Teacher Sorting and Preferences over School Disadvantage: Evidence from Performance Pay in Texas*

Patrick L. Massey†

Abstract

In this paper I study how performance-based compensation affects teacher mobility and sorting using the Texas Teacher Incentive Allotment (TIA), a statewide program introduced in 2019. TIA gives teachers a tiered quality designation, Recognized, Exemplary, or Master, based on performance evaluations and value-added measures. These designations are portable across schools and come with a salary supplement that rises with school disadvantage and rural status. Using administrative data on Texas public school teachers, I combine a staggered difference-in-differences event-study design with a discrete choice model of school choice that allows for heterogeneous teacher preferences over school SES. I find that designation reduces mobility on net: teachers must remain in their district to collect the first allotment, and the recovery in mobility afterward does not offset the moves foregone during that year. Designation shifts teachers toward more advantaged campuses: designated teachers become substantially less likely to leave already-advantaged schools, while exits from the highest-need schools fall only in the first designation year. This response is ordered by designation level, with Recognized teachers becoming more mobile and sorting toward advantage while Master teachers become less mobile and less likely to leave disadvantaged campuses. Preference estimates imply that the average designated teacher would require roughly $6,940 more per year to accept a school with conditions associated with a 20 percentage point higher economically disadvantaged share. While the salary gradient created by TIA falls short of that benchmark at every tier, but the gap is smallest at the highest tier. These results suggest that TIA's portable credential expands effective teachers' outside options at the levels where the pay gradient is shallowest and that the gradient becomes steep enough to hold teachers at disadvantaged campuses only at the highest tier.

JEL Codes: I21, I24, I28, J33, J45, J31

Keywords: Teacher sorting; performance pay; teacher preferences; compensating differentials; educational inequality

*I gratefully acknowledge that this research was made possible through data provided by the University of Houston Education Research Center. The conclusions of this research do not necessarily reflect the opinion or official position of the University of Houston Education Research Center, the Texas Education Agency, the Texas Higher Education Coordinating Board, the Texas Workforce Commission, or the State of Texas. The research reported here was supported by the Institute of Education Sciences, U.S. Department of Education, through Grant R305B200009 to Michigan State University. The opinions expressed are those of the author and do not represent the views of the Institute or the U.S. Department of Education. I also thank the National Academy of Education for support through the NAEd/Spencer Dissertation Fellowship award.

†Michigan State University, pmassey@msu.edu

1 Introduction

Teacher quality is regularly recognized as one of the most significant within-school determinants of student success, influencing educational achievement and attainment, as well as long-term outcomes such as earnings (Chetty et al., 2014b; Jackson, 2018). Yet, access to effective teachers remains highly unequal across schools (Lankford et al., 2002; Hanushek et al., 2004; Boyd et al., 2005b; Isenberg et al., 2013; Goldhaber et al., 2015). The recruitment and retention of effective teachers in schools serving economically disadvantaged students is a persistent policy challenge because these schools experience disproportionately high teacher turnover. Thus, financial incentives are an increasingly common tool used to influence where effective teachers work.

There is some evidence that teachers respond to these incentives. Retention bonuses can decrease turnover, and sufficiently large compensation differentials can incentivize teachers to stay in or move to hard-to-staff schools (Clotfelter et al., 2008; Glazerman et al., 2013; Springer et al., 2016). At the same time, much of this evidence comes from temporary bonuses, district-level initiatives, or relatively small-scale interventions. There is little evidence on whether compensation can meaningfully alter teacher mobility and sorting at scale because few policies provide incentives that are both substantial and sustained. This limitation is especially important given evidence that inducing teachers to work in high-poverty or otherwise hard-to-staff schools may require sizable compensation differentials (Hanushek et al., 2004; Imazeki, 2005; Clotfelter et al., 2011).

Texas's Teacher Incentive Allotment (TIA) provides a novel setting in which to study this question. TIA is a statewide, performance-based compensation program launched in 2019 that provides designated teachers with recurring annual payments, called allotments, ranging from $3,000 to $32,000 on top of their base salary. Participating districts designate eligible teachers at one of three levels: Recognized, Exemplary, or Master. The size of a teacher's annual allotment depends on both the designation level and the characteristics of the school in which the teacher works. Payments increase with the share of economically disadvantaged students at a campus and are further increased for rural schools. As a result, TIA creates considerable variation in wages that a teacher can receive across different campuses both within and across districts.

TIA also creates a second, potentially opposing, force. Designations are valid for five years and are portable between campuses. Thus, receiving a designation does more than raise wages: it can also signal teacher quality to other campuses and improve a teacher's opportunities elsewhere in the labor market. A policy designed to attract and retain effective teachers in underserved schools may make those schools more financially attractive while also making designated teachers more appealing to more advantaged schools.

I formalize this tension in a random utility framework where TIA operates through two channels, a compensation channel and a credentialing channel. The compensation channel makes high-need schools relatively more attractive through higher wages. Through the credentialing channel, designation may reduce information frictions surrounding teacher quality and expand the set of schools available to designated teachers. Both channels may influence mobility but do not need to pull sorting in the same direction. If teachers are on average more likely to prefer schools with more advantaged students, credentialing may drive movement toward those campuses, while compensation pulls in the opposite direction. The net effect then depends on whether the compensation gradient generated by TIA is large enough to offset teachers' preferences over school characteristics and an expanded set of outside options created by designation.

Using restricted-use administrative data from the University of Houston Education Research Center (UHERC), I examine how receiving a TIA designation affects teachers' labor-market decisions. First, I estimate the causal effect of designation on teacher mobility and sorting using a difference-in-differences event-study framework that exploits the staggered timing of teacher designations across districts. I examine

not only whether designated teachers become more or less likely to move, but also the characteristics of the schools they leave and enter. I then estimate a mixed logit model of school choice to estimate teachers' preferences over salary and campus socioeconomic disadvantage. This model allows me to quantify teachers' preferences over school disadvantage in dollars and to directly compare the compensation teachers need to work in higher-need schools with the additional compensation generated by TIA.

I find that designation does not increase mobility on net. Teachers must remain in their district through the designation year to receive their first allotment, and mobility drops sharply in that year. The suppression carries into the following year and then reverses, with moves becoming more common from two years after designation onward. Cumulating the event-time estimates from the designation year forward, this later increase does not make up for the moves foregone at designation. Designation also shifts teachers toward more advantaged campuses, with the average change in campus disadvantage negative in every post-designation year. This shift runs mainly through where designated teachers stay rather than through movement into advantaged campuses. Departures from the most advantaged campuses fall in every post-designation period, while departures from the most disadvantaged campuses show no comparable change.

These average effects conceal substantial heterogeneity across designation levels. Recognized teachers become more mobile following designation and increasingly sort toward more advantaged campuses. By contrast, Master teachers become less mobile and less likely to leave disadvantaged campuses. This ordering is consistent with the varying strength of the financial incentive across designation levels. Recognized teachers receive the smallest allotments, while Master teachers face substantially larger compensation gradients across schools. The reduced-form results thus suggest that the credentialing channel is more important at lower designation levels, and the compensation channel becomes more important as the size of the incentive increases.

The school-choice estimates provide evidence on the magnitude of compensation needed to offset teachers' preferences over school disadvantage. I estimate that the average designated teacher would require an additional $6,941 in annual compensation to accept a one-standard-deviation increase in campus socioeconomic disadvantage. The additional TIA compensation generated by moving from a more advantaged to a more disadvantaged campus is less than this amount at every designation level, but the gap shrinks with designation level and is smallest for Master teachers. The school-choice estimates therefore match the ordering of the reduced-form patterns: where the gap is largest, teachers shift toward more advantaged schools, and where it is smallest, designated teachers become less likely to leave disadvantaged campuses.

This paper makes three contributions. First, I provide teacher-level evidence on how a large and sustained statewide performance-pay program affects teacher mobility and sorting. Second, I estimate a mixed logit model of realized school choices to estimate the willingness-to-pay associated with campus socioeconomic disadvantage and compare it with the compensation gradient induced by TIA. Third, I show how combining school-specific compensation with a portable performance designation can create opposing incentives. Differences across designation levels suggest that compensation becomes relatively more important as the financial incentive increases.

The remainder of the paper proceeds as follows. Section 2 reviews the relevant literature. Section 3 describes the TIA program. Section 4 presents the conceptual framework. Section 5 describes the data. Section 6 provides a descriptive analysis of the pre-TIA teacher labor market. Section 7 presents the difference-in-differences empirical strategy and results. Section 8 describes the discrete choice model and its results. Section 9 reports robustness checks. Section 10 concludes.

2 Literature Review

This paper contributes to the literature on teacher preferences for school characteristics and the compensation required to attract teachers to disadvantaged schools. Teachers tend, on average, to prefer schools serving more socioeconomically advantaged students, but this does not always reflect student demographic preferences. Schools that serve lower-income, rural, and other disadvantaged populations often have different organizational and working conditions that teachers value. Ingersoll (2001) highlights the significance of organizational conditions and job dissatisfaction in explaining teacher turnover, demonstrating that staffing issues often occur when teachers depart specific schools rather than a general lack of qualified teachers. Further research has confirmed the importance of school leadership, class size, peers, and culture (Boyd et al., 2011; Feng & Sass, 2017). Geography also plays an important role in the supply of teachers. Teachers typically get hired in relatively small labor markets and near where they grew up (Boyd et al., 2005a), so pay must compensate not only for less-desired working conditions but also for the cost of moving away from preferred locations. Meanwhile, teachers self-select into schools with higher-achieving and more socioeconomically advantaged students (Hanushek et al., 2004; Boyd et al., 2005b; Feng & Sass, 2017). Thus, the observed sorting by school socioeconomic status is likely to be at least partially due to differences in organizational and workplace conditions that are systematically associated with student disadvantage.

