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Preferences, Inequities, and Incentives in the Substitute Teacher Labor Market

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Abstract

We examine the labor supply decisions of substitute teachers – a large, on-demand market with broad shortages and inequitable supply. In 2018, Chicago Public Schools implemented a targeted bonus program designed to reduce unfilled teacher absences in largely segregated Black schools with historically low substitute coverage rates. Using a regression discontinuity design, we find that incentive pay substantially improved coverage equity and raised student achievement. Changes in labor supply were concentrated among Black and Hispanic substitutes from nearby neighborhoods with experience in incentive schools. Wage elasticity estimates suggest incentives would need to be 50% of daily wages to close fill-rate gaps.

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The choices educators make about where to supply their labor have important consequences for educational equity. Educators' preferences for schools based on non-pecuniary aspects of a job such as a school's location and working conditions can create inequitable access to learning opportunties for students.[1] One possible policy lever to address these market inequities is to move away from uniform salary schedules to ones that offer a wage premium for working in hard-to-staff schools. Several studies evaluating targetted pay programs have found that teachers' labor supply decisions are responsive to wage incentives and that these choices affect students' academic outcomes.[2] We examine the nature of labor supply among an important but far less studied group of employees in public education, substitute teachers.

Substitute teaching is among the largest on-demand labor sectors in the U.S. with almost 600,000 substitutes covering over 30 million teacher absences in K-12 schools each year.[3] Demand for substitute teachers has traditionally exceeded labor supply at prevailing wages. Prior to the COVID-19 pandemic, one of every five substitute requests in the U.S. went unfilled (Frontline Education 2019).[4] The health and economic impacts of the pandemic have further exacerbated longstanding substitute shortages, with 77% of school districts reporting acute challenges in staffing substitute positions (Schwartz and Diliberti 2022). Some districts have even been forced to take emergency measures such as waiving college experience requirements, calling on the National Guard, and temporarily closing schools (Blad 2022; Heyward 2021; Hughes 2021).

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[1] See for example Boyd et al. (2005); Boyd et al. (2011); Boyd et al. (2013); Clotfelter, Ladd, and Vigdor (2011); Feng and Sass (2018); Hanushek, Kain, and Rivkin (2004); James, Kraft, and Papay (2022).
[2] See for example Bobba et al., (2021); Cabrera and Webbink (2020); Clotfelter et al. (2008); Feng and Sass (2018); Glazerman et al. (2013); Kho et al. (2019); Pugatch & Schroeder (2018); Steele, Murnane, and Willett (2010).
[3] We estimate total number of teacher absences based on an average of 11 absences per teacher (Frontline Education 2019; Joseph, Waymack, and Zielaski 2014) and 3.6 million K-12 teachers.
[4] Similar shortages exist in other high-income countries such as France (Benhenda 2022).

Although substitute shortages are a widespread challenge, the nature of the substitute labor market distributes these shortages among schools in inequitable ways. In the private sector, compensating wage differentials typically serve to "level the playing field" across jobs with different working conditions (Rosen 1986; Smith 1979). In the public education sector, fixed salary schedules within districts substantially constrain variation in wages across jobs, placing some schools at a substantial disadvantage (Feng 2020). Unlike private firms, traditional public schools cannot control many aspects of their working conditions such as their location or what challenges their students may face outside of school. These unequal characteristics have particularly stark consequences for the distribution of substitute labor because of the limited labor supply and the one-sided nature of the market. When schools post requests for substitutes, they have little choice over who fills the requests. While schools primarily compete over the qualifications and effectiveness of the teachers they hire, competition for substitutes is often on the margin of whether schools can successfully secure any substitute at all to cover a teacher's absence.

In this paper, we study the labor supply decisions of substitute teachers in the Chicago Public Schools (CPS) and examine the potential of a differentiated wage structure to reduce inequities in substitute fill rates. CPS schools in the bottom quintile of fill rates have, on average, 50% of substitute requests filled, compared to an average fill rate of over 95% in the top quintile. This inequitable distribution of substitute coverage matters because unfilled teacher absences can have far-reaching consequences for students and schools. Substitutes play an important role in ensuring the organizational stability of schools and minimizing the negative effects of teacher absences on student achievement (Benhenda 2022; Clotfelter, Ladd, and Vigdor 2009; Herrmann and Rockoff 2012; Miller, Murnane, and Willett 2008). Teacher absences that go uncovered force schools to make difficult choices between redistributing students across other classes, pulling other school personnel away from their duties to cover the class, or placing students in the gym, cafeteria, or library with minimal supervision. Each of these responses creates negative externalities that spill over beyond an absent teacher's classroom.

Understanding the labor supply decisions of substitute teachers is also critical for advancing educational equity because differences in substitute coverage often fall sharply along racial and socioeconomic lines (Gershenson 2012; 2013; Liu, Loeb, and Shi 2022). In cities like Chicago, substitute labor market dynamics are deeply affected by the racial and socioeconomic segregation of neighborhoods and schools (Ewing 2018), with students of color and low-income students disproportionately bearing the burden of substitute shortages.[5] In 2017-18, Black students in CPS experienced uncovered teacher absences at three times the rate of their white peers (33% vs. 10%) and students from low-income backgrounds faced uncovered absences twice as often as their more affluent peers (26% vs. 12%). Substitute coverage in CPS and other large urban districts is a civil rights issue.

Aiming to address this inequity, CPS collaborated with our research team to design a targeted bonus-pay program for substitute requests in the 75 schools with the lowest historical fill rates in the district. We apply a sharp regression discontinuity (RD) design to evaluate the causal effects of the program on substitute coverage and student outcomes. In the following year, the district expanded the program from an initial 75 to 125 schools, allowing us to reexamine its efficacy after a year of implementation and evaluate a different local margin of treatment.

We find that substitute labor supply was substantially affected by the targeted incentives, with a 23 percentage-point increase in the share of substitute requests filled – an almost 50% increase in

[5] Similar patterns of inequitable substitute coverage are evident in the Columbus City Schools (Curriculum Management Solutions, Inc. 2020, 207).

treated schools' fill rates. As a result, substitutes covered an additional 114 teacher absences, on average, in each incentive school that otherwise would have been unfilled – equivalent to over 13,000 total student-hours of classroom coverage per school.[6] We conduct a range of robustness tests for potential negative spillover effects of the targeted incentives on non-incentive schools both broadly and among those schools concentrated just below the treatment cutoff. These analyses suggest that the discontinuities we estimate at the treatment cutoff are driven almost entirely by increases in fill rates among treated schools rather than substitute coverage declines among non-incentive schools caused by negative spillover effects.

Increased substitute labor supply on the extensive margin (from the incentive schools' perspective) appears to be the primary driver of increased coverage. We find the increase in fill rates is mostly explained by more substitutes working in schools with historically low fill rates, rather than an increase in the average number of days substitutes work in these schools. The increased number of unique substitutes working in incentive schools were almost entirely Black and Hispanic substitute teachers who lived within a convenient, but not immediate, commute to an incentive school. This reflects the highly segregated nature of the incentive schools' neighborhoods. Disaggregating results by substitutes' prior work histories, we also find that effects were concentrated among substitutes who had previously demonstrated a willingness to work in incentive schools but were not working in them exclusively. Thus, the incentives appear to have shaped the labor supply decisions of only a subset of the CPS substitute teacher workforce. Estimates of substitutes' wage elasticity of daily labor supply at incentive schools

[6] We estimate this as follows: 23% of the total number of substitute requests by treated schools in 2018-19 was 8,550. Dividing this number by the 75 treated schools produces 114 additional filled absences per school. We then multiple 114 by the average number of students teachers teach in incentive schools (20) and the number of instructional hours in a school day (6 hours in CPS elementary schools).

suggest that the targeted bonus pay would need to be roughly doubled to $80, or 50% of daily substitute pay, in order to entirely close the coverage gap faced by incentive schools.

We further test for effects on other school outcomes such as teacher retention and student achievement. We find no effect on teacher turnover, but a small positive effect of 0.05 standard deviations on achievement in English Language Arts. Results for math are of similar magnitude but are imprecisely estimated. This is equivalent to moving a typical incentive school from the 21st to the 27th percentile in the district-wide distribution of average achievement.

Estimates for the second year of the expanded incentive program reveal similar results. We find the program increased fill rates among incentive schools by 21 percentage points with a pattern of substitute labor supply responses consistent with the year prior. These results enhance the overall generalizability of our findings as we estimate the second-year effects of the program at a different local margin of treatment. Unlike the first year, we find evidence that the second-year program successfully attracted substitutes who did not work the prior year to return to work at schools with incentive pay. Finally, we find that the second-year program increased teacher absences in incentive schools by 3.5 days, driven almost entirely by absences for professional development rather than sick or personal days.

This research contributes to several related literatures. Our paper is most directly related to the labor and personnel economics literatures examining educators' labor supply preferences and the effect of compensating wage differentials in the public education sector (Boyd et al. 2005; 2011; 2013; Bueno and Sass 2018; Cabrera and Webbink 2020; Clotfelter et al. 2008; Feng and Sass 2018; Glazerman et al. 2013; Hanushek, Kain, and Rivkin 2004; Kho et al. 2019; Pugatch and Schroeder 2018; Steele, Murnane, and Willett 2010). We provide the first direct estimates of substitute teachers' labor supply response to differentiated wages, a policy that has been proposed in prior research (Gershenson 2012) and is currently used in some school districts (Liu, Loeb, and Shi 2022) but has not yet been evaluated in the field.

We also leverage our setting to estimate the wage elasticity of daily labor supply for substitutes. Prior studies have shown that teachers' annual labor supply is wage-elastic on the extensive margin of whether to join/remain at a school, suggesting that compensating differentials may decrease disparities in substitutes' labor supply across schools (Falch 2010; 2011; Ransom and Sims 2010). Our findings are strikingly similar to prior work examining the wage elasticity of teachers' annual labor supply decisions despite the different margins these wage elasticities represent. Our work also complements recent model-based analyses exploring the efficiency and equity consequences of different pay regimes and assignment mechanism in the teacher labor market (Bates et al. 2022; Biasi, Fu, and Stromme 2021; Bobba et al. 2021; Graham et al. 2022; Tincani 2021).

Prior research on the daily labor supply decisions of on-demand workers has primarily focused on taxi and ride-hailing drivers (Camerer et al. 1997; Chen et al. 2020; Chen and Sheldon 2015; Farber 2005; 2008; 2015; Thakral and Tô 2021), which make up approximately 370,000 jobs in the U.S.[7] We extend this body of work by examining substitute teachers, a larger and far less studied occupation in the on-demand labor market. Our study provides further evidence of an income effect in the context of on-demand workers' decisions about where to provide their labor (Allon, Cohen, and Sinchaisri 2018; Camerer et al. 1997; M. K. Chen and Sheldon 2015; Fehr and Goette 2007). Finally, these findings are relevant for larger efforts to address inequity in the U.S. public education system and illuminate the consequences of residential and school segregation on school quality and educator labor supply.

[7] Employed population estimates are from the Bureau of Labor Statistics' 2018 employment data (2018a; 2018b).

1. Hypothesized Effects of Uncovered Teacher Absences

Uncovered teacher absences affect students, teachers, and administrators throughout a school via a variety of direct and indirect channels. When schools fail to secure a substitute teacher, students likely lose instructional time. Although securing a substitute does not guarantee that students will have a productive learning environment, it at least ensures supervision and possible support for students to complete independent work. Sending students to be supervised by non-instructional staff in school gyms, auditoriums, and cafeterias – a sometimes necessary solution – limits the potential use of instructional time and can convey to students an implicit disregard for their learning. Teachers often must spend additional instructional time upon their return addressing students' uneven progress and any disciplinary issues that arose when substitutes can't be found.

Unfilled absences also have the potential to spill over beyond a single classroom by having a domino-like effect throughout a school (Vialet and von Moos 2020). The impact of re-allocating students and staff during an uncovered absence affects the administrative functioning and overall orderliness of the school environment. Administrators must abandon their planned work schedule to prioritize finding classroom coverage. Disruptions from unsupervised students, frequent student additions from uncovered classes, and repeated requests to fill in for absent colleagues might engender resentment among teachers and cause them to feel like their work is undervalued. This could, in turn, lead to animus towards school leadership, a decreased commitment to the school, higher rates of teacher absences, and lower rates of teacher retention.

Students in other classes can be affected when their class receives students whose teacher is absent or when their teacher has to cover for the absence of a colleague. Redistributing students across other classes increases class sizes, which can create distractions and make classroom management more challenging for teachers. Teachers often cover absences during their prep-period, losing time they would have dedicated to planning lessons, communicating with families, and collaborating with other grade- and subject-level teams. Schools frequently have special-educators, paraprofessionals, or enrichment teachers (e.g., art, music, dance, and physical education) step away from their primary duties to cover a class when substitutes are unavailable. This shortchanges students with learning differences who have a legal right to specialized support services under the Individuals with Disabilities Education Act. In some cases, school principals and administrators are even forced to step in as substitutes.

2. Research Design

2.1 Chicago Public Schools

The Chicago Public School District (CPS), the fourth largest school district in the U.S., is comprised of 508 traditional public schools. The district served 370,000 students in the 2017-18 school year, with a staff of over 22,000 teachers and 5,500 substitutes. The concentration of students of color and low-income students in Chicago is similar to other large, urban districts such as New York City, Los Angeles, and Miami-Dade. Black and Hispanic students make up 37% and 47% of the district, respectively. White and Asian students comprise 10% and 4% of the student population. Seventy-seven percent of students are eligible for free or reduced-price lunch (FRPL), a proxy for household income.

The composition of the teacher workforce and, to a lesser degree, the substitute workforce are not representative of the students they serve. Teachers in CPS are 78% women, 50% white, 21% Black, 21% Hispanic, and 4% Asian. Substitutes are 72% women, 42% white, 39% Black, 10% Hispanic, and 3% Asian. The average teacher in Chicago was absent 12 times in 2017-18, similar to teacher absence rates in other large, urban districts (Joseph, Waymack, and

Zielaski 2014). Twenty-three percent of substitute requests went unfilled in Chicago in 2017-18 – five percentage points higher than similar urban districts in that year (Frontline Education 2019).

CPS is also one of the most racially segregated school districts in the country (Potter 2022a; 2022b). As shown in Figure 1, nearly every school in CPS serves a population of students where a single race comprises over 50% of the student body. Black students are concentrated in the South Side and West Side of the city. Hispanic students are concentrated in the Southwest Side and Northwest Side of the city. White students are concentrated in the North Side and Far North Side of the city, and Asian students are concentrated in Central Chicago. One in three schools in Chicago serves an almost exclusively Black student population (>85%) and one in five schools serves an almost exclusively Hispanic population (>85%).

2.2 Substitute Teachers in CPS

Substitute labor supply in Chicago is shaped by state regulations, district policies, and substitute preferences. During our study period, the state of Illinois required substitutes to hold a bachelor's degree and state-issued substitute teaching licensure. Substitute teachers in CPS are part of the larger Chicago Teachers Union which negotiates on their behalf. CPS employs two primary classes of substitutes: day-to-day and cadre. Day-to-day substitutes – 92% of all substitutes on the roster – earn $165 for a 6.5-hour workday and do not earn benefits. They face no minimum requirements for how much they work and are free to fulfill any request on any day up until the time it starts. Principals directly hire cadre substitutes – the remaining 8% of the substitute roster – at specific schools to cover teacher absences. Cadres earn $186 a day, are eligible for benefits, and generally take on a floating teacher role for the school. Cadres must be available to work every day of the school year and must accept any assignment they are given.

Substitute requests in CPS are made and fulfilled using an absence management platform developed by Frontline Education, an administration software company. Teachers accrue one sick day per month and three personal days per school year. Sick days are the most common type of absence, making up 48% of requests from teachers. When teachers are going to be absent, they or a school administrator enter the expected absence or position vacancy into Frontline. Substitutes can then view absences that need coverage and elect to take jobs by selecting them in the Frontline platform. The platform allows users to have certain jobs or substitutes appear first and to create alerts for jobs at preferred schools or classrooms. Entries in Frontline are highly reliable because they are used to populate substitute payroll. While schools might contact favored substitutes outside of the system to notify them of an opening, all jobs must be entered within the system for substitutes to be paid.

Many substitute teachers in CPS do not choose to work every day. Figure 2 depicts trends in total substitute requests made, requests filled, and the total number of allowable days CPS substitutes could work between 2013-14 and 2018-19. This figure captures the gradual growth and stability of substitute teacher supply over time. It also illustrates that if every active substitute in CPS worked their maximum allowable days, they would more than cover the rising demand for substitutes.

CPS experiences large inequities in substitute coverage across schools. In 2017-18, school average fill rates across the district ranged from as low as 14% to as high as 100%. These gaps are relatively stable over time and are not a function of differential rates of substitute demand. In fact, the typical school in the bottom quintile of fill rates submits less than two thirds the number of requests, on average, as the typical school in the top quintile. Inequities in coverage are likely a symptom of long-standing neighborhood segregation by race and income in Chicago, which has contributed to differences in school quality (Spirou and Bennett 2016).

2.3 Intervention

In the 2018-19 school year, we worked in collaboration with CPS to develop a targeted pay program for substitutes at schools facing the most acute shortages. CPS selected the 75 schools with the lowest historical coverage rates into the incentive program using a weighted average of 2014-15 to 2017-18 school year request fill rates, shown in Figure 3.[8] All schools selected for the incentive pay fully participated in the program. CPS divided these incentive schools into three tiers of 25 and assigned pay stipends of $30, $35, or $40, according to the severity of historical need, with the lowest coverage rates receiving the highest stipend. Relative to day-to-day substitutes' daily pay – $165 – stipends represented a substantial 18% to 24% increase in wages.[9] Substitutes earned the stipend on top of their base pay for each day worked at an incentive school and earned only their base pay at all other schools. The district advertised the incentive pay program repeatedly to substitutes via email, on the Frontline platform, and during a training session prior to the start of the school year. Individual substitute requests made by incentive schools were labeled as "Tier 1," "Tier 2," or "Tier 3" in the Frontline system.

Substitute fill rates also vary considerably throughout the school year in Chicago as shown in Figure 4. CPS offered an additional stipend at incentive schools on 12 high-demand days to address elevated shortages on the Mondays and Fridays between May 3rd through June 17th in the spring of the 2018-19 school year (highlighted in Figure 4).[10] For these days, the

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[8] The 2017-18 school year was weighted 75% and the average fill rate across 2014-15, 2015-16, and 2016-17 school years was weighted 25%.
[9] School-based cadre substitutes were not eligible for the bonuses.
[10] The Friday before Memorial Day (5/24/2019) and Memorial Day (5/27/2019) were excluded from the additional "high-demand" day incentives.

district paid total stipends of $40, $50, or $60 dollars, across the three tiers of treated schools. These "high-demand" days made up 32% of school days in May, 40% of school days in June, and 7% of the school year. CPS advertised augmented, high-demand stipend days in district-wide emails to substitutes shortly before the relevant days. These days with differential stipends were not indicated in a specific way on the Frontline platform.

In Table 1, we provide school-level average characteristics for the district as well as for incentive schools and non-incentive schools in the year prior to the bonus pay program (2017-18). Substitutes filled 47% of incentive schools' job requests, on average, relative to 82% in the 433 non-incentive schools. The typical incentive school served substantially larger proportions of Black (78.5% vs. 40.6%) and FRPL-eligible (86.9% vs. 75.6%) students relative to non-incentive schools. As shown in Figure 3, most initial incentive schools (72%) were highly segregated, serving almost exclusively Black students (>85%). Incentive schools were rated meaningfully lower than non-incentive schools, on average, on the district school quality rating scale (3.1 vs. 3.7 on a scale of 1 to 5).[11] Students in incentive schools scored substantially lower on state tests, with an average difference of over 0.50 standard deviations. The typical incentive school was also more likely to serve elementary and middle grades, with only 2 of the 75 treated schools serving high school students. Violent crime rates were also more than twice as high in incentive schools' neighborhoods as non-incentive schools' neighborhoods.

For the 2019-20 school year, CPS extended treatment to an additional 50 schools for a total of 125 incentive schools. CPS also raised and simplified the stipend structure to be a $45 bonus across all treated schools on all days – a 27% wage premium. The initial 75 treated

[11] The School Quality Ratings range from 1 to 5 and are comprised of several measures including school-average test score growth on standardized assessments and national school attainment percentile, average student daily attendance, and environmental survey ratings. In 2017-18 average school was rated 3.6 and the school-level standard deviation was 0.7.

schools were grandfathered into treatment in 2019-20. CPS selected additional schools using an updated weighted average of historical fill rates which included the 2018-19 school year. The use of two sorting variables across the two selection processes determined treatment status by two distinct thresholds.[12] We visually present how schools were sorted into treatment across these two thresholds in Appendix B Figure B.1. Conceptually, this new cutoff constitutes an extension of treatment to schools with less-severe historical need than the initial 75 schools.

2.4 Data

Administrative Records. We leverage detailed substitute request data from the Frontline platform combined with district administrative data. We observe requests from the 2013-14 school year through March 16, 2020 when CPS closed school buildings due to the COVID-19 pandemic. We link substitute request records for this panel to teacher, substitute, student, and school characteristics using administrative rosters and publicly available district data. Teacher and substitute records include demographics, hire date, job title, and residential address. School data include student body demographics, total enrollment, and school quality ratings. Student-level data include achievement on all state- and district-mandated tests, demographics, and attendance as a percent of each school year. State and district standardized test scores are not available in the second year of the intervention (2019-20) because of COVID-19-induced school closures.

Substitute Surveys. CPS administered an anonymous survey to capture qualitative information about substitutes' behaviors and perceptions of the incentive intervention. Substitutes received this survey in the fall of 2019, the beginning of the second year of the targeted bonus pay program. The survey was sent to all substitutes via their CPS email accounts

[12] The new weighted average placed 75% of the weight on fill rates from 2018-19 and 25% of the weight on the average fill rate across 2015-16, 2016-17, 2017-18 school years.

and consisted of a mix of Likert-scale, multiple choice, and open response items. Questions covered topics such as capacity to work more, motivation for working as a substitute, reasons for not wanting to work at certain schools, and what administrative supports improve their work experiences. Forty percent of active substitutes responded to the survey. We integrate results from substitutes' survey responses throughout the paper to provide further context for interpreting our findings.

2.5 Substitute Supply Outcomes

Our primary outcomes relate to the nature of substitute labor supply at schools. Our focal outcome of interest is the percent of substitute requests filled at a school in a school year. We explore the mechanisms of any effects on fill rates by examining the number of unique substitutes who work at a school in a year and the average number of days a substitute works at a school. The number of substitutes that work at a school approximates the school-specific extensive margin of substitute labor supply, while the average number of days substitutes work at a school approximates the school-specific intensive margin.

We explore potential heterogeneity in responsiveness to incentive pay across a range of substitute characteristics. First, we disaggregate the number of substitutes at a school into five mutually exclusive and exhaustive groups based on their prior-year work history. These groups include substitutes who: 1) had worked at incentive schools only, 2) had worked at comparison schools only, 3) had worked at both incentive and comparison schools, 4) were new to the district, or 5) were "lapsed" substitutes who did not fill a request in the prior year. We also decompose overall effects on the number of unique substitutes by substitute race as well as estimated commute time to a school.[13] We disaggregate the number of unique substitutes at a school in a year by whether they commuted 10 minutes or less, between 10 and 20 minutes, or more than 20 minutes to explore the localness of the response to incentives.[14] These measures allow us to examine which substitute teachers were most responsive to incentive pay.

2.6 School-Wide Teacher and Student Outcomes

We also examine a set of school-wide outcomes to explore the effect of the substitute bonus pay program on teacher attendance and retention as well as student academic achievement. We first examine whether the substitute bonus pay program affected average teacher absences in a school. Second, we construct a binary measure of teacher retention (whether a teacher returned to their school the following year) as an indirect measure of the policy's effect on teachers. We then disaggregate the converse of this measure (teacher turnover) into two mutually exclusive and exhaustive indicators for whether teachers transferred to another school in CPS and whether teachers exited the district.

We test whether incentives have indirect effects on student achievement in math and English language arts (ELA). Our achievement measures leverage a wide range of assessments to create an indexed subject score for each student, standardized by grade and year. Tests include the Measures of Academic Progress (MAP), Partnership for Assessment of Readiness for College and Careers (PARCC), Illinois Assessment of Readiness (IAR), Preliminary SAT (PSAT), and SAT assessments. All of these tests are mandatory in the district, with participation

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[13] We were able to geocode just under 80% of commutes for filled requests over the 2016-17 through 2019-20 school years. We calculated the driving commute time under normal traffic conditions from a substitute's address to the school (Weber and Péclat 2017).

[14] We selected these commute time thresholds for their heuristic relevance. In 2017-18, 29 percent of observed commutes were 10 minutes or less, 36 percent were between 10 and 20 minutes, and 35 percent were more than 20 minutes.

rates between 95% and 99%. Together, we observe scores for 75% of all students in CPS schools in 2018-19 and 95% of all students in grades 3 through 8.

3. Econometric Methods

The treatment-sorting process described above introduced a discrete treatment cutoff that we leverage to estimate the causal effects of incentive pay using a sharp regression discontinuity (RD) design (Calonico et al. 2017; Imbens and Lemieux 2008; Lee and Lemieux 2010; Cattaneo and Titiunik 2022). This design isolates the effect of the incentive pay for those schools at the margin of receiving treatment. The key identifying assumption is that it was impossible for schools to manipulate their historical fill rates to qualify for the incentive program. Conceptually, this assumption implies that schools near the program eligibility cutoff are comparable in all aspects except for their treatment status (Cattaneo, Idrobo, and Titiunik 2019). This assumption clearly holds in our case where the CPS central office developed the targeted incentive pay program without schools' knowledge and used historical data on fill rates collected in the Frontline data portal that schools could not manipulate retrospectively. Schools were also not aware of the weighting formula the district used to calculate historical fill rates or the exact cutoff used to determine eligibility.

We include a standard set of tests in Appendix A to affirm the validity of our RD design. These include documenting that a range of school characteristics are smooth functions across the treatment threshold and testing for covariate balance. We also examine the density of the sorting variable across the treatment threshold and again find a smooth distribution. A nonparametric manipulation test (Cattaneo, Jansson, and Ma 2020) fails to reject the null, with a p-value of 0.18.

