is $sd(\hat{g}_j)$ and we simulate data from the model embedded in the previous two equation using the observed $\{c_{j0}, c_{j1}\}$.

We find that the estimated standard deviation is 0.114, so the visual depiction of noise is in line with the underlying truth. If we take the minimum distance estimate at face value, while on average teachers have higher application probabilities to non-Title I schools, about 44% of teachers have higher application probabilities to Title I instead. Hence, this suggests that even though on average teachers prefer non-Title I schools, there is a substantial amount of heterogeneity in the applicant pool. Depending on how such preference heterogeneity maps into the existing allocation of teachers to schools, policies that make Title I schools more attractive could have small or large effects on teachers' application rates.

E Selection into the transfer market

What explains the differences in student gains between Table 2 and the results depicted in Figure 7b? Here, we compare the teacher transfer market to the broader sample of teachers and positions. We first consider the representation of schools in the transfer market. Unsurprisingly, we see significant over-representation for positions in schools with high proportions of economically disadvantaged students. Appendix Table A14 shows that a 10 percentage point increase in the share of economically disadvantaged students is associated with 0.15 more positions posted. Because the overrepresented type of school is already the more common one (more than half of the students in our district are economically disadvantaged) this means that gains from sorting on comparative advantage are going to be understated in our transfer sample.

The pattern is less pronounced for the number of students a teacher instructs. Though the point estimate implies that an additional 10 students per teacher lowers the number of positions a school posts by 0.2, this relationship is largely driven by outliers, as shown in Appendix Figure A19.

To examine the selection of teachers into the transfer market, we first look at four cohorts, 2010-2013, such that we can follow them for five years. We further restrict attention to those for whom we can measure productivity, leaving us with 553 teachers who entered the state's data during those years. Of those, 207 applied to transfer at some point during the first five years. Only 124 remain in their original school and have not applied to transfer within five years of entering the district. The remaining 287 leave the district. Appendix Table A15 shows that there is very little difference in comparative advantage between teachers who applied to transfer and the teachers who did not. Teachers who apply for transfer have lower and less variable absolute advantage.

Accordingly, it is unlikely that the difference in per-student potential gains is due to teacher selection into transferring, particularly with regard to comparative disadvantage (with disadvantaged students). It is possible that the small differences in absolute disadvantage interacted with the under-representation of large classes accounts for some of the gap. The clearest selection into the transferring market, however, comes from the over-representation of schools with a high concentration of disadvantaged students. With a limited distribution of schools, there is less room to realize the gains from teachers' comparative advantages.

F Principal preferences estimation

We estimate principal preferences via maximum simulated likelihood, where we simulate from the normal distributions of the random effect at the level of the position-year. Let n index each simulation iteration and let $B_{jptn}(\theta)$ be the model-predicted probability that p rates j positively in year t in simulation iteration n at parameter vector θ. For each position p in year t, we construct the simulated likelihood as:

$$L_{pt} = \frac{1}{100}\sum_{n=1}^{100}\prod_{j\in\mathcal{J}_{pt}}(b_{jpt}B_{jptn}(\theta)+(1-b_{jpt})(1-B_{jptn}(\theta))), \tag{A17}$$

where $\mathcal{J}_{pt}$ is the set of teachers who applied to a position p in year t and b_{jpt} is an indicator for whether p rated j positively in the data. Our full simulated log likelihood function is:

$$l = \frac{1}{P}\sum_{p} logL_{pt}. \tag{A18}$$

Table A1: Sample and summary statistics

	Full Sample	Elementary Sample	Value-Added Sample	2015 Sample
Applications				
N	2,163,711	337,754	13,819	2,702
On-Cycle	0.68	1.00	1.00	1.00
Instructional	0.70	1.00	1.00	1.00
Elementary	0.39	1.00	1.00	1.00
Applicants				
N	104,795	14,864	867	178
Female		0.92	0.87	0.89
Black		0.24	0.30	0.25
Hispanic		0.03	0.01	0.03
In-District		0.12	0.43	0.44
Choice Set Size		159.10	151.14	151.35
Application Rate		0.18	0.11	0.10
Transferred		0.23	0.43	0.51
Mean Commute Time		17.78	22.57	22.50
Experience		5.81	9.22	9.89
VA Econ Adv		-0.03	-0.03	-0.04
VA Econ Disadv		-0.02	-0.02	-0.03
Abs Adv		-0.03	-0.03	-0.03
Comp Adv in Econ Disadv		0.01	0.01	0.01
Positions				
N	38,921	1,824	1,784	296
Choice Set Size		1,293.54	71.89	88.63
Application Rate		0.14	0.11	0.10
Mean Class Size		26.40	26.40	25.69
Frac Econ Disadv		0.65	0.65	0.68
Frac Black		0.43	0.43	0.45
Frac Hispanic		0.24	0.24	0.25

The table shows count or mean statistics across different samples. The “Full Sample” includes all of the raw data, the “Elementary Sample” restricts to on-cycle elementary school instructional positions without specialization, the “Value-Added Sample” further restricts to teachers with value-added forecasts based on prior years, and the “2015 Sample” further restricts to the 2015 application cycle (for positions in the 2016 school year). We use the “Elementary Sample” for estimating principal preferences, the “Value-Added Sample” for estimating teacher preferences, and the “2015 Sample” for estimating counterfactual allocations. We do not include mean statistics for applicants and positions for the complete sample because we built the data on the subsample. Commute time is measured in minutes, absolute advantage is value-added at the representative school in the district, and choice set size is the number of positions in a teacher’s choice set (Applicants panel) or the number of teachers with the position in their choice set (Positions panel).

Table A2: Teacher Value-Added Structural Parameters

	Estimates	Standard Errors	95% CI Lower Bound	95% CI Upper Bound
$\sigma_{\varepsilon 1}$	0.450	0.000	0.456	0.457
$\sigma_{\varepsilon 2}$	0.470	0.000	0.477	0.479
$\sigma_{\theta 1}$	0.110	0.007	0.108	0.137
$\sigma_{\theta 2}$	0.088	0.015	0.089	0.143
correlation$(\theta_{c0t}, \theta_{c1t})$	0.657	0.162	0.126	0.844
$\sigma_{\mu 1}$	0.249	0.007	0.262	0.284
$\sigma_{\mu 2}$	0.243	0.015	0.254	0.316
correlation(μ_{j0t}, μ_{j1t})	0.859	0.035	0.729	0.872

The table shows the estimates of a subset of the structural parameters of the production model – specifically the parameters corresponding to contemporaneous output. Non-disadvantaged students have index 1 while disadvantaged students have index 2. ε is the student-year idiosyncratic component, θ captures classroom effects, and μ describes a teacher's value-added. The remaining structural parameters describe the drift process of teacher value-added over time. Standard errors and confidence intervals are estimated with 100 bootstrap iterations.

Table A3: Estimated Experience Returns to Teacher Value-Added

	1	2	3	4	5	6	7+
Estimate	0.056	0.077	0.083	0.088	0.088	0.091	0.070
Standard Error	0.004	0.004	0.005	0.005	0.005	0.005	0.005

The table shows the estimated experience returns for math test scores, where the scores have been normalized to have mean 0 and standard deviation 1 for students in a given grade-year. Columns designate the number of prior years of experience. The omitted category is teachers with no prior experience.

Table A4: Potential Gains from Reassignment – Test Score Percentiles

	Per-Student Gains (σ)	As a Fraction of (Best-Actual)	Non-Disadvantaged	Disadvantaged
Alternate Allocations				
Best	0.050		0.089	0.016
Random	-0.003	-0.19	0.017	-0.021
Worst	-0.053	-3.63	-0.052	-0.054
Alternate Policies				
Best w/i School	0.012	0.98	0.015	0.010
Replace Bottom 5% of Teachers	0.011	0.74	0.012	0.010
Targeting Student Types				
Max Non-Disadvantaged VA	0.024	1.66	0.130	-0.072
Max Disadvantaged VA	0.016	1.12	-0.047	0.074

The table shows the potential gains from reassignments of teachers to different schools. Test scores are constructed as the raw score percentile (from 0 to 1), where percentiles are calculated for each grade-year in the state. We then normalize the test scores to be in standard deviation units based on the standard deviation of the uniform distribution. The sample is all teachers with non-missing value-added measures in 2016, along with their corresponding 2016 assignments. Gains come from better matching of teachers to students, as teachers' effectiveness may differ across student types. The first column shows the per-student gains from various allocations relative to the actual allocation. The second column shows the gain as a fraction of the full difference between the best (output-maximizing) and actual allocations. The third and fourth columns show the per-student gains, relative to the actual allocation, for non-disadvantaged and disadvantaged students. The best within school allocation only changes the teacher-classroom assignments within a school. "Replacing Bottom 5% of Teachers" refers to replacing the bottom 5% of teachers according to realized per-student output with teachers with median value-added for each student type. The targeting student types allocations are the ones that maximize per-student output for students of one type only. We assign classrooms the mean student composition and class sizes in that school in 2016 in all allocations except the "Best w/i School" and "Constant Class Size" allocations.