In most U.S. public school districts, wages are determined by salary schedules that are based primarily on experience and credentials, leaving little opportunity to adjust pay for differences in working conditions. In the standard compensating-differentials framework, schools with less desirable working conditions could use higher wages to attract and retain teachers (Rosen, 1986). This adjustment is constrained by rigid salary schedules, and current estimates indicate that the differentials required may be substantial. For turnover in central-city Texas schools to be at the level of a typical suburban school, women's salaries would need to be increased by 25% to 43% and men's salaries by about 10% (Hanushek et al., 2004). Increases of 15% to 20% may not be enough to reduce attrition from urban districts to the level of an average district in Wisconsin (Imazeki, 2005). Using North Carolina data, Clotfelter et al. (2011) estimates that salary differentials of around 10% to 58% would be necessary to retain teachers with good preservice qualifications across schools with differing nonwhite student shares ranging from 10 to 50 percentage points.

The difficulty in estimating teacher preferences from realized labor-market outcomes is that observed matches are the result of decisions made by both teachers and schools. Boyd et al. (2013) address this problem using a game-theoretic two-sided matching model, estimating preferences of teachers over jobs and preferences of employers over teacher characteristics separately. The results suggest that teachers prefer higher-income schools, shorter travel distances, and suburban schools, while employers value some measures of teacher academic qualifications more. The two-sided approach highlights that the observed teacher-school match is not a one-sided teacher choice, as the teacher must prefer the school and the school has to prefer hiring the teacher. Another challenge is that standard teacher pay scales produce little independent wage variation across schools, while school characteristics may be correlated with unobserved amenities. Under TIA the incentive amount varies systematically with school disadvantage, and the relationship between disadvantage and compensation varies by designation level. Additionally, base salary varies separately from any TIA incentive. These additional sources of variation in wages help to disentangle the salary coefficient from the disutility of campus SES. However, realized matches are still the outcome of a two-sided hiring process.

Recently, Johnston (2025) ran a discrete-choice experiment in a large Texas school district to directly identify teacher preferences. Johnston exploits alternative hypothetical sets of job choices to identify pref-

erences over pay, working conditions, and student and principal characteristics. Respondents were told to imagine the schools as identical in all characteristics not shown, allowing the experiment to isolate willingness to pay for the attributes in question. His conditional-logit estimates suggest that teachers would require about $282 to accept a 10-percentage-point increase in the share of low-income students and $575 to compensate for a 10-percentile decline in average student achievement. Working conditions are also valued by teachers. They value a supportive principal at about $7,500 per year, and principal support eliminates the majority of the estimated disutility associated with high-poverty and low-achieving schools.

Related literature shows that differentiated compensation can alter teacher labor-market behavior. Clotfelter et al. (2008) analyze a North Carolina program that provided a $1,800 per year bonus for math, science, and special education teachers in high-poverty or low-performing schools, and find that the policy reduced turnover by 17%. Denver Public Schools used differentiated compensation through its ProComp program to improve retention in hard-to-staff schools (Fulbeck, 2014). Springer et al. (2016) find that $5,000 bonuses improved retention of effective teachers in Tennessee's lowest-performing schools. Dee & Wyckoff (2015) examine the IMPACT program, which combined dismissal threats for low-performing teachers with financial rewards for high performers, such as bonuses up to $25,000 and permanent base-pay increases. Dismissal threats increased voluntary attrition among low performers while improving subsequent performance among those who stayed, whereas financial incentives improved high performers' subsequent performance but had no discernible effect on retention.

Other programs have used large incentives specifically to alter where teachers work. Glazerman et al. (2013) study the Talent Transfer Initiative, a randomized intervention that offered highly effective teachers $20,000 over two years to transfer into academically struggling schools. The program was successful in attracting highly effective teachers into high-need schools and improving their retention for as long as the incentive was offered. California's Governor's Teaching Fellowship also gave $20,000 to high-achieving prospective teachers who pledged to teach in underperforming schools for four years. Recipients of the fellowship were more likely to subsequently work in low-performing schools (Steele et al., 2010). These studies suggest that large enough incentives can shift teachers across schools, but the programs were temporary, externally funded, or targeted relatively narrow groups. This means that there is less evidence on the implications of sizable persistent differences in pay for teacher sorting when they are embedded within a broader pay system.

More generally, changes in how wages are set suggest that relaxing rigidity of wages could affect the distribution of teachers. Districts that adopted flexible pay after Wisconsin's 2011 Act 10 paid higher quality teachers more, attracted high-quality teachers from neighboring districts that maintained rigid salary schedules, and saw gains in student achievement (Biasi, 2021). Biasi et al. (2025) extends this evidence in a structural equilibrium model in which districts compete for heterogeneous teachers through wage and hiring strategies. Wage flexibility improves the matching of teachers to districts, but it can also increase inequality because more advantaged districts can attract the teachers they prefer more easily. They also examine state-funded bonuses based on effectiveness targeted to disadvantaged districts. In certain situations, these targeted bonuses can boost both equity and efficiency at the same time. TIA provides an empirical setting with similar targeting: monetary incentives ranging from $3,000 to $32,000 per year that systematically vary with school disadvantage and where the resulting cross-campus compensation gradient is determined by a statewide formula, rather than by district wage-setting choices alone.

Another source of friction is imperfect information on teacher quality. Current principals may learn a lot about the effectiveness of the teachers they supervise, but that information does not always follow a

teacher when she decides to move. Instead, schools hiring rely on interviews, references, credentials, and other imperfect signals, and observable characteristics at the time of application are only weakly predictive of subsequent teacher effectiveness (Jacob et al., 2018). Thus, information frictions can limit the outside options of good teachers even when their productivity would be valuable to other schools.

Bates (2020) finds that information about teacher effectiveness affects mobility and sorting. In two North Carolina districts, after value-added measures were disclosed to hiring principals, high-value-added teachers were more likely to move, especially to higher-performing schools. Districts that did not adopt saw weaker effects, while lower value-added teachers increasingly moved to uninformed districts. This contrast indicates that mobility responded to districts' information rather than evaluation itself and increased the sorting of effective teachers toward more-advantaged schools. Bates et al. (2025) extend this mechanism to the equilibrium allocation in a two-sided matching model of teacher applications and principal evaluations. They find that teachers have heterogeneous preferences for schools with fewer economically disadvantaged students, but there is a weak correlation between principals' evaluations and teacher value added. Consequently, bonuses that attract teachers to disadvantaged schools produce limited improvements in teacher quality unless principals select more effective applicants. Targeting bonuses to effective teachers and placing greater weight on effectiveness in hiring produce larger gains. TIA operates on both margins: differentiated compensation is restricted to designated teachers, while the designation itself provides a portable performance signal to prospective employers.

National Board Certification (NBC) provides a similar portable signal of teacher quality, and some states offer targeted compensation. The Cowan & Goldhaber (2018) study examines Washington State's combination of NBC and bonuses for certified teachers in hard-to-staff schools and finds that the incentives reduce turnover among certified teachers at eligible schools. But NBC is different from TIA in several important ways. Certification is voluntary, costs are paid by teachers to obtain certification, and it is generally separate from regular district evaluation systems. Instead, TIA incorporates a portable performance label into the evaluation and compensation system and ties the financial value of that label to the school in which the teacher works. Hence, the same policy has both a signal of teacher performance and a school-specific incentive for compensation.

The closest study to date examining TIA is Kirksey et al. (2024), who use a synthetic control design on the first cohort of participating districts and find a decline in district-level turnover of around 5 percentage points and modest gains in student achievement. Their data do not identify teachers, so their estimates reflect changes in the aggregate across participating districts rather than the behavior of teachers directly exposed to the policy. TIA provides the opportunity to link evidence on compensating differentials, financial incentives, and teacher preferences in a statewide setting where teacher-school matches are two-sided and both the size of the incentive and the visibility of teacher quality vary simultaneously.

These literatures identify the components of the problem separately. Estimates of the compensation required to work in disadvantaged schools are based either on realized matches under rigid salary schedules, which provide little independent wage variation across campuses, or on stated-preference designs that experimentally vary pay and school attributes independently. Temporary, externally funded, or narrowly targeted bonuses provide evidence that compensation can influence teacher behavior. Evidence that information about teacher quality influences mobility comes from settings where there is no monetary compensation. TIA combines these features. The compensation gradient across campuses is determined by a statewide formula rather than district wage setting; it is large and recurring rather than a one-time bonus; and it is associated with a designation that is portable across districts and visible to prospective employers. This

setting allows me to examine how school-specific compensation and a portable performance designation jointly affect mobility and sorting and to compare the actual compensation gradient with the compensating differential implied by realized school choices.