For a given school-level outcome, we fit a simple local linear regression model within a fixed bandwidth around the treatment cutoff as follows:

$$Y_s = \alpha + \beta TREATED_s + f(HFR_s) + \delta X_s + \gamma_g + \varepsilon_s \qquad (1)$$

The outcome, Y_s, is a school-level measure such as the percent of requests filled by substitutes at school *s*. Our primary independent variables are school-level indicators of treatment status, the continuous historical fill rate forcing variable HFR_s that CPS used to sort schools into treatment, and their interaction to allow for the slope of HFR_s to differ on each side of the cutoff. We also include fixed effects for grade ranges (elementary, middle, high), γ_g given the large concentration of elementary schools among treated schools. We use a uniform kernel weighting function and a bandwidth of +/- 0.12 across all models to allow for a common analytic sample across outcomes, selecting the largest optimal bandwidth among our set of primary outcomes following Calonico et al. (2017).[15] For school-level outcomes, we estimate heteroskedasticity-robust standard errors. We adapt equation (1) to model teacher- and student-level outcomes where teachers and students are nested within schools and cluster our standard errors at the school level. We further test the robustness of our modeling decisions by applying a range of alternative bandwidths, functional forms, and kernel weights. Results from these analyses shown in Appendix A suggest that our findings are consistent across alternative modeling specifications.

[15] We privilege a uniform kernel over other weighting approaches that place higher weight on observations closer to the cutoff because of the highly segregated nature of schools in CPS, which causes the racial composition of students to vary widely across individual schools. Estimates using a triangular kernel reported in Appendix Tables A4 and A7 show our overall findings are not sensitive to the choice of weighting functions.

For all outcomes, we present unconditional models as well as models that include covariates intended to increase the precision of our estimates (Calonico et al. 2019). Covariates include a school-level vector of controls, X_s, for student body racial composition, FRPL-eligibility, special education status, English language-learner status, total enrollment, and school-level lagged achievement in math and ELA. For teacher-level outcomes, we complement this school-level vector with a vector of teacher characteristics that includes indicators for teacher race, gender, and tenure (3-5, 6-10, 11-20, and 21+ years working in the district). For student-level outcomes, we replace this teacher-level vector with a student-level vector that includes indicators for student race, FRPL-eligibility, special education status, English language-learner status, grade, and a cubic of the lagged outcome.

We estimate treatment effects separately for the first and second years of the incentive pay program given the change in the local margin of treatment. For the 2019-20 school year, we focus on the treatment effect at the 125th school cutoff by excluding the original 75 schools sorted into treatment in 2018-19 from our analytic sample. We also report estimates at the original 75th school cutoff in Appendix B for completeness. Estimating effects in an RD framework at both thresholds is possible because CPS used an updated running variable in the second year that re-sorted schools and created a two-dimensional treatment discontinuity as illustrated in Appendix B Figure B.1. We again include the standard set of RD robustness checks for the 125th-school cutoff using our second year of data in Appendix A, all of which confirm the validity of the core design assumptions.

One potential concern with targeted incentive programs is that they have the potential to cause negative spillover effects on non-treated units (Card and Giuliano 2016; Kho et al. 2019; Cabrera and Webbink 2020; Pugatch and Schroeder 2018). To better understand the threat that negative spillover would pose to our research design, it is helpful to conceptualize our RD estimates as the results of a localized randomization design (Cattaneo, Titiunik, and Vazquez-Bare 2017). Any negative spillovers of the targeted incentives on non-incentive schools would violate the Stable Unit Treatment Value Assumption (SUTVA) of experimental designs which assumes that the treatment effect for each unit is independent of the effect of treatment on other units. Schools that were not eligible for incentive pay might have experienced a spillover effect due to substitutes reallocating their labor to jobs in incentivized schools. If this were the case, we might expect that our RD estimates overstate the positive effect of treatment on incentive schools because this discontinuity captures both an increase in fill rates for incentive schools and a decrease in fill rates for non-incentive schools.

We conduct a series of descriptive bounding exercises and formal robustness tests to assess the degree to which our RD estimates are possibly inflated by negative spillover. The results of these tests, which we describe below, confirm that any negative spillover effects are at most very minimal in our context. Although the targeted bonus did incentivize a subset of substitutes to shift more of their labor to the incentive schools, negative spillover effects were largely offset by a district-wide increase in substitute labor supply on the intensive margin. This was possible because most substitutes do not work to their full potential number of days.

4. Findings

4.1 Effects on Substitute Labor Supply

We find that the targeted bonus pay program substantially increased substitute fill rates. We present our empirical estimates in Table 2 Panel A and a visualization of the discontinuity in Figure 5. Visual evidence from Figure 5 illustrates a large jump in fill rates among incentive schools. The figure also depicts a relatively smooth linear trend in fill rates among non-incentive schools ruling out any concentrated negative spillover effects for those schools just below the assignment cutoff. In our preferred model with covariates, the treatment effect of the targeted incentives among schools just over the treatment cutoff is a 23 percentage-point increase in substitute request fill rates. This amounts to a 1.4 standard deviation increase in the distribution of school-level fill rates across CPS schools in 2017-18, a move from the 6th percentile to the 28th percentile of the empirical distribution, or nearly a 50% increase in the pre-treatment average of treated schools.

Our daily records on substitute requests allow us to explore the dynamic nature of the treatment over the course of the academic year. Treatment effects averaged across the 2018-19 academic year may mask important heterogeneity in substitutes' responses to incentives and experiences working in treated schools if, for example, the targeted bonuses increased substitutes willingness to try working in incentive schools early in the fall but their experiences made them unwilling to return. In Figure 6, we present estimated treatment effects on substitute request fill rates separately by month. Large effects emerge in November (September and October have a low volume of requests and generally higher fill rates) and are sustained across the school year with a spike in June possibly due to the concentration of additional high-demand day incentives. Further, fill rates for just high-demand days in May and June produce a 28 percentage-point increase in coverage (p<0.000), suggesting the additional day-specific stipends increased coverage by another 5 percentage points.

We find strong evidence that these higher fill rates across the school year were driven by increased labor supply of substitute teachers on the school-specific extensive margin. As shown in Table 2 Panel A, we estimate that treatment increased the number of unique substitutes at a treated school across the academic year by 13 individuals in our preferred model, relative to schools with similar histories of unmet need for substitutes – a 26% increase from the pre-treatment average of 50. We find no effects on the school-specific intensive margin of substitutes' labor supply in 2018-19. Estimates for effects on the average number of days substitutes worked at given school are positive, but small and not statistically significant. See Appendix B for corresponding regression discontinuity plots of these analyses and all other outcomes.

4.2 Heterogeneous Responses Across Substitutes

Disaggregating the unique substitutes at each school in 2018-19 by where they worked in 2017-18, we find the increase in the number of substitutes was primarily driven by those who had prior experience working in incentive schools but did not exclusively work in these schools. As shown in Table 2 Panel B, the number of substitutes with all other work histories appear unchanged by incentives. Our survey results echo this heterogeneity, with 43% of respondents saying they were "not at all" or only "slightly" interested in taking a job at an incentive school despite the bonus pay. Half of all survey respondents also reported that they refuse to work in at least one school in the district based on prior experiences. Bonus pay appears to have attracted substitutes to incentive schools who were already willing to work in similar schools, but was less enticing to new substitutes and those who had not recently worked in incentive schools.

We also observe heterogenous responsiveness to incentives by substitute proximity to a given school. In Panel A of Table 3, we present our treatment effect estimates for substitutes living within different commuting times of a school. We find no effect on the number of substitutes commuting 10 minutes or less, a significant increase of 8 substitutes commuting between 10 and 20 minutes, and an increase of 5 individuals commuting more than 20 minutes. Comparing the pre-treatment means presented in Table 3, this change in substitutes commuting

10 to 20 minutes is equivalent to a 43% increase. This suggests the incentives were most effective for substitutes who already lived within a convenient, but not immediate, distance from a school and to a lesser degree those substitutes that had longer commutes.

Lastly, we examine heterogeneity in substitute responsiveness by race and gender. We report effects for the number of unique substitutes at a school in a year for the four largest racial groups and by gender in Panel B of Table 3. Focusing on estimates from our controlled models, we find that only Black and Hispanic substitutes were responsive to the targeted bonus to work in incentive schools, which serve almost exclusively Black and Hispanic students. The number of Black substitutes at incentive schools increased by 10, a 33% increase compared to the prior year average. The number of Hispanic substitutes increased by more than 2, a 57% increase. White substitutes were, on average, unresponsive to the incentives even though they constitute the largest racial group of substitutes in the district (42%). We also find that women were more responsive to the incentives than men. The increase in unique substitutes who are women – who constituted 74% of substitutes at the average treated school in 2017-18 – accounts for 90% of the entire treatment effect increase of 13 individuals. We find only a small and insignificant increase in the number of men substituting at incentive schools.

4.3 Effects on Teacher and Student Outcomes

In Table 4, we present our estimated treatment effects at the cutoff for teacher outcomes and student achievement. We find positive but insignificant point estimates for teacher retention of 4 and 2 percentage points in our unconditional and conditional models, respectively. Disaggregating teacher turnover into transfers and district exits, we find that point estimates for retention appears to be driven by a reduction in teacher transfers across schools rather than exits from the district. We also estimate a positive but insignificant increase in the number of days teachers are absent by 1.5 days.

At the student-level, we find consistently positive estimates for effects on student achievement in both math and ELA of similar magnitude. We estimate the targeted incentive program increased average student achievement in ELA by a statistically significant 0.05 standard deviations. This estimate is uniformly larger (0.05 to 0.11) and significant across models using a triangular kernel, alternative bandwidths, and polynomial specifications (Appendix Tables A2 and A8). Estimates for effects on math achievement from our controlled model are also 0.05 standard deviations but do not achieve statistical significance. Including our vector of covariates and lagged achievement measures substantially improves our precision while leaving the effect magnitude largely unchanged. These effects are small in absolute magnitude but meaningful given that this is an average effect for all students in a school. This represents a 0.10 standard deviation increase in the distribution of school-level average achievement.[16]

Putting these estimates in context with other studies helps to highlight how uncovered teacher absences are particularly detrimental to student achievement. Teachers in the incentive schools were absent an average of 11.4 times per year in 2018-19, suggesting that the intervention caused students to have roughly three additional days of teacher absences covered by a substitute. Prior studies of the effect of teacher absences *when substitutes are present* find that 10 days of covered teacher absences lowers student achievement by roughly 0.01 to 0.03 standard deviations (Benhenda 2022; Clotfelter, Ladd, and Vigdor 2009; Herrmann and Rockoff 2012; Miller, Murnane, and Willett 2008). These studies also find evidence that substitute quality

[16] The standard deviation of school-level average achievement in our panel is 0.52 in math and 0.50 in ELA.

matters suggesting that the effects we find are larger because they might operate both on the extensive margin of securing a substitute (and thereby avoiding the negative consequences of uncovered absences) as well as the intensive margin of attracting higher-quality substitutes.

4.4 Second Year Program Impacts

Overall, we find that the incentive pay program had largely similar effects in terms of both magnitude and mechanisms across both years despite the different margins at which we estimate treatment effects. As reported in Table 5 and shown in Figure 7, we find a 21 percentage-point increase in fill rates in 2019-20. Figure 7 illustrates a relatively smooth linear trend in fill rates among non-incentive schools suggesting that there were no concentrated negative spillover effects for those schools just below this new threshold. We also show that this improved coverage for the newly added cohort of 50 schools in the second year of the program did not come at the cost of dampened incentive effects amongst the initial 75 treated schools (Appendix B Table B.1).[17]

We again find large effects on the number of unique substitutes, and a small, insignificant negative effect on the number of days substitutes work suggesting that increases in coverage were again driven by more substitutes working in a school, on average. Our heterogeneity analyses according to where substitutes worked in the year prior again suggest that substitutes who worked at both treated and comparison schools were the most responsive to incentives. Encouragingly, we also find a statistically significant increase in the number of lapsed substitutes of 2.28 who did not work the prior year. We present heterogeneity analysis by substitute

[17] For results at the 75th-school cutoff for our substitute heterogeneity outcomes and teacher-level outcomes, see Appendix B Tables B.2 and B.3, respectively. Month-level fill rate estimates are shown in Figure B.24. We present visual results of raw discontinuities in Appendix B Figures B.25 through B.41.

commute, race, and gender and our teacher-level outcomes in Tables 6 and 7, respectively. These results also echo the patterns found for the first treated cohort.

We find no effects on teacher retention in the second year although these estimates based on end-of-year outcomes are likely less generalizable given the context of the COVID-19 pandemic and move to emergency remote teaching in the spring of 2020. The estimated magnitude of effects on teacher absences become larger in the second year, suggesting that teachers were absent 3.5 more days, on average, because of the substitute incentive pay program. The increased substitute coverage from the intervention might have made teachers less reticent about being absent knowing that their colleagues would be less likely to bear the burden. Further analyses reveal that this increase is driven almost entirely by an increase in absences for professional development rather than absences for sick leave or personal days (see Appendix B Table B.4). It appears the increase in substitute coverage allowed teachers in incentive schools to be more willing and able to engage in professional learning.

4.5 Negative Spillover Effects

Both descriptive evidence and empirical tests confirm that the large positive treatment effects we find are not driven by negative spillover effects. To start, visual evidence from Figures 5 and 7 is inconsistent any concentrated spillover effects at the treatment discontinuity. The rate of unfilled absences among non-incentive schools is a steady, continuous line all the way up to the incentive cutoff. Thus, any potential spillover effects would be diluted across the much larger sample of control schools given there are more than five times as many control schools as incentive schools.

A simple bounding exercise suggests that negative spillover effects account for at most a small fraction of our overall RD treatment estimates. To show this, we present a time series of fill rates for the entire district and for treatment and control schools separately in Figure 8. Between 2017-18 and 2018-19, the aggregate, district-wide substitute coverage rate declined from 79.9% to 73.6%, a decline entirely driven by an increase in total substitute requests (see Figure 2). This captures a district-wide secular decline that would be unaffected by any shifts in substitute labor supply from non-incentive to incentive schools. Despite this district-wide decline, the aggregate fill rate for the original 75 incentive schools increased by 14.6 percentage points between 2017-18 and 2018-19. This establishes a lower bound for our treatment effects under the implausible assumption that incentive schools did not experience a secular decline in fill rates during the year of the intervention. If anything, any secular decline was likely to be concentrated in these schools with historically low substitute fill rates. Historical patterns show larger drops in substitute coverage for schools with persistently low fill rates compared to schools with persistently high fill rates. If we assume that incentive schools experienced even an average secular decline in fill rates of 6.3 percentage points, then the treatment resulted in a net positive effect of 20.9 percentage points (14.6 + 6.3), very close to the entire estimated effect from our preferred models.

Data on the raw aggregate number of days worked by substitute teachers helps to illustrate why negative spillover effects were minimal in this context. Substitutes worked a total of 286,232 days in 2017-18, only 7.5% of which were in incentive schools. The number of substitute days worked in incentive schools rose from 21,538 in 2017-18 to 36,306 in 2018-19, a 69% increase. This increase in labor supplied to incentive schools was possible, in part, because CPS substitutes' total labor supply also increased in 2018-19 to 293,588 days. The large number of non-incentive schools also served to minimize negative spillover such that the total labor supply in non-incentive schools fell by only 2.8%, from 264,694 to 257,282 days.

We next conduct more formal robustness tests where we remove sets of non-incentive schools most likely to be susceptible to negative spillover effects and re-estimate our RD models. These results further demonstrate that our estimates are not driven by negative spillover effects. We construct two proxy measures for the degree to which schools are likely to be affected by negative spillovers. The first measure is the percentage of substitute teachers at a school who taught in both incentive and control schools in the year prior to the targeted incentive pay program. As we describe in our findings above, substitute teachers who had previously worked in both incentive and non-incentive schools were more responsive to the targeted bonus, leaving the non-incentive schools with larger proportions of these substitutes susceptible to losing more coverage. The second measure is a count of the number of incentive schools within a three-mile radius of a given non-incentive school, which reflects the intensity of the local market competition for substitute labor.[18] This captures the ease with which substitutes could shift their labor away from a non-incentive school to an incentive school without meaningfully increasing their commute time.

We then re-estimate our RD models, omitting non-incentive schools in either the top quarter or upper half of the distributions of these sensitivity-to-spillover proxies. This allows us to estimate the counterfactual projection of the fill rates for treatment schools close the cutoff without including comparison schools most likely to suffer from potential negative spillover effects. Estimates across our models shown in Table 8 range between 20 to 24 percentage points, strikingly similar to our preferred estimate of 23 percentage points. We replicate these robustness tests for the second year and again find our results are largely unchanged with estimates ranging between 18 to 25 percentage points (Appendix B Table B.5).

[18] The number of incentive schools within a 3-mile radius ranges from 0 to 41, with a median of 7. Estimates based on a 5-mile radius produce extremely similar results.

5. Extensions

5.1 Equity Implications

Inequities in substitute coverage across schools in CPS motivated the design of the incentive pay program. The large increase in fill rates at schools with historically low substitute coverage caused by the targeted incentives redistributed the burden of unfilled teacher absences more equitably across schools. We quantify this equity-enhancing effect by plotting Lorenz curves of the cumulative density of total unfilled substitute requests across schools sorted by the historical fill rate forcing variable from our RD analysis in Figure 9. The 75 schools in the first cohort of the incentive program (15% of all CPS schools) accounted for 29% of the district's unfilled absences, on average, across the five years prior to the reforms. This share of all unfilled absences dropped to 18% in the first treated year. The corresponding Gini coefficient – an equity indicator ranging from 0 to 1 where higher values represent greater inequity – decreased from 0.38 to 0.24 with the introduction of the targeted incentives in 2018-19.

5.2 Program Costs

The total cost of the additional stipends in 2018-19 was $1.1 million, on top of the $42 million cost of base pay for substitutes in that year. The incentive costs represent less than 0.03% of the district's total instructional expenditures and less than 0.01% of the $6 billion total operating budget. This translates to an average cost of $14,253 per incentive school or $34 per-pupil in incentive schools. With the increased stipend amount and program size in 2019-20, the program expenses through mid-March 2020 totaled $1.7 million, $13,902 per treated school, or $34 per student. Per-school costs were on track to be slightly higher than the first year of the program if the remainder of the school year had not been shifted to emergency remote learning due to the COVID-19 pandemic. Overall, these per-pupil costs of the intervention are very low relative to most education interventions and highly cost-effective even with a modest 0.05 standard deviation effect on achievement (Kraft 2020). The program cost $128 per additional filled substitute request that the incentives generated.[19] In comparison, the district spends roughly 54% of its $6 billion dollar budget on instructional expenditures which amounts to a loss of $1,283 per unfilled teacher absence when no instruction occurs.[20] Securing a substitute with a small bonus stipend makes it possible to recuperate at least a portion of this lost investment by making learning more likely.

5.3. Substitute Wage Elasticities

We leverage the variation in daily wage rates across schools (created by the targeted and tiered incentive pay plan) and days (created by the additional stipends for high-demand days) to estimate substitutes' wage elasticity of daily labor supply for incentive schools. Using a panel of school-by-day-level data from 2018-19, we regress the logarithm of fill rates on the logarithm of wages with school and day fixed effects. We estimate substitutes' wage elasticity of daily labor supply to incentive schools is 1.5, very similar to the 1.4 short-run elasticity of teacher's annual labor supply estimated by Falch (2011). Extrapolating from these estimates, it would take almost an $80 bonus – almost half of the day-to-day substitute daily rate – to raise the 47% pre-treatment average fill rate in the initial 75 treated schools to the 82% fill rate observed in comparison schools. We emphasize, however, that this elasticity estimate likely does not generalize to all CPS substitutes given that many were not responsive to the incentive program.

5.4 Why Targeted Incentives Didn't Change Some Substitutes' Labor Supply

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[19] We estimate this cost by diving the total program cost by the estimated number of filled substitute requests the treatment induced.

[20] We obtain our per student per day estimate by dividing $9,138 in instructional expenditures per pupil in 2018-19 by 178 school days. We then multiply this by 25, the average class size across all CPS, to obtain the lost investment per teacher absence.

When asked whether they would take a job at one of the 125 incentive schools in CPS in the fall of 2019, 23.3% of substitutes said they were not at all likely and another 19.4% were only slightly likely. What explains these respondents' hesitation to work in incentive schools despite up to a 27% wage premium? The most common explanations substitutes indicated on our survey were distance (34.5%) and the neighborhood where a school was located (23.2%). Consistent with prior research (Gershenson 2013; Liu, Loeb, and Shi 2022), distance appears to be a first-order factor shaping substitutes' labor supply. In Figure 10, we illustrate this with a map of the district from the 2016-17 school year. The figure shows how fill rates in CPS schools are systemically lower in areas of the city where fewer substitutes live. The actual and perceived safety of a school's surrounding neighborhood is also likely an important factor given the substantially higher crime rates in the areas where incentive schools are located (see Table 1). In Chicago, preferences for shorter commutes and certain neighborhoods are also deeply enmeshed with the legacy of racial and socioeconomic segregation. This can be seen clearly by comparing Figure 1, which shows the geography of school segregation in CPS, with Figure 10. The schools that struggle most to attract substitute teachers are almost exclusively in Black or Hispanic neighborhoods and serve almost entirely Black or Hispanic students.

6. Discussion and Conclusion

The design of wage structures and choices in the on-demand labor market have important consequences for whether and where daily workers supply their labor as well as for consumer welfare. Whether a worker chooses to join the on-demand sector depends largely on factors such as relative wages, job location, working conditions, schedule flexibility, and entry requirements. The substitute teaching profession is a large on-demand sector where wages range between $15 to $25 an hour, demand exists nationwide in almost every community, working conditions vary substantially, and barriers to entry (e.g., B.A. or A.A. degree) can be high relative to similar-paying jobs. It is also a setting where workers have strong local preferences and daily wages are fixed across sizable geographic areas. As we show, this type of market design can substantially distort the supply of on-demand labor within metropolitan areas. Equal pay leads to dramatically unequal rates of substitute coverage.

Our analysis of the substitute teacher labor market in CPS illustrates how the on-demand employment structure for substitutes has resulted in negative and inequitable consequences for schools and students. At a basic level, the uniform per-diem rate in CPS is below the market wage that would result in full substitute coverage for most schools. With wages fixed for all jobs in the district, substitutes fill requests based on their preferences across locations and working conditions. These features differ substantially across schools in large urban districts like CPS, leading to inequities in access to substitute coverage. CPS's move towards differentiated pay across schools demonstrates that some substitutes have more malleable preferences and will change where they work in response to financial incentives. For many more substitutes, a bonus equivalent to a 18%-27% raise was not large enough to compensate for their differential preferences.

The targeted intervention also demonstrates the important consequences of substitute labor supply for student performance in school. Increasing fill rates in incentive schools by 23 percentage points resulted in an average, school-wide increase in achievement of 0.05 standard deviations. Uncovered teacher absences are not just an operational challenge; they have direct effects on students' opportunities to learn both within the classroom of an absent teacher and also in other classrooms across a school.

We see several potential paths forward to reduce the large inequities in substitute coverage found in CPS and other large, urban school districts. Our study suggests differentiated compensation is a promising market mechanism that is also feasible from a policy perspective. Most substitutes are not unionized allowing districts to set wages unilaterally rather than through a collectively bargained process. States could also lower barriers to entry as several states have done by requiring only an associate's degree or allowing student-teachers to serve as substitutes (Goldhaber and Payne 2022). Another alternative is investing in full-time substitute positions such as cadre roles where substitutes either work exclusively at one school or must accept any job assigned by the central office. This change could have its own supply challenges given that many workers in on-demand markets highly value flexible work hours (Chen et al., 2020).

Other avenues include improving substitute training and working conditions. Substitutes might be willing to teach in a wider range of schools if they were better trained to foster classroom community and built rapport with students. Nationally, almost half of all districts do not provide any training whatsoever for substitute teachers (Kelly Education, n.d.). Efforts to equalize working conditions for substitutes across schools could also serve to reduce inequities in substitute coverage. Based on survey responses, substitutes are most satisfied working in schools where high-quality lesson plans, school maps, and schedules are provided and a staff member is available to answer questions that the substitute may have.

None of these efforts, however, would directly address the challenge presented by substitutes' strong preferences for short commute times and safer neighborhoods. Until recently, CPS required substitutes to be college-educated workers. Because Chicago residents with college degrees are more likely to live in higher-income areas in the north and near south of the city, this requirement placed schools in low-substitute-density areas at a distinct disadvantage. These low-density areas reflect the stark neighborhood segregation of Chicago: schools with acutely low fill rates are primarily those located in historically Black and Hispanic neighborhoods. Not only do fewer potential substitutes live in these segregated neighborhoods, but substitutes in other surrounding areas may also be less willing to commute to schools in these communities because of the higher rates of crime. One possible solution might be to recruit a more diverse substitute labor workforce from these low-density areas given that Black and Hispanic substitutes appear most responsive to incentives for taking jobs in segregated schools serving almost entirely Black and Hispanic students. These efforts might be linked with “Grow Your Own” teacher pipeline programs aiming to increase the supply and diversity of new teachers. Ultimately, educational equity will likely require addressing the racial and economic segregation that is prevalent in many American cities such as Chicago and that underlies differential crime rates, school working conditions, and the supply of local substitutes.