Table A5: Potential Gains from Reassignment – Constant Class Size

	Per-Student Gains (σ)	As a Fraction of (Best-Actual)	non-Disadvantaged	Disadvantaged
Alternate Allocations				
Best	0.021		0.018	0.023
Random	0.000	0.02	0.002	-0.001
Worst	-0.020	-0.94	-0.015	-0.023
Alternate Policies				
Best w/i School	0.003	0.16	0.007	0.000
Replace Bottom 5% of Teachers	0.017	0.82	0.023	0.012
Targeting Student Types				
Max Non-Disadvantaged VA	0.003	0.17	0.123	-0.090
Max Disadvantaged VA	0.005	0.22	-0.111	0.096

The table shows the potential gains from reassignments of teachers to different schools where each school has the same number (but possibly different composition) of students per class. The sample is all teachers with non-missing value-added measures in 2016, along with their corresponding 2016 assignments. Gains come from better matching of teachers to students, as teachers' effectiveness may differ across student types. The first column shows the per-student gains from various allocations relative to the actual allocation. Gains are measured in student standard deviations (σ). The second column shows the gain as a fraction of the full difference between the best (output-maximizing) and actual allocations. The third and fourth columns show the per-student gains, relative to the actual allocation, for non-disadvantaged and disadvantaged students. The best within school allocation only changes the teacher-classroom assignments within a school. "Replacing Bottom 5% of Teachers" refers to replacing the bottom 5% of teachers according to realized per-student output with teachers with median value-added for each student type. The targeting student types allocations are the ones that maximize per-student output for students of one type only. We assign classrooms the mean student composition and class sizes in that school in 2016 in all allocations except the "Best w/i School" and "Constant Class Size" allocations.

Table A6: Same-Race and Same-Gender Effects on Test Scores

	Student Res
Black Teacher - Black Student	0.00225
	(0.00164)
Hispanic Teacher - Hispanic Student	-0.00556
	(0.00549)
Female Teacher - Female Student	0.00478
	(0.000550)
Fixed Effects	Teacher, School
Mean DV	0.0000115
Clusters	37940
N	5158740

An observation is a student-year and the outcome is the student's math score residualized by student demographics including lagged scores, school fixed effects, and teacher experience measures. The regressors include measures of demographic match between student and teacher. The regression includes school fixed effects and teacher fixed effects. Standard errors are clustered at the teacher level.

Table A7: Teacher Value-Added Structural Parameters with Alternate Forms of Heterogeneity

	Race	Achievement
$\sigma_{\varepsilon 1}$	0.465	0.481
$\sigma_{\varepsilon 2}$	0.457	0.439
$\sigma_{\theta 1}$	0.091	0.099
$\sigma_{\theta 2}$	0.110	0.102
correlation$(\theta_{c0t}, \theta_{c1t})$	0.637	0.628
$\sigma_{\mu 1}$	0.233	0.240
$\sigma_{\mu 2}$	0.261	0.282
correlation(μ_{j0t}, μ_{j1t})	0.900	0.844

The table shows the estimates of a subset of the structural parameters of production models with alternate forms of heterogeneous teacher effects – specifically by race and prior achievement. In the first column, non-white students have index 1 while White students have index 2. In the second column, students with below median prior math achievement have index 1 while students with above median prior math achievement have index 2. ε is the student-year idiosyncratic component, θ captures classroom effects, and μ describes a teacher's value-added. The remaining structural parameters describe the drift process of teacher value-added over time.

Table A8: Application timing

	Obs	Mean days	Median days	Share 0 days
Stock	196,779	3.6	0	0.72
Flow	146,382	2.1	0	0.75

(a) Wait times until applying

	Obs	Mean fraction of days	Mean fraction of applications	Mean days since posting
First day	14,864	0.61	0.65	23.47
Subsequent days	40,850	0.14	0.13	11.55

(b) First day versus subsequent days

	Obs	April or before	May	June	July	August
First day (all teachers)	14,864	0.20	0.25	0.22	0.18	0.15
Last day (all teachers)	14,864	0.09	0.15	0.21	0.26	0.29
First day (transfers)	2,547	0.27	0.30	0.24	0.14	0.05
Last day (transfers)	2,547	0.10	0.17	0.25	0.29	0.19

(c) Timing of first and last days

The tables show statistics related to application timing. Panel (a) shows how long it took an applicant to apply to positions that were in "stock" (already posted) on the day the teacher first applied on the platform or in "flow" (posted after the day the teacher first applied on the platform). Panel (b) shows application statistics for the first day a teacher applied on the platform in a cycle versus subsequent days. "Mean days since posting" is the mean number of days a vacancy had been posted at the time the teacher applied. Panel (c) shows the (monthly) timing of when an applicant's first and last application days of the cycle occurred. "All teachers" includes all applicants while "transfers" includes just teachers who ended up in new schools.

Table A9: Multi-classroom teacher prevalence

Year	All	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8
2012	0.264	0.109	0.187	0.618	0.621	0.631
2013	0.287	0.124	0.210	0.636	0.631	0.649
2014	0.300	0.152	0.227	0.633	0.625	0.644
2015	0.363	0.256	0.345	0.615	0.598	0.602
2016	0.391	0.305	0.392	0.595	0.591	0.595
2017	0.385	0.291	0.399	0.612	0.569	0.596
2018	0.393	0.307	0.425	0.596	0.586	0.578
Estimation sample	0.417					

The table shows the prevalence of teachers having multiple classrooms, separately by teacher's grade and year. The sample includes teachers for whom we can calculate math value-added. Our estimation sample consists of teachers, with value-added forecasts, who applied to elementary school positions.

Table A10: Predicting Student Residuals by Teacher Type

	Student res	Student res
Share disadvantaged	-0.0549	-0.0409
	(0.0251)	(0.0202)
Share disadvantaged x CA in disadvantaged	0.0820	0.0697
	(0.0356)	(0.0283)
Num teachers	3214	3214
Num students	157671	157671
Mean CA	-0.00805	-0.00805
SD CA	0.0624	0.0624
Controls	No	Yes

The table assesses whether changes in the share of economically disadvantaged students predict changes in student test score residuals differently by teacher type across transfers. Teacher type is defined by comparative advantage in pre-transfer schools, with "CA in disadvantaged" an indicator for whether the teacher is above median in comparative advantage in teaching disadvantaged students. The outcome is changes in average teacher-by-school student residuals across transfers. "Share disadvantaged" is the change in the average share of economically disadvantaged students teacher j taught when moving from one school to another. Controls include a cubic in average experience in the school, an indicator for experience missingness, and transfer year indicators. Standard errors are clustered at the teacher level.

Table A11: Autocorrelations in class size

Class size, school level

Variables	Class Size t	Class Size t-1	Class Size t-2	Class Size t-3	Class Size t-4
Class Size t	1.0000				
Class Size t-1	0.7329	1.0000			
	(0.0000)				
Class Size t-2	0.6248	0.6966	1.0000		
	(0.0000)	(0.0000)			
Class Size t-3	0.4093	0.5261	0.6598	1.0000	
	(0.0000)	(0.0000)	(0.0000)		
Class Size t-4	0.3722	0.3746	0.4365	0.5796	1.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	

Nb. obs. : 247

Class size, teacher level

Variables	(Res.) Size t	(Res.) Size t-1	(Res.) Size t-2	(Res.) Size t-3	(Res.) Size t-4
(Res.) Size t	1.0000				
(Res.) Size t-1	0.3668	1.0000			
	(0.0000)				
(Res.) Size t-2	0.2688	0.3717	1.0000		
	(0.0000)	(0.0000)			
(Res.) Size t-3	0.2900	0.1272	0.2699	1.0000	
	(0.0000)	(0.0186)	(0.0000)		
(Res.) Size t-4	0.1173	0.1438	0.0698	0.3098	1.0000
	(0.0301)	(0.0077)	(0.1978)	(0.0000)	

Nb. obs. : 342

The table shows correlations (within unit) between class size in one year and class size in a prior year. In the top panel, a unit of analysis is a school and class size is the mean across all of the school's classrooms (that generate math test scores). In the bottom panel, a unit of analysis is a teacher and class size is residualized by school-year fixed effects such that residual class size compares how a teacher's class size deviates from the school-year mean.

Table A12: Autocorrelations in class composition

Class composition, school level

Variables	Frac Disadv t	Frac Disadv t-1	Frac Disadv t-2	Frac Disadv t-3	Frac Disadv t-4
Frac Disadv t	1.0000				
Frac Disadv t-1	0.9602	1.0000			
	(0.0000)				
Frac Disadv t-2	0.9430	0.9555	1.0000		
	(0.0000)	(0.0000)			
Frac Disadv t-3	0.9363	0.9370	0.9496	1.0000	
	(0.0000)	(0.0000)	(0.0000)		
Frac Disadv t-4	0.9435	0.9467	0.9554	0.9775	1.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	

Nb. obs. : 247

Class composition, teacher level

Variables	(Res.) Dis t	(Res.) Dis t-1	(Res.) Dis t-2	(Res.) Dis t-3	(Res.) Dis t-4
(Res.) Dis t	1.0000				
(Res.) Dis t-1	0.3170	1.0000			
	(0.0000)				
(Res.) Dis t-2	0.2898	0.3200	1.0000		
	(0.0000)	(0.0000)			
(Res.) Dis t-3	0.1524	0.2076	0.3723	1.0000	
	(0.0047)	(0.0001)	(0.0000)		
(Res.) Dis t-4	0.0921	0.0512	0.2203	0.3925	1.0000
	(0.0889)	(0.3450)	(0.0000)	(0.0000)	

Nb. obs. : 342

The table shows correlations (within unit) between class composition (fraction of students that are economically disadvantaged) in one year and class composition in a prior year. In the top panel, a unit of analysis is a school and class composition is the (weighted) mean across all of the school's classrooms (that generate math test scores). In the bottom panel, a unit of analysis is a teacher and class composition is residualized by school-year fixed effects such that residual class composition compares how a teacher's class composition deviates from the school-year mean.