3 Teacher Incentive Allotment [1]

3.1 Program Overview

TIA is a statewide performance-based compensation program implemented in Texas in 2019 as part of House Bill 3, a comprehensive revision of the state's school finance system that increased public school funding while providing property tax relief (Svitek, 2019). Its stated objective is to establish a structured pathway for teachers to reach six-figure salaries, with particular emphasis on rewarding teachers in rural and low-income schools. This policy also represents a large investment in teachers by the state. During the 2023-2024 school year, Texas allocated $290 million in incentive payments through TIA (Texas Education Agency, 2024).

Participation is voluntary at the district level. School districts and charter networks apply to the Texas Education Agency (TEA), and once approved, may award teachers one of three designation levels: Recognized, Exemplary, or Master. The program launched with an initial cohort of 26 districts and has expanded substantially each year since.

The allotment is the per-teacher payment the state sends to a district for each designated teacher it employs. Because the payment follows the teacher and scales with the school she works at, TIA effectively subsidizes competition for designated teachers across Texas public schools. Districts must direct at least 90% of the allotment to compensation at the campus where the designated teacher works, preventing cross-campus redistribution within the district, and may retain up to 10% for administrative costs. In practice, most districts pass the full 90% directly to the teacher. The size of the allotment varies by designation level and by school characteristics, as described next.

3.2 The Allotment Formula

The defining feature of TIA, and the one most consequential for teacher sorting, is that the per-teacher allotment is not a flat bonus. It scales with the socioeconomic composition of the school. The per-teacher allotment is calculated at the campus level according to:

$$\text{Allotment} = \underbrace{B_k}_{\text{base}} + \underbrace{m_k \times \bar{s}_j}_{\text{demographic supplement}} \tag{1}$$

where B_k and m_k are the base allotment and scaling multiplier for designation level k, and $\bar{s}_j$ is the Average Student Point Value (ASPV) at school j, which ranges from 0 at the least disadvantaged campus to 4 at the most disadvantaged. Base allotments are $3,000, $6,000, and $12,000 for Recognized, Exemplary, and Master teachers, respectively. The scaling multipliers are $1,500, $3,000, and $5,000. Total allotments therefore range from $3,000 to $9,000 for Recognized, $6,000 to $18,000 for Exemplary, and $12,000 to $32,000 for Master teachers.

The scaling term $\bar{s}_j$ captures how disadvantaged a campus's students are on average, with higher values meaning more disadvantaged. It is constructed in three steps. First, every Census block in Texas is sorted

[1] Many of these institutional details have been pulled from the Teacher Incentive Allotment 2023 - 2024 Guidebook; further details can be found at: `https://tiatexas.org/wp-content/uploads/2023/08/tia_guidebook_2023_FINAL_8.31.2023.pdf`

into a tier from 1 to 5 using measures such as single-parent household rates, educational attainment, home ownership, and median income, with higher tiers corresponding to more disadvantaged blocks. Second, each student is assigned a tier from 0 to 5: students who do not qualify for free or reduced-price lunch (FRPL) fall into tier 0, FRPL-eligible students take the tier of the Census block they live in, and students at rural campuses receive a two-tier bump up to a maximum of 5. Third, each tier is worth a set number of points and $\bar{s}_j$ is the average of those points across all students enrolled at campus j. This calculation is performed for every campus in Texas, regardless of district participation, and is updated each year.

Tier	Point Value	Average Median Income	Rural Tier
0	0	—	2
1	0.5	$116,706	3
2	1	$72,229	4
3	2	$57,339	5
4	3	$45,554	5
5	4	$33,544	5

Source: TEA Census Block Tier Mapping for 2024, which can be found at:
`https://tea.texas.gov/texas-schools/general-information/census-block-group-tools`

Table 1: TEA Tiering System with Average Median Income

Two features of this design are worth emphasizing. First, the allotment can differ substantially across campuses within the same district. Second, because $\bar{s}_j$ is computed for every campus in the state, the scaling follows a designated teacher to any campus she moves to, including campuses in non-participating districts.

3.3 Earning a Designation

The primary path to earning a designation goes through a participating district's evaluation process. A teacher employed by a participating district earns a designation by meeting the district's performance standards, which TEA requires combine student growth measures with classroom observations. Districts set the weights on these components in their applications. Student growth is a measure that captures how far a student progressed from a starting point rather than measuring absolute proficiency. The combined score determines designation level, with TEA recommending that roughly the top 33%, 20%, and 5% of teachers statewide receive Recognized, Exemplary, and Master designations, respectively. A teacher may also receive a Recognized designation by holding a National Board Certification; TEA awards this designation to certified National Board teachers regardless of whether their district participates in TIA.

Once earned, a designation attaches to the teacher's state-issued teaching certificate and remains effective for five years. It cannot be revoked unless the teacher leaves the profession, and a higher designation earned later resets the five-year clock. Teachers who move to a non-participating district lose eligibility for redesignation once those five years lapse.

Notably, the designation is portable. If a designated teacher moves to a new campus, her designation travels with her, and the new campus's $\bar{s}_j$ determines the allotment she receives. This holds even when the receiving district does not participate in TIA: any campus employing a designated teacher receives the corresponding allotment from the state. Portability converts the designation into a labor-market credential that can be exercised across the full set of Texas public schools, not only within the designating district.

4 Conceptual Framework

I argue that TIA affects teacher mobility and sorting through two policy-induced changes that I model as a random utility problem. A *compensation channel* raises pay for designated teachers by amounts that grow with school disadvantage. A *credentialing channel* makes teacher quality publicly observable through a portable designation. The compensation channel raises the relative attractiveness of disadvantaged schools; the credentialing channel expands designated teachers' outside options. If teachers on average prefer more advantaged schools, the two channels have opposing implications for the distribution of teachers across schools.

4.1 Setup

Consider a labor market with a continuum of teachers indexed by i and a set of schools indexed by j, where $d(j)$ denotes the district of school j. Schools differ along a socioeconomic characteristic $\bar{s}_j$, with higher values corresponding to more disadvantaged schools. Teachers are heterogeneous in their preferences over $\bar{s}_j$.

Teacher i chooses among schools to maximize utility:

$$U_{ij} = \alpha w_{ij} + \beta_i \bar{s}_j + \varepsilon_{ij} \tag{2}$$

where w_{ij} is the salary teacher i earns at school j, ε_{ij} is an idiosyncratic match component and $\alpha > 0$ is common to all teachers. The key parameter β_i captures teacher i's preference over SES: a negative β_i indicates a preference for less disadvantaged schools, and a positive β_i indicates a preference for serving more disadvantaged students. Consistent with prior literature, I assume β_i is negative on average.

Not all schools are equally feasible options for every teacher. Let $A_i \subseteq J$ denote the set of schools that are feasible matches for teacher i. Before TIA, information frictions limit A_i because teacher quality is not publicly observable to outside employers. After TIA, designation relaxes this friction and expands the feasible set for designated teachers.

Moving between schools is also costly. A teacher who leaves school j for school j' incurs a utility cost that depends on whether the move is to a school within $d(j)$ or outside of it. Specifically, she incurs cost $c_{jj'} = c_S + c_D$, where c_S is the cost incurred from switching schools, and c_D is the cost incurred from switching districts.

Teacher i, currently employed at school j, chooses the school that maximizes utility net of moving costs over her feasible set, which includes her current school:

$$\max_{j' \in A_i} U_{ij'} - c_{jj'}$$

4.2 Pre-TIA Environment

Before TIA, two features of the labor market prevent schools from using compensation to compete for effective teachers.

First, teacher compensation in Texas public schools is largely determined by a uniform district salary schedule based on experience and degree level. Schools within the same district cannot offer differential pay to attract or retain teachers, and effectiveness plays no role in salary determination. The pre-TIA salary for teacher i at school j is:

$$w_{ij} = \bar{w}_{d(j)}(exp_i, degree_i)$$

where $\bar{w}_{d(j)}(\cdot)$ is the salary schedule in district $d(j)$. Schools within a district cannot systematically offer higher pay to attract effective teachers to hard-to-staff campuses[2]. Schools serving more disadvantaged populations therefore cannot compensate teachers for less desirable working conditions through higher salaries.

Second, teacher quality is only imperfectly observed outside the current school or district. Principals may learn about a teacher's effectiveness on the job, but absent a credible and portable signal, that information is difficult to transmit across schools and even harder to transmit across districts. Highly effective teachers face constrained outside options even when other schools would value them highly. Quality is therefore only weakly reflected in both compensation and mobility opportunities[3].

4.3 Post-TIA Environment

TIA introduces two changes that affect these two channels simultaneously.

4.3.1 The Credentialing Channel

TIA creates a publicly observable, portable designation based on a composite evaluation of teacher effectiveness. This designation reduces information frictions by allowing schools to verify a teacher's quality through an official signal rather than informal or noisy proxies. Teacher quality moves from a private good known only within her network to a public credential that follows her across the labor market. Formally, designation expands the feasible set of schools A_i available to a teacher.

This channel can increase mobility even holding compensation fixed. Before TIA, a teacher may be unable to move to a preferred school simply because that school cannot distinguish her from other candidates. Designation relaxes this constraint, allowing teachers to act on preferences that were previously muted by limited observability. The credentialing channel therefore increases the extent to which preferences are expressed through mobility.