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Figures

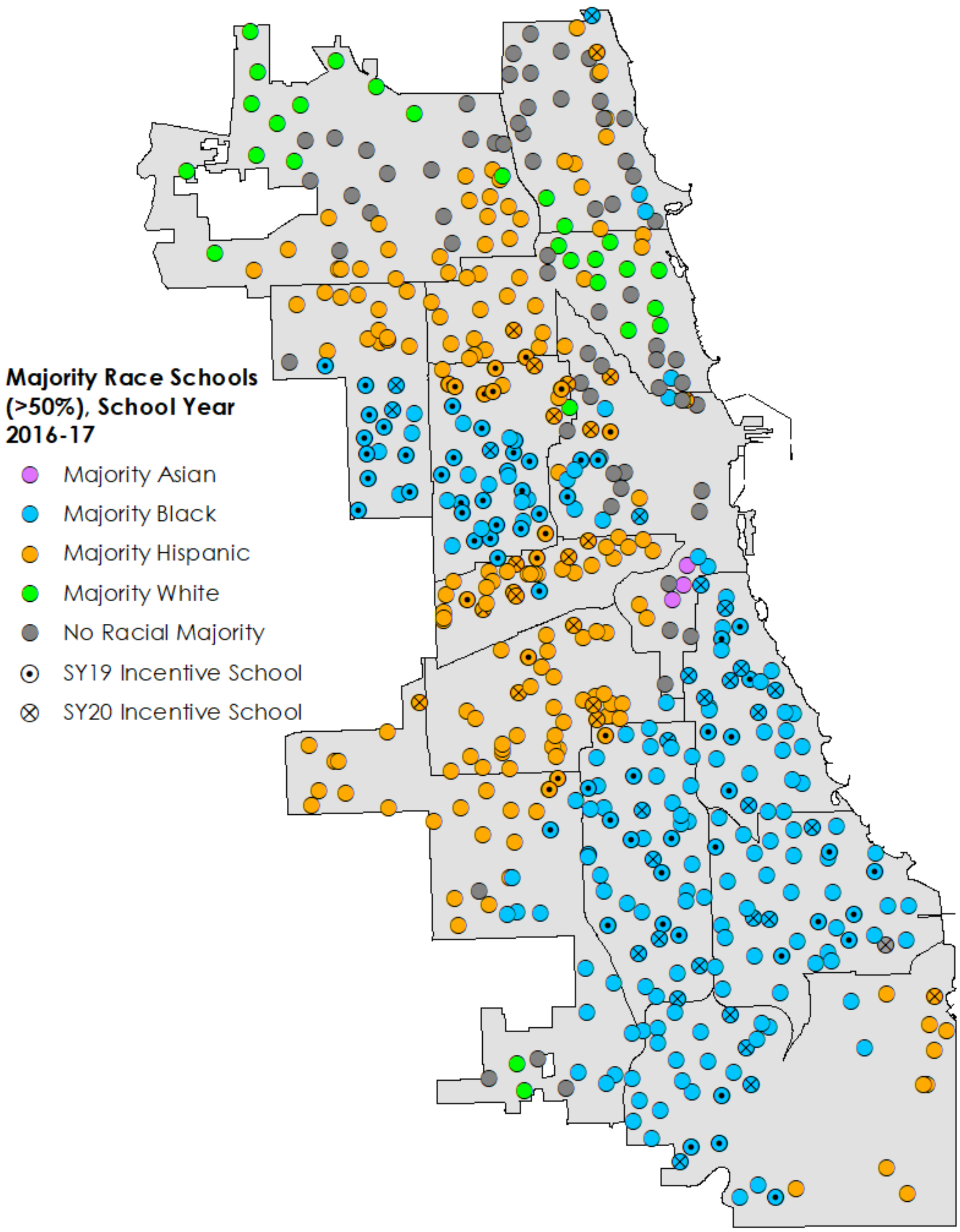

Figure 1. The geography of racial segregation across Chicago Public Schools, 2016-2017.

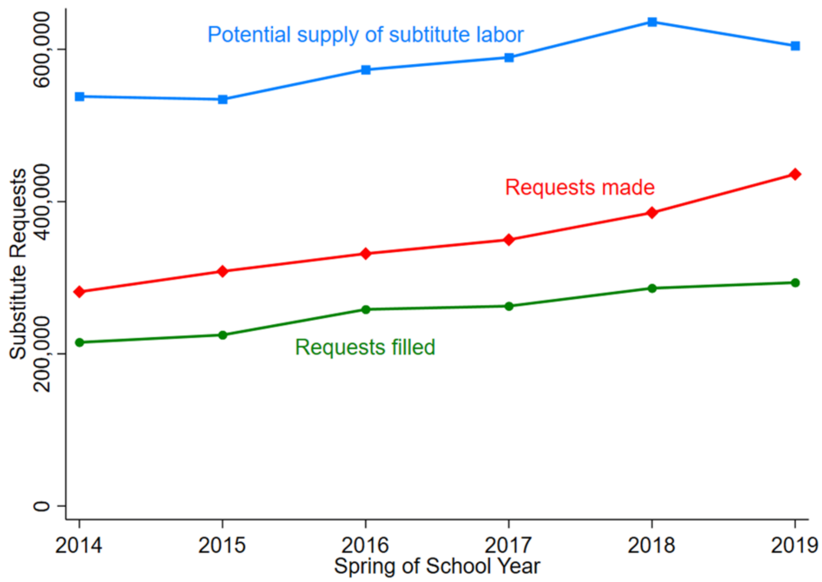

Figure 2: Potential, requested, and realized substitute labor supply

Notes: We approximate potential supply of substitute labor by aggregating the maximum allowable days for all substitutes we observe working in a given year. This is equivalent to the average observed substitute working 140 school days each year, 77 days more than the average substitute worked in 2017-18.

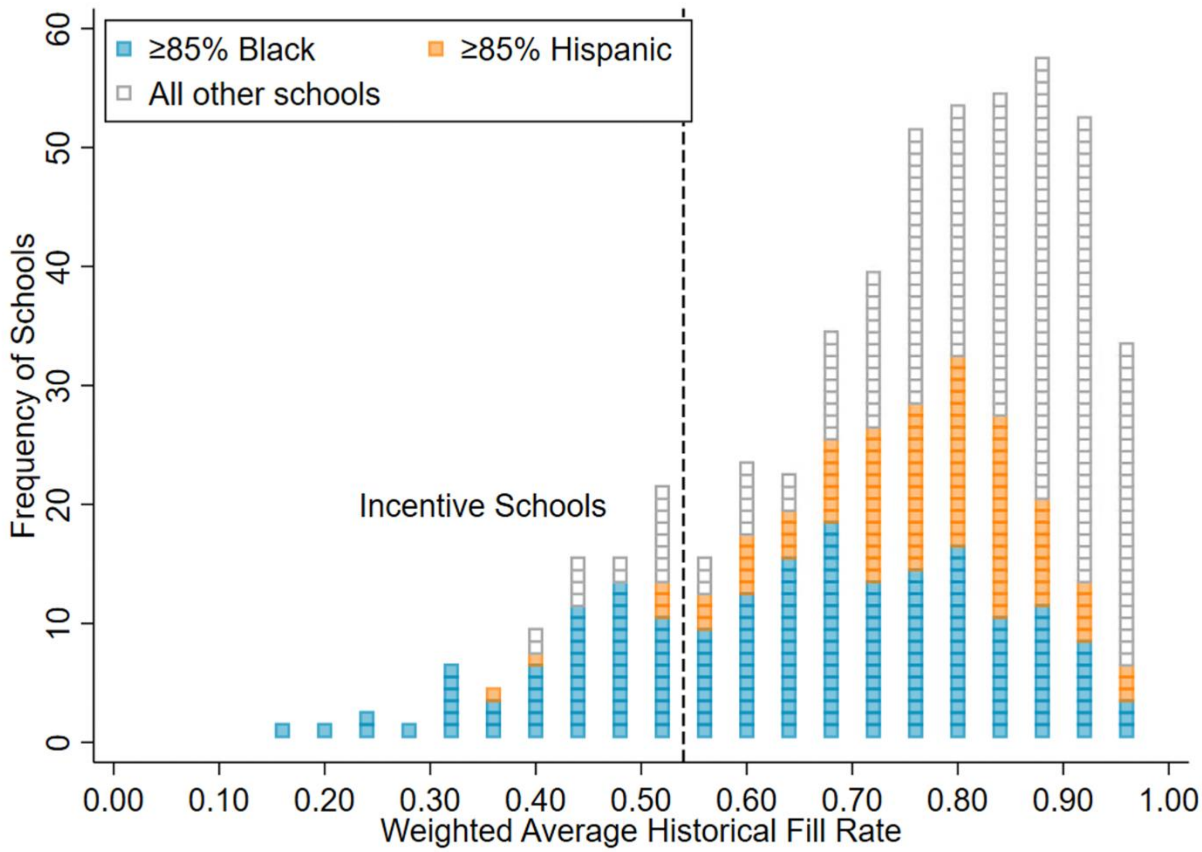

Figure 3: Histogram of treatment sorting variable

Notes: Each square represents a single school. Histograms bins are 4 percentage points wide. All incentive schools are to the left of the dashed vertical line.

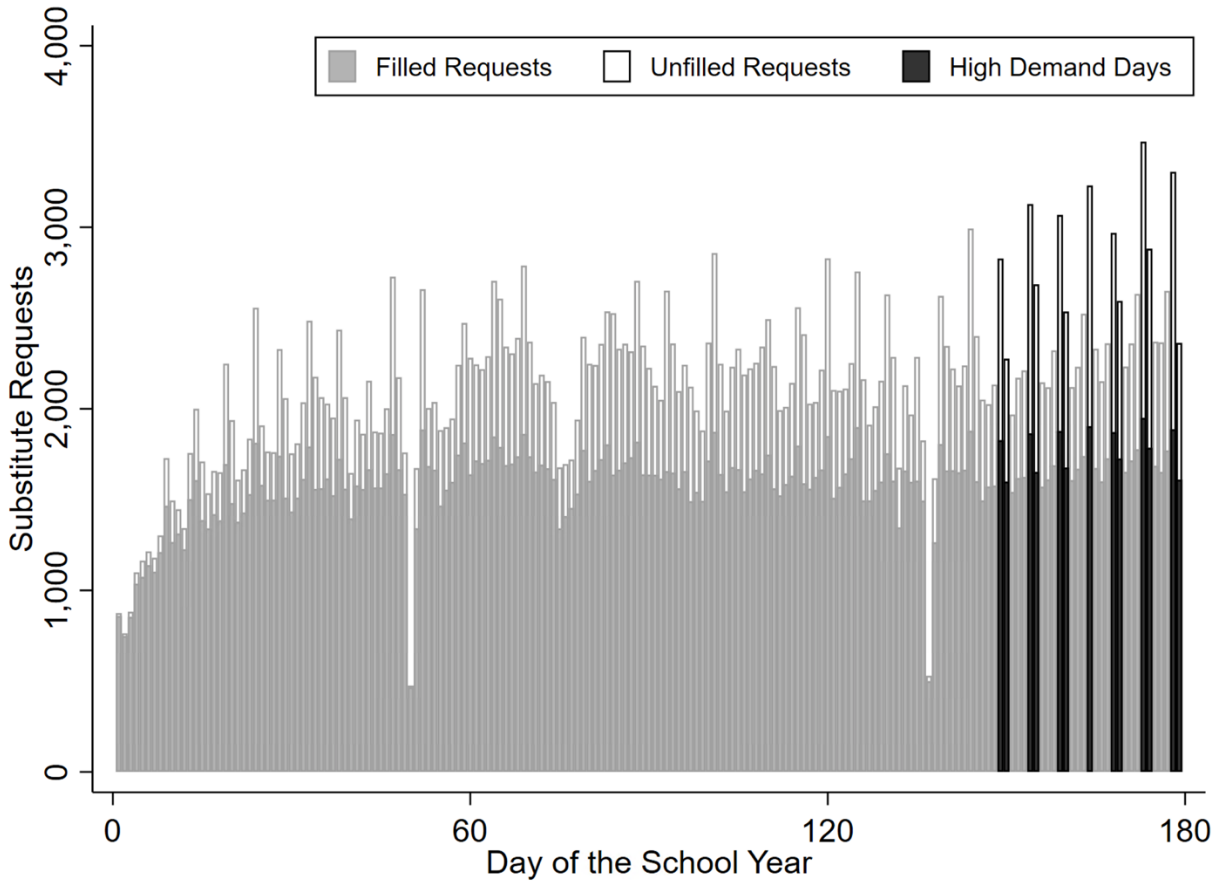

Figure 4: District-wide, day-level substitute request volume and coverage in 2017-18

Notes: Each bar represents the total requests in the Chicago Public Schools District on a given day of the school year, which we disaggregate by substitute coverage. We present the pre-treatment year to show that high-demand days were selected for additional day-specific stipends because of their relatively high volume of requests and relatively low coverage rates.

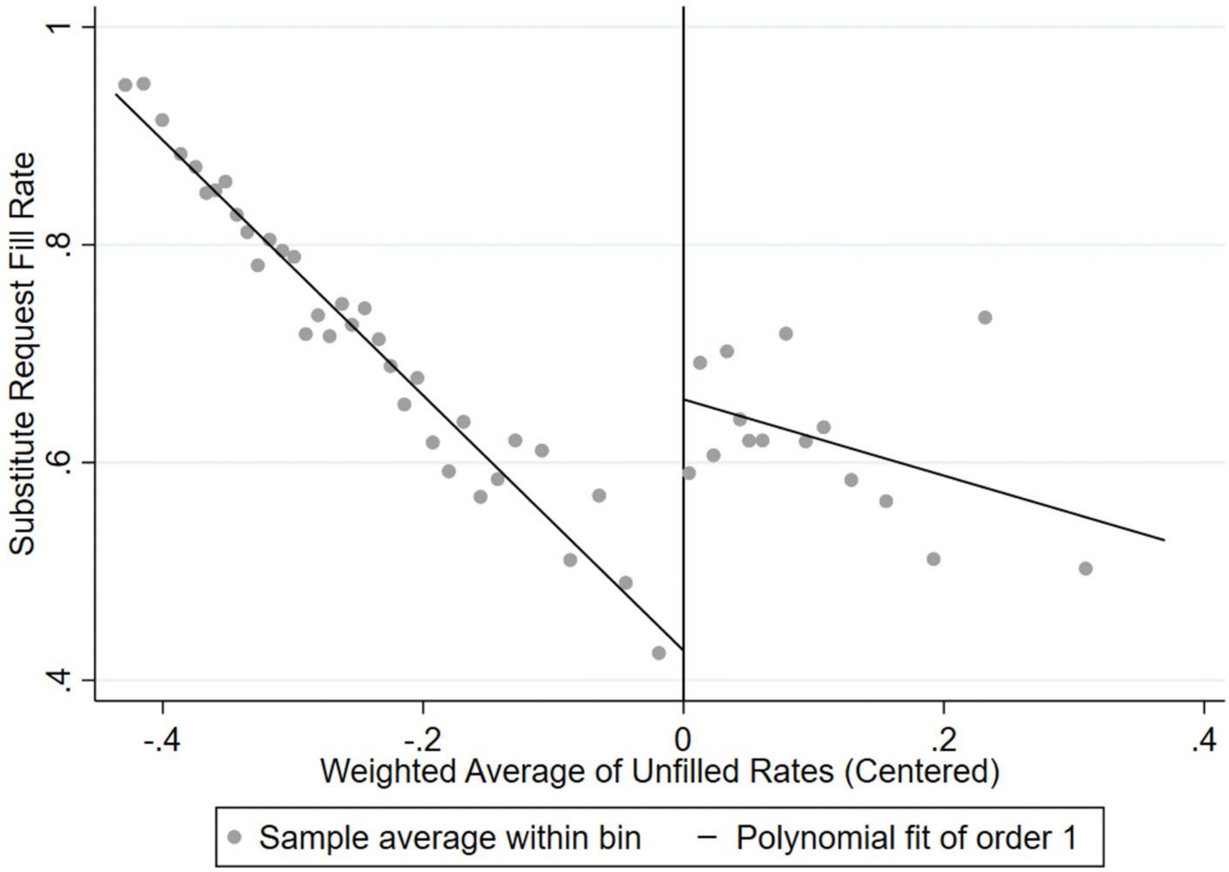

Figure 5: Raw fill rate discontinuity, 2018-19 forcing variable

Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. The dependent variable is the school-level substitute request fill rate for the 2018-19 school year. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

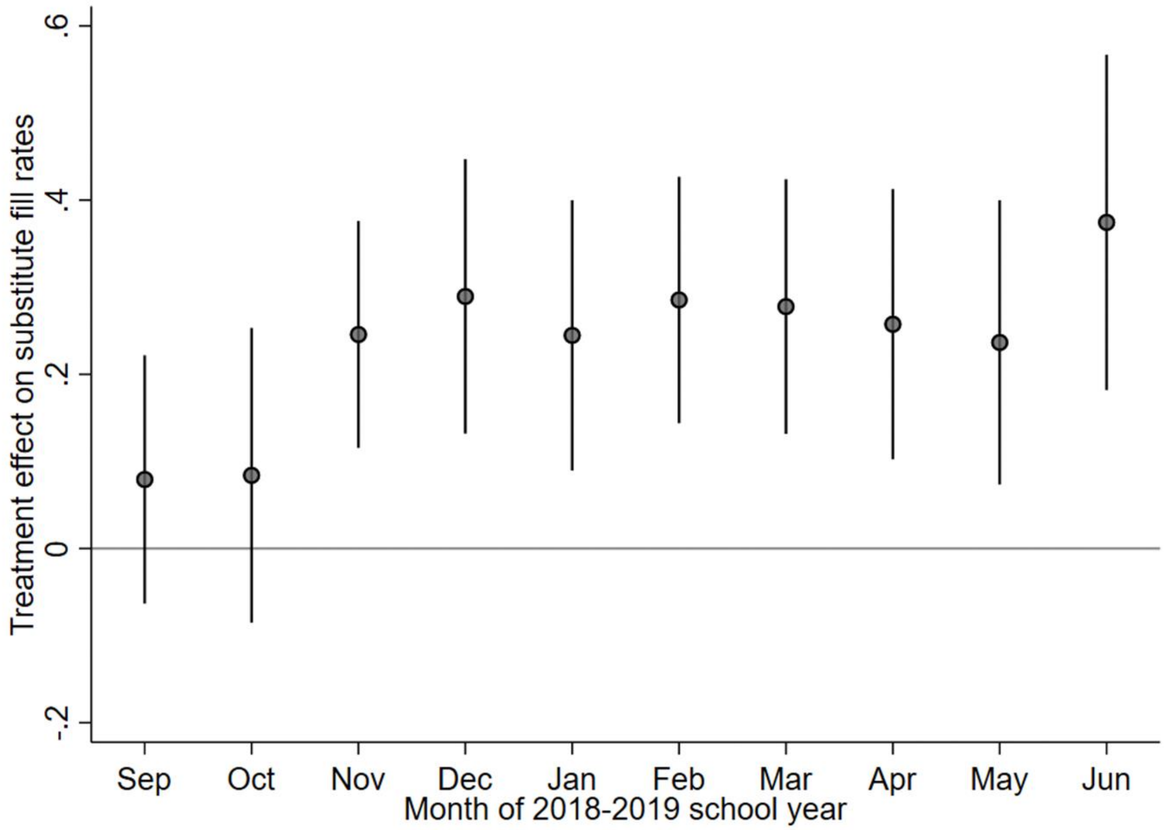

Figure 6: Month-level regression discontinuity estimates, 2018-19

Notes: We plot average treatment effects at the cutoff from our RD model on fill-rates for specific months during the school year, controlling for school-level covariates. Vertical bars demarcate 95% confidence intervals for each estimate. Seven of the 22 school days in May and 5 of the 12 school days in June were high-demand days with additional stipends.

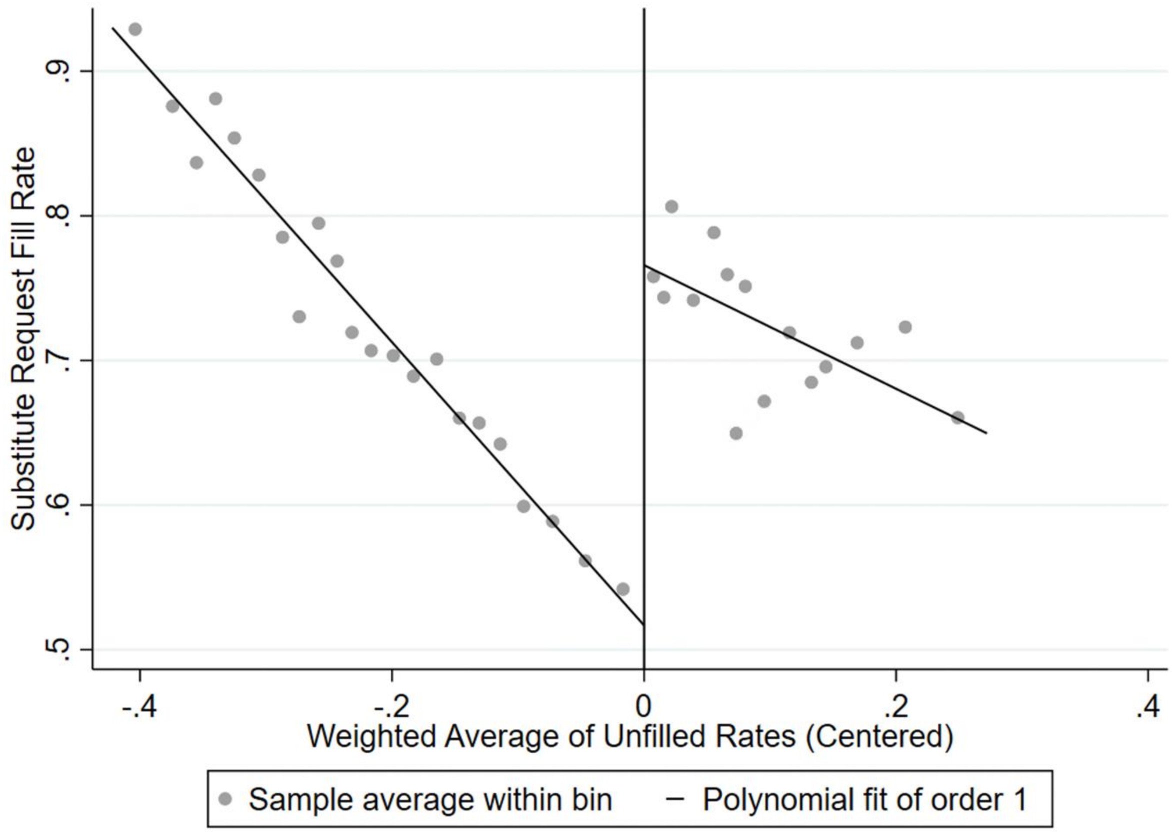

Figure 7: Raw fill rate discontinuity, 2019-20 forcing variable

Notes: The independent variable isolates the 125th school treatment cutoff, centered at zero, with the initial cohort of 75 treated schools omitted. Treated schools are on the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the mimicking variance evenly-spaced method using spacings estimators.

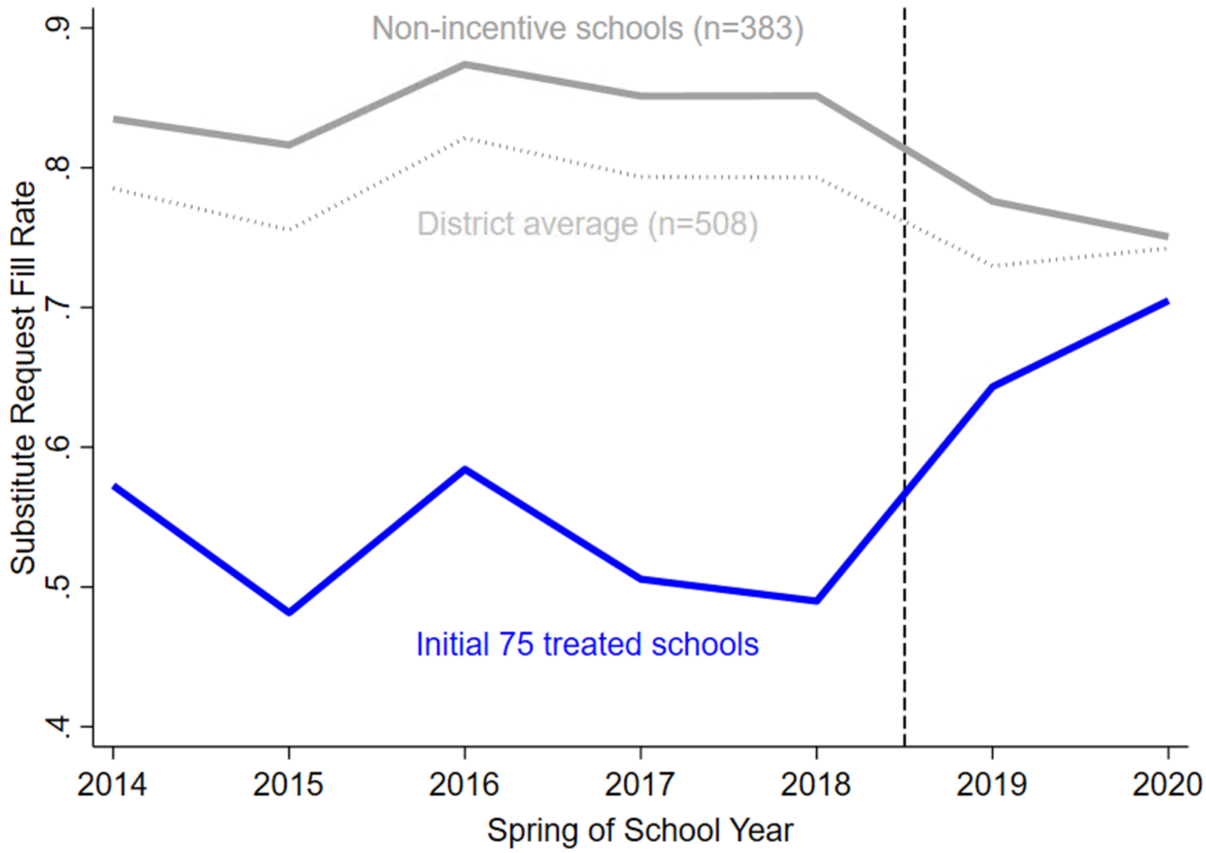

Figure 8: Aggregate substitute coverage over time

Notes: Aggregate fill rates are the percent of all requests among a group of schools that are filled by a substitute. The vertical dashed line marks the introduction of the targeted incentive pay program for substitutes. Only the initial 75 treated schools were treated in 2018-19. We omit the 50 additional schools (added to the treatment group 2019-20) from the non-incentive schools group but include them in the district overall average (dotted line).