Table A13: Teacher experience and student assignment

	(1) Outcome	(2) Outcome	(3) Outcome	(4) Outcome	(5) Outcome
Outcome: Share economically disadvantaged students assigned					
log(experience)	-0.0369	-0.0311	-0.0063	-0.0029	-0.0021
	(0.0013)	(0.0028)	(0.0005)	(0.0011)	(0.0011)
Outcome: Share Black students assigned					
log(experience)	-0.0331	-0.0195	-0.0010	-0.0008	-0.0005
	(0.0010)	(0.0023)	(0.0004)	(0.0008)	(0.0010)
Outcome: Average student lagged math score					
log(experience)	0.0887	0.0474	0.0461	0.0173	0.0115
	(0.0023)	(0.0049)	(0.0016)	(0.0033)	(0.0041)
Outcome: Share gifted status					
log(experience)	0.0231	0.0106	0.0161	0.0053	0.0074
	(0.0007)	(0.0014)	(0.0006)	(0.0012)	(0.0016)
New only		X		X	X
Year FE			X	X	
School FE			X	X	
School-year FE					X
N	1,879,666	258,723	1,879,666	258,723	258,723

Standard errors in parentheses.

The table shows separate regression results for different outcomes on the log of a teacher's prior experience. Outcomes are mean characteristics of the students in a teacher's classroom. "New only" indicates that the sample only includes teachers new to the school; thus, the regression compares outcomes across teachers new to the school depending on the teacher's experience.

Table A14: Predicting posted positions

	(1) Positions	(2) Positions
Class size	-0.0199	
	(0.0125)	
Fraction disadvantaged		1.503
		(0.544)
N	116	116

An observation is a school-year. The outcome is the number of positions posted in an application cycle and the regressors are characteristics of the school's mean class. Robust standard errors are in parentheses.

Table A15: Transferring and non-transferring teachers' value added

	(1) Did not apply			(2) Applied to transfer		
	mean	sd	count	mean	sd	count
Comparative advantage	0.0001	0.0351	528	-0.0002	0.0367	506
Absolute advantage	0.0034	0.1210	528	0.0219	0.1508	506

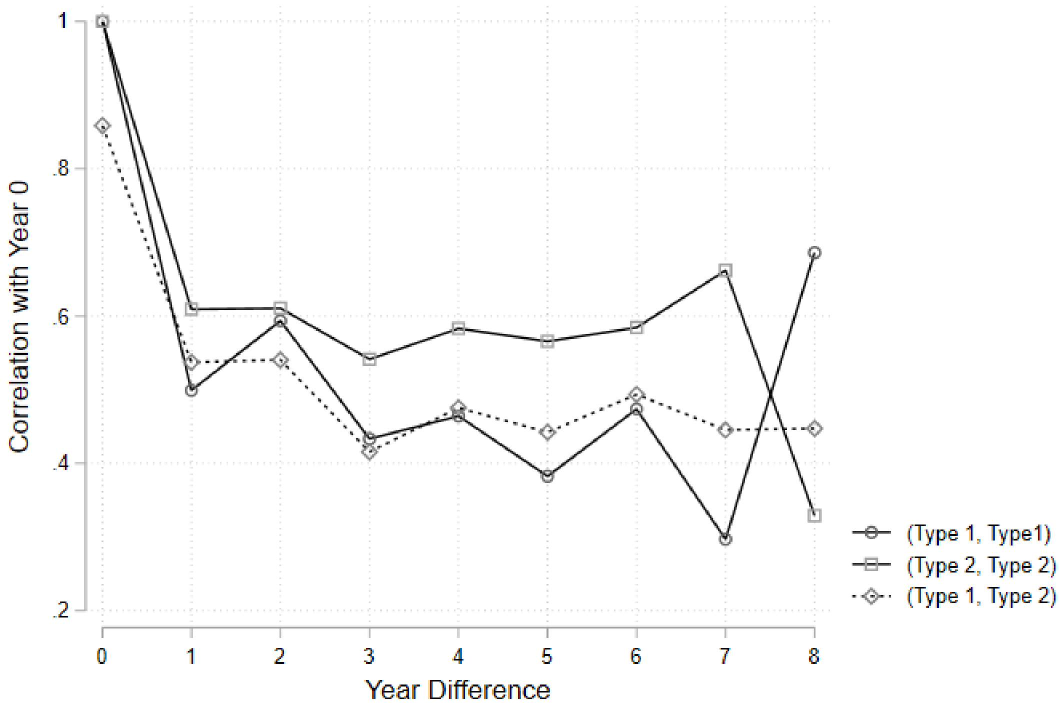
The table shows the means and standard deviations of absolute and comparative advantage for teaching economically advantaged students by whether the teacher ever submits an application to transfer. An observation is a teacher with a value-added forecast. These are pooled over years 2010 through 2018.

Table A16: Applications to Title I and non-Title schools

	Obs	Mean choice set	Median	Mean prob.	25th	50th	75th	Std. dev.	Overall mean prob.
Title I	14,747	85.3	68	0.176	0.010	0.056	0.264	0.237	0.137
non-Title I	14,747	74.0	66	0.176	0.013	0.084	0.270	0.217	0.155
Gap	14,747			-0.001	-0.049	0.003	0.041	0.134	-0.018

The table shows application statistics to positions at Title I and non-Title I schools. Columns (2) and (3) show the mean and median choice set sizes for an applicant. “Gap” shows the difference in statistics across the two school types.

Figure A1: Value-Added Drift Parameters



The figure shows the estimated correlations between teacher value-added in different years. The x-axis captures the year difference between the teacher's value-added measures. The three lines reflect correlations in teacher value-added within student type (1 for non-disadvantaged students, 2 for disadvantaged students) or across student type.

Figure A2: Math Comparative Advantage Forecast Unbiasedness

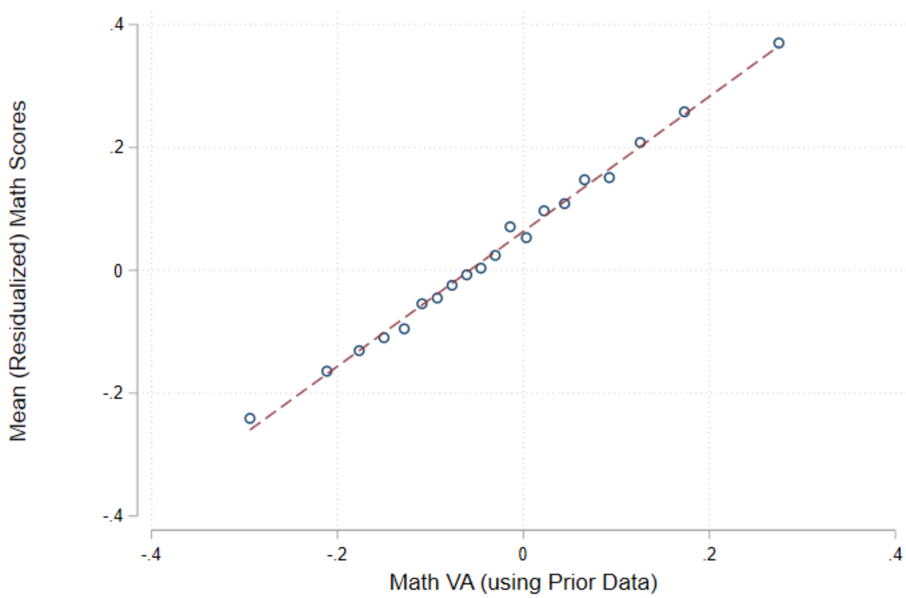
The figure is a binscatter, where an observation is a teacher-year and “Difference in VA” is the difference in a teacher’s math value-added between economically disadvantaged and advantaged students. Value-added estimates are predictions using data from prior years. Units are student standard deviations. The y-axis is the difference in mean student math test score, residualized by student demographics including lagged scores, school fixed effects, and teacher experience measures. The mean is taken over all students (of a given type) for a given teacher-year and the difference is between a teacher’s economically disadvantaged and advantaged students.

Figure A3: Transfer event study

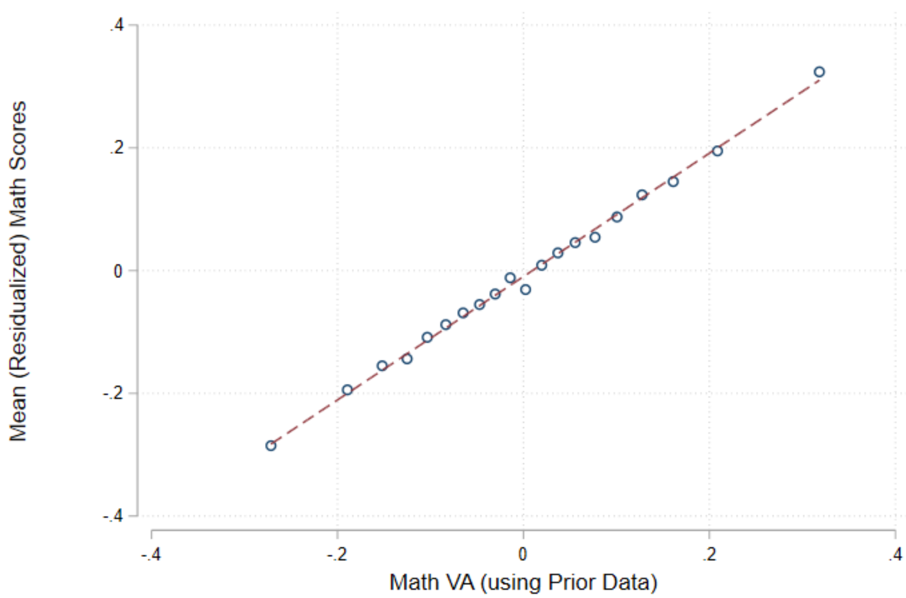
The figure shows event study coefficient estimates and 95% confidence intervals. The outcome is residualized math test score (residualized by student observables including lagged scores, school fixed effects, and an experience function), in student standard deviation units. The event is the teacher's first transfer from one school to another school in the state, where non-transfers do not have an event. We include teacher and year fixed effects and follow Sun and Abraham (2021) in constructing the estimates.