Importantly, the credentialing channel does not by itself determine the direction of sorting. It expands opportunities, but the direction of resulting moves depends on the distribution of β_i. If teachers on average prefer more advantaged schools, expanding mobility opportunities will tilt sorting toward those schools.

4.3.2 The Compensation Channel

TIA also introduces a salary supplement that varies with both designation level and school disadvantage. Designated teachers receive the allotment described in equation 1 layered on top of their current salary. The post-TIA salary for a designated teacher at designation level k is:

$$w_{ij} = \bar{w}_{d(j)}(exp_i, degree_i) + B_k + m_k \bar{s}_j \tag{3}$$

The base allotment B_k is earned at every school and does not affect school choice. What matters for sorting is the demographic supplement $m_k \bar{s}_j$, which varies across schools. Substituting equation 3 into equation 2, a designated teacher's utility at school j is:

$$U_{ij} = \alpha \bar{w}_{d(j)}(exp_i, degree_i) + \alpha B_k + (\beta_i + \alpha m_k)\bar{s}_j + \varepsilon_{ij}$$

[2]Some teachers in hard-to-staff subjects receive additional incentives, but these remain subject to the same experience- and degree-based schedule.

[3]The notable exception is Dallas ISD, one of the largest school districts in Texas, which started an incentive pay program in 2014 known as the Teacher Excellence Initiative (TEI). See Hanushek et al. (2026) for a thorough analysis.

The key object is the effective preference for disadvantage:

$$\beta_i^* = \beta_i + \alpha m_k$$

TIA shifts every designated teacher's effective preference toward disadvantaged schools by αm_k[4]. A teacher who was mildly averse to disadvantaged placements (β_i slightly below zero) may now have $\beta_i^* > 0$, tipping her school choice toward higher-need schools. Whether she moves also depends on the cost of moving.

Operating in isolation, the compensation channel pulls designated teachers toward more disadvantaged schools.

4.3.3 Interaction Between Channels

The two channels have different implications for both mobility and sorting. The credentialing channel expands outside options by relaxing information frictions, which can only increase mobility. The compensation channel changes the relative attractiveness of schools by raising pay more at disadvantaged campuses. It can increase mobility by drawing teachers toward higher-need schools, or reduce it by retaining teachers already there. The two channels also do not necessarily move teachers in the same direction. If teachers on average prefer more advantaged schools, the credentialing channel increases access to those schools, while the compensation channel pushes in the opposite direction by making disadvantaged schools financially more attractive. Whether TIA improves the distribution of teachers across schools depends on whether the pay premium attached to disadvantaged schools is large enough to offset teachers' underlying preferences for more advantaged environments.

4.4 Sorting Implications

Consider a designated teacher i deciding whether to move from her current school j to another school j'. She moves if:

$$\alpha(w_{ij'} - w_{ij}) + \beta_i(\bar{s}_{j'} - \bar{s}_j) + (\varepsilon_{ij'} - \varepsilon_{ij}) > c_{jj'} \tag{4}$$

The first term reflects the post-TIA salary structure from equation 3. For a designated teacher at a lower-need school contemplating a move to a higher-need school ($\bar{s}_{j'} > \bar{s}_j$), the base allotment B_k is earned at both schools and cancels out, while the demographic supplement increases, providing a financial incentive to move. The second term captures preferences: a teacher with $\beta_i > 0$ gains utility from moving to a higher-need school, reinforcing the salary incentive, while a teacher with $\beta_i < 0$ loses utility from the same move. If her aversion to disadvantage is strong enough, the financial incentive will not be enough to induce the move. Even when the gain from moving is positive, she stays unless it exceeds the cost of moving.

4.4.1 Cross-District Sorting

A teacher considering a move to another district must weigh both the base salary and the TIA supplement. Let $\Delta s = \bar{s}_{j'} - \bar{s}_j$ and $\Delta w = \bar{w}_{d(j')}(exp_i, degree_i) - \bar{w}_{d(j)}(exp_i, degree_i)$, where neither difference is restricted in sign. Abstracting from the idiosyncratic term, equation 4 becomes:

$$\beta_i^* \Delta s + \alpha \Delta w > c_S + c_D \tag{5}$$

[4]In practice, districts retain a share of the allotment and may distribute the remainder differently across designated teachers. However, allotment amounts are publicly reported before any district adjustments, and teachers likely make mobility decisions based on the reported figure rather than the realized amount. The empirical analysis uses the publicly reported allotment.

The left-hand side is her gain from moving. The supplement contributes $\alpha m_k \Delta s$ to this gain, which is positive for a move to a higher-need school and negative for a move to a lower-need school. A designated teacher moves if and only if her gain from moving exceeds the cost of moving.

For a move toward a higher-need school ($\Delta s > 0$), rearranging equation 5, she moves if $\beta_i \Delta s > c_S + c_D - \alpha(\Delta w + m_k \Delta s)$. Her own preference for SES must cover whatever part of the moving cost the pay gain does not. The supplement lowers this threshold by $\alpha m_k \Delta s$. If the pay gain is smaller than the cost of moving, $\alpha(\Delta w + m_k \Delta s) < c_S + c_D$, the right-hand side is positive, so only teachers who already prefer higher-need schools ($\beta_i > 0$) make the move.

The moving costs also create a retention range. Reversing the direction of a move flips the sign of the gain but not the cost, so teachers at either school stay where they are when

$$-(c_S + c_D) \leq \beta_i^* \Delta s + \alpha \Delta w \leq c_S + c_D \tag{6}$$

I call this the retention range. A teacher stays even if she values the other school more, as long as her gain from moving does not exceed the cost of moving. Because the supplement raises every designated teacher's effective preference by αm_k, some teachers at the more disadvantaged schools who would have left before TIA now stay.

4.4.2 Within-District Sorting

Within a district, the base salary schedule cancels, so $\Delta w = 0$ and the moving cost includes only c_S. The moving condition in equation 5 reduces to:

$$\beta_i^* \Delta s = (\beta_i + \alpha m_k) \Delta s > c_S$$

Sorting within a district is therefore determined by the TIA demographic component and the within-district moving cost. A designated teacher moves toward a more disadvantaged school if and only if $\beta_i^* \Delta s$ exceeds c_S. A teacher with $0 < \beta_i^* \Delta s \leq c_S$ prefers the higher-need school, but not by enough to cover the moving costs. If $\alpha m_k \Delta s < c_S$, only teachers who already prefer higher-need schools make the move. The retention range becomes $-c_S \leq \beta_i^* \Delta s \leq c_S$.

5 Data

I use restricted-access administrative data accessed through the University of Houston Education Research Center (UHERC). Within the UHERC I observe teacher- and student-level data from the Texas Education Agency (TEA), covering all Texas public K-12 schools from the 2011–2012 through 2024–2025 school years. For ease of reading I refer to school years by the fall semester, so the 2011–2012 school year is the 2011 school year. The teacher-level data report a fall snapshot of each teacher's campus assignment, base and total salary, education level, and demographics, including sex and race/ethnicity. The student-level data report a similar fall snapshot, demographics, and state test scores, with student-teacher links. I also use administrative data from the TIA program, available for the 2019 through 2024 school years. The TIA-specific data include (1) school-level allocation amounts for each designation level, (2) designation records identifying designated teachers, their designation level, and corresponding allotments, and (3) district TIA applications containing the allocation sharing percentages. I merge the TIA and teacher-level datasets using

unique teacher and campus identifiers to construct an unbalanced teacher-year panel spanning the 2011 to 2024 school years. The unbalanced structure reflects natural entry and exit from teaching over this period.

5.1 TIA Rollout

Table 2 summarizes TIA adoption across district cohorts. Districts opted into the program on a rolling basis beginning in 2019, with adoption rapidly expanding after 2021. By the end of the period I study, 451 of roughly 1,200 Texas districts had adopted TIA, with 757 remaining as never-treated. Early adopters represent substantially larger districts and serve more low-income students. This is likely due to larger districts already having systems in place to evaluate teachers according to TIA standards while smaller districts needed time to develop such systems. The 2019 and 2020 cohorts had average student enrollments of 16,482 and 14,356, respectively, substantially larger than the 2024 cohort and never-treated districts, which had average enrollments of 7,420 and 3,257. The share of FRPL students follows a similar pattern, with 72.3% and 73.0% in the 2019 and 2020 cohorts, respectively, compared with 62.6% and 58.4% in the 2024 and never-treated districts.

The employee composition of early-adopting districts reflects these demographic differences. The 2019 and 2020 cohorts employ substantially higher shares of Hispanic teachers (31.9% and 51.5%, respectively) than the 2024 and never-treated districts (21.7% and 15.9%, respectively). Similarly, the 2019 cohort had significantly higher shares of Black employees (16.2%), nearly double that of any other cohort. This pattern has implications for the composition of the analysis sample.