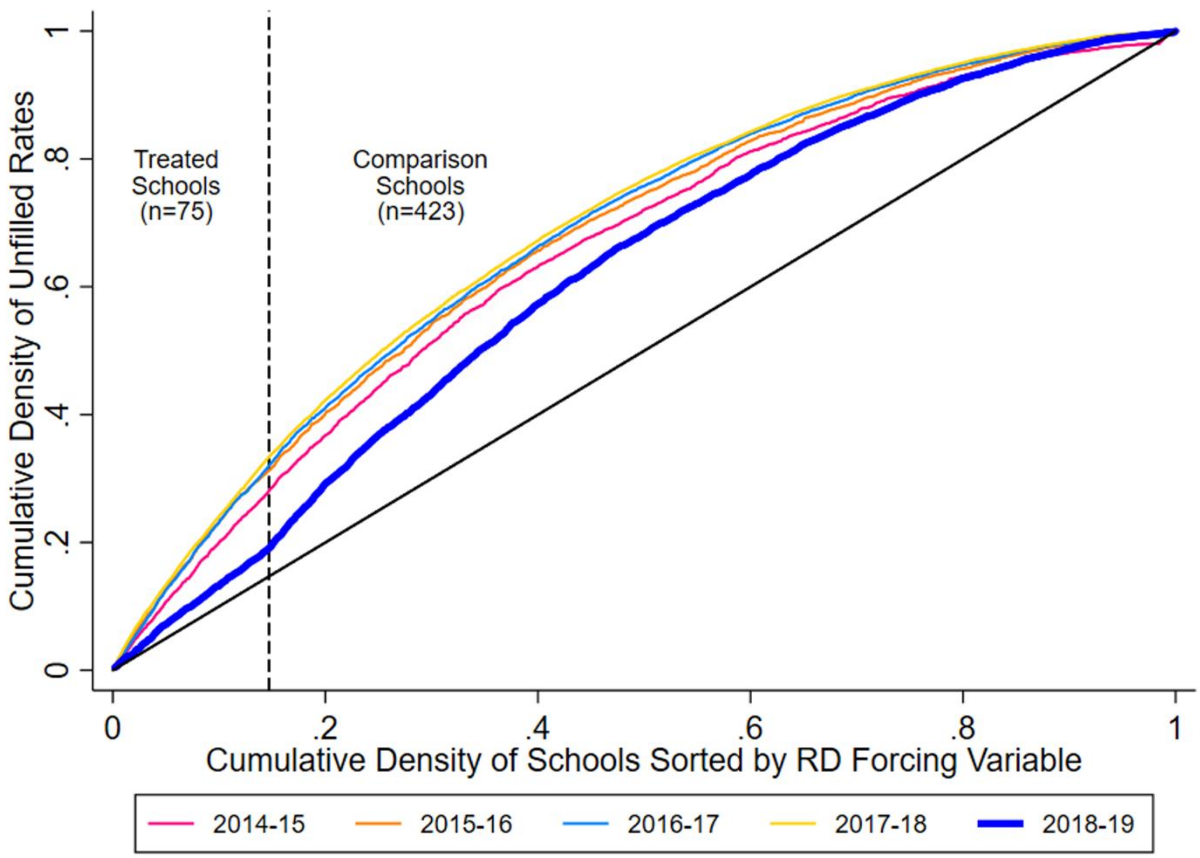

Figure 9: Lorenz curves of unfilled rates across all schools

Notes: The black 45° line represents perfect equity, here defined as each school experiencing the same rate of unfilled requests. The area between a plotted arc and the 45° line represents the inequity in the rates of unfilled requests experienced across the district in that given school year. Smaller areas between the distribution arc and the 45° line represent a more equitable distribution of unfilled rates across schools. The 75 schools to the left of the vertical dashed line received the incentive pay treatment in 2018-19.

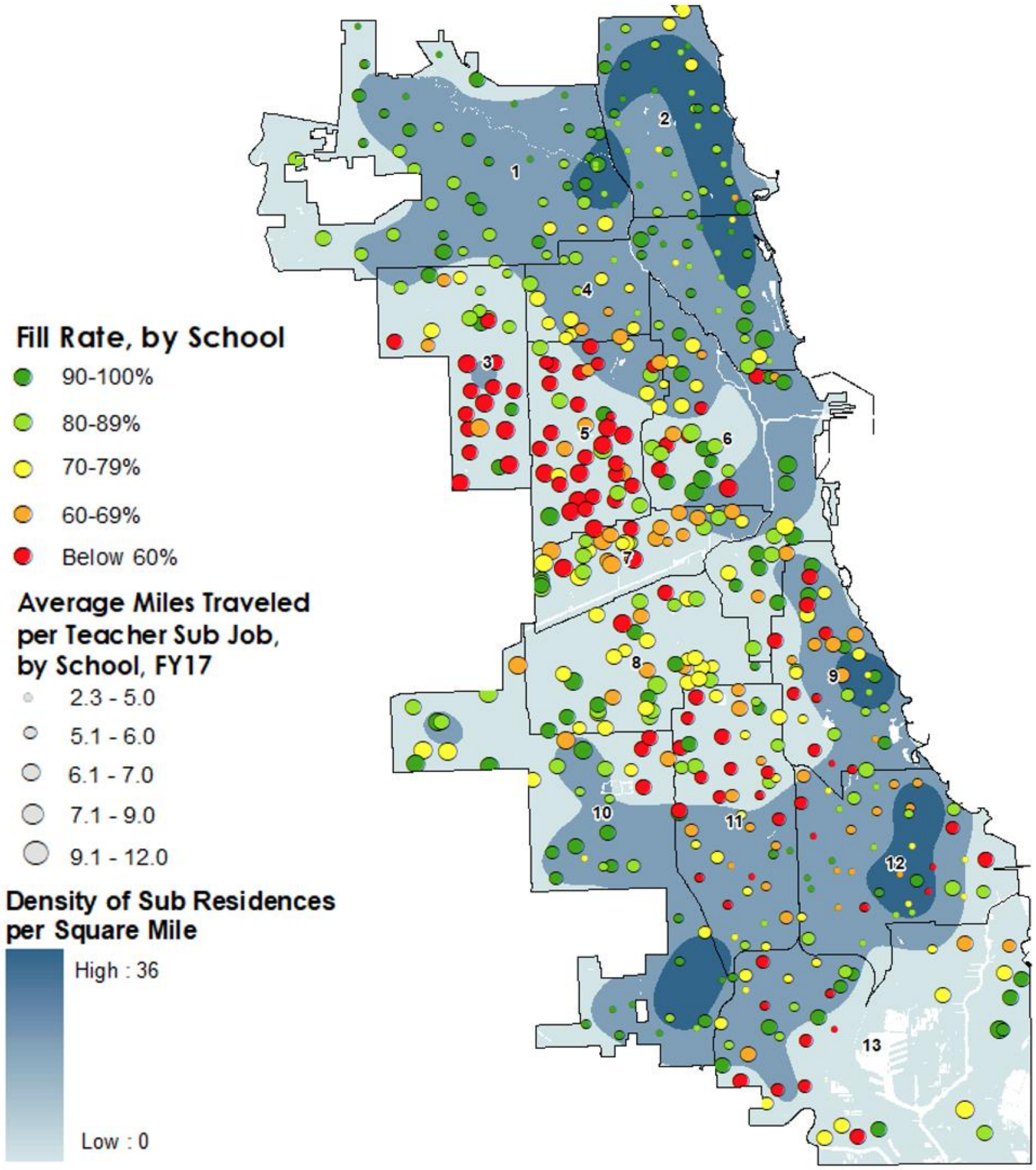

Figure 10: Substitute residential density, school-average commute distances, and substitute fill rates across Chicago Public School, 2016-17

Tables

Table 1: Pre-treatment Characteristics of Incentive and Non-Incentive Schools

	District	2018-19 Treatment		2019-20 Treatment	
		Incentive	Non-incentive	Incentive	Non-incentive
Substitute behavior measures:					
Fill rate %	76.7	47.3	81.9	54.8	83.9
Number of substitutes engaged	64.8	50.4	67.3	55.5	67.8
Average days substitutes work	8.1	4.7	8.7	5.2	9.1
Average observed commute (min)	17.1	18.1	17.0	18.0	16.8
Number of substitutes commuting ≤10min	14.3	9.1	15.2	10.4	15.5
Number of substitutes commuting 10-20min	23.8	17.5	24.9	20.1	25.0
Number of substitutes commuting >20min	25.0	22.3	25.5	23.3	25.6
School characteristics:					
Average teacher absences	11.9	11.4	11.9	11.9	11.8
Teacher Retention %	0.8	0.8	0.8	0.8	0.8
Misconduct incidents per student	0.32	0.27	0.33	0.27	0.34
Enrollment	601	445	627	437	654
Math achievement	-0.10	-0.41	-0.04	-0.37	-0.01
English achievement	-0.09	-0.38	-0.04	-0.34	0.00
Neighborhood violent crimes / 100 residents	0.45	0.82	0.39	0.70	0.37
All neighborhood crimes / 100 residents	9.77	14.60	8.94	13.05	8.70
School Quality Rating (scale 1-5)	3.6	3.1	3.7	3.3	3.7
Student body demographics:					
English language-learner %	16.8	8.6	18.2	12.3	18.2
Special education %	15.5	14.8	15.6	14.7	15.7
Free/Reduced price lunch eligible %	77.3	86.9	75.6	86.6	74.3
Asian %	3.4	0.2	4.0	0.3	4.5
Black %	46.2	78.5	40.6	70.3	38.4
Hawaiian %	0.2	0.1	0.2	0.1	0.2
Hispanic %	39.0	19.8	42.4	27.6	42.8
Multi-racial %	1.1	0.4	1.2	0.5	1.3
Native American %	0.3	0.2	0.3	0.2	0.3
White %	9.5	0.8	11.1	1.0	12.3
Race not reported %	0.2	0.1	0.2	0.1	0.2
School levels represented					
Elementary grades only	33	0	33	3	30
Elementary & middle grades	377	73	304	116	261
Middle grades only	8	0	8	3	5
Middle & high school grades	11	0	11	0	11
High school grades only	79	2	77	3	76
n	508	75	433	125	383

Notes: All values are averages of school-level data for the indicated group in the 2017-18 school year. The "District" column presents the school-level average across all observed traditional public schools.

Table 2: Effects of Targeted Incentives on Substitute Labor Supply 2018-19

	Incentive school mean 2017-18	(1)	(2)
Panel A. Substitute behavior measures			
Substitute request fill rate	0.47	0.22***	0.23***
		(0.05)	(0.05)
Number of unique substitutes at a school	50.40	17.88**	13.02***
		(7.23)	(4.73)
Average substitute's total days worked at a school	4.65	1.36	1.57
		(1.63)	(1.44)
Panel B. Number of unique substitutes in 2018-19 by prior work history			
Prior work in treated schools only	0.73	0.12	0.14
		(0.23)	(0.22)
Prior work in comparison schools only	5.47	2.47	0.47
		(1.62)	(1.00)
Prior work in both treated & comparison schools	27.73	14.93***	13.57***
		(4.24)	(3.27)
New to the substitute roster in 2018-19	13.11	-0.28	-1.30
		(1.87)	(1.38)
Lapsed substitutes	3.36	0.64	0.14
		(0.64)	(0.52)
School covariate vector		No	Yes
n		111	111

Notes: Heteroskedasticity-robust standard errors are reported in parentheses (* p<.10, ** p<.05, *** p<.01). Panel B disaggregates unique substitutes at a school in 2018-19 by where they worked in the prior year. Lapsed substitutes have been on the roster but did not take any substituting jobs in the year prior. We include schooling-level fixed effects in all models. Our school covariate vector includes controls for student body race demographics, free/reduced price lunch eligibility, special education status, English as a second language status, total enrollment, and school-level lagged achievement in math and English. All models use a bandwidth of +/- 0.12 and are weighted by a uniform kernel.

Table 3: Heterogeneous Effects of Targeted Incentives on Substitute Labor Supply 2018-19

	Incentive school mean 2017-18	(1)	(2)
Panel A. Substitute localness			
Number of substitutes commuting <10min	9.05	2.26	1.97
		(1.59)	(1.30)
Number of substitutes commuting 10-20min	17.52	7.49**	7.59***
		(3.02)	(2.42)
Number of substitutes commuting >20min	22.29	8.87***	5.24***
		(2.99)	(1.97)
Panel B. Number of unique substitutes in 2018-19 by demographic groups			
Asian	0.64	-0.07	-0.44*
		(0.42)	(0.25)
Black	30.96	9.29	10.03**
		(6.28)	(4.47)
Hispanic	3.95	3.25*	2.25***
		(1.67)	(0.86)
White	9.81	3.68	-0.21
		(3.33)	(1.66)
Female	37.23	14.35***	11.69***
		(5.50)	(3.70)
Male	13.17	3.53	1.33
		(2.68)	(1.72)
School covariate vector		No	Yes
n		111	111

Notes: Heteroskedasticity-robust standard errors are reported in parentheses (* p<.10, ** p<.05, *** p<.01). We include schooling-level fixed effects in all models. Our school covariate vector includes controls for student body race demographics, free/reduced price lunch eligibility, special education status, English as a second language status, total enrollment, and school-level lagged achievement in math and English. All models use a bandwidth of +/- 0.12 and are weighted by a uniform kernel.

Table 4: Effects of Targeted Incentives on Teacher and Student Outcomes 2018-19

	Incentive school mean 2017-18	(1)	(2)
	Panel A. Teacher-level measures		
Total absences	11.701	2.662*	1.477
		(1.376)	(1.241)
		n = 2,609	
Retained	0.780	0.040	0.022
		(0.038)	(0.035)
		n = 2,609	
Transferred	0.115	-0.035	-0.030
		(0.033)	(0.031)
		n = 2,609	
Left district	0.104	-0.005	0.009
		(0.024)	(0.020)
		n = 2,609	
	Panel B. Student-level measures		
Index math achievement	-0.406	0.032	0.044
		(0.113)	(0.031)
		n = 28,046	
Index ELA achievement	-0.378	0.061	0.046*
		(0.119)	(0.024)
		n = 27,733	
School covariate vector		No	Yes
Individual covariate vector		No	Yes

Notes: Heteroskedasticity-robust standard errors are clustered by school and reported in parentheses (* p<.10, ** p<.05, *** p<.01). Model (2) includes vectors of school covariates as well as individual covariates. For teachers, the individual covariate vector indicates race, gender, and binned tenure indicators for 3-5, 6-10, 11-20, and 21+ years (1-2 years omitted category). Student-level covariates include student race indicators, gender, English as a second language status, free/reduced price lunch eligibility, special education status, and a lagged cubic of both math and English achievement. Student outcomes are modeled for the subsample of students with lagged outcomes to improve precision; this does not materially impact our estimates. Schooling-level fixed effects are included in all models. We weight observations within the bandwidth or +/- 0.12 with a uniform kernel.

Table 5: Effects of Targeted Incentives on Substitute Labor Supply 2019-20

	Incentive school mean 2017-18	(1)	(2)
Panel A. Substitute behavior measures			
Substitute request fill rate	0.66	0.25***	0.21***
		(0.04)	(0.04)
Number of unique substitutes at a school	63.12	34.69***	34.91***
		(8.61)	(6.98)
Average substitute's total days worked at a school	6.09	-1.58	-1.87
		(1.25)	(1.19)
Panel B. Number of unique subs in 2019-20 by prior work history			
Prior work in treated schools only	0.26	1.71***	1.70***
		(0.48)	(0.41)
Prior work in comparison schools only	10.44	4.82*	3.49
		(2.75)	(2.29)
Prior work in both treated & comparison schools	32.14	22.32***	23.73***
		(4.47)	(3.71)
New to the substitute roster	15.56	3.60*	3.71**
		(1.97)	(1.69)
Lapsed substitutes	4.72	2.24**	2.28**
		(1.02)	(0.93)
School covariate vector		No	Yes
n		93	93

Notes: Heteroskedasticity-robust standard errors are reported in parentheses (* p<.10, ** p<.05, *** p<.01). We exclude schools sorted into treatment in 2018-19. Schooling-level fixed effects are included in all models. The school covariate vector includes controls for student body race demographics, representation of free/reduced price lunch eligibility, special education status, English as a second language status, total school enrollment, and lagged school-level math and ELA achievement. Observations within the bandwidth of +/- 0.10 are weighted by a uniform kernel. The pre-treatment mean is reported for the 50 schools added to treatment in 2019-20.

Table 6: Heterogeneous Effects of Targeted Incentives on Substitute Labor Supply 2019-20

	Incentive school mean 2017-18	(1)	(2)
Panel A. Substitute localness			
Number of substitutes commuting <10min	12.54	2.48	1.77
		(2.22)	(1.50)
Number of substitutes commuting 10-20min	24.00	12.96***	13.23***
		(2.54)	(2.31)
Number of substitutes commuting >20min	24.86	6.68**	7.62***
		(2.73)	(1.83)
Panel B. Number of unique substitutes in 2018-19 by demographic groups			
Asian	0.96	0.13	0.01
		(0.32)	(0.24)
Black	36.32	26.58***	27.84***
		(7.95)	(5.90)
Hispanic	6.04	0.05	0.26
		(1.73)	(0.80)
White	14.70	3.80	2.78
		(4.00)	(2.94)
Female	45.04	28.48***	29.01***
		(7.13)	(5.99)
Male	18.08	6.21**	5.90***
		(2.53)	(1.66)
School covariate vector		No	Yes
n		93	93

Notes: Heteroskedasticity-robust standard errors are reported in parentheses (* p<.10, ** p<.05, *** p<.01). We exclude schools sorted into treatment in 2018-19. Schooling-level fixed effects are included in all models. The school covariate vector includes controls for student body race demographics, representation of free/reduced price lunch eligibility, special education status, English as a second language status, total school enrollment, and lagged school-level math and ELA achievement. Observations within the bandwidth of +/- 0.10 are weighted by a uniform kernel. The pre-treatment mean reported is for the 50 schools added to treatment in 2019-20.

Table 7: Effects of Targeted Incentives on Teacher Outcomes 2019-20

	Incentive school mean 2017-18	(1)	(2)
Total absences	12.560	4.315**	3.502**
		(1.782)	(1.510)
Retained at school	0.776	-0.026	-0.053
		(0.048)	(0.043)
Transferred	0.117	0.021	0.038
		(0.039)	(0.037)
Left district	0.107	0.005	0.016
		(0.023)	(0.019)
School covariate vector		No	Yes
Teacher covariate vector		No	Yes
n		2,174	2,174

Notes: Heteroskedasticity-robust standard errors are reported in parentheses (* p<.10, ** p<.05, *** p<.01). Schooling-level fixed effects are included in all models. The school covariate vector includes controls for student body race demographics, representation of free/reduced price lunch eligibility, special education status, English as a second language status, total school enrollment, and lagged school-level math and ELA achievement. The teacher-level covariate vector includes binned tenure indicators (3-5, 6-10, 11-20, 21+ years, with 0-2 years omitted), gender, and race /ethnicity indicators. Observations within the bandwidth of +/- 0.10 are weighted by a uniform kernel. The pre-treatment mean reported is for the 50 schools added to treatment in 2019-20.

Table 8: Sensitivity Analysis

	(1)	(2)
Panel A. Exclude Top 25% of Schools by % of Substitutes Worked at Both Treatment and Comparison Schools Last Year		
Substitute request fill rates	0.24***	0.21***
	(0.07)	(0.07)
	n=81	
Panel B. Exclude Top 50% of Schools by % of Substitutes Worked at Both Treatment and Comparison Schools Last Year		
Substitute request fill rates	0.20***	0.23***
	(0.08)	(0.06)
	n=56	
Panel C. Exclude Top 25% of Schools by Number of Treated Schools Within 3 Miles		
Substitute request fill rates	0.23***	0.24***
	(0.05)	(0.05)
	n=85	
Panel D. Exclude Top 50% of Schools by Number of Treated Schools Within 3 Miles		
Substitute request fill rates	0.20***	0.22***
	(0.06)	(0.05)
	n=67	
School covariate vector	No	Yes

Notes: Heteroskedasticity-robust standard errors are reported in parentheses (* p<.10, ** p<.05, *** p<.01). We include schooling-level fixed effects and our school covariate vector in all models. Observations are weighted by a uniform kernel. Estimation bandwidth is 0.12 for all panels.

Appendix A: Regression Discontinuity Design Validity

We conduct a range of empirical tests to examine the validity of our regression discontinuity design. First, we present histograms of our sorting variable for the 2018-19 and 2019-20 RD models in Figures A.1 and A.2, respectively. These figures illustrate no evidence of manipulation around either treatment cutoff. This makes sense given schools were unaware of the program or the decision to use a weighted historical fill-rate variable as the assignment mechanism until after the incentive schools were announced. Using historical data in the sorting process ensured that schools could not manipulate their position on either side of the cutoff. We perform formal nonparametric density tests to ensure school assignment above or below the cutoff was not manipulated following Cattaneo et al. (2020). We pass these formal tests (p=0.18 in 2018-19 and p=0.93 in 2019-20) and display the results in Figures A.3 and A.4.

We also test for discontinuities in school characteristics across each year's treatment threshold. These tests are intended to validate the integrity of the foundational assumption of pseudo-random selection into treatment and comparison groups close to the treatment cutoff. We show the distribution of these characteristics across the 2018-19 and 2019-20 sorting variables in Figures A.5 and A.6. In Table A.1 we present the point estimates of discontinuities at the threshold for several covariates by using these variables as outcomes in our 2018-19 and 2019-20 RD models. We find small and statistically insignificant effects across the ten covariates we test for both our 2018-19 and 2019-20 RD models with the exception of a 4 percentage-point difference in concentration of special education students across the 2019-20 125th school cutoff. We interpret these overall findings as evidence for the validity of the pseudo-random assignment of schools across the treatment cutoff.

To ensure the robustness of our results to model specification, we examine the sensitivity of our main results to our selection of bandwidth and functional form in Table A.2. We use a single bandwidth across outcomes in our main tables to allow for a stable sample. We selected the maximum of the mean-square-error minimizing bandwidth from Calonico et al., (2017; 2020) calculated for each outcome, which fell in the range of 6 to 12 percentage points in our 2018-19 model, rounded to the nearest hundredth. In Tables A.2 and A.3, below, we present RD estimates for our key outcomes across both years with bandwidths of +/- 6, 8, 10, and 12 percentage points across linear, quadratic, and cubic functional forms. We also graphically illustrate the stability of our estimates for 2018-19 across bandwidths as in Lee & Lemieux (2010) for fill rates in Figures A.7 and A.8. Similar to Fan & Gijbels (1996) and Ludwig & Miller (2007), we find that the choice of kernel does not materially impact our results, as shown in Tables A.4, A.5, A.6, and A.7 where we report estimate from models using a triangular kernel weight.

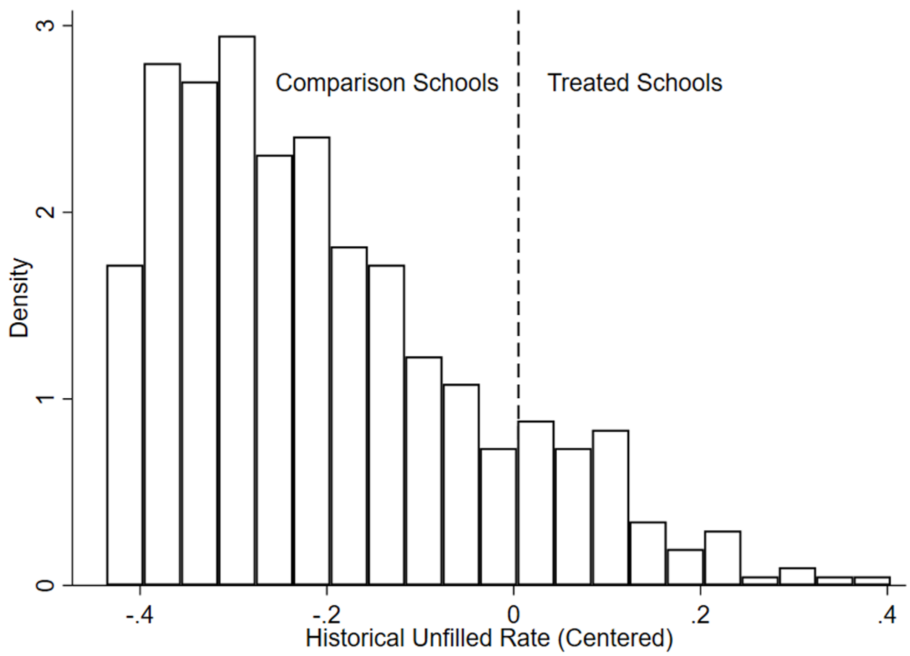

Figure A.1 Sorting variable density 2018-19

Notes: Histogram of schools along the 2018-19 sorting variable; bin width is 4 percentage points.

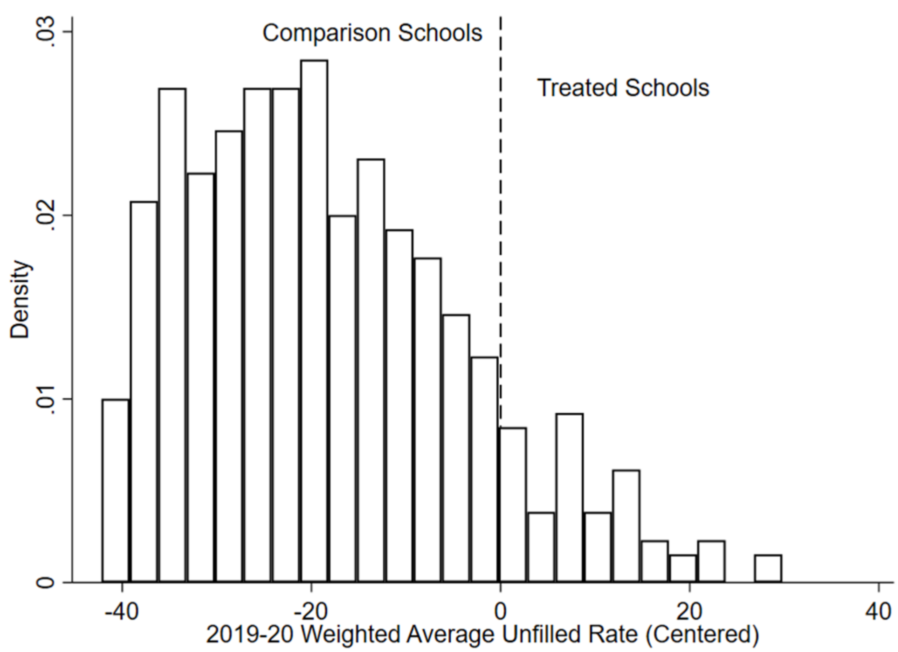

Figure A.2 Sorting variable density 2019-20

Notes: Histogram of schools along the 2019-20 sorting variable; bin width is 3 percentage points. Schools sorted into treatment in 2017-18 are omitted.