Figure A4: Forecast Unbiasedness for Large Changes in Class Size

(a) Large decreases in class size

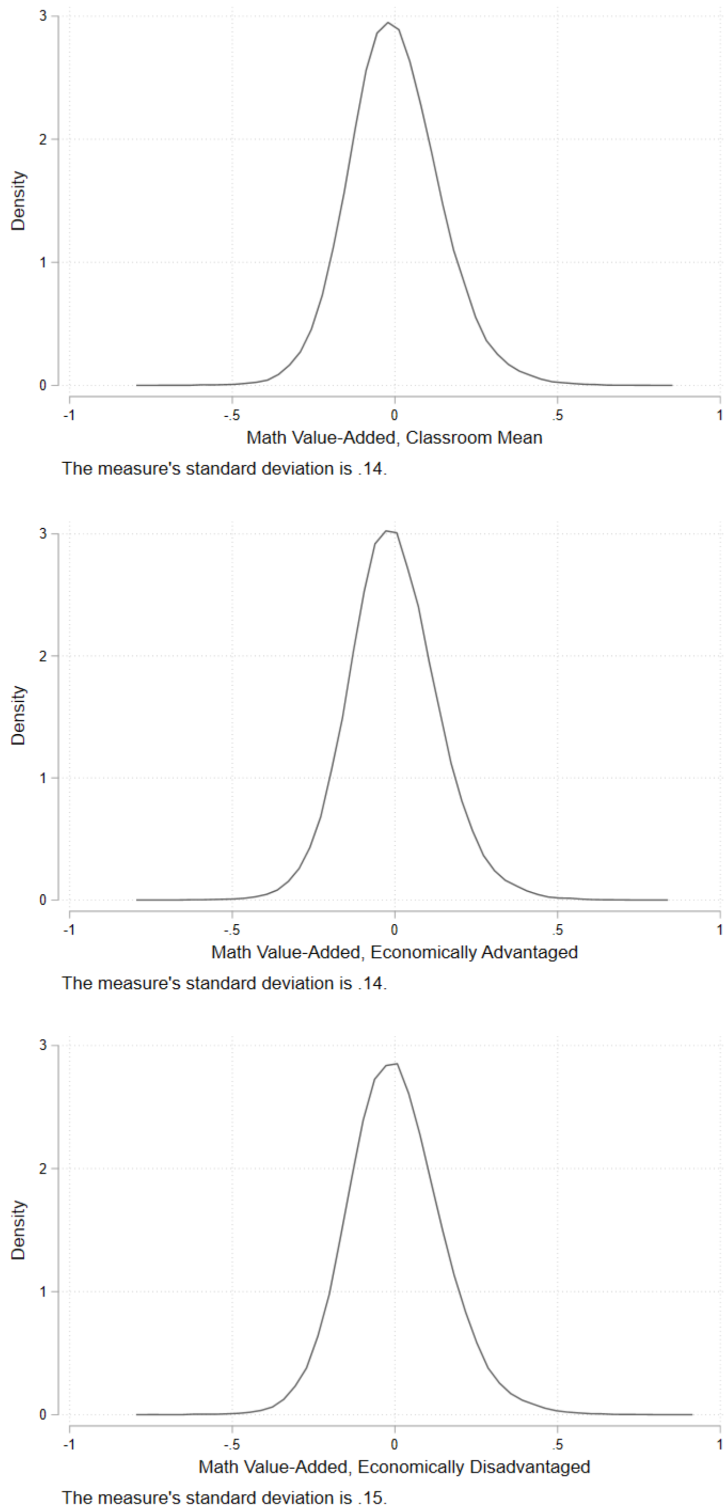


(b) Large increases in class size



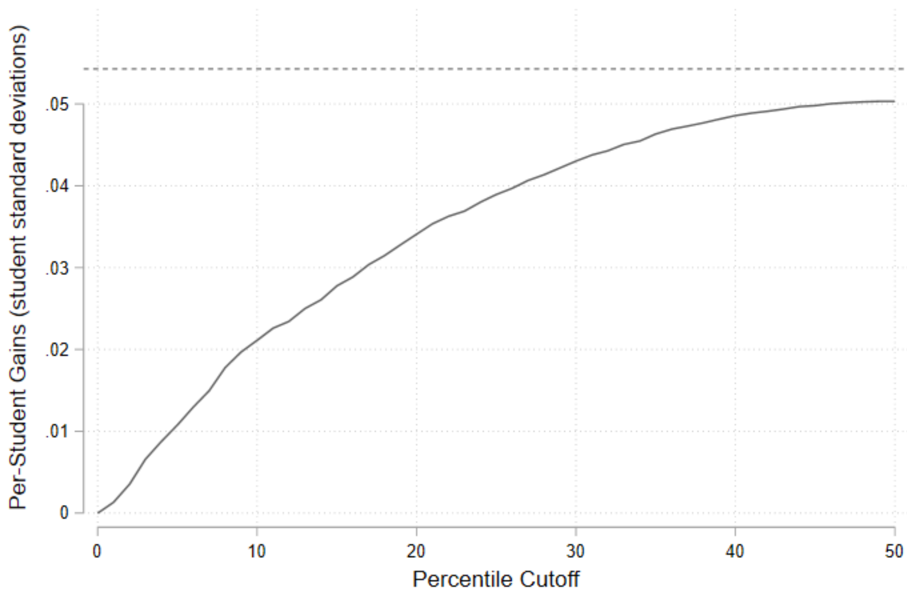
The figure shows a binscatter of student residual test scores by value-added prediction where an observation is a teacher-year. For decreases, the sample consists of all teachers where the class size used for prediction exceeds the class size in the target by more than 10 students. For increases, the sample consists of all teachers where the class size used for prediction is less than the class size in the target by more than 10 students.

Figure A5: Value-Added Distribution

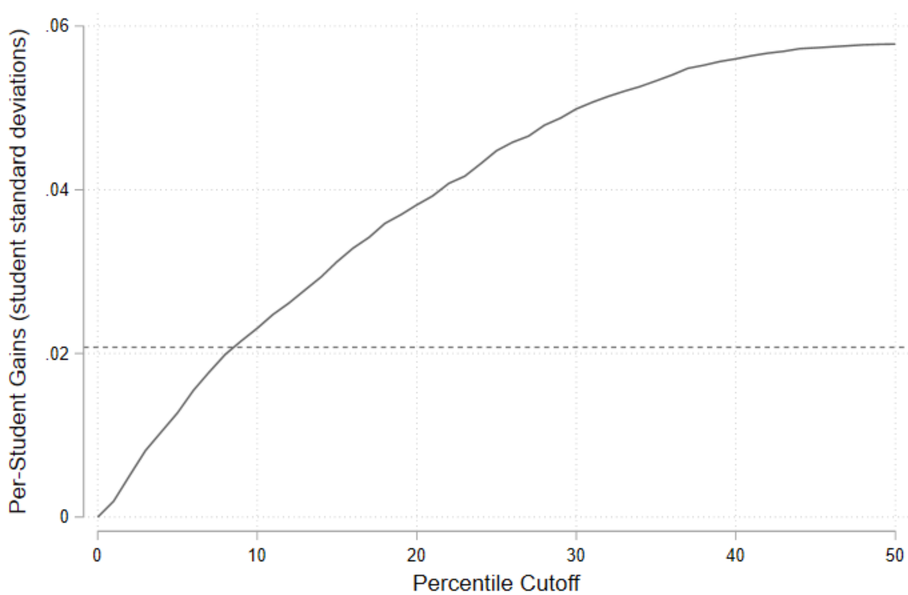


The figures show kernel density plots of our forecast of a teacher's value-added in a given year at the school they actually teach at (panel A), for economically advantaged students (panel B), and for economically disadvantaged students (panel C). The forecast uses only data from prior years. The units are student standard deviations.