District Cohort	2019	2020	2021	2022	2023	2024	Never Treated
Student Enrollment	16,482	14,356	7,056	6,981	5,611	7,420	3,257
	(33,030)	(19,129)	(12,600)	(14,013)	(9,674)	(19,088)	(11,589)
Share of FRPL Students	0.723	0.730	0.670	0.686	0.632	0.626	0.584
	(0.154)	(0.150)	(0.183)	(0.179)	(0.202)	(0.159)	(0.212)
Employee Count	1,696	1,434	706	710	583	748	332
	(4879)	(1055)	(1071)	(1197)	(830)	(1570)	(1507)
Share of White Employees	0.470	0.356	0.620	0.610	0.695	0.703	0.745
	(0.271)	(0.203)	(0.273)	(0.300)	(0.281)	(0.257)	(0.259)
Share of Black Employees	0.162	0.073	0.091	0.086	0.076	0.053	0.074
	(0.185)	(0.065)	(0.115)	(0.131)	(0.121)	(0.072)	(0.167)
Share of Hispanic Employees	0.319	0.515	0.267	0.286	0.208	0.217	0.159
	(0.289)	(0.267)	(0.288)	(0.294)	(0.274)	(0.242)	(0.198)
Number of Districts	26	6	39	144	134	102	757
Cumulative Treated Districts	26	32	71	215	349	451	—
Total Districts in State	1202	1204	1207	1209	1207	1208	—

Notes: Means across districts within cohort; standard deviations in parentheses. District cohort is defined by the first year the district participated in TIA. Districts also include charter school systems. Never-treated districts are those with no TIA participation through the end of the observation window.

Table 2: TIA Rollout by District Cohort

5.2 Sample

I refine the full dataset to construct an analysis sample suited to the relevant policy question. The question of interest is not whether these incentives induce the movement of highly effective teachers relative to the general teaching population, but whether they induce movement relative to similarly effective teachers. I therefore restrict the sample in two major ways.

First, I exclude teachers who were ever reported as being employed at multiple campuses within the same year. I do this as it is conceptually difficult to define a move when a teacher has more than one origin campus. Second, I restrict the sample to teachers who will eventually receive a TIA designation at some point during the study period. Table 3 presents the number of teachers newly designated each year, as well as the cumulative count of designated teachers over time. I use teachers first designated in 2024 as the control group. Because their designation occurs in the final observed year and mobility outcomes are forward-looking, there are no post-treatment outcomes for this group of teachers. They serve as a natural comparison group of similarly effective teachers who remain untreated within the study window.

After applying these refinements, the final analysis sample consists of 329,984 teacher-year observations[5], with 213,794 treated observations and 116,190 untreated observations.

Year	New Teachers Designated	Total Teachers Designated	Total Teachers in State
2019	3,631	3,631	301,626
2020	602	4,233	308,487
2021	1,507	5,740	310,712
2022	6,680	12,420	315,042
2023	11,797	24,217	320,470
2024	15,239	39,456	320,191

Notes: Counts reflect teachers newly designated in each year and the running cumulative total. The 2024 cohort serves as the control group in the analysis; because designation occurs in the final observed year, there are no post-treatment mobility outcomes for this group.

Table 3: New and Cumulative TIA Designations

Table 4 reports the summary statistics for treated teachers, the control group, and the statewide teacher population. Overall, the treated and control groups are quite similar along observable characteristics, which is consistent with the control group being composed of teachers who are designated just outside the observation window.

In terms of demographics, both treated and control teachers are predominantly female, with shares of 85.8% and 86.4%, respectively, compared to 78.8% statewide. The racial composition of the treated and control groups is also broadly similar to each other, though both differ somewhat from the statewide population in ways that align with the district adoption pattern described above. Because early adopters were disproportionately large urban districts with more diverse workforces, treated teachers are less likely to be White (45.9% versus 58.5% statewide) and more likely to be Hispanic (39.9%) or Black (11.8%). The control group mirrors this pattern, as 2024-adopting districts are drawn from a similar pool, though control teachers are somewhat less likely to be Black (7.6%). Treated teachers are also slightly more experienced than control teachers (10.7 versus 9.3 years) and have modestly higher base salaries ($53,618 versus $52,384). The primary compensation difference is the TIA allotment itself, which averages approximately $10,300 among treated teachers. This represents a substantial increase relative to baseline salary levels and highlights the potential for the program to meaningfully alter teachers' financial incentives.

[5]The observation count differs from that reported in the results tables because mobility outcomes are coded as missing in the final observed year.

	Treated	Control	Statewide
Female	0.858	0.864	0.788
	(0.349)	(0.343)	(0.408)
White	0.459	0.480	0.585
	(0.498)	(0.500)	(0.493)
Black	0.118	0.076	0.114
	(0.322)	(0.266)	(0.318)
Hispanic	0.399	0.426	0.281
	(0.490)	(0.494)	(0.450)
Years of Experience	10.710	9.285	10.934
	(8.222)	(7.506)	(9.211)
Base Annual Salary	53,618	52,384	54,850
	(10,564)	(10,502)	(10,202)
Total Annual Salary	54,676	53,340	56,152
	(11,135)	(11,035)	(10,887)
Allotment Amount	10,322	—	—
	(6,048)	—	—
N	213,794	116,190	4,174,761

Notes: Standard deviations in parentheses. Treated teachers are those designated in the years 2019, 2020, 2021, 2022, and 2023. Control teachers are those designated in 2024. The statewide population is measured after removing teachers who teach at multiple campuses in a year.

Table 4: Summary Statistics

To examine the effect of designation on teacher mobility, I construct forward-looking outcome variables: a teacher's outcome in period t is defined by her origin campus in t and her destination campus in $t+1$. A teacher is classified as having moved if her destination campus differs from her origin campus.

In addition to estimating the overall effect of TIA on mobility, I examine how teachers sort into different types of campuses by estimating heterogeneous effects by campus-level SES and rural classification.

I use the Average Student Point Value (ASPV) to classify campuses by their SES status. With the data available, I could back out the actual ASPV used in the allotment calculation. However, this value includes an adjustment for the rural status of the school, which is not possible to account for with the data I have. I use a value that has been adjusted by the TEA to remove the rural adjustment of the ASPV. This provides a measure that is based solely on the SES of the schools' student population. Because of how this measure is constructed, it can be thought of as a function of both the number of free and reduced-price lunch students and the socioeconomic conditions of the neighborhoods that feed into the school. However, because this score is only observed for the years in which TIA is implemented, I compute a time-invariant measure by averaging values across available years. This approach relies on the assumption that campus-level socioeconomic composition remains relatively stable in the short term. Using the share of FRPL students as a proxy for campus SES, between-campus variation accounts for about 93% of the total variation (between-campus SD = 0.245; within-campus SD = 0.067), supporting the use of a time-invariant measure of campus SES.

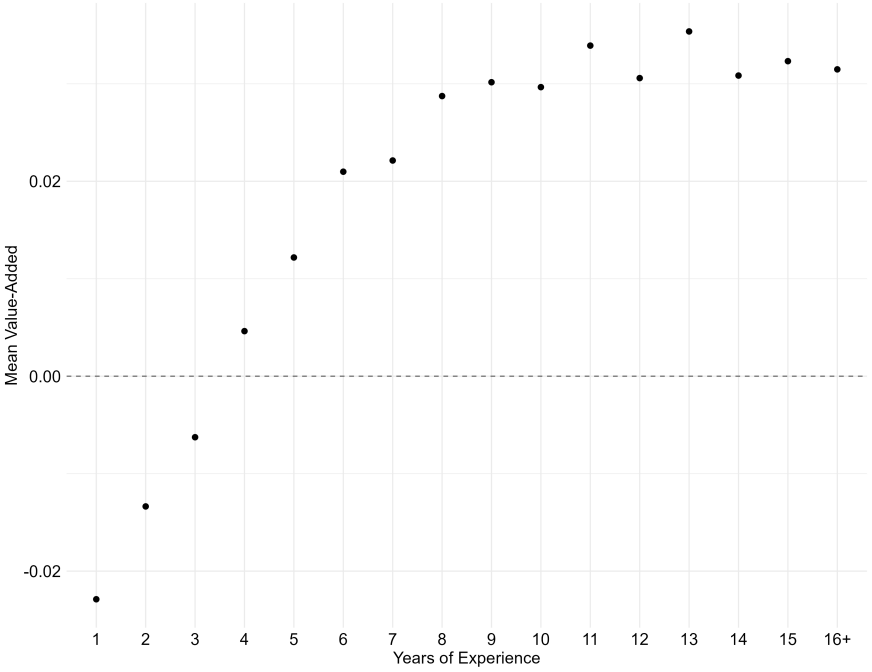
Using the distribution of ASPV across campuses, I assign each campus a percentile rank. Because ASPV is increasing in campus disadvantage, a rank of 100 corresponds to the lowest-income campuses and a rank

of 1 to the highest-income campuses. I classify campuses in the top quartile (most disadvantaged) of the distribution as low-SES campuses and those in the bottom quartile (most advantaged) as high-SES campuses. I then define two quartile-based mobility outcomes: a teacher is classified as moving to a high-SES campus if her origin campus is not high-SES and her destination campus is high-SES, and similarly for moves to a low-SES campus. Additionally, I use the percentile ranks of teachers' origin and destination campuses to construct ΔSES = destination percentile − origin percentile, where $\Delta SES > 0$ indicates a move toward a more disadvantaged campus and $\Delta SES < 0$ a move toward a higher-income campus. Because ASPV is time-invariant, stayers have a $\Delta SES = 0$. Together, these measures capture sorting patterns across the full SES distribution, including moves between non-extreme campuses that the quartile indicators alone would miss.