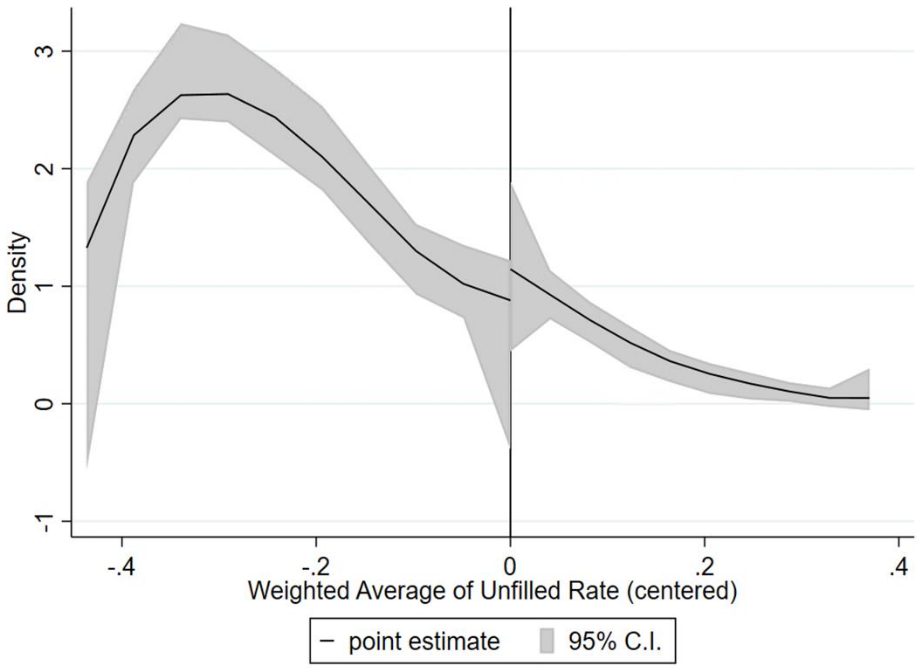

Figure A.3 Sorting variable density test 2018-19
Notes: Visual results from the nonparametric density test (Cattaneo, Jansson, and Ma 2018) for the sorting variable used to determine treatment in the 2018-19 school year.

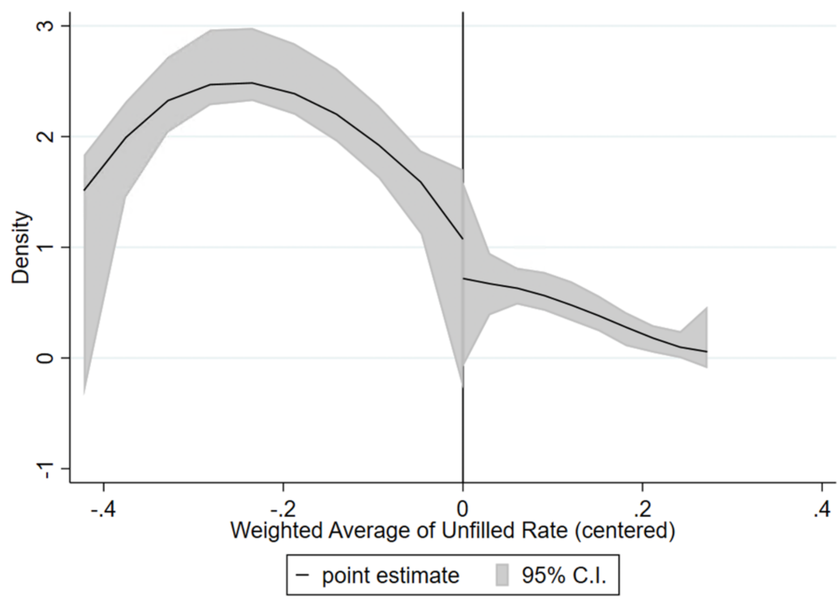

Figure A.4 Sorting variable density test 2019-20
Notes: Visual results from the nonparametric density test (Cattaneo, Jansson, and Ma 2018) for the sorting variable used to determine treatment in the 2019-20 school year. We exclude schools treated in 2018-19.

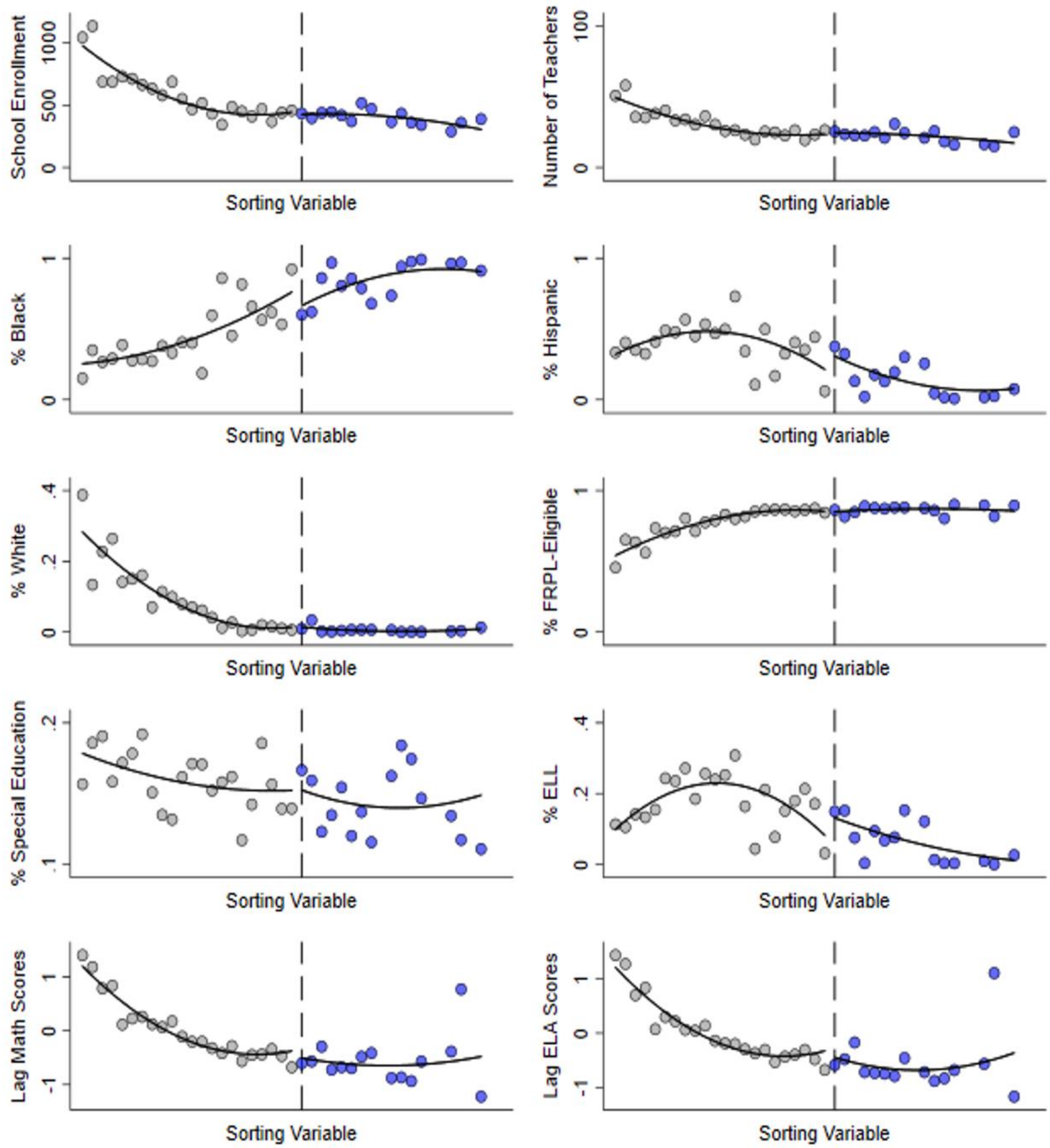

Figure A.5 Covariate balance 2018-19 forcing variable

Notes: Each covariate plot is a scatter of bin averages across the district; bin width is 2 percentage points. Treated schools are on the right of each vertical dashed line. Each covariate is a school-level measure for the 2018-19 school year.

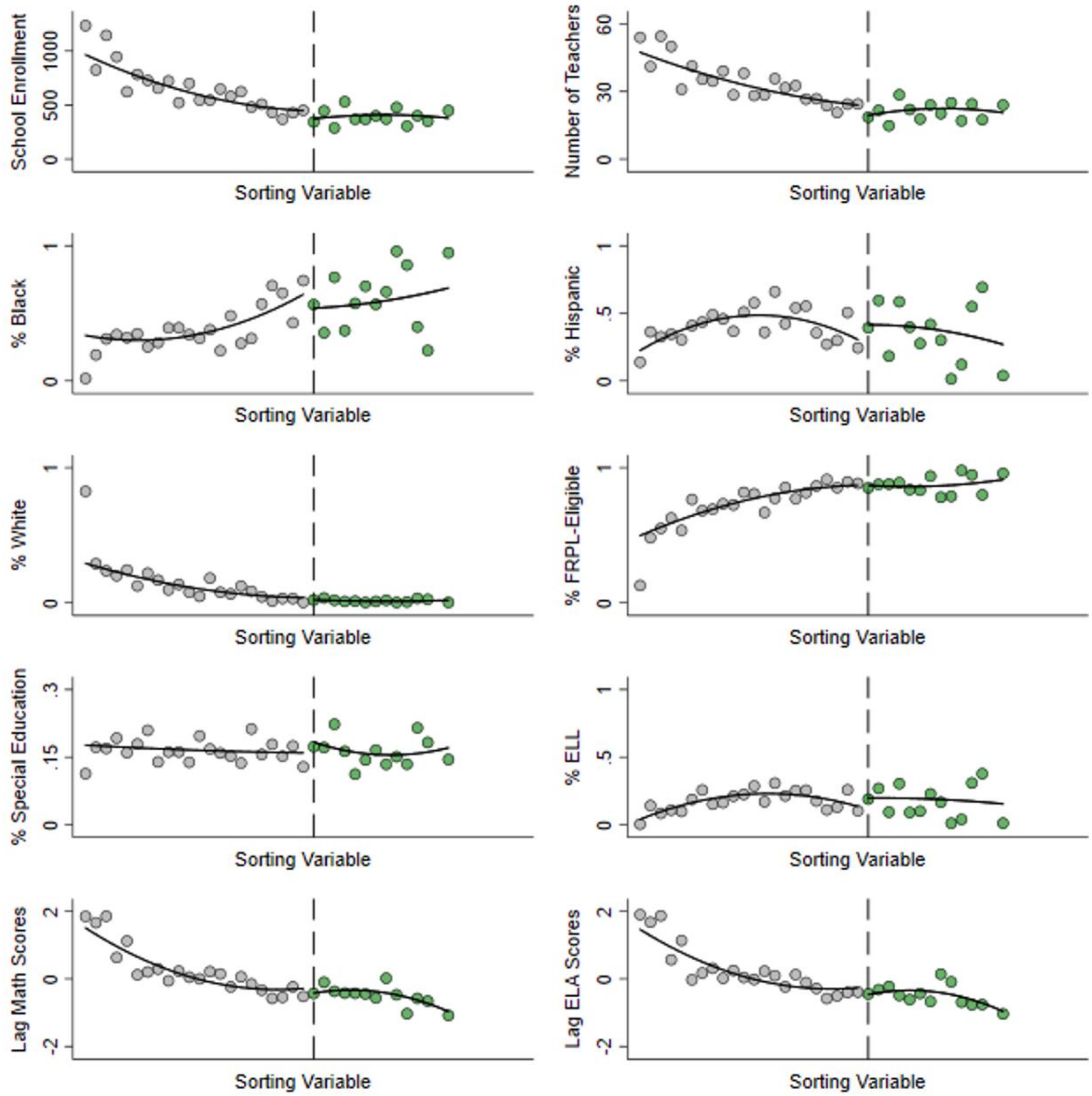

Figure A.6 Covariate Balance 2019-20 forcing variable

Notes: Each covariate plot is a scatter of bin averages across the district; bin width is 2 percentage points. Treated schools are on the right of each vertical dashed line. Each covariate is a school-level measure for the 2019-20 school year. The initial 75 treated schools are omitted.

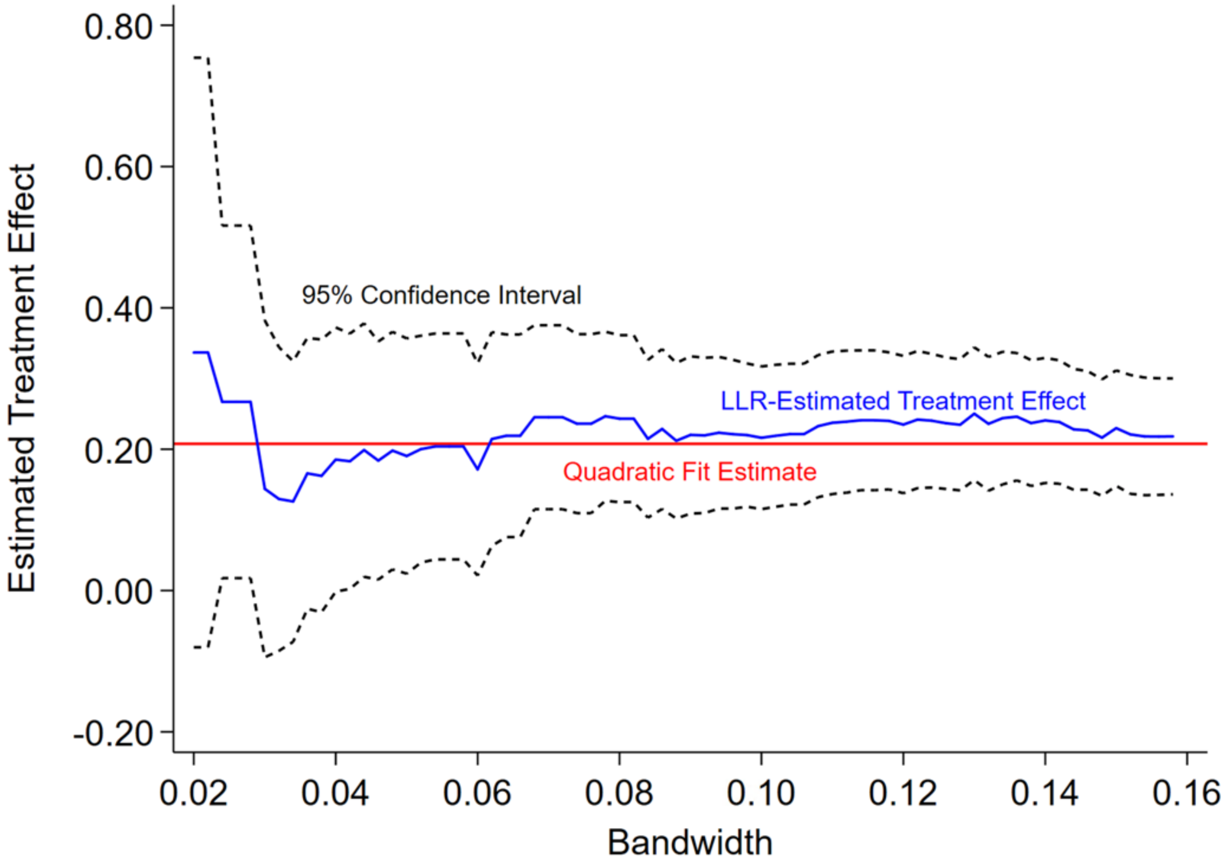

Figure A.7 Stability of fill rate results across bandwidths, 2018-19

Notes: Quadratic fit estimate uses an optimized bandwidth calculated according to Calonico et al., (2017). Local linear regression (LLR) estimates on fill rates are for the bandwidth indicated on the x axis. 95% confidence intervals are for corresponding LLR-estimated effect.

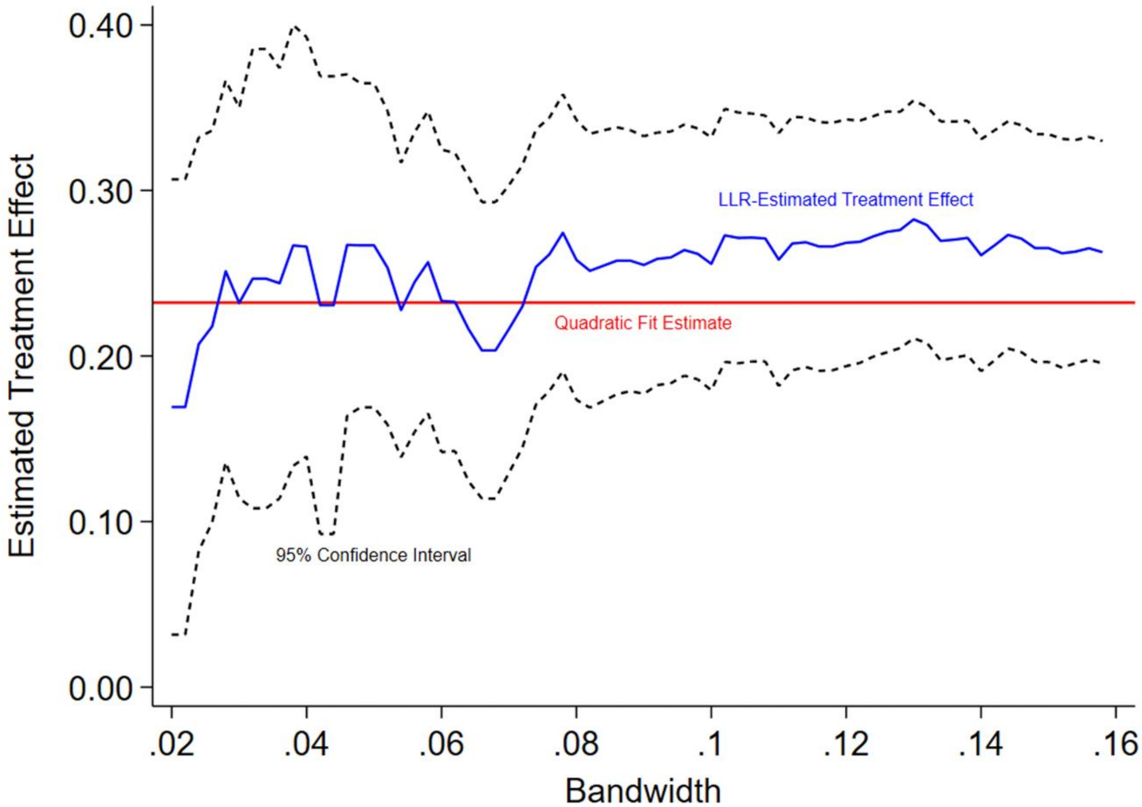

Figure A.8 Stability of fill rate results across bandwidths, 2019-20

Notes: Quadratic fit estimate uses an optimized bandwidth calculated according to Calonico et al., (2017). Local linear regression (LLR) estimates on fill rates are for the bandwidth indicated on the x axis. 95% confidence intervals are for corresponding LLR-estimated effect. The 75 schools in the 2018-19 treated cohort are omitted from all estimates.

Table A.1: Covariate Balance across Treatment Thresholds for Main Specifications

	2018-19 75th School Cutoff	2019-20 125th School Cutoff
Outcome	(1)	(2)
Enrollment	26.41	-71.70
	(62.40)	(75.52)
Number of teachers	2.00	-5.86
	(3.89)	(4.14)
Black %	-0.00	-0.00
	(0.00)	(0.01)
Hispanic %	-0.08	-0.00
	(0.14)	(0.18)
White %	0.08	-0.02
	(0.13)	(0.17)
FRPL eligible %	0.00	0.02
	(0.01)	(0.01)
Special education %	-0.01	-0.04
	(0.02)	(0.04)
ELL %	0.00	0.04
	(0.02)	(0.02)
Lagged Math	0.01	-0.02
	(0.07)	(0.08)
Lagged ELA	-0.02	0.03
	(0.22)	(0.28)
Bandwidth	0.12	0.10
n	111	93

Notes: Heteroskedasticity-robust standard errors are reported in parentheses (* p<.10, ** p<.05, *** p<.01). All models include schooling-level fixed effects and weight observations with a uniform kernel.

Table A.2: Stability of Results to Bandwidth and Functional Form Choice 2018-19

Bandwidth		0.06	0.08	0.10	0.12
		Panel A. Substitute behavior measures			
Substitute request fill rate	Linear	0.223***	0.252***	0.213***	0.226***
	Quadratic	0.310***	0.241**	0.228***	0.246***
Number of unique substitutes at a school	Linear	21.990***	17.677**	12.190**	13.020***
	Quadratic	29.558***	30.084***	21.618**	17.075**
Average substitutes' total days worked at a school	Linear	0.358	1.088	1.523	1.572
	Quadratic	-0.182	-1.289	1.439	3.015
		Panel B. Teacher outcomes			
Total absences	Linear	2.033	0.454	1.233	1.477
	Quadratic	5.133**	3.478*	-0.005	0.100
School retention	Linear	0.032	0.030	0.029	0.022
	Quadratic	0.033	0.044	0.044	0.047
Transfers	Linear	-0.063	-0.041	-0.028	-0.030
	Quadratic	-0.105*	-0.105**	-0.077	-0.066
Exits	Linear	0.031	0.012	-0.001	0.009
	Quadratic	0.073**	0.060**	0.033	0.019
		Panel C. Student outcomes			
Index math	Linear	0.089***	0.093**	0.032	0.044
	Quadratic	0.004	0.027	0.067	0.036
Index ELA	Linear	0.109***	0.113***	0.072***	0.046*
	Quadratic	0.054*	0.055*	0.053*	0.082***

Notes: Heteroskedasticity-robust standard errors are reported in parentheses and clustered by school for teacher- and student-level models (* p<.10, ** p<.05, *** p<.01). We include schooling-level fixed effects and our school covariate vector in all models. Teacher- and student-level models also include individual-level control vectors. Observations within the bandwidth indicated are weighted by a uniform kernel.

Table A.3: Stability of Results to Bandwidth and Functional Form Choice 2019-20

Bandwidth		0.06	0.08	0.10	0.12
		Panel A. Substitute behavior measures			
Substitute request fill rate	Linear	0.157***	0.155***	0.213***	0.237***
	Quadratic	0.171***	0.130**	0.149***	0.208***
Number of unique substitutes at a school	Linear	37.214***	33.814***	34.908***	32.783***
	Quadratic	24.554**	36.051***	37.754***	38.401***
Average substitutes' total days worked at a school	Linear	-1.764	-3.043**	-1.875	-2.010*
	Quadratic	1.432	-0.369	-0.472	-0.871
		Panel B. Teacher outcomes			
Total absences	Linear	4.986***	3.000**	3.502**	2.571*
	Quadratic	6.989***	5.354**	4.208*	4.590**
School retention	Linear	-0.055	-0.052	-0.053	-0.053
	Quadratic	-0.174**	-0.122**	-0.145**	-0.050
Transfers	Linear	0.062	0.033	0.038	0.037
	Quadratic	0.095	0.060	0.079	0.033
Exits	Linear	-0.008	0.018	0.016	0.016
	Quadratic	0.079***	0.062**	0.066**	0.017

Notes: Heteroskedasticity-robust standard errors are reported in parentheses and clustered by school for teacher- and student-level models (* p<.10, ** p<.05, *** p<.01). We include schooling-level fixed effects and our school covariate vector in all models. Teacher- and student-level models also include individual-level control vectors. Observations within the bandwidth indicated are weighted by a uniform kernel.

Table A.4: Effects of Targeted Incentives on Substitute Labor Supply 2018-19
Triangular Kernel

	Incentive school mean 2017-18	(1)	(2)
Panel A. Substitute behavior measures			
Substitute request fill rate	0.47	0.21***	0.23***
		(0.05)	(0.05)
Number of unique substitutes at a school	50.40	21.88***	15.19***
		(8.05)	(5.09)
Average substitutes' total days worked at a school	4.65	0.81	1.62
		(1.62)	(1.35)
Panel B. Number of unique substitutes in 2018-19 by prior work history			
Prior work in treated schools only	0.73	0.01	0.11
		(0.23)	(0.22)
Prior work in comparison schools only	5.47	4.14**	0.93
		(1.77)	(0.98)
Prior work in both treated & comparison schools	27.73	16.68***	15.44***
		(4.71)	(3.48)
New to the substitute roster in 2018-19	13.11	0.50	-1.23
		(2.04)	(1.49)
Lapsed substitutes	3.36	0.57	-0.07
		(0.73)	(0.54)
School covariate vector		No	Yes
n		111	111

Notes: Heteroskedasticity-robust standard errors are reported in parentheses (* p<.10, ** p<.05, *** p<.01). Panel B disaggregates unique substitutes at a school in 2018-19 by where they worked in the prior year. Lapsed substitutes have been on the roster but did not take any substituting jobs in the year prior. We include schooling-level fixed effects in all models. Our school covariate vector includes controls for student body race demographics, free/reduced price lunch eligibility, special education status, English as a second language status, total enrollment, and school-level lagged achievement in math and English. All models use a bandwidth of +/- 0.12 and are weighted by a triangular kernel.

Table A.5: Heterogeneous Effects of Targeted Incentives on Substitute Labor Supply 2018-19 Triangular Kernel

	Incentive school mean 2017-18	(1)	(2)
Panel A. Substitute localness			
Number of substitutes commuting <10min	9.05	2.10	1.54
		(1.76)	(1.39)
Number of substitutes commuting 10-20min	17.52	9.44***	10.34***
		(3.30)	(2.49)
Number of substitutes commuting >20min	22.29	10.33***	5.04***
		(3.13)	(1.81)
Panel B. Number of unique substitutes in 2018-19 by demographic groups			
Asian	0.64	0.35	-0.38
		(0.48)	(0.28)
Black	30.96	5.61	11.91**
		(7.11)	(4.66)
Hispanic	3.95	5.22***	2.16**
		(1.78)	(0.90)
White	9.81	8.38**	-0.09
		(3.59)	(1.57)
Female	37.23	15.67**	13.71***
		(6.17)	(3.93)
Male	13.17	6.21**	1.48
		(2.95)	(1.79)
School covariate vector		No	Yes
n		111	111

Notes: Heteroskedasticity-robust standard errors are reported in parentheses (* p<.10, ** p<.05, *** p<.01). We include schooling-level fixed effects in all models. Our school covariate vector includes controls for student body race demographics, free/reduced price lunch eligibility, special education status, English as a second language status, total enrollment, and school-level lagged achievement in math and English. All models use a bandwidth of +/- 0.12 and are weighted by a triangular kernel.

Table A.6: Effects of Targeted Incentives on Teacher and Student Outcomes 2018-19 Triangular Kernel

	Incentive school mean 2017-18	(1)	(2)
Panel A. Teacher-level measures			
Total absences	11.701	2.744*	1.188
		(1.546)	(1.263)
		n = 2,609	
Retained	0.780	0.055	0.026
		(0.036)	(0.034)
		n = 2,609	
Transferred	0.115	0.055	0.026
		(0.036)	(0.034)
		n = 2,609	
Left district	0.104	-0.001	0.014
		(0.025)	(0.020)
		n = 2,609	
Panel B. Student-level measures			
Index math achievement	-0.406	0.142	0.046
		(0.120)	(0.029)
		n = 28,046	
Index ELA achievement	-0.378	0.152	0.074***
		(0.134)	(0.021)
		n = 27,733	
School covariate vector		No	Yes
Individual covariate vector		No	Yes

Notes: Heteroskedasticity-robust standard errors are clustered by school and reported in parentheses (* p<.10, ** p<.05, *** p<.01). Model (2) includes vectors of school covariates as well as individual covariates. For teachers, the individual covariate vector indicates race, gender, and binned tenure indicators (3-5, 6-10, 11-20, 21+ years, 0-2 years omitted). Student-level covariates include student race, gender, English as a second language status, free/reduced price lunch eligibility, special education status, and a lagged cubic of student-level achievement in both math and English. Student outcomes are modeled for the subsample of students with lagged outcomes to improve precision; this does not materially impact our estimates. Schooling-level fixed effects are included in all models. Observations within the bandwidth or +/- 0.12 are weighted by a triangular kernel.