Figure A6: Gains from teacher replacement



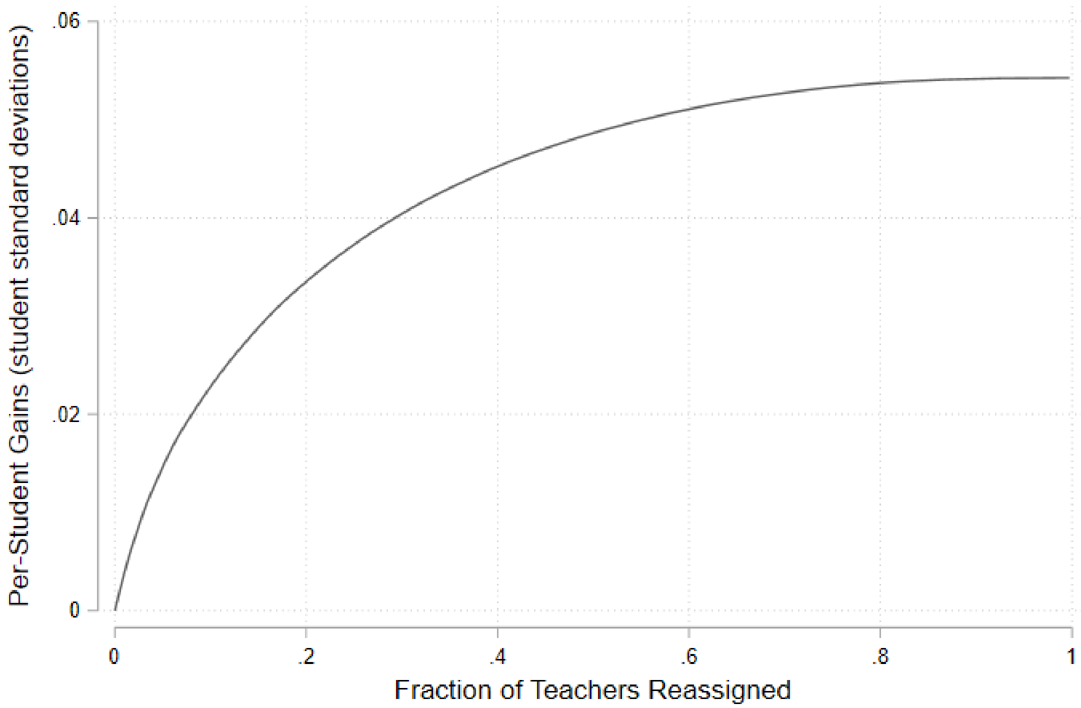
With class size variation



Constant class size

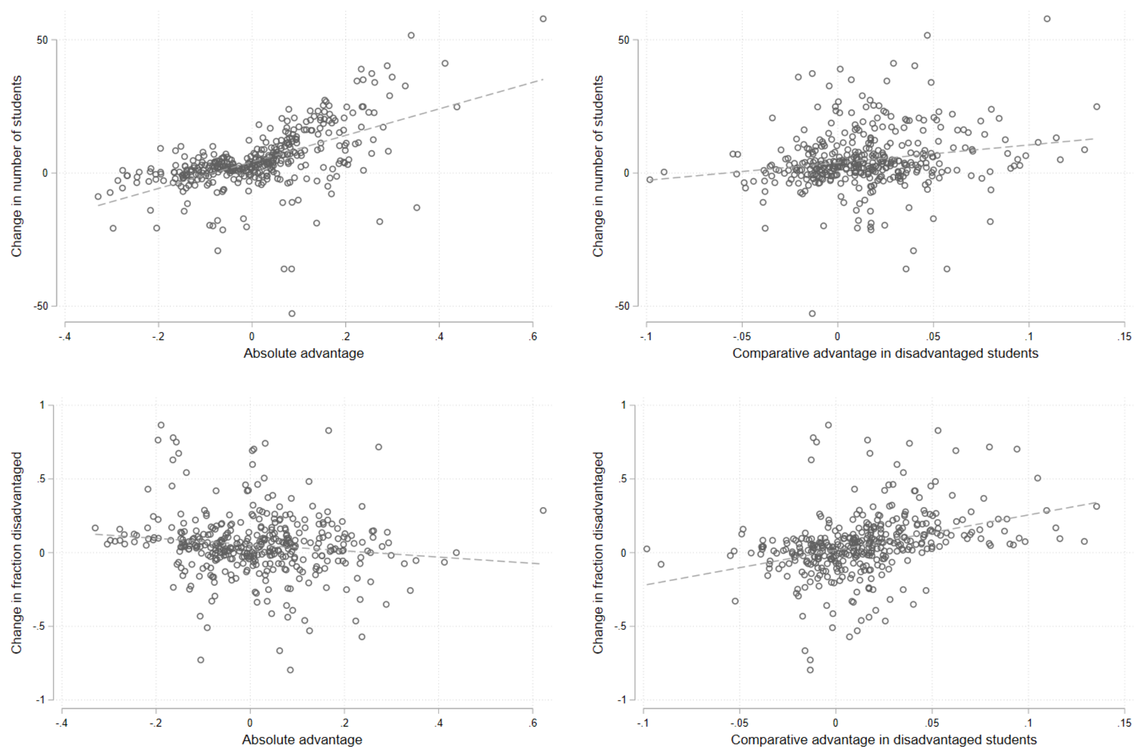
This Figure shows the results from policies that replace the X% of low-performing teachers with median value-added teachers, where the x-axis shows different values of X. The sample is the 2016 teachers with value-added forecasts. We assess performance based on realized value-added in the data (i.e., at the schools and classrooms a teacher is actually at in the data), and the median value-added teacher has median values for both dimensions of value-added. The y-axis is per-student gains in achievement. The top panel uses class size variation while the bottom panel imposes constant class sizes (at the district mean). The horizontal dashed lines are the gains from the output-maximizing allocation of existing teachers across schools in the district.

Figure A7: Student Gains by Fraction of Teachers Reassigned



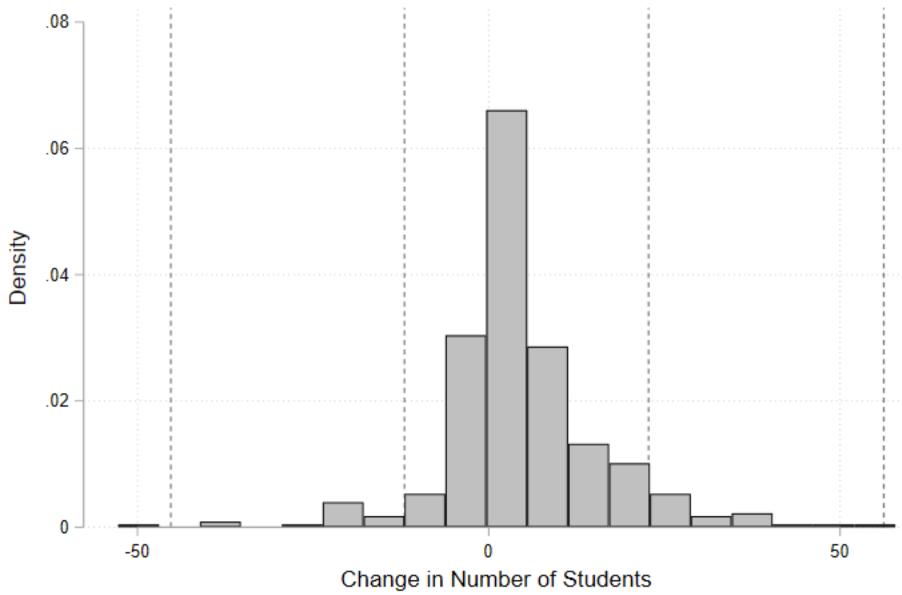
The figure plots the potential per-student math test score gains (in student standard deviation units) as a function of the fraction of teachers that are assigned to a school different than their actual school. The sample consists of the 2016 teachers with math value-added scores.

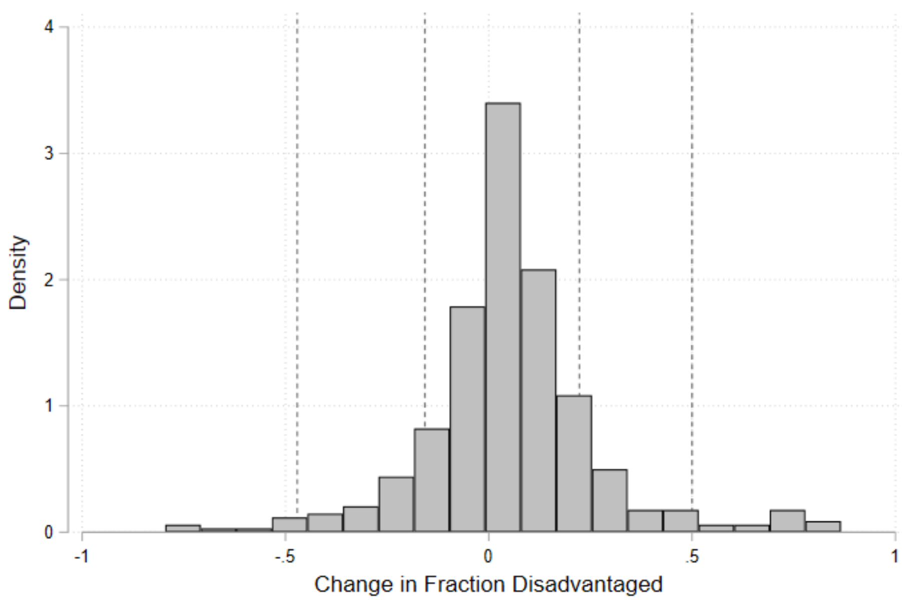
Figure A8: Changes in a teacher's classroom composition and size between the output-maximizing and actual allocations



The figures show scatterplots and lines of best fit for the 2016 sample of teachers with value-added scores. In the top row, the variable of interest is the difference in the number of students a teacher teaches between the output-maximizing and actual allocations. Positive numbers are teachers who have more students in the output-maximizing allocation than in the actual. In the bottom row, the variable of interest is the difference in the fraction of disadvantaged students a teacher teaches between the output-maximizing and actual allocations. In the left column, teachers are ordered on the x-axis by absolute advantage (value-added at a representative school). In the right column, the teachers are sorted by comparative advantage in teaching economically disadvantaged students.

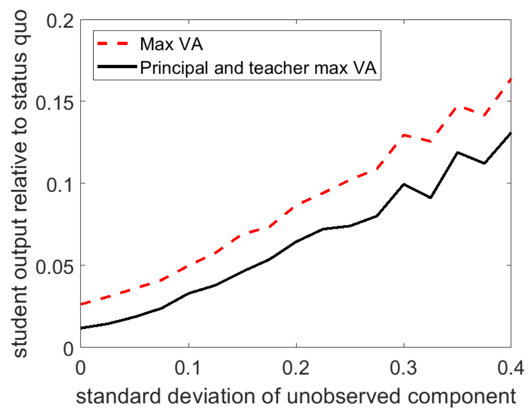
Figure A9: Optimal teacher placement relative to placement that generated value-added



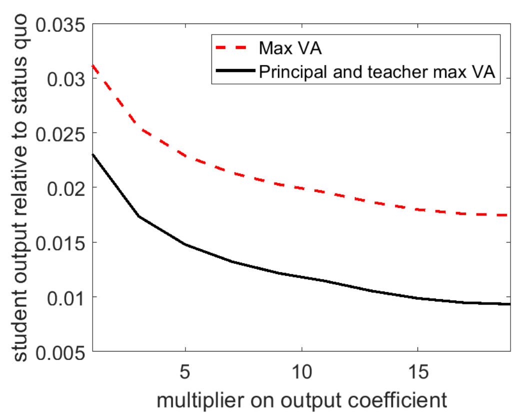


The figures show histograms for the 2016 sample of teachers with value-added scores. In the top panel, the variable of interest is the difference in the number of students a teacher teaches between the output-maximizing allocation and the classrooms that generated the teacher's value-added forecast. Positive numbers are teachers who have more students in the output-maximizing allocation than in the estimation data. In the bottom row, the variable of interest is the difference in the fraction of disadvantaged students a teacher teaches between the output-maximizing allocation and the classrooms that generated the teacher's value-added forecast. The vertical dashed lines represent the 1st, 10th, 90th, and 99th percentiles of the distribution we use for validation of our value-added measures in Table 1.