6 Descriptive Evidence on Pre-TIA Teacher Mobility

Before moving into the causal analysis, I document stylized facts on teachers and their mobility in Texas prior to TIA. To look at the pre-TIA environment, I pool all teacher observations from the 2014 through the 2018 school years. I disaggregate mobility outcomes by observed teacher value-added, which I construct following methods from Chetty et al. (2014a) and Jackson (2018). I provide a detailed overview of how I construct this measure in Appendix A. Note that I can only construct value-added measures for a subset of teachers, specifically 4th through 8th grade reading and math teachers. To extend the descriptive analysis to all teachers regardless of subject, I also report results that proxy quality with experience. Figure 1 shows the well-documented fact that teacher quality is increasing and concave in experience (Rockoff, 2004; Papay & Kraft, 2015). I partition teachers into four value-added quartiles and four experience bins: early-career (0–3 years), early-mid career (4–9 years), mid-career (10–20 years), and senior (21+ years).

Figure 1: Teacher Value-Added by Years of Experience

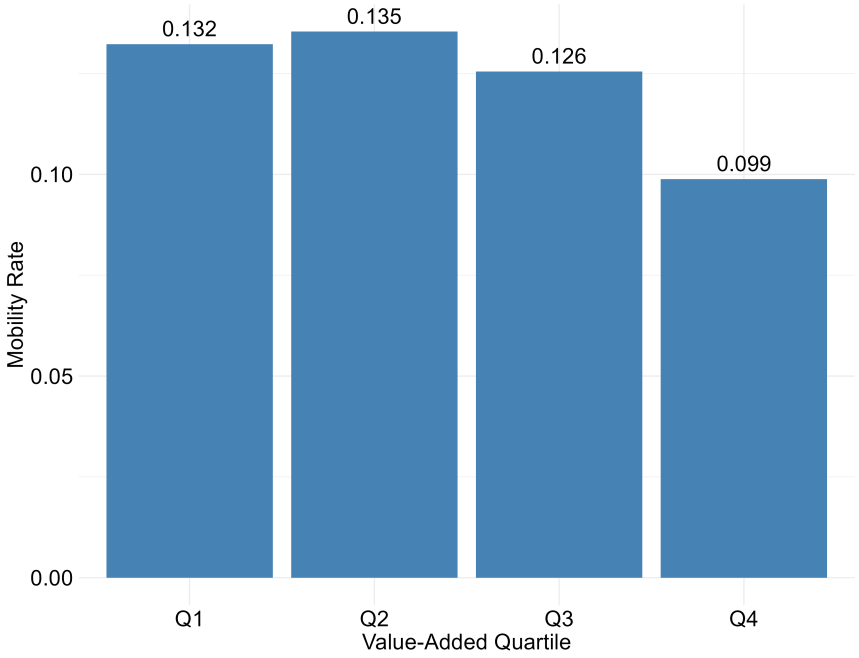


Notes: This figure reports mean teacher empirical Bayes shrunken value-added estimates by years of experience.

6.1 Baseline Mobility Rates

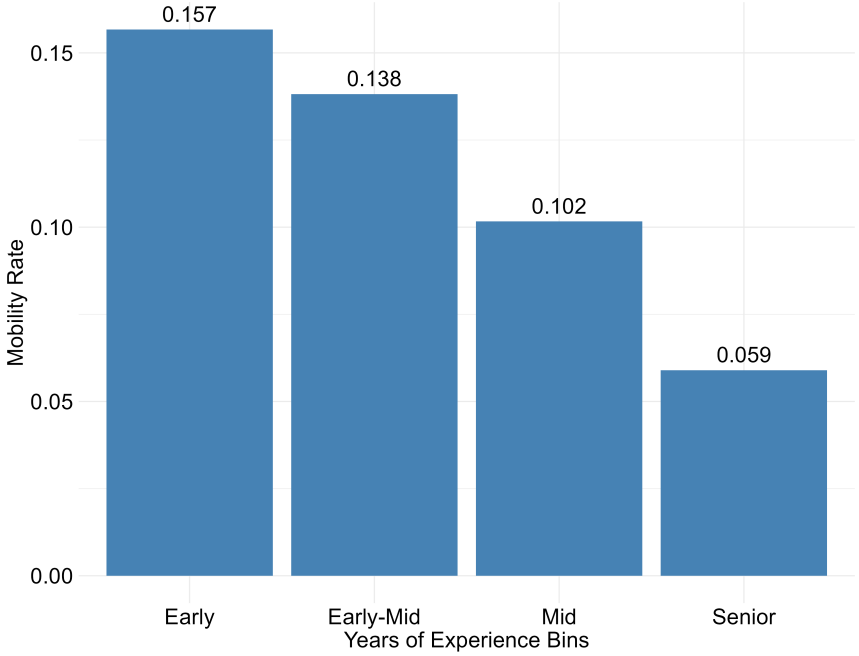
Figure 2 shows that higher value-added teachers are less mobile: teachers in the top quartile of the value-added distribution are approximately three percentage points less likely to move than those in the bottom quartile. The pattern is slightly more pronounced when teachers are partitioned by experience, which I show in figure 3. Early-career teachers move at a rate of approximately 16% annually, declining to around 6% among senior teachers. These differences establish that both quality and experience are negatively associated with mobility in the pre-TIA environment, providing context for interpreting any treatment-induced changes.

Figure 2: Teacher Mobility by Value-Added Quartile



Notes: This figure reports pre-TIA teacher mobility rates by teacher value-added quartile, where Q1 denotes the lowest quartile of value-added and Q4 denotes the highest quartile. Mobility is defined as teaching in a different campus in the following academic year.

Figure 3: Teacher Mobility by Experience Level

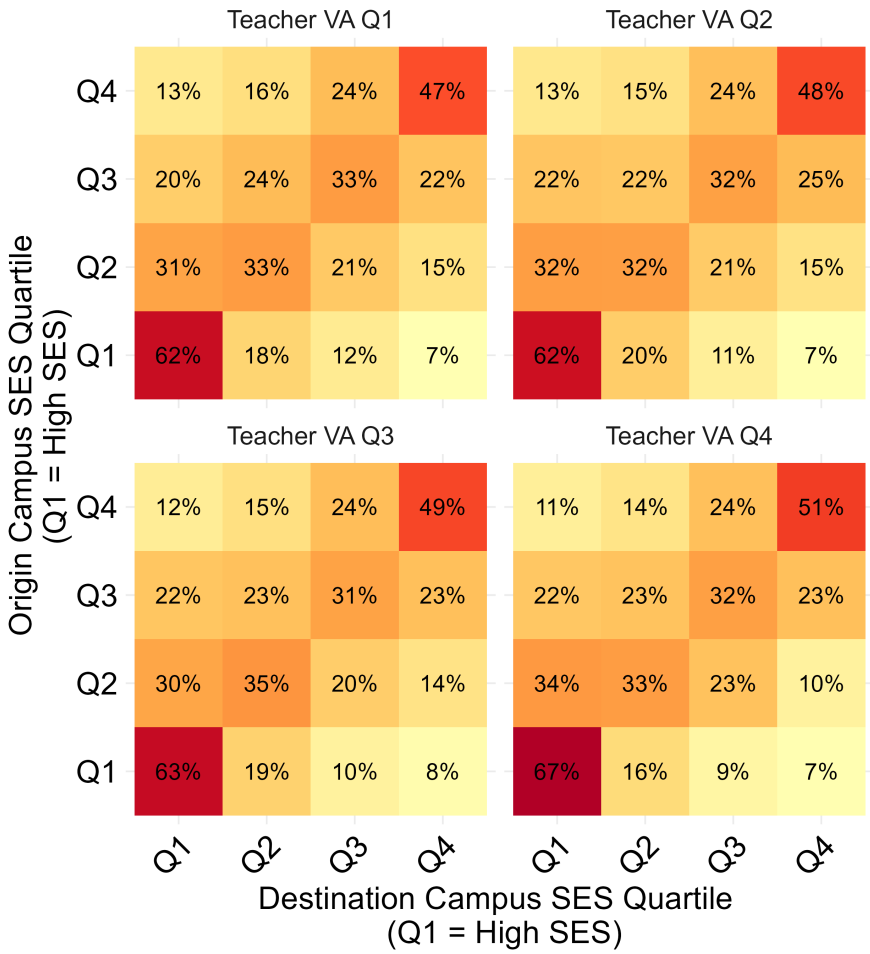


Notes: This figure reports pre-TIA teacher mobility rates by teacher experience bins. Mobility is defined as teaching in a different campus in the following academic year.