Table A.7: Effects of Targeted Incentives on Substitute Labor Supply 2019-20 Triangular Kernel

	Incentive school mean 2017-18	(1)	(2)
Panel A. Substitute behavior measures			
Substitute request fill rate	0.66	0.24***	0.17***
		(0.04)	(0.03)
Number of unique substitutes at a school	63.12	34.20***	33.95***
		(9.05)	(6.89)
Average substitutes' total days worked at a school	6.08	-1.20	-2.25**
		(1.22)	(1.03)
Panel B. Number of unique substitutes in 2019-20 by prior work history			
Prior work in treated schools only	0.26	1.80***	1.66***
		(0.48)	(0.34)
Prior work in comparison schools only	10.44	5.04*	3.97**
		(2.82)	(1.98)
Prior work in both treated & comparison schools	32.14	21.86***	22.78***
		(4.72)	(3.72)
New to the substitute roster in 2019-20	15.56	3.24*	3.01*
		(1.92)	(1.62)
Lapsed substitutes	4.72	2.27**	2.52***
		(0.99)	(0.79)
School covariate vector		No	Yes
n		93	93

Notes: Heteroskedasticity-robust standard errors are reported in parentheses (* p<.10, ** p<.05, *** p<.01). Panel B disaggregates unique substitutes at a school in 2019-20 by where they worked in the prior year. Lapsed substitutes have been on the roster but did not take any substituting jobs in the year prior. We include schooling-level fixed effects in all models. Our school covariate vector includes controls for student body race demographics, free/reduced price lunch eligibility, special education status, English as a second language status, total enrollment, and school-level lagged achievement in math and English. All models use a bandwidth of +/- 0.10 and are weighted by a triangular kernel.

Appendix B: Supplemental Figures and Tables

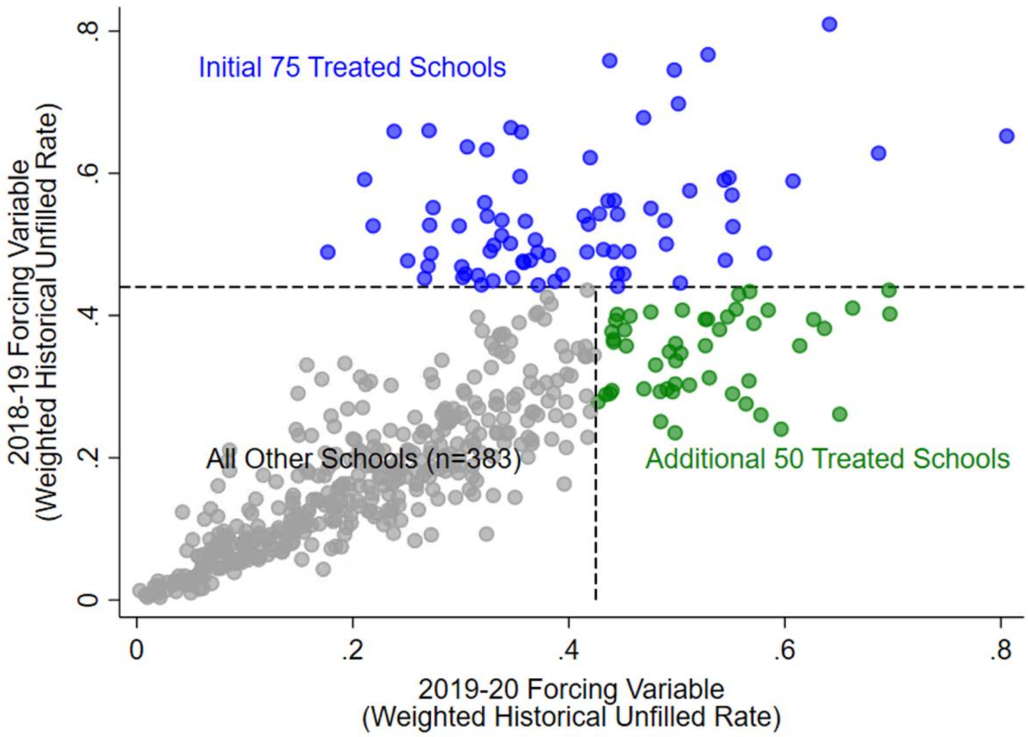

Figure B.1 District distribution across both treatment sorting variables

Notes: Demarcated regions indicate treatment status as determined by each year's treatment cutoff. Both the initial 75 treated schools and additional 50 treated schools received treatment in 2019-20.

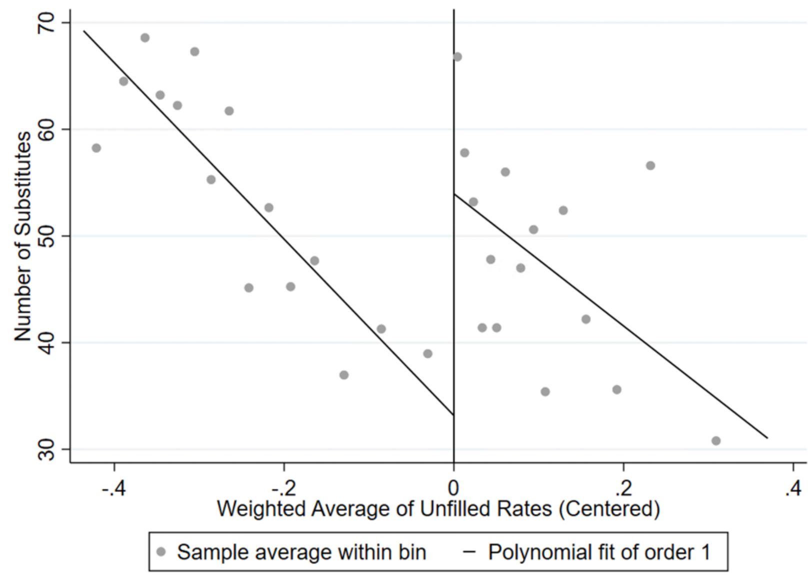

Figure B.2 Raw discontinuity in unique substitutes at each school, 2018-19
Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

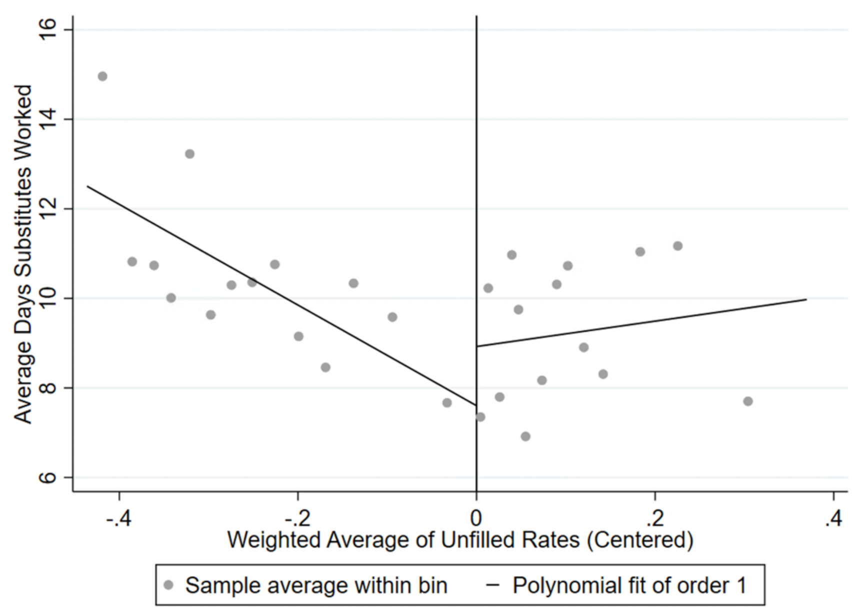

Figure B.3 Raw discontinuity in the average days substitutes worked at each school, 2018-19
Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

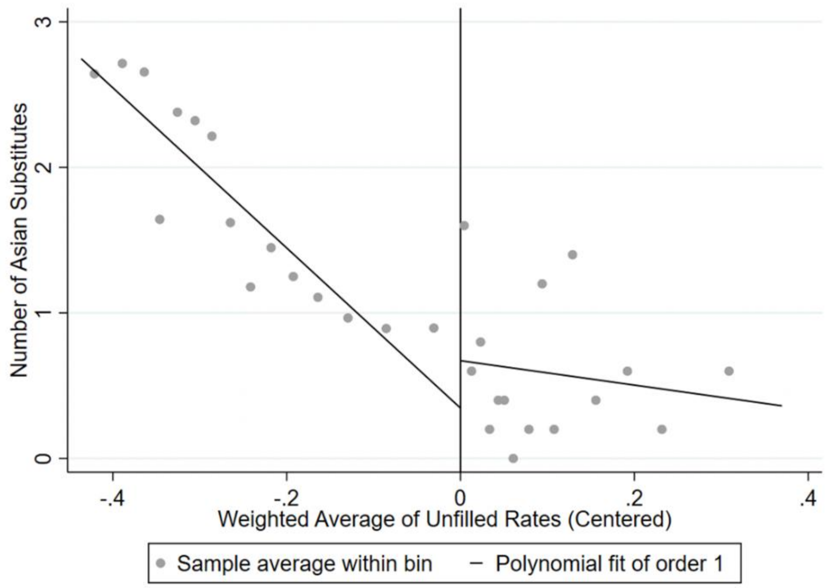

Figure B.4 Raw discontinuity in unique Asian substitutes at each school, 2018-19
Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

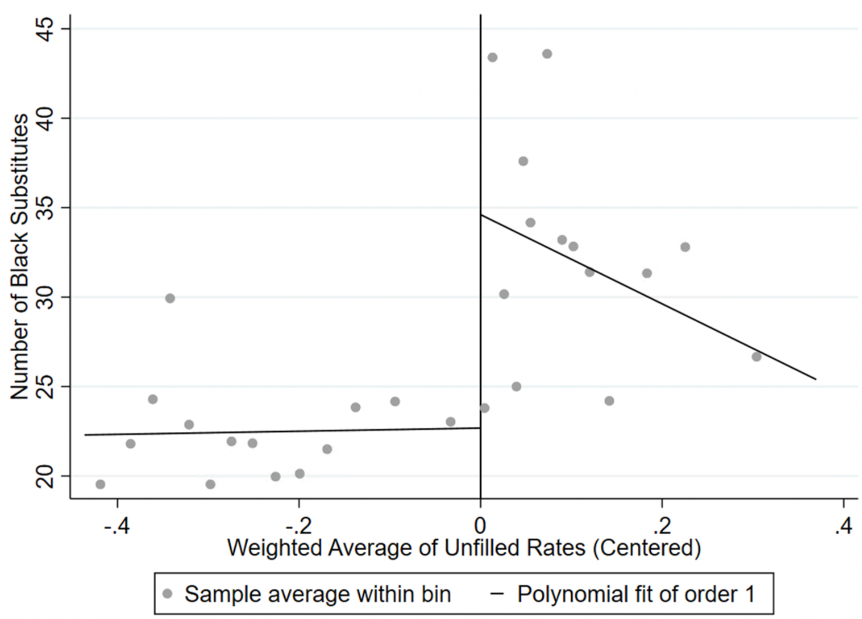

Figure B.5 Raw discontinuity in unique Black substitutes at each school, 2018-19
Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

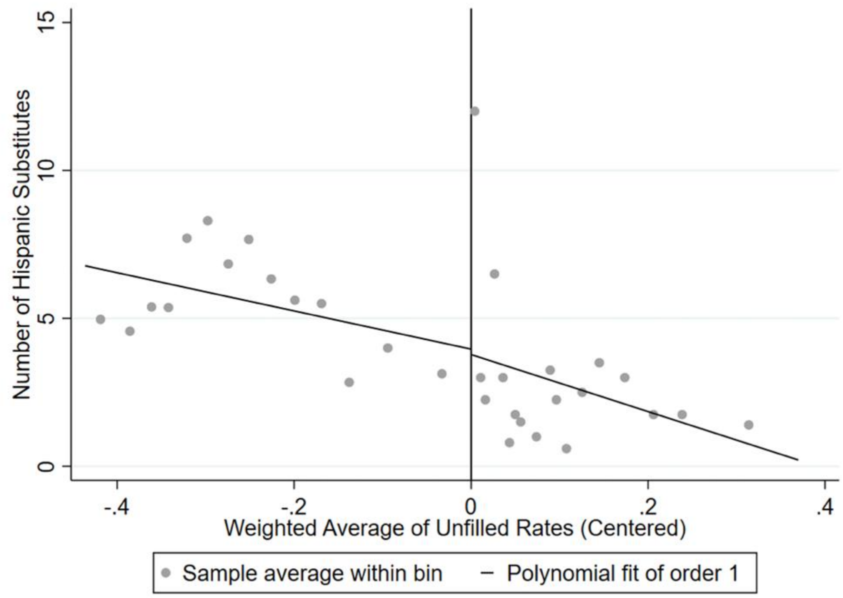

Figure B.6 Raw discontinuity in unique Hispanic substitutes at each school, 2018-19
Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

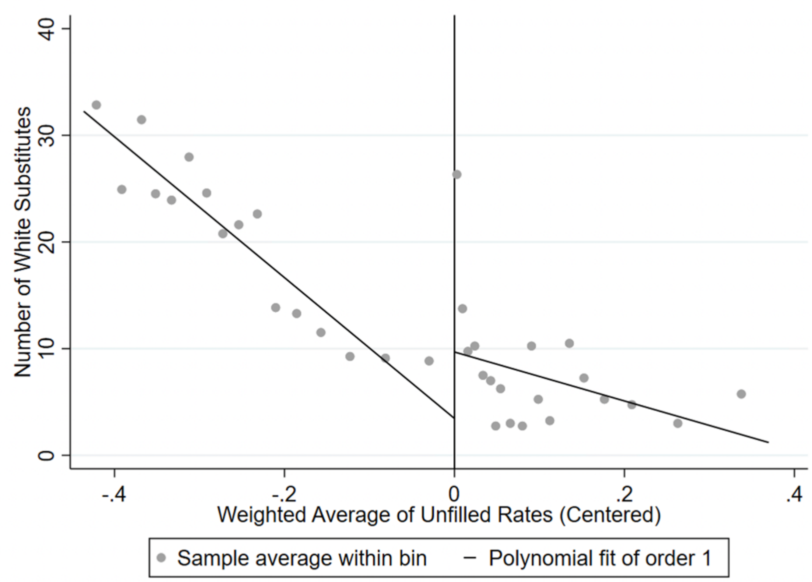

Figure B.7 Raw discontinuity in unique white substitutes at each school, 2018-19
Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

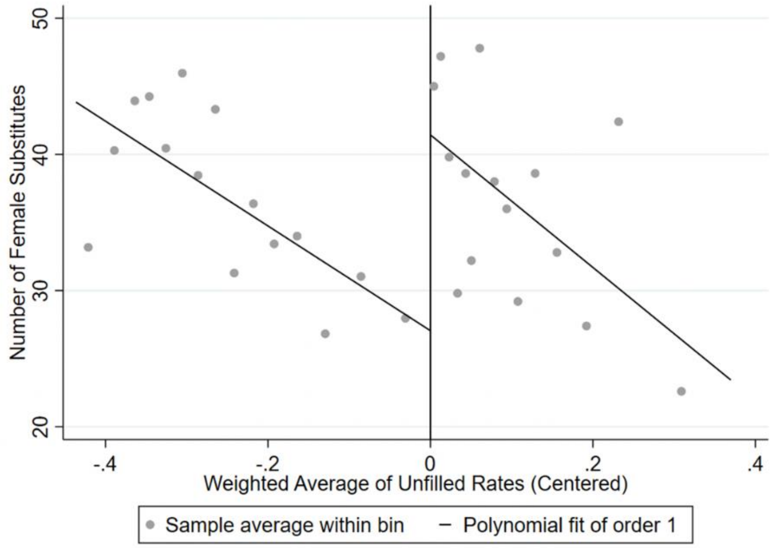

Figure B.8 Raw discontinuity in unique female substitutes at each school, 2018-19
Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

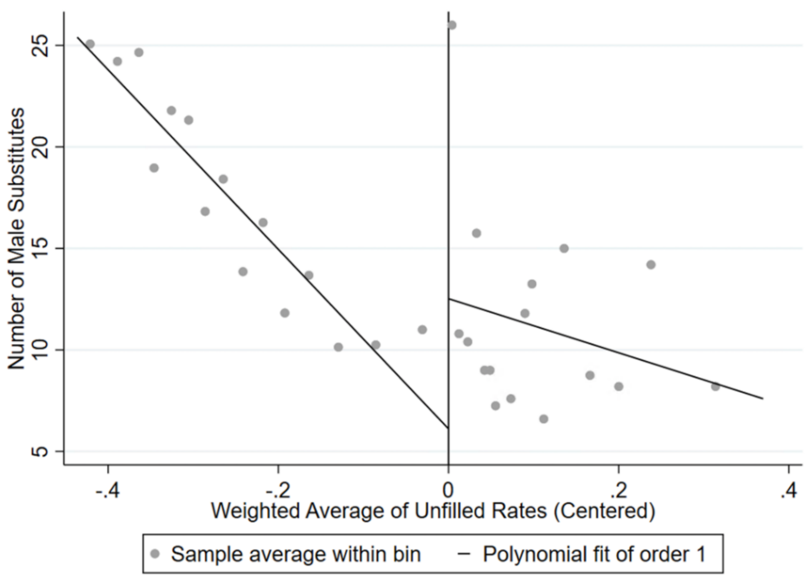

Figure B.9 Raw discontinuity in unique male substitutes at each school, 2018-19
Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

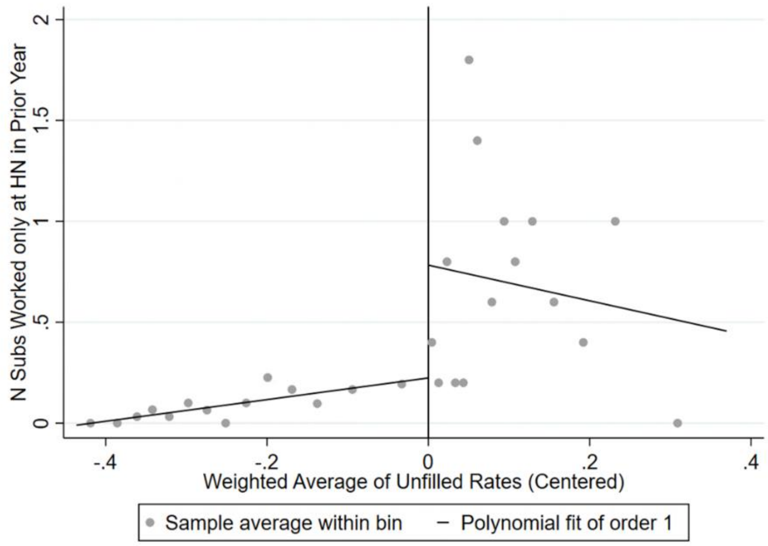

Figure B.10 Raw discontinuity in the number of unique substitutes in 2018-19 who worked only at incentive schools in the prior year

Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

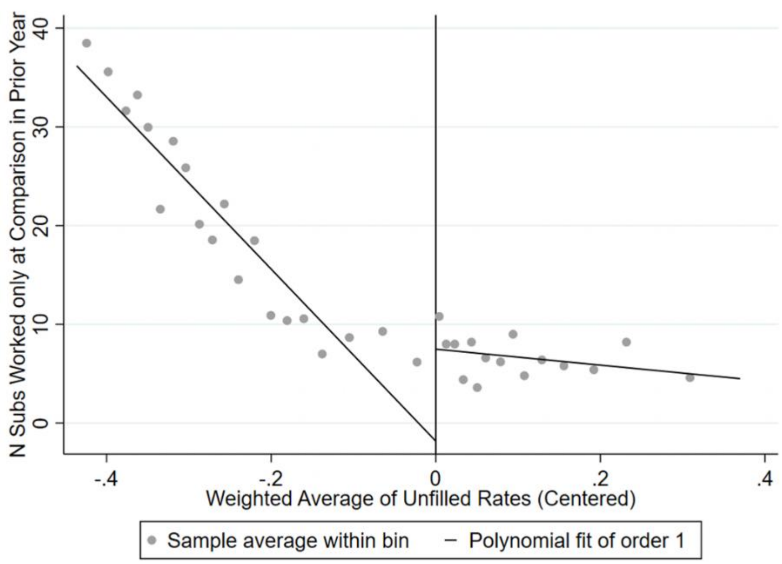

Figure B.11 Raw discontinuity in the number of unique substitutes in 2018-19 who worked only at non-incentive schools in the prior year

Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

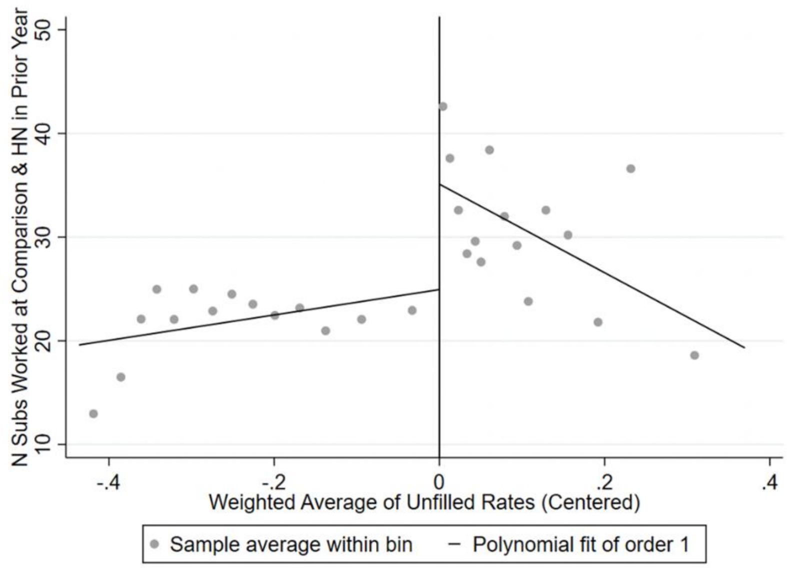

Figure B.12 Raw discontinuity in the number of unique substitutes in 2018-19 who worked only at both incentive and non-incentive schools in the prior year

Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

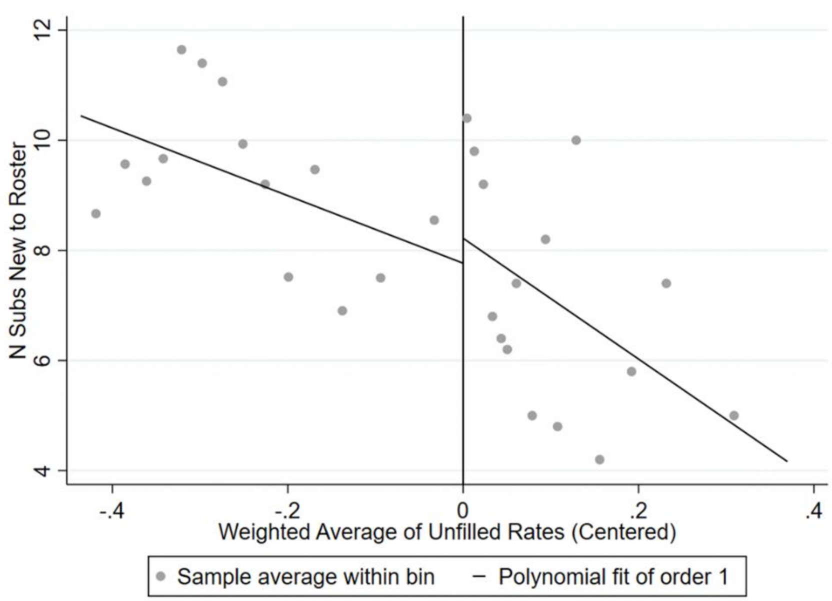

Figure B.13 Raw discontinuity in the number of unique substitutes new to the roster in 2018-19

Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

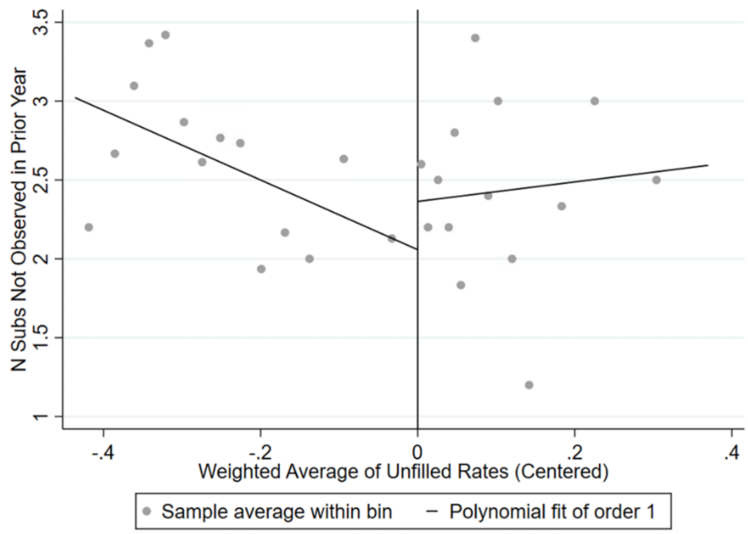

Figure B.14 Raw discontinuity in the number of unique substitutes in 2018-19 not observed in the prior year

Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

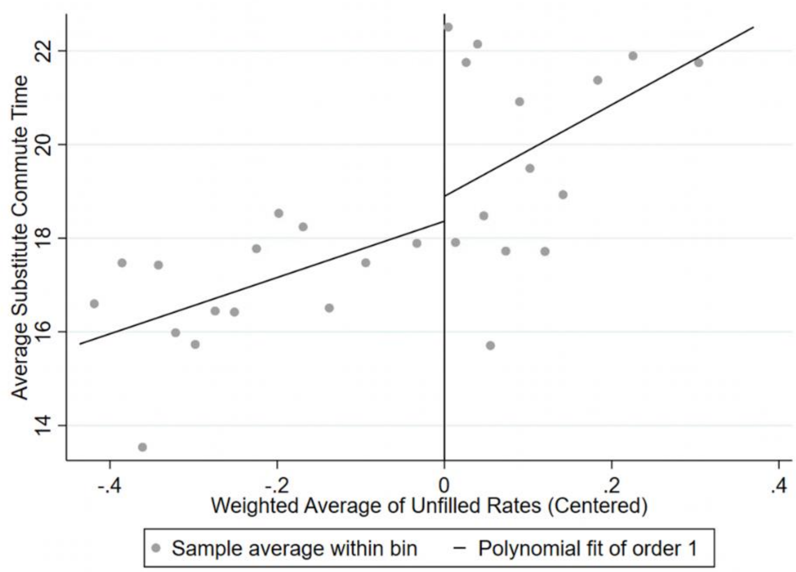

Figure B.15 Raw discontinuity in average substitute commute time 2018-19

Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

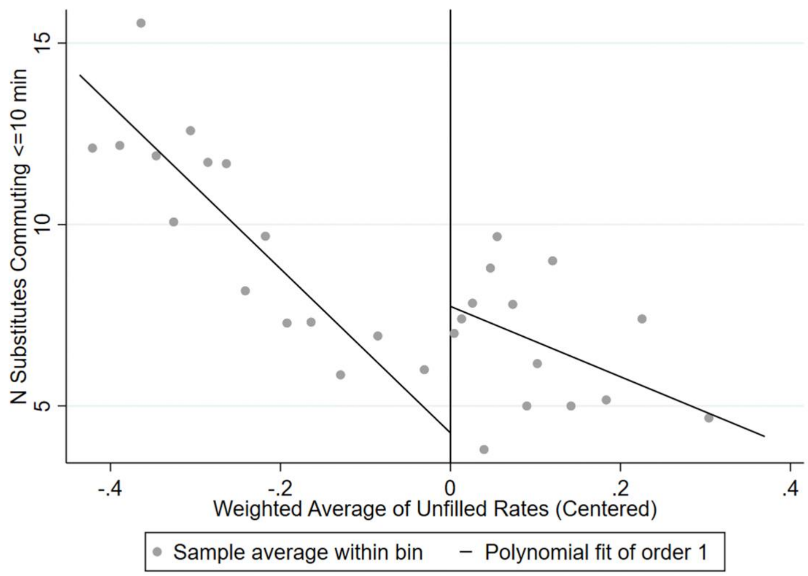

Figure B.16 Raw discontinuity in unique substitutes commuting less than ten minutes in 2018-19
Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

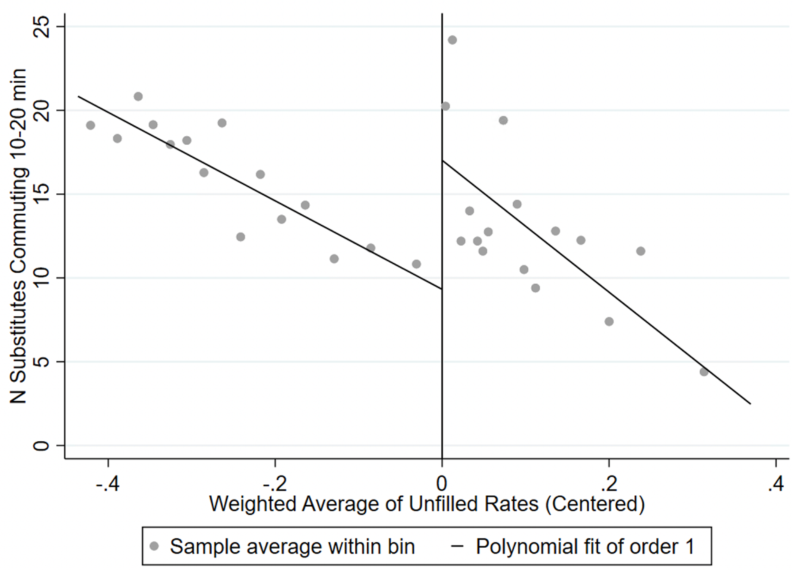

Figure B.17 Raw discontinuity in unique substitutes commuting 10 to 20 minutes in 2018-19
Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

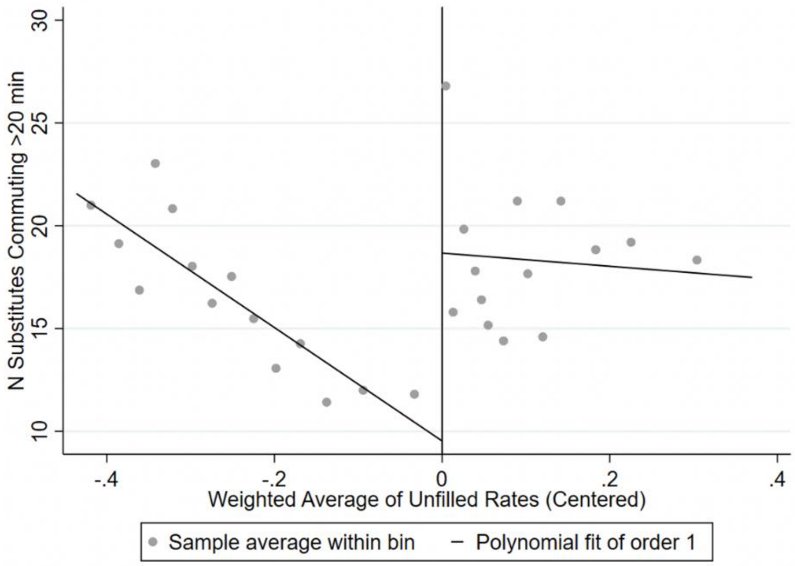

Figure B.18 Raw discontinuity in unique substitutes commuting >20 minutes in 2018-19

Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

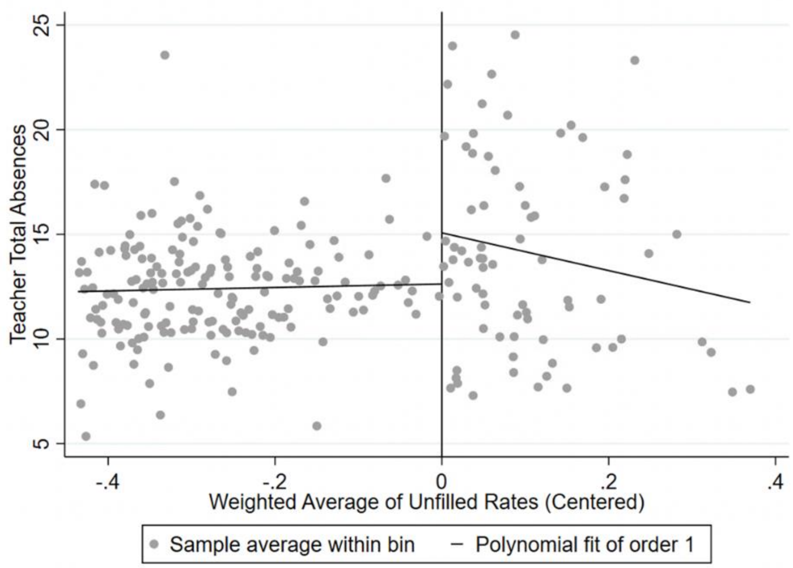

Figure B.19 Raw discontinuity in teacher-level absences in 2018-19

Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Teachers working in treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

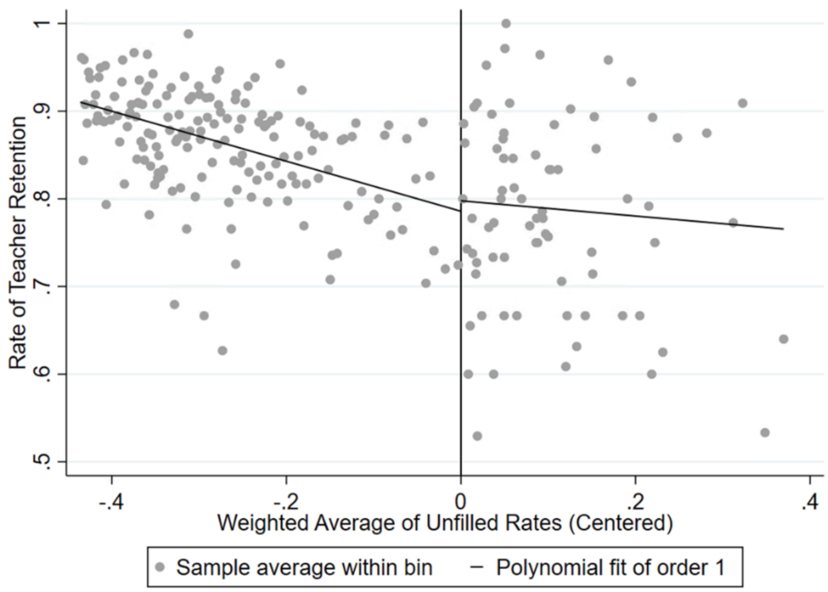

Figure B.20 Raw discontinuity in teacher-level retention in 2018-19
Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Teachers working in treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

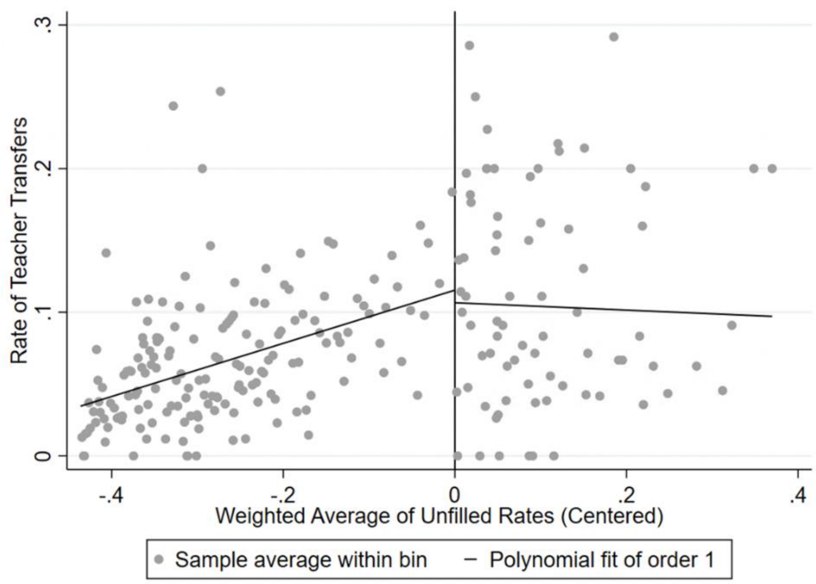

Figure B.21 Raw discontinuity in teacher-level transfers in 2018-19
Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Teachers working in treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

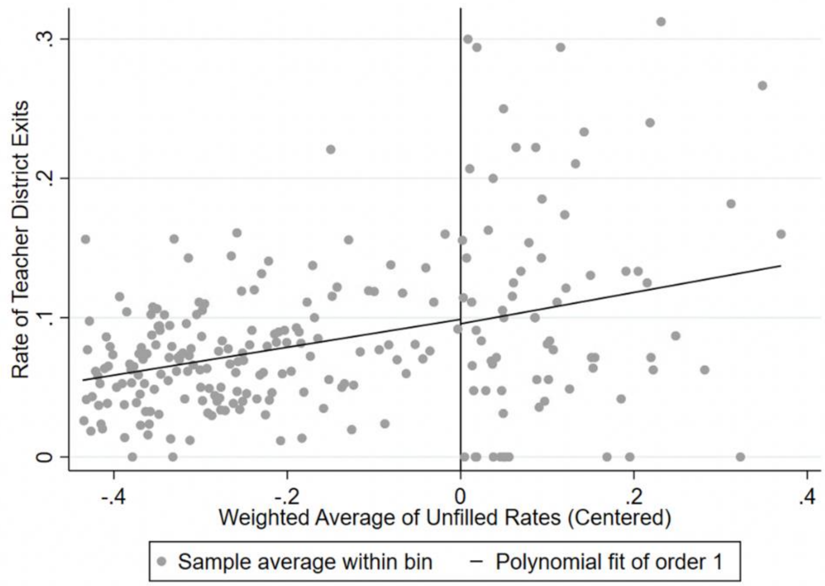

Figure B.22 Raw discontinuity in teacher-level district exits in 2018-19
Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Teachers working in treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

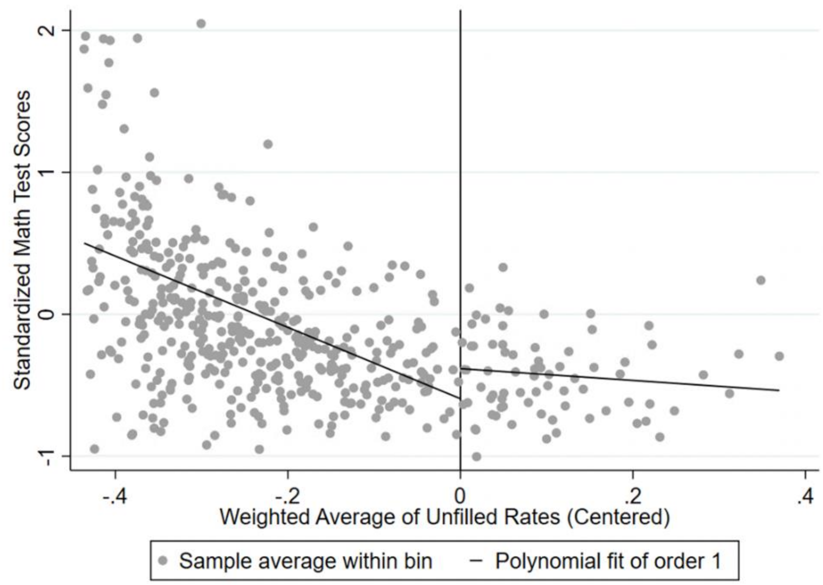

Figure B.22 Raw discontinuity in student-level math test scores in 2018-19
Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Students in treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

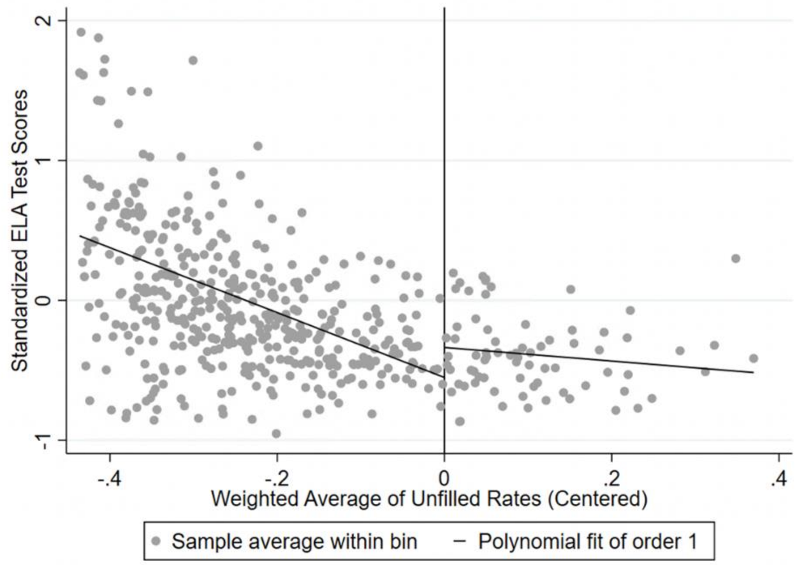

Figure B.23 Raw discontinuity in student-level ELA test scores in 2018-19
Notes: The independent variable is the forcing variable used to select schools for treatment, centered at the treatment cutoff. Students in treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

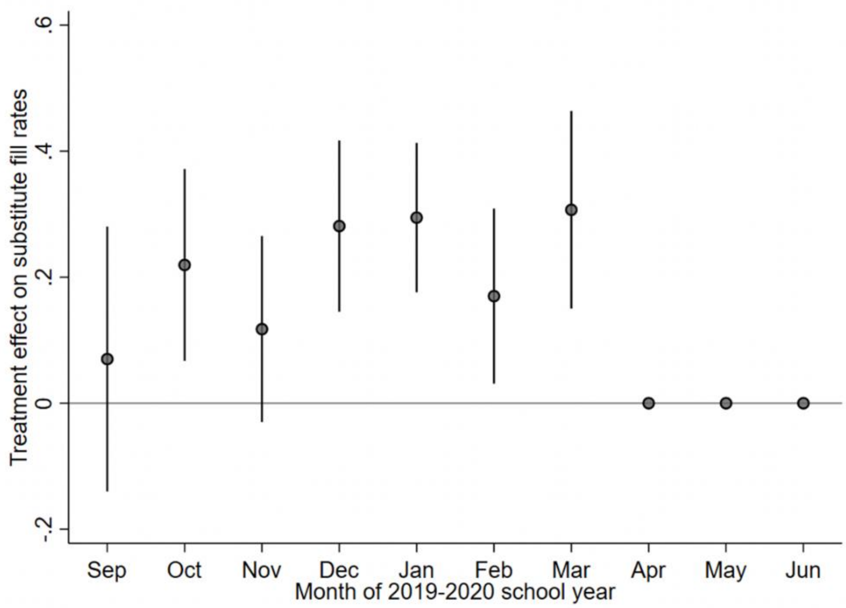

Figure B.24 Month-level regression discontinuity estimates, 2019-20
Notes: We plot average treatment effects at the 125th school cutoff from our RD model on fill-rates for specific months during the school year, controlling for school-level covariates. Vertical bars demarcate 95% confidence intervals for each estimate.

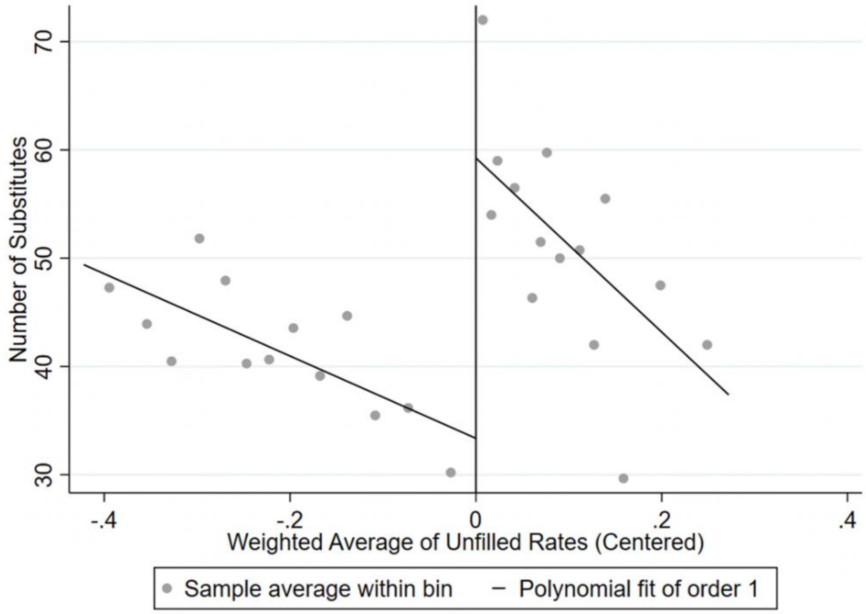

Figure B.25 Raw discontinuity in unique substitutes at each school, 2019-20
Notes: The independent variable was used to select schools for treatment in the second program year, centered at zero. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

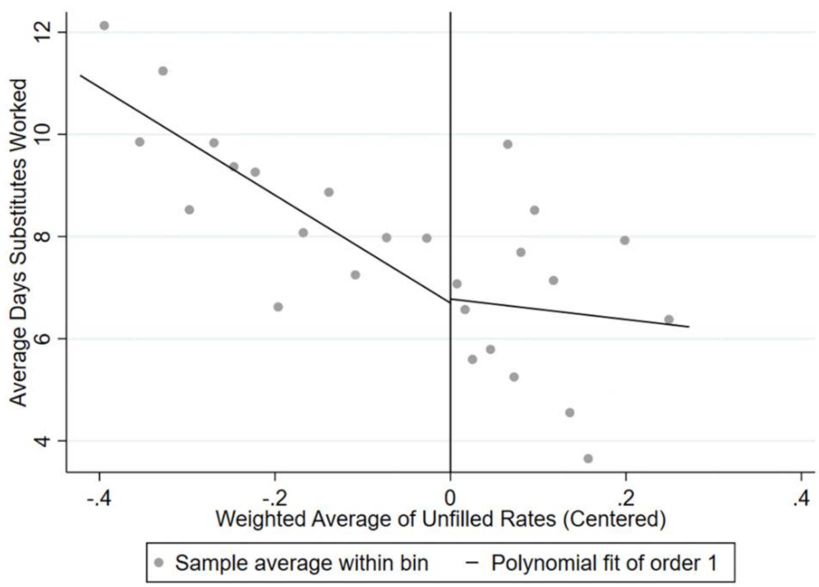

Figure B.26 Raw discontinuity in the average days substitutes worked at each school, 2019-20
Notes: The independent variable was used to select schools for treatment in the second program year, centered at zero. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

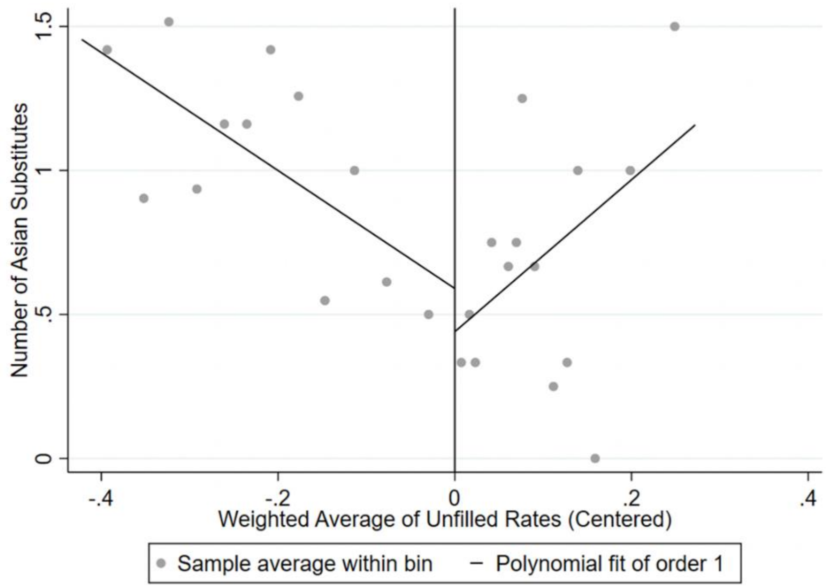

Figure B.27 Raw discontinuity in number of Asian substitutes at each school, 2019-20
Notes: The independent variable was used to select schools for treatment in the second program year, centered at zero. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

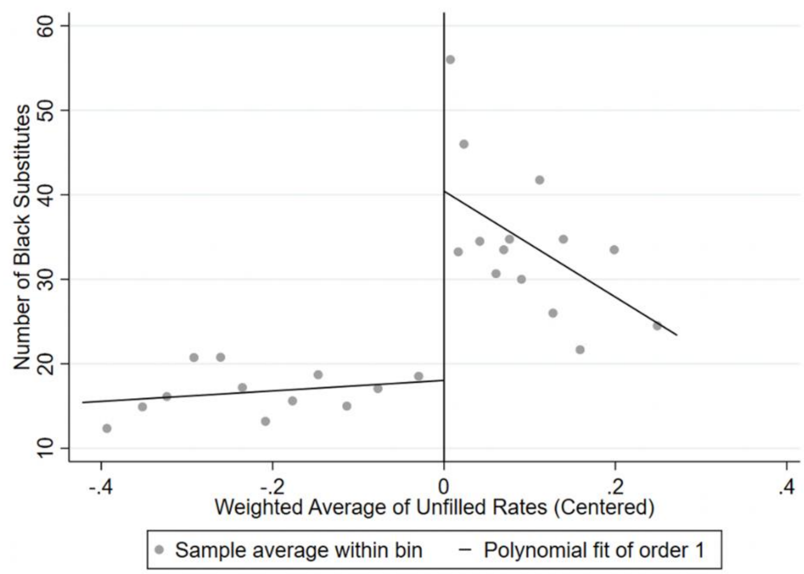

Figure B.28 Raw discontinuity in number of Black substitutes at each school, 2019-20
Notes: The independent variable was used to select schools for treatment in the second program year, centered at zero. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

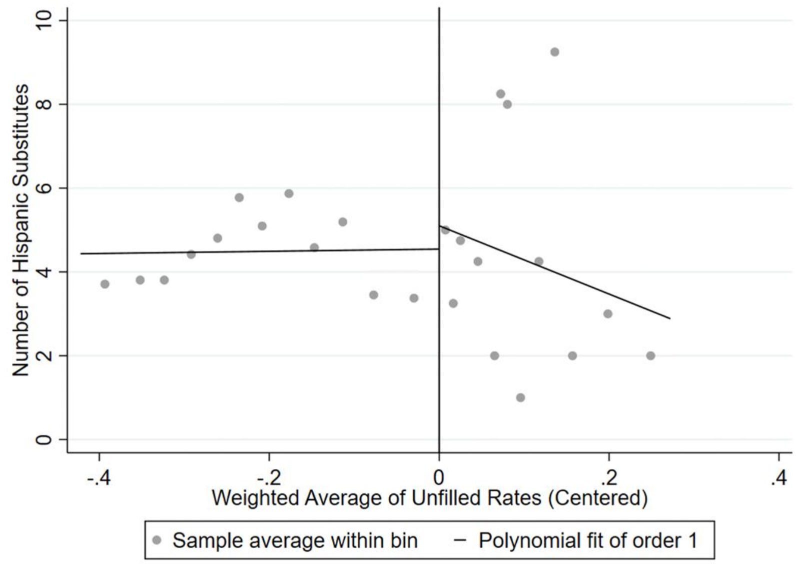

Figure B.29 Raw discontinuity in number of Hispanic substitutes at each school, 2019-20
Notes: The independent variable was used to select schools for treatment in the second program year, centered at zero. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