Figure A10: Simulations of two forms of misspecification



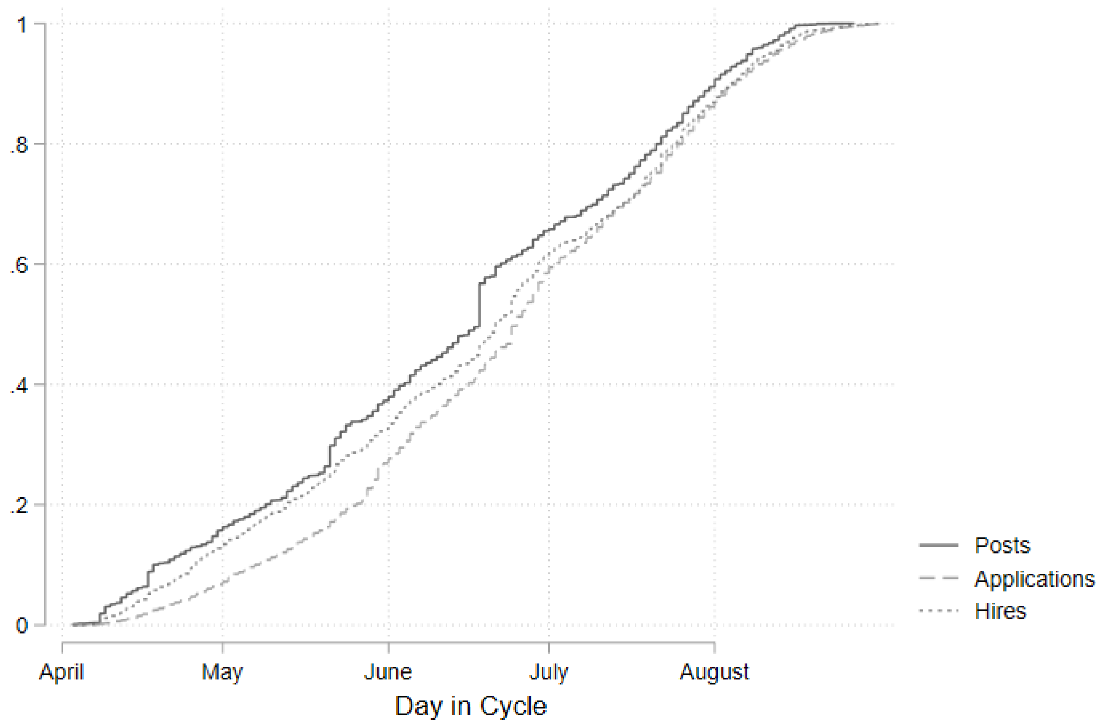
Unmodelled match effects



Attenuation in coefficient on output in teacher preferences

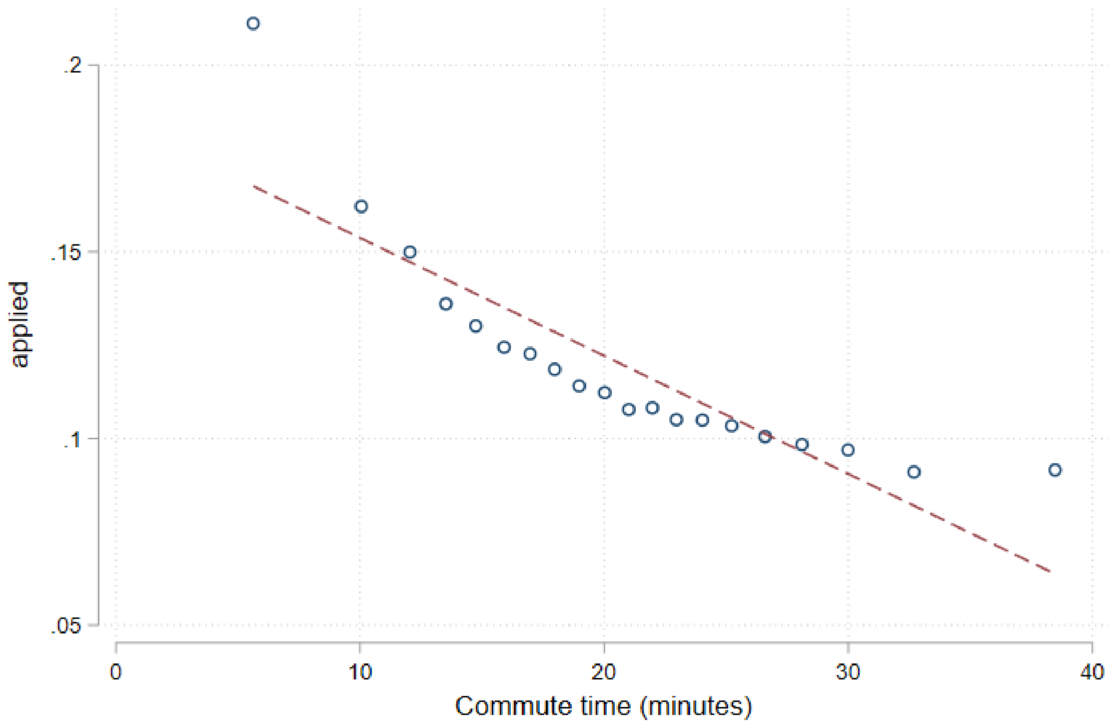
The figures show results from simulation exercises where we vary parameters related to match effects. In each figure the y-axis is the mean student achievement relative to the status quo. The dashed red line is the achievement in the output-maximizing allocation while the solid black line is the achievement in the equilibrium where principals and teachers each have preferences in order of value-added produced. The top panel adds an iid unobserved component to match effects, where the x-axis is the standard deviation of this component. The bottom panel varies the coefficient in teacher preferences on value-added. If our model misses match effects that teachers are aware of, then the preference coefficient might be attenuated. The x-axis in the bottom panel shows by how much we multiply our estimated coefficient on value-added.

Figure A11: Market timing



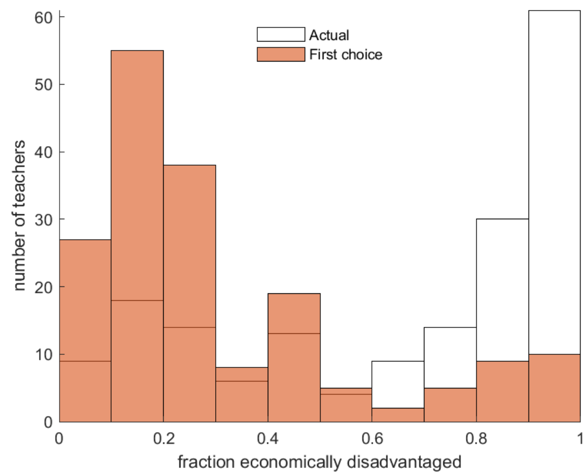
The figure shows CDFs for postings, applications, and hires (the application date of the application that led to a hire).

Figure A12: Application probabilities against commute time



This Figure plots the probability of applying against commute time (measured in one-way minutes). The Figure residualizes for applicant fixed effects.

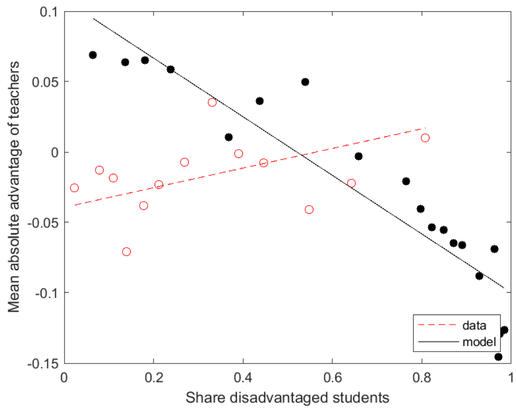
Figure A13: Number of teachers by fraction economically disadvantaged



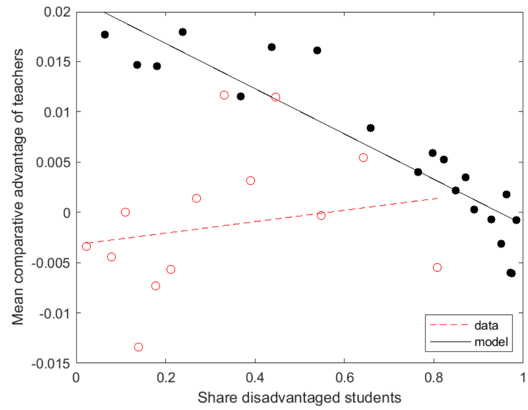
This Figure plots histograms of the number of teachers, by the fraction of students who are economically disadvantaged. The histograms are for the actual positions in the data (in white) and the positions teachers would have if they could all have their top choice (in red).