6.2 Sorting Patterns Across Campus SES

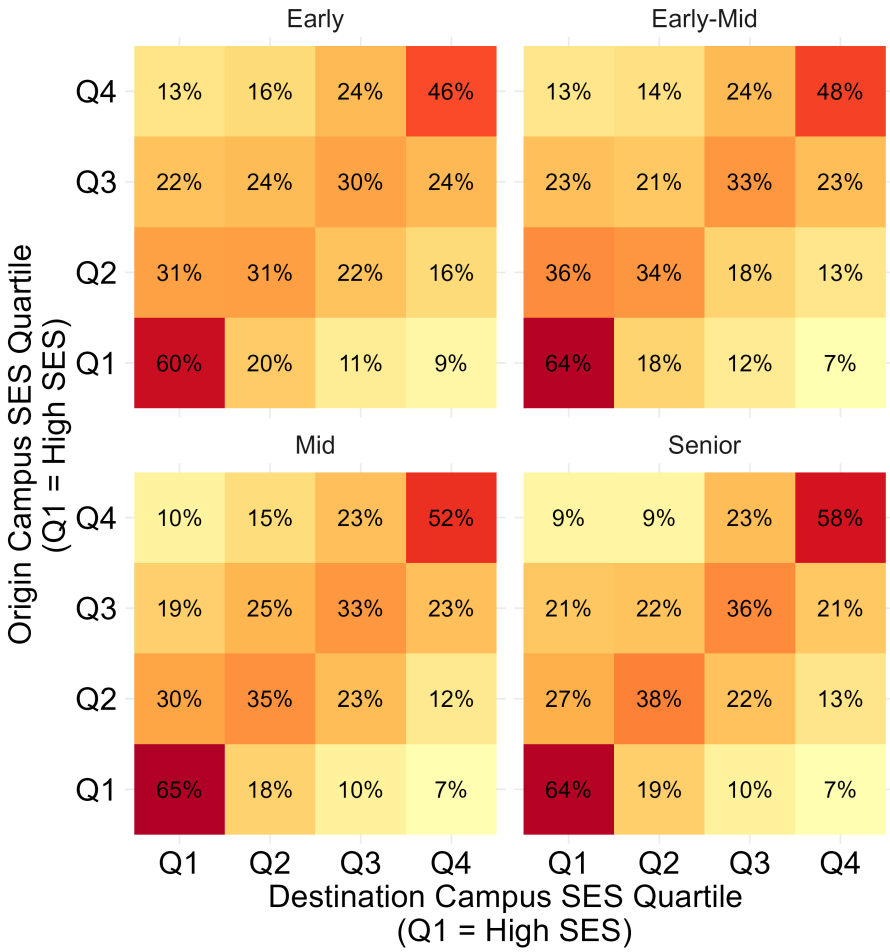
Figure 4 shows substantial persistence in the income distribution of schools teachers move between. Teachers who begin in lower-income campuses are disproportionately likely to move to other lower-income campuses, while teachers who begin in higher-income campuses are disproportionately likely to move to other higher-income campuses. These transition patterns are broadly similar across teacher value-added quartiles, suggesting that sorting across campus SES does not vary sharply by measured teacher quality. Figure 5 shows a similar pattern when moves are disaggregated by experience bin, although persistence appears somewhat weaker at the top of the campus income distribution.

Figure 4: Mobility Patterns Across Campus SES Quartiles by Teacher Value-Added



Notes: This figure reports transition rates between origin and destination campus socioeconomic status (SES) quartiles for teachers who changed campuses prior to TIA implementation, separately by teacher value-added quartile. Campus SES quartiles are based on the school SES measure, where Q1 denotes the highest-SES (most advantaged) campuses and Q4 denotes the lowest-SES (most disadvantaged) campuses. Rows indicate the origin campus quartile, and columns indicate the destination campus quartile.

Figure 5: Mobility Patterns Across Campus SES Quartiles by Teacher Experience



Notes: This figure reports transition rates between origin and destination campus socioeconomic status (SES) quartiles for teachers who changed campuses prior to TIA implementation, separately by teacher experience. Campus SES quartiles are based on the school SES measure, where Q1 denotes the highest-SES (most advantaged) campuses and Q4 denotes the lowest-SES (most disadvantaged) campuses. Rows indicate the origin campus quartile, and columns indicate the destination campus quartile.

6.3 Replacement Patterns at Low-SES Campuses

In addition to understanding how teachers move, it is also important to understand who replaces teachers when they leave. I focus specifically on when teachers leave low-SES campuses. In Tables 5 and 6, I examine the distribution of value-added and experience of teachers entering low-SES campuses in the year after a teacher exits. Entrants are defined as teachers who were not in the same campus in the prior year, so these tables describe the distribution of incoming teachers at the campus-year level rather than exact one-for-one replacements.

Table 5 shows that the value-added distribution of incoming teachers is broadly similar whether low-SES schools are replacing any departing teacher or specifically a high-value-added departing teacher. When any teacher leaves, .265 of entrants are low-value-added (Q1) and .214 are high-value-added (Q4); when the departing teacher is high-value-added, those shares are .257 and .228, respectively. The middle of the

distribution changes very little across the two cases, suggesting only modest differences in the value-added composition of incoming teachers following the exit of a high-value-added teacher relative to teacher turnover more generally.

Entrant VA Quartile	Any Teacher Leaves	High-VA Teacher Leaves
Q1	.265	.257
Q2	.279	.275
Q3	.242	.239
Q4	.214	.228

Table 5: Value-Added Quartile of Incoming Teachers Following Teacher Exit in Low-SES Schools

Notes: This table reports the value-added quartile distribution of teachers entering a school in the year after a teacher leaves a low-SES school. The table separately considers departures by high-value-added teachers (Q4) and departures by any teacher, regardless of value added. Entrants are defined as teachers who were not in that same school in the prior year, so the estimates capture the composition of incoming teachers at the school-year level rather than exact one-for-one replacements. Shares may not sum to 1 exactly because of rounding.

Table 6 shows a similarly modest pattern by experience. Most incoming teachers are early-career regardless of which teacher exits, though schools losing a mid- or senior-career teacher receive slightly more experienced entrants on average. When any teacher leaves, .506 of entrants are early-career, .207 are mid-career, and .077 are senior; when a mid- or senior-career teacher leaves, those shares are .488, .214, and .079, respectively. Taken together, the two tables suggest that replacement patterns in low-SES schools differ only slightly when the departing teacher has a high value-added or is more experienced.

Entrant Experience Category	Any Teacher Leaves	Mid or Senior Teacher Leaves
Early	.506	.488
Early-Mid	.210	.219
Mid	.207	.214
Senior	.077	.079

Table 6: Experience Category of Incoming Teachers Following Teacher Exit in Low-SES Schools

Notes: This table reports the experience-category distribution of teachers entering a school in the year after a teacher leaves a low-SES school. The table separately considers departures by mid- or senior-career teachers and departures by any teacher, regardless of experience. Entrants are defined as teachers who were not in that same school in the prior year, so the estimates capture the composition of incoming teachers at the school-year level rather than exact one-for-one replacements. Shares may not sum to 1 exactly because of rounding.

6.4 Pre-Treatment Outcome Means

Table 7 shows that treated and control teachers exhibit similar baseline mobility patterns, supporting the validity of the comparison group. About 9.8% of treated teacher-year observations involve a campus move in the following year, compared with 10.6% for controls. This provides a useful baseline for interpreting the magnitude of the treatment effects, since any changes in mobility should be evaluated relative to this underlying rate.

Movement across the SES distribution is small in magnitude but directionally informative. The unconditional mean of ΔSES is -0.471 percentile points for treated teachers and -0.725 for controls. Because

ΔSES is zero by construction for the roughly 90% of teacher-years that do not involve a move, these averages are mechanically compressed. Conditional on moving, treated teachers shift roughly 4.8 percentile points toward more advantaged campuses ($-0.471/0.098 \approx -4.81$), while control teachers shift 6.8 points ($-0.725/0.106 \approx -6.84$). Both groups exhibit upward SES sorting among movers, with the difference between them small relative to the standard deviations of the unconditional measure. Moves into the tails of the SES distribution are rare for both groups: roughly 1.1 to 1.6% of teacher-year observations involve a move to a high-SES campus and 1.1% to a low-SES campus. Moves to rural campuses are similarly uncommon, at 1.6% in both groups.

Finally, when teachers do move, those moves tend to be geographically local. About 5.0% of treated and 4.5% of control teacher-year observations involve a within-district move, 7.0% in both groups within-county, and 8.0% and 8.4% within TEA region. Relative to the overall mobility rate, this implies that most moves occur within geographically proximate areas, with roughly half being within-district transfers. This pattern holds for both groups, suggesting that teacher mobility operates mainly within local labor markets rather than across more distant locations.

Variable	Treated	Control
Move Campus	0.098	0.106
	(0.298)	(0.308)
ΔSES	-0.471	-0.725
	(8.973)	(9.916)
Move to High-SES Campus	0.011	0.016
	(0.106)	(0.125)
Move to Low-SES Campus	0.011	0.011
	(0.106)	(0.102)
Move to Rural Campus	0.016	0.016
	(0.125)	(0.126)
Move from High-SES Campus	0.006	0.008
	(0.080)	(0.091)
Move from Low-SES Campus	0.018	0.019
	(0.132)	(0.136)
Move from Rural Campus	0.017	0.019
	(0.130)	(0.135)
Move within School District	0.050	0.045
	(0.218)	(0.207)
Move within County	0.070	0.070
	(0.255)	(0.255)
Move within TEA Region	0.080	0.084
	(0.271)	(0.278)
N	183,528	113,781

Standard deviations in parentheses. Values for treated teachers are derived using all observed periods prior to treatment. Values for control teachers are derived using all observations of the 2024 group of designated teachers.