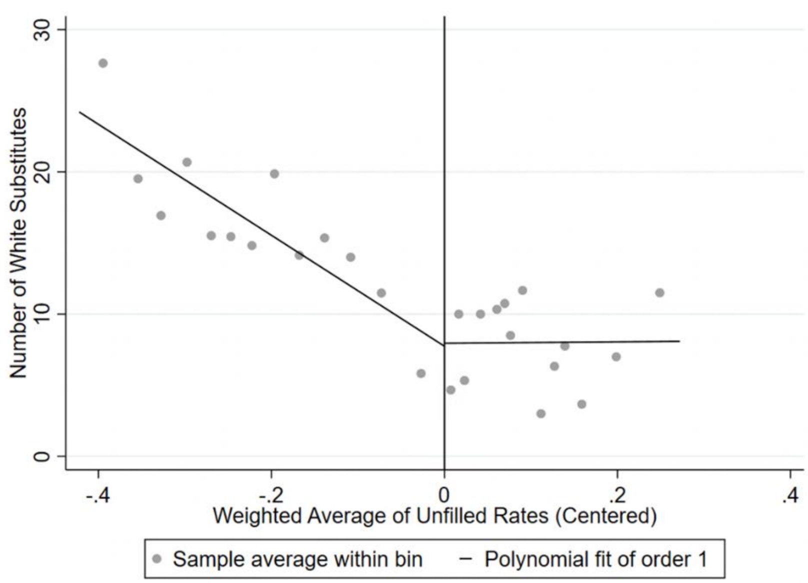

Figure B.30 Raw discontinuity in number of white substitutes at each school, 2019-20
Notes: The independent variable was used to select schools for treatment in the second program year, centered at zero. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

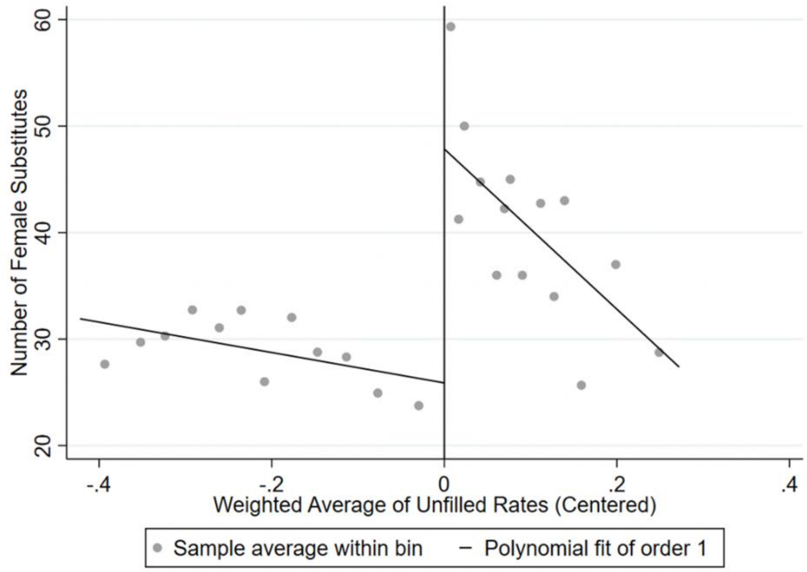

Figure B.31 Raw discontinuity in number of female substitutes at each school, 2019-20

Notes: The independent variable was used to select schools for treatment in the second program year, centered at zero. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

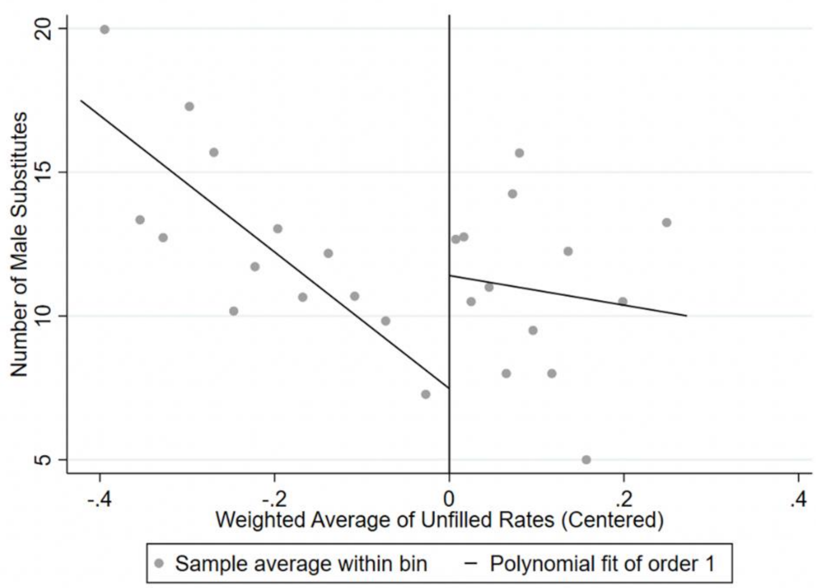

Figure B.32 Raw discontinuity in number of male substitutes at each school, 2019-20

Notes: The independent variable was used to select schools for treatment in the second program year, centered at zero. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

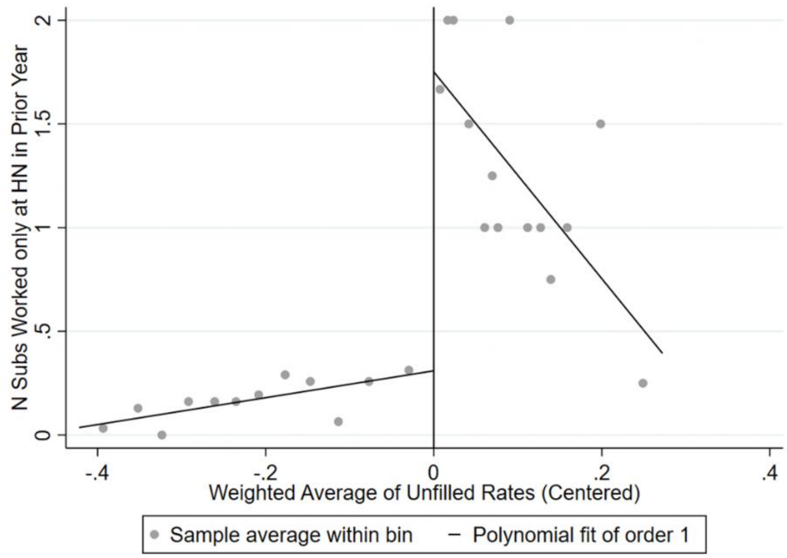

Figure B.33 Raw discontinuity in the number of unique substitutes in 2019-20 who worked only at incentive schools in the prior year

Notes: The independent variable was used to select schools for treatment in the second program year, centered at zero. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

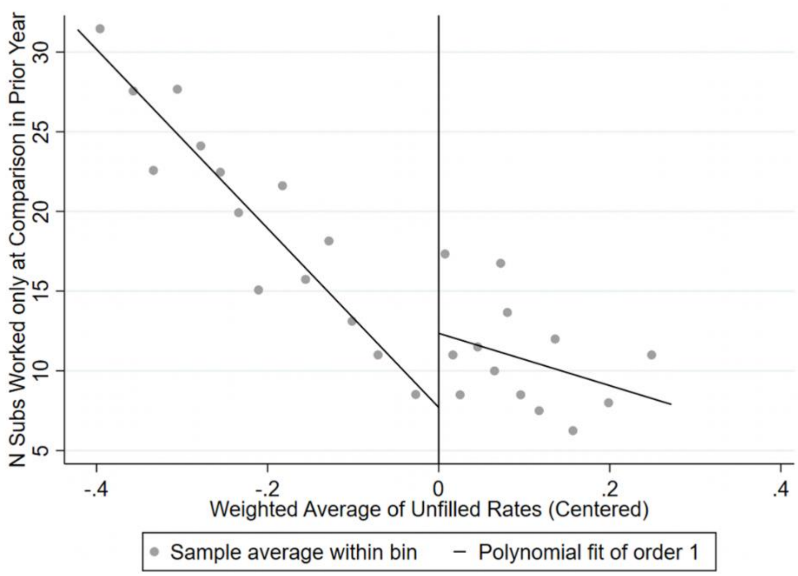

Figure B.34 Raw discontinuity in the number of unique substitutes in 2019-20 who worked only at non-incentive schools in the prior year

Notes: The independent variable was used to select schools for treatment in the second program year, centered at zero. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

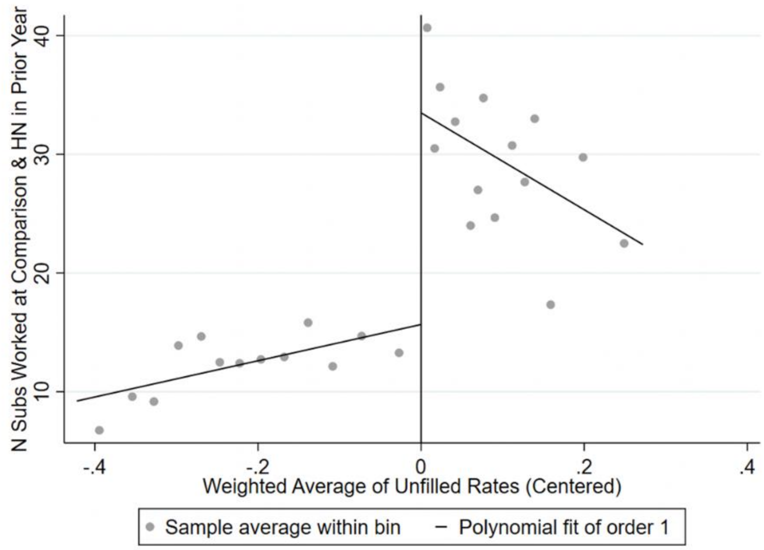

Figure B.35 Raw discontinuity in the number of unique substitutes in 2019-20 who worked only at both incentive and non-incentive schools in the prior year

Notes: The independent variable was used to select schools for treatment in the second program year, centered at zero. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

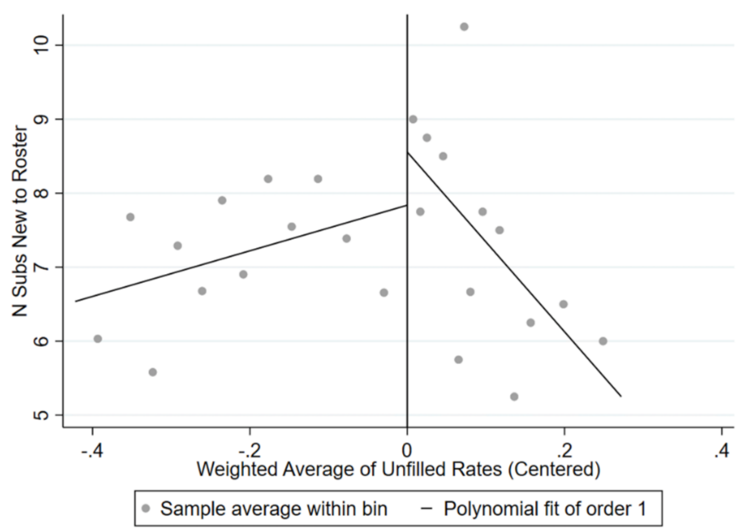

Figure B.36 Raw discontinuity in the number of unique substitutes new to the roster in 2019-20

Notes: The independent variable was used to select schools for treatment in the second program year, centered at zero. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

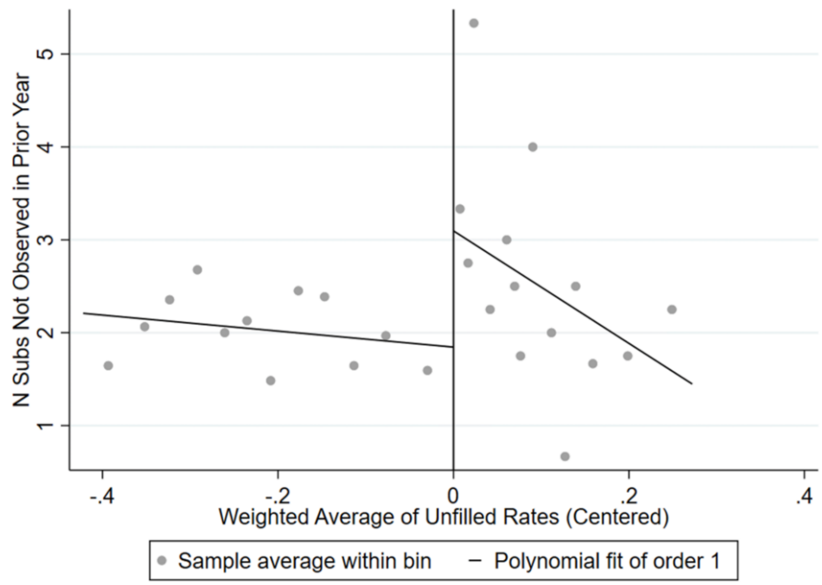

Figure B.37 Raw discontinuity in the number of unique substitutes in 2019-20 who worked only at incentive schools in the prior year

Notes: The independent variable was used to select schools for treatment in the second program year, centered at zero. Treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

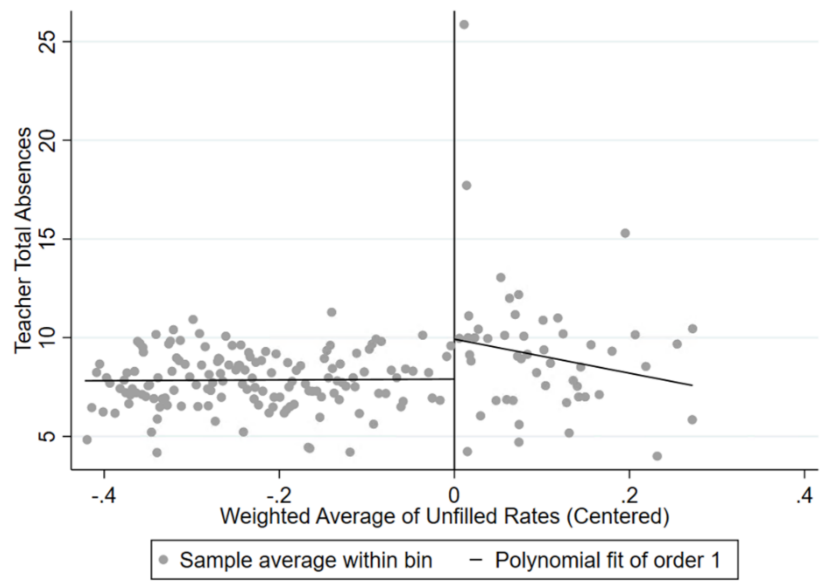

Figure B.38 Raw discontinuity in teacher-level total absences in 2019-20

Notes: The independent variable was used to select schools for treatment in the second program year, centered at zero. Teachers working in treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

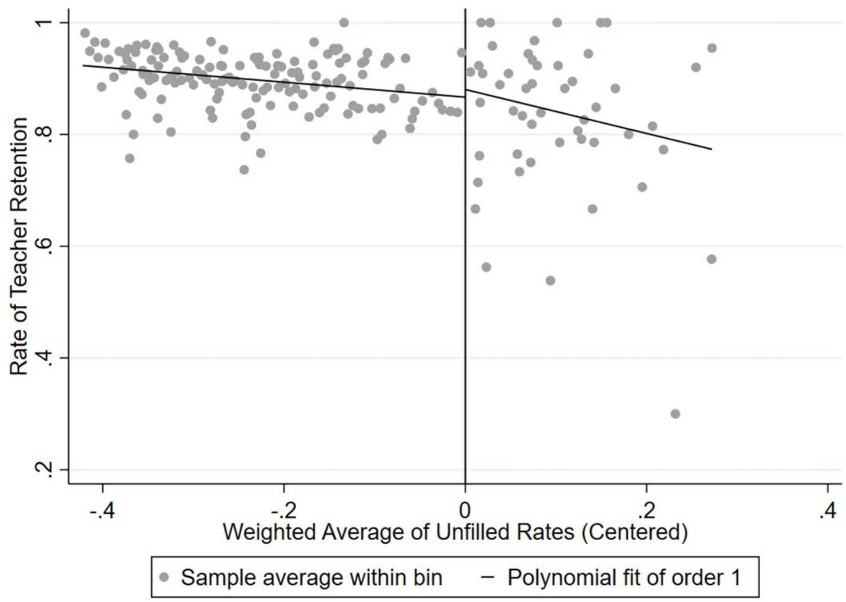

Figure B.39 Raw discontinuity in teacher-level retention in 2019-20

Notes: The independent variable was used to select schools for treatment in the second program year, centered at zero. Teachers working in treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

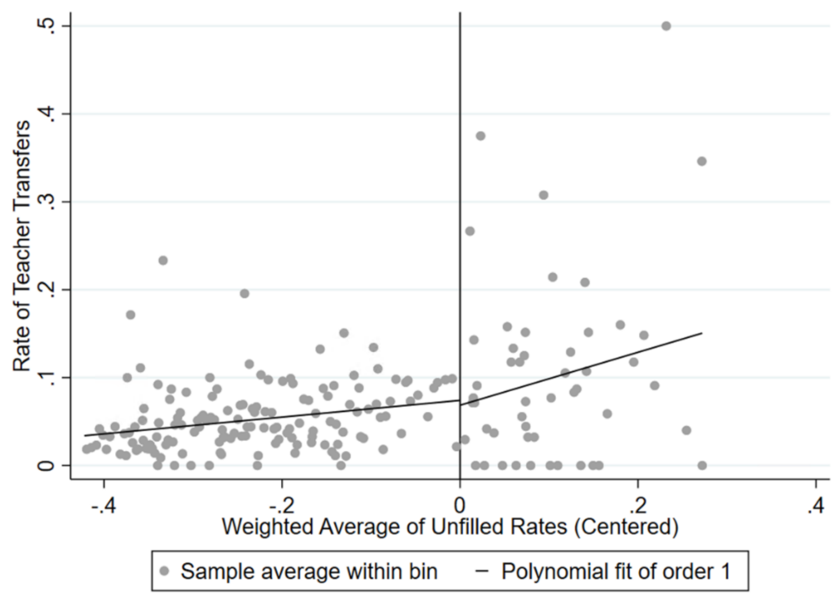

Figure B.40 Raw discontinuity in teacher-level transfers in 2019-20

Notes: The independent variable was used to select schools for treatment in the second program year, centered at zero. Teachers working in treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

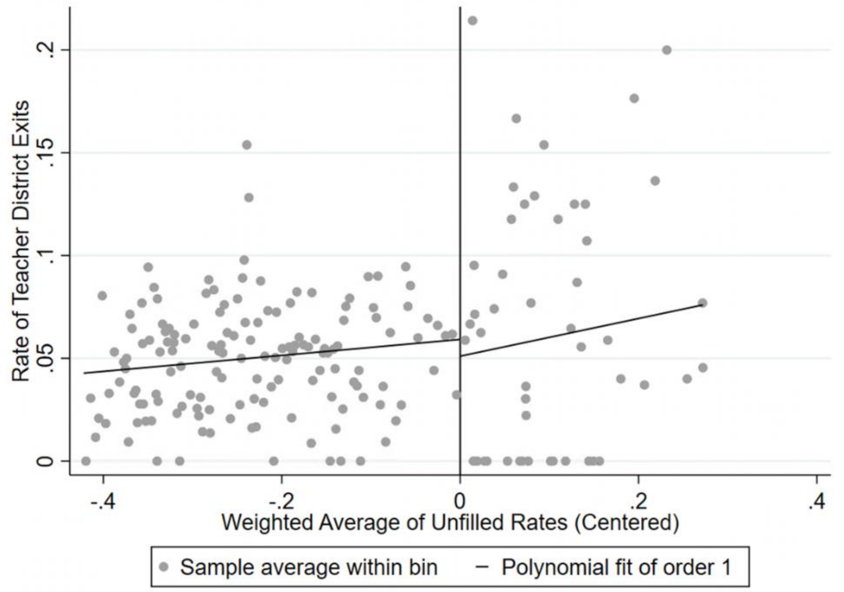

Figure B.41 Raw discontinuity in teacher-level exits in 2019-20

Notes: The independent variable was used to select schools for treatment in the second program year, centered at zero. Teachers working in treated schools are to the right of the cutoff. We follow Calonico et al. (2015) by selecting bin-width with the evenly-spaced mimicking variance method using spacings estimators.

Table B.1: Effects of Targeted Incentives on Substitute Labor Supply 2019-20 75th School Cutoff

	Incentive school mean 2017-18	(1)	(2)
Panel A. Substitute behavior measures			
Substitute request fill rate	0.47	0.22**	0.21**
		(0.10)	(0.09)
Number of unique substitutes at a school	50.40	29.10***	27.81***
		(8.89)	(6.79)
Average substitutes' total days worked at a school	4.65	-2.57	-1.49
		(2.39)	(2.48)
Panel B. Number of unique subs in 2019-20 by prior work history			
Prior work in treated schools only	0.73	1.70***	1.89***
		(0.42)	(0.33)
Prior work in comparison schools only	5.47	1.10	0.08
		(2.80)	(1.92)
Prior work in both treated & comparison schools	27.73	22.62***	22.34***
		(4.76)	(4.01)
New to the substitute roster	13.11	2.62	2.68*
		(2.07)	(1.52)
Lapsed substitutes	3.36	1.05	0.81
		(0.67)	(0.56)
School covariate vector		No	Yes
n		75	75

Notes: Heteroskedasticity-robust standard errors are reported in parentheses (* p<.10, ** p<.05, *** p<.01). Models exclude schools sorted into treatment in 2019-20. Schooling-level fixed effects are included in all models. The school covariate vector includes controls for student body race demographics, representation of free/reduced price lunch eligibility, special education status, English as a second language status, total school enrollment, and lagged school-level math and ELA achievement. Observations within the bandwidth of +/- 0.11 are weighted by a uniform kernel. The pre-treatment mean reported is for the initial 75 treated schools.

Table B.2: Heterogeneous Effects of Targeted Incentives on Substitute Labor Supply 2019-20 75th School Cutoff

	Incentive school mean 2017-18	(1)	(2)
	Panel A. Substitute localness		
Number of substitutes commuting <10min	9.05	2.80	2.25*
		(1.76)	(1.25)
Number of substitutes commuting 10-20min	17.52	7.64**	7.37***
		(3.06)	(2.43)
Number of substitutes commuting >20min	22.29	7.69***	6.64***
		(2.92)	(2.41)
Panel B. Number of unique substitutes in 2018-19 by demographic groups			
Asian	0.64	1.36**	0.55
		(0.58)	(0.40)
Black	30.96	18.14**	23.24***
		(7.13)	(6.06)
Latinx	3.95	2.09	0.62
		(2.25)	(0.74)
White	9.81	4.89	1.44
		(3.10)	(1.77)
Female	37.23	21.43***	21.44***
		(7.33)	(5.43)
Male	13.17	7.67***	6.36***
		(2.40)	(1.85)
School covariate vector		No	Yes
n		75	75

Notes: Heteroskedasticity-robust standard errors are reported in parentheses (* p<.10, ** p<.05, *** p<.01). Models exclude schools sorted into treatment in 2019-20. Schooling-level fixed effects are included in all models. The school covariate vector includes controls for student body race demographics, representation of free/reduced price lunch eligibility, special education status, English as a second language status, total school enrollment, and lagged school-level math and ELA achievement. Observations within the bandwidth of +/- 0.11 are weighted by a uniform kernel.

Table B.3: Effects of Targeted Incentives on Teacher Outcomes 2019-20 75th School Cutoff

	Incentive school mean 2017-18	(1)	(2)
Total absences	11.700	1.739	2.506***
		(1.380)	(0.844)
Retained at school	0.780	-0.051	-0.038
		(0.053)	(0.040)
Transferred	0.115	0.025	0.004
		(0.032)	(0.032)
Left district	0.104	0.026	0.034
		(0.037)	(0.027)
School covariate vector		No	Yes
Teacher covariate vector		No	Yes
n		1,684	1,684

Notes: Heteroskedasticity-robust standard errors are reported in parentheses (* p<.10, ** p<.05, *** p<.01). Models exclude schools sorted into treatment in 2019-20. Schooling-level fixed effects are included in all models. The school covariate vector includes controls for student body race demographics, representation of free/reduced price lunch eligibility, special education status, English as a second language status, total school enrollment, and lagged school-level math and ELA achievement. The teacher covariate vector includes binned tenure indicators (3-5, 6-10, 11-20, 21+ years, with 0-2 years omitted), gender, and race / ethnicity indicators. Observations within the bandwidth of +/- 0.11 are weighted by a uniform kernel.

Table B.4: Regression discontinuity estimates for teacher absence types, 2019-20

	Incentive school mean 2017-18	(1)	(2)
Total Absences	12.56	4.315**	3.502**
		(1.782)	(1.510)
Sick	5.45	-0.009	-0.362
		(0.482)	(0.397)
Personal day	2.03	0.250*	0.155
		(0.143)	(0.123)
Professional development	2.49	2.903*	2.884*
		(1.741)	(1.527)
Leave of absence	1.42	0.162	0.033
		(0.308)	(0.306)
School covariate vector		No	Yes
Individual covariate vector		No	Yes
n		2174	2174

Notes: Heteroskedasticity-robust standard errors are reported in parentheses (* p<.10, ** p<.05, *** p<.01). Models exclude the 75 schools sorted into treatment in 2018-19. Schooling-level fixed effects are included in all models. The school covariate vector includes controls for student body race demographics, representation of free/reduced price lunch eligibility, special education status, English as a second language status, total school enrollment, and lagged school-level math and ELA achievement. Observations within the bandwidth of +/- 0.10 are weighted by a uniform kernel. The pre-treatment mean reported is at the teacher-level, for the 50 treated schools added to treatment in 2019-20.

Table B.5. Sensitivity Analysis for 2019-20 125th Cutoff Regression Discontinuity

	(1)	(2)
Panel A. Exclude Top 25% of Schools by % of Substitutes Worked at Both Treatment and Comparison Schools Last Year		
Substitute request fill rates	0.25***	0.24***
	(0.04)	(0.04)
	n=61	
Panel B. Exclude Top 50% of Schools by % of Substitutes Worked at Both Treatment and Comparison Schools Last Year		
Substitute request fill rates	0.16**	0.25***
	(0.07)	(0.05)
	n=38	
Panel C. Exclude Top 25% of Schools by Number of Treated Schools Within 3 Miles		
Substitute request fill rates	0.18***	0.18***
	(0.04)	(0.04)
	n=59	
Panel D. Exclude Top 50% of Schools by Number of Treated Schools Within 3 Miles		
Substitute request fill rates	0.22***	0.18***
	(0.04)	(0.04)
	n=44	
School covariate vector	No	Yes

Notes: Heteroskedasticity-robust standard errors are reported in parentheses (* p<.10, ** p<.05, *** p<.01). We include schooling-level fixed effects and our school covariate vector in all models. Observations are weighted by a uniform kernel. Estimation Bandwidth is 0.10 for all panels. All models exclude the initial 75 treated schools, focusing on the 125th treatment-school cutoff.