Figure A14: Model fit: teacher serial dictatorship based on absolute advantage (descending)

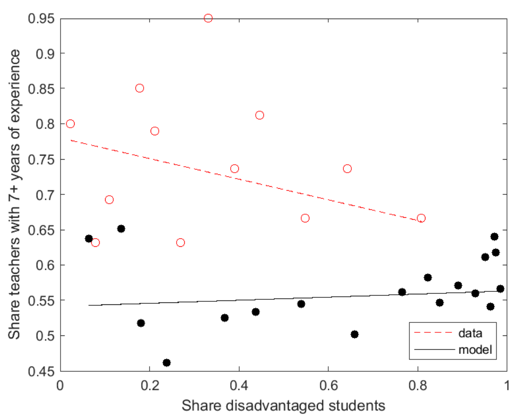
(a) Teacher absolute advantage



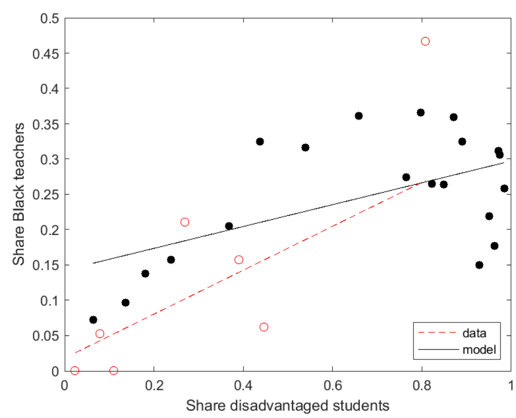
(b) Teacher comparative advantage



(c) Teachers with 7+ years of experience



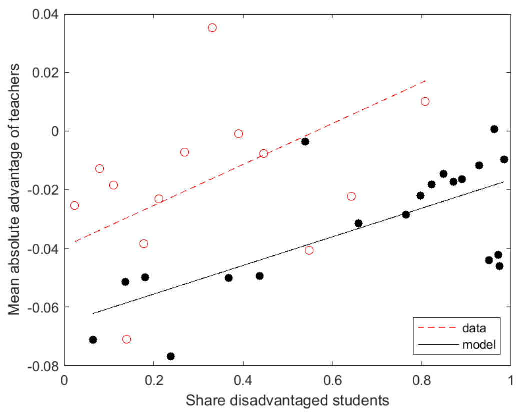
(d) Teachers that are Black



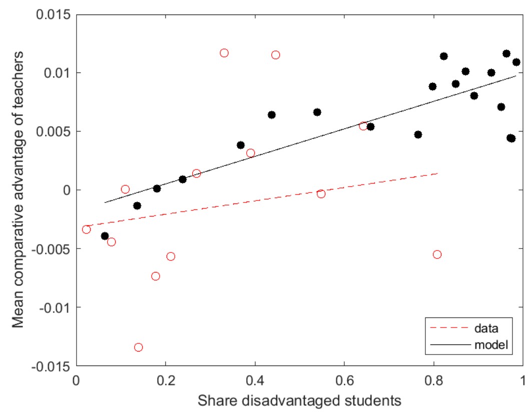
This Figure compares the allocations implied by a model in which the allocation is determined by a serial dictatorship where teachers go in descending order of their absolute advantage to the allocations we observe in the data. The data refers to all teachers in the district. The model refers to the teachers who apply in the transfer system for whom we have value-added scores.

Figure A15: Model fit: teacher serial dictatorship based on experience (descending)

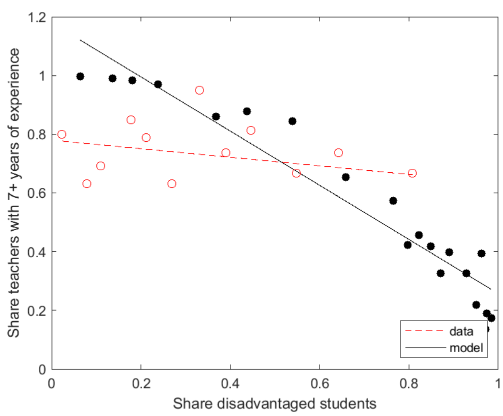
(a) Teacher absolute advantage



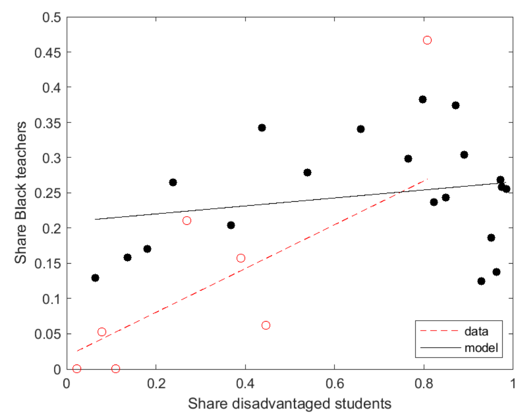
(b) Teacher comparative advantage



(c) Teachers with 7+ years of experience



(d) Teachers that are Black



This Figure compares the allocations implied by a model in which the allocation is determined by a serial dictatorship where teachers go in descending order of their experience to the allocations we observe in the data. The data refers to all teachers in the district. The model refers to the teachers who apply in the transfer system for whom we have value-added scores.

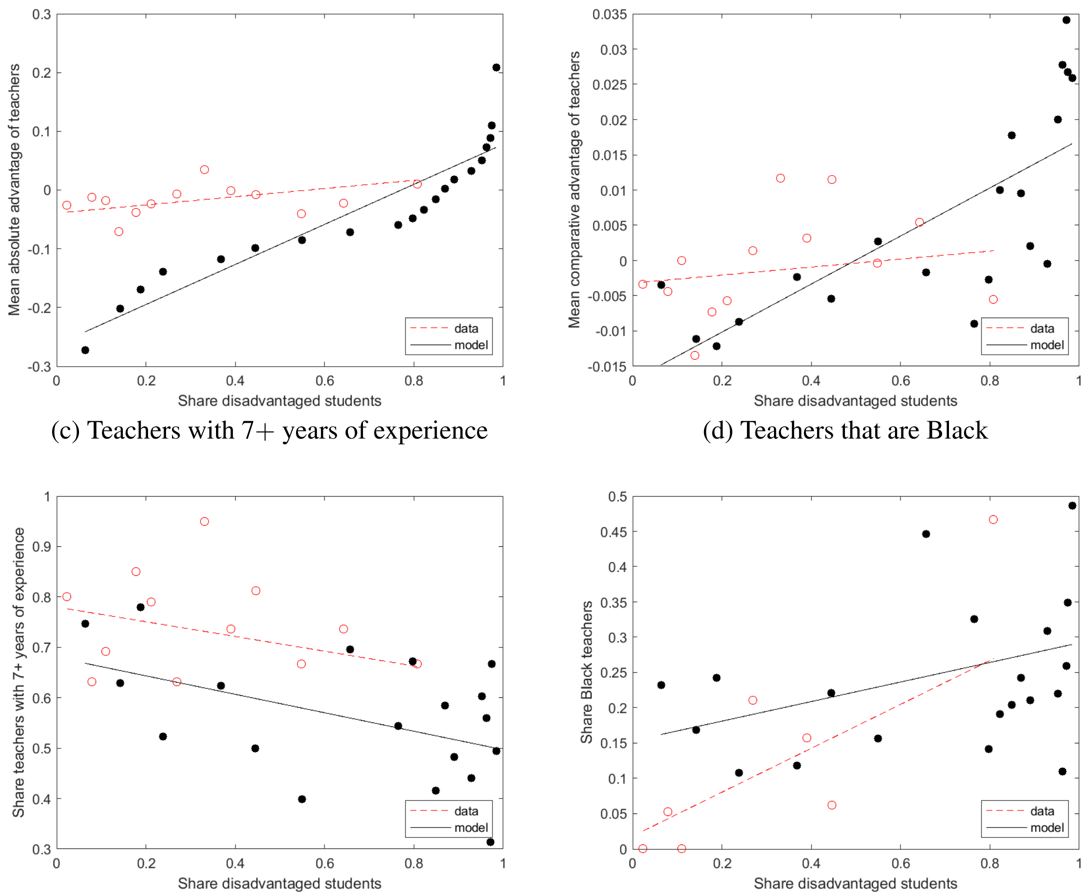
Figure A16: Model fit: school serial dictatorship based on fraction disadvantaged (descending)

(a) Teacher absolute advantage

(b) Teacher comparative advantage

(c) Teachers with 7+ years of experience

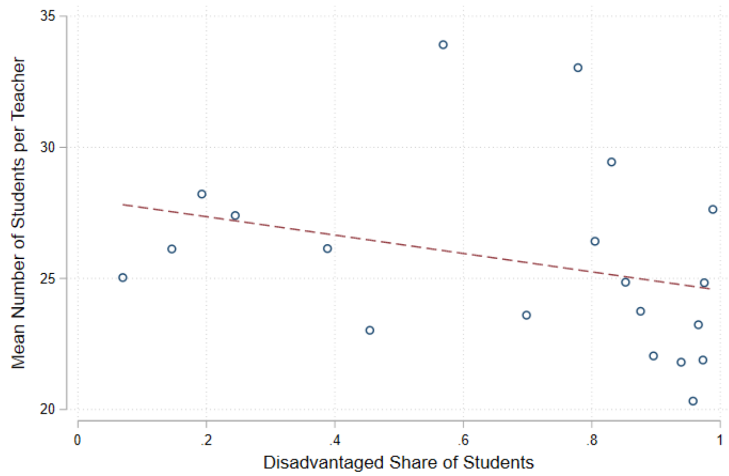
(d) Teachers that are Black



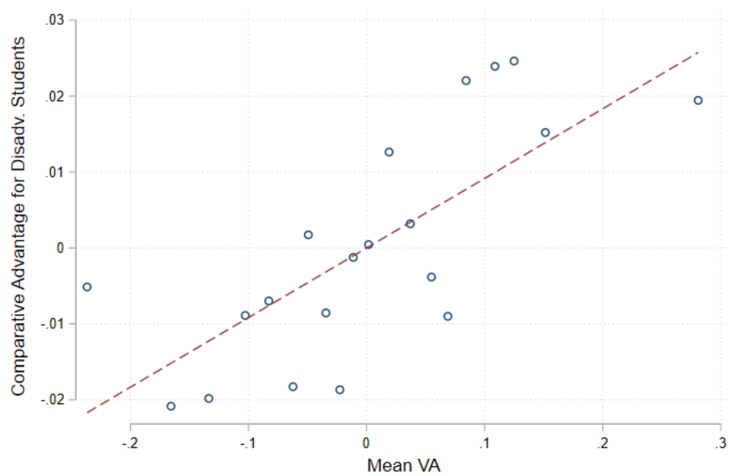
This Figure compares the allocations implied by a model in which the allocation is determined by a serial dictatorship where schools go in descending order of their fraction of disadvantaged students to the allocations we observe in the data. The data refers to all teachers in the district. The model refers to the teachers who apply in the transfer system for whom we have value-added scores.

Figure A17: Features of classes and teachers – transfer sample

(a) Class size and fraction disadvantaged

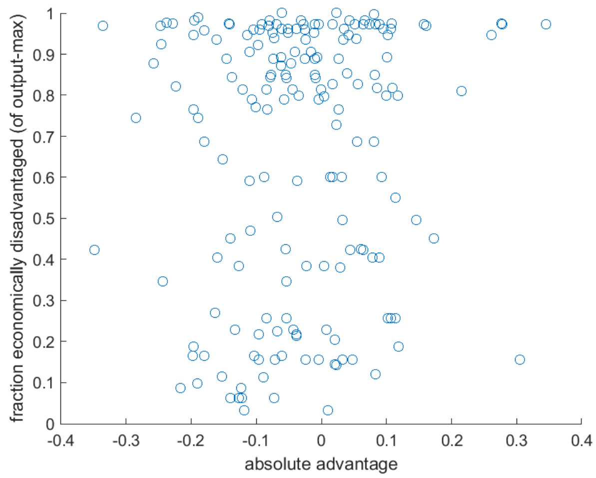


(b) Comparative advantage for disadvantaged students, and absolute advantage



The figures show binscatters related to classroom characteristics and teacher characteristics in the transfer sample used for counterfactual analysis. The top panel shows the relationship between a school's (mean) disadvantaged share of students and a school's (mean) number of students per teacher. The bottom panel shows the relationship between a teacher's absolute advantage (x-axis) and comparative advantage in teaching economically disadvantaged students (y-axis). For this figure, absolute advantage is the average value-added across students types (rather than the value-added at a representative school) to avoid mechanical correlations between absolute and comparative advantage.

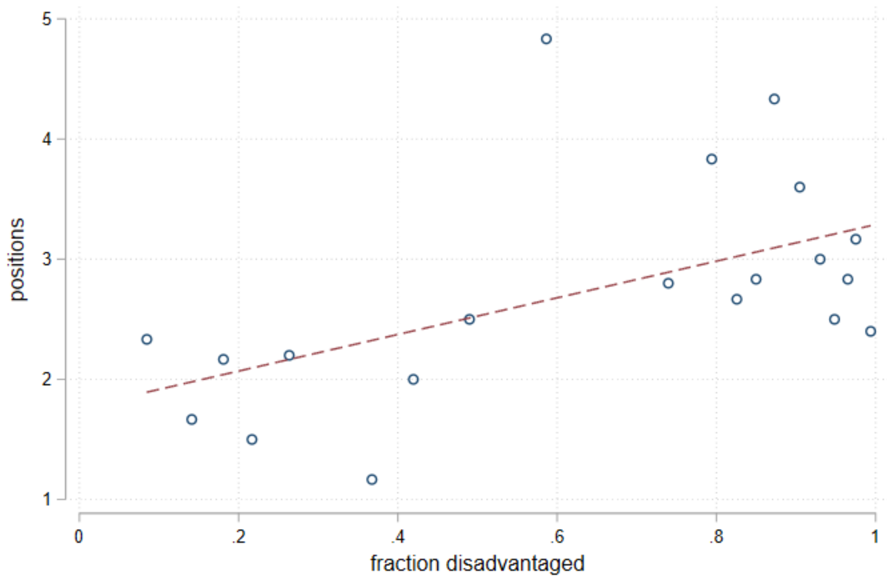
Figure A18: First-best allocation's placement of teachers, by absolute advantage



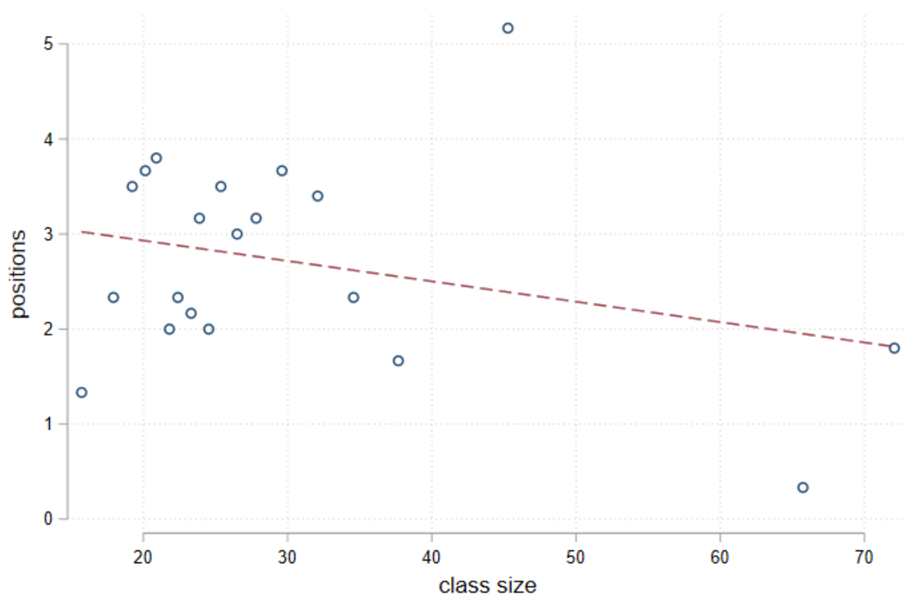
This Figure plots the first-best allocation in our transfer sample, where we divide teachers by absolute advantage and positions by fraction of students that are economically advantaged. Each point is an assignment of a teacher to a position.

Figure A19: Postings selection in the transfer market

(a) Positions and fraction of disadvantaged students

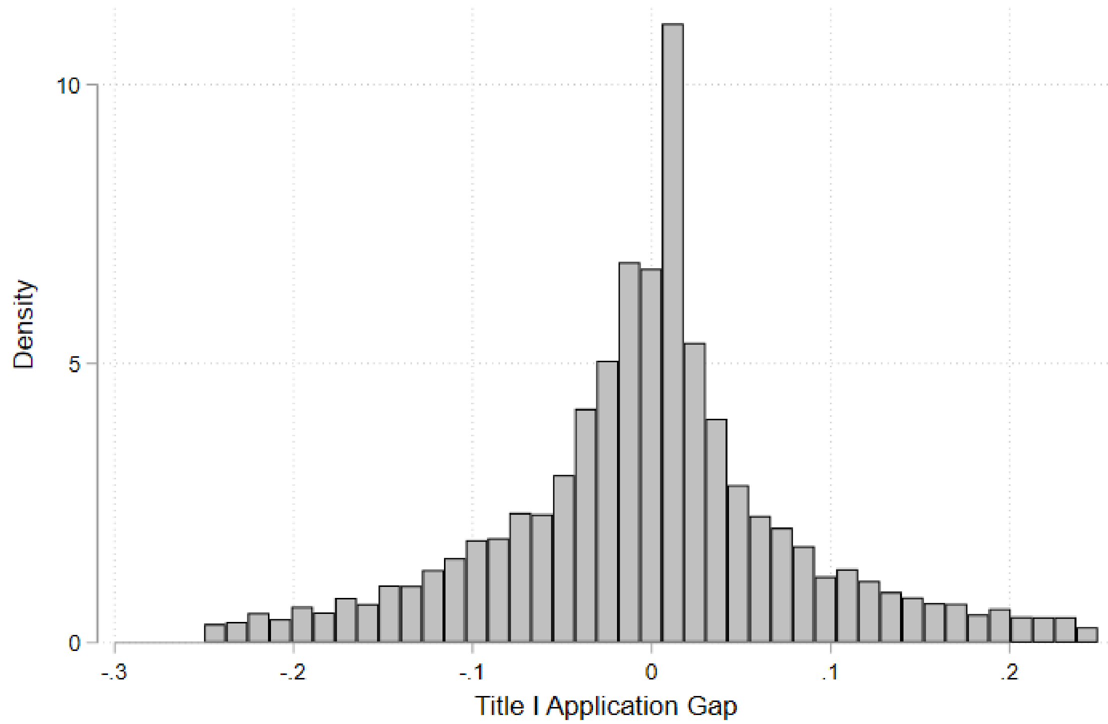


(b) Positions and class size



This figure shows the relationship between number of positions posted and (a) a school's fraction of students that are economically disadvantaged and (b) a school's class size. An observation is a school.

Figure A20: Title I Application Gap



This Figure plots the distribution of the individual-level Title I application rate minus the individual-level non-Title application rate. Thus, the positive entries indicate that a teacher applies to a greater share of the Title I schools in their choice set than to the non-Title I schools.