Table 7: Baseline Means of Dependent Variables for Treated and Control Teachers

7 Effect of Designation on Teacher Mobility

7.1 Event-Study Difference-in-Differences

In order to understand how designation affects the sorting behavior of teachers, I first look at whether receiving a designation makes a teacher more likely to move schools. I then look at the types of schools that designated teachers move into, focusing on whether they are more likely to move into low-SES schools, high-SES schools, or rural schools.

My empirical strategy exploits two sources of variation: whether a teacher ever becomes designated and the timing of designation. This setup allows me to compare designated teachers to themselves before and after designation, while using the last set of designated teachers as the comparison group. Formally I use the following event-study difference-in-differences:

$$y_{it} = \alpha_i + \lambda_t + \sum_{\tau=-5}^{4} \delta_\tau \, 1\{t - (D_i - 1) = \tau\} + X_{it}\beta + \varepsilon_{it} \tag{7}$$

where y_{it} denotes an outcome for teacher i in year t, which is associated with the teacher's placement in period $t+1$. The vector X_{it} contains the teacher's years of experience. I include teacher fixed effects (α_i) and year fixed effects (λ_t), and I cluster standard errors at the teacher level. The coefficients of primary interest, $\delta_{\tau \geq 0}$, capture how the probability of each outcome changes in the years after designation, relative to similarly effective teachers.

For the estimates of δ_τ to be valid, two assumptions must be satisfied. First, teachers must not anticipate treatment (no anticipatory effects). Second, designated and control teachers should follow similar mobility patterns prior to designation (parallel trends). Anticipation is a particular concern in this setting because teachers are evaluated in the year prior to designation and must remain in their current district through the first year of designation in order to receive that first allotment. Therefore, I define $\tau = 0$ as the year a teacher is evaluated and $\tau = 1$ as the first year of their designation. Because teachers must remain in their current district through the designation year, I expect a mechanical decline in mobility at $\tau = 0$, with any behavioral response to designation appearing at $\tau = 1$ or later.

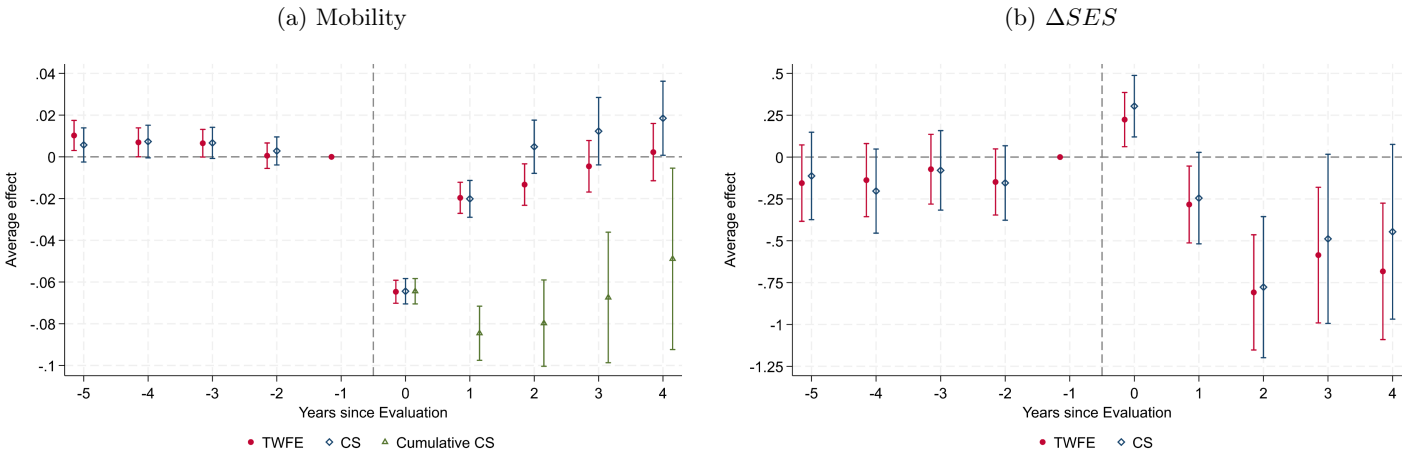
Given the lock-in mechanism described above, I expect treatment effects to evolve over time. Mobility is mechanically suppressed at $\tau = 0$ and may increase as teachers respond to designation in subsequent periods. Estimating dynamic effects also addresses concerns regarding the staggered nature of designation. In staggered settings, two-way fixed effects (TWFE) estimators can assign negative weights to certain observations when aggregating treatment effects (de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021; Callaway & Sant'Anna, 2021), an issue that is especially problematic when treatment effects are heterogeneous over time.

To address these concerns, my primary specification uses the estimator proposed by Callaway & Sant'Anna (2021). This estimator computes group-time average treatment effects for each cohort of newly designated teachers, using the last-designated cohort as the comparison group, and then aggregates these into event-study estimates. I also report TWFE estimates as a robustness check. In all specifications I omit δ_{-1} so that treatment effects are measured relative to the year prior to evaluation.

7.2 Results

I present the event-study estimates in three figures. The first reports the effect of designation on overall teacher mobility and on the net change in campus SES, measured by ΔSES. The second reports destination- and origin-specific sorting at the tails of the campus SES distribution, separating moves into and out of high-SES and low-SES campuses. The third reports the corresponding results for rural campuses. Estimates for the geographic scope of mobility are reported in Appendix Figure C.2. As defined earlier in the paper, the destination- and origin-specific measures are transition outcomes; for example, moving to a low-SES campus is defined for teachers whose origin campus is not low-SES and whose destination campus is low-SES. For completeness, all Callaway & Sant'Anna (2021) event-study estimates are reported in Appendix Tables B.2, B.3, and B.4.

Figure 6: Estimated Effects of Designation on Mobility and ΔSES



Notes: Time period $t = 0$ represents the school year in which a teacher is evaluated for designation. Error bars represent 95% confidence intervals. Red circles represent two-way fixed effects estimates. Blue diamonds represent Callaway & Sant'Anna (2021) estimates. Green triangles represent cumulative sums of the Callaway & Sant'Anna (2021) event-time estimates, such that the estimate at event time t equals $\sum_{\tau=0}^{t} \hat{\delta}_\tau$. The outcome in Panel A is an indicator for being observed at a different campus in $t+1$ relative to period t. The outcome in Panel B is defined as ΔSES = destination percentile − origin percentile, where $\Delta SES < 0$ indicates a move to a more advantaged campus.

Panel A of Figure 6 reports the estimated effects of designation on overall teacher mobility. In period 0, the probability of changing campuses in the following year falls by 6.4 percentage points (pp). This decline is mechanical, not behavioral. Teachers must remain in their district to receive the designation, which temporarily compresses between-district mobility. Within-district moves do not decline in period 0, as I show in Panel A of Figure C.2, providing additional evidence that the result reflects the institutional rule rather than a broader change in willingness to move.

Mobility remains suppressed in the first designation year, falling 2.0 pp in period 1, and then recovers. Designated teachers are 0.5 pp more likely to change campuses in period 2 and 1.2 pp more likely in period 3, neither statistically significant, with the effect reaching 1.9 pp in period 4. Against a baseline annual mobility rate of roughly 10%, these are modest increases. One possibility is that the lock-in period simply postpones moves rather than suppressing them. The cumulative estimates suggest otherwise. Cumulative mobility remains negative at every horizon, reaching −4.9 pp by period 4, implying a net decline in mobility over the full post-designation period. Taken together, these results suggest that the recovery in mobility after the temporary constraint is lifted does not offset the moves foregone during the lock-in year.

Panel B reports the estimated effects on ΔSES, the net change in campus SES percentile between origin and destination. The period 0 estimate is positive and statistically significant. This would suggest treated teachers move to more disadvantaged campuses during their lock-in period. However, the lock-in is what is primarily driving this result. This estimate is a mechanical consequence of the sharp mobility decline during the lock-in year shown in Panel A.

Because ΔSES is destination campus SES percentile minus origin campus SES percentile, stayers contribute 0 by construction, and the group mean at any event time can be written as $\bar{X}_{g,t} = p_{gt} m_{g,t}$, where $p_{g,t}$ is the mobility rate and $m_{g,t} = E[\Delta SES \mid Moving]$. In the raw pre-treatment data, as I showed earlier, treated teachers have $E[\Delta SES \mid Moving] \approx -4.81$. The period 0 difference-in-differences estimate can be written as:

$$\delta_0^{\Delta SES} = \left[\bar{X}_{Treated,0} - \bar{X}_{Treated,-1}\right] - \left[\bar{X}_{Control,0} - \bar{X}_{Control,-1}\right]$$

Substituting for $\bar{X}_{g,t}$ using the expression above and assuming that (1) the average ΔSES among treated teachers who move is unchanged between period -1 and period 0 ($m_{T,0} = m_{T,-1} = m_T$) and (2) that controls are stable in both mobility and destination changes $\left(\left[\bar{X}_{Control,0} - \bar{X}_{Control,-1}\right] = 0\right)$, then I can rewrite $\delta_0^{\Delta SES} = (p_{T,0} - p_{T,-1}) m_T$. Using a similar set of arguments, I can show that $\delta_0^{move} = (p_{T,0} - p_{T,-1})$. Plugging in values, I get:

$$\delta_0^{\Delta SES} \approx \delta_0^{move} m_T = -0.064 \times -4.81 \approx 0.308$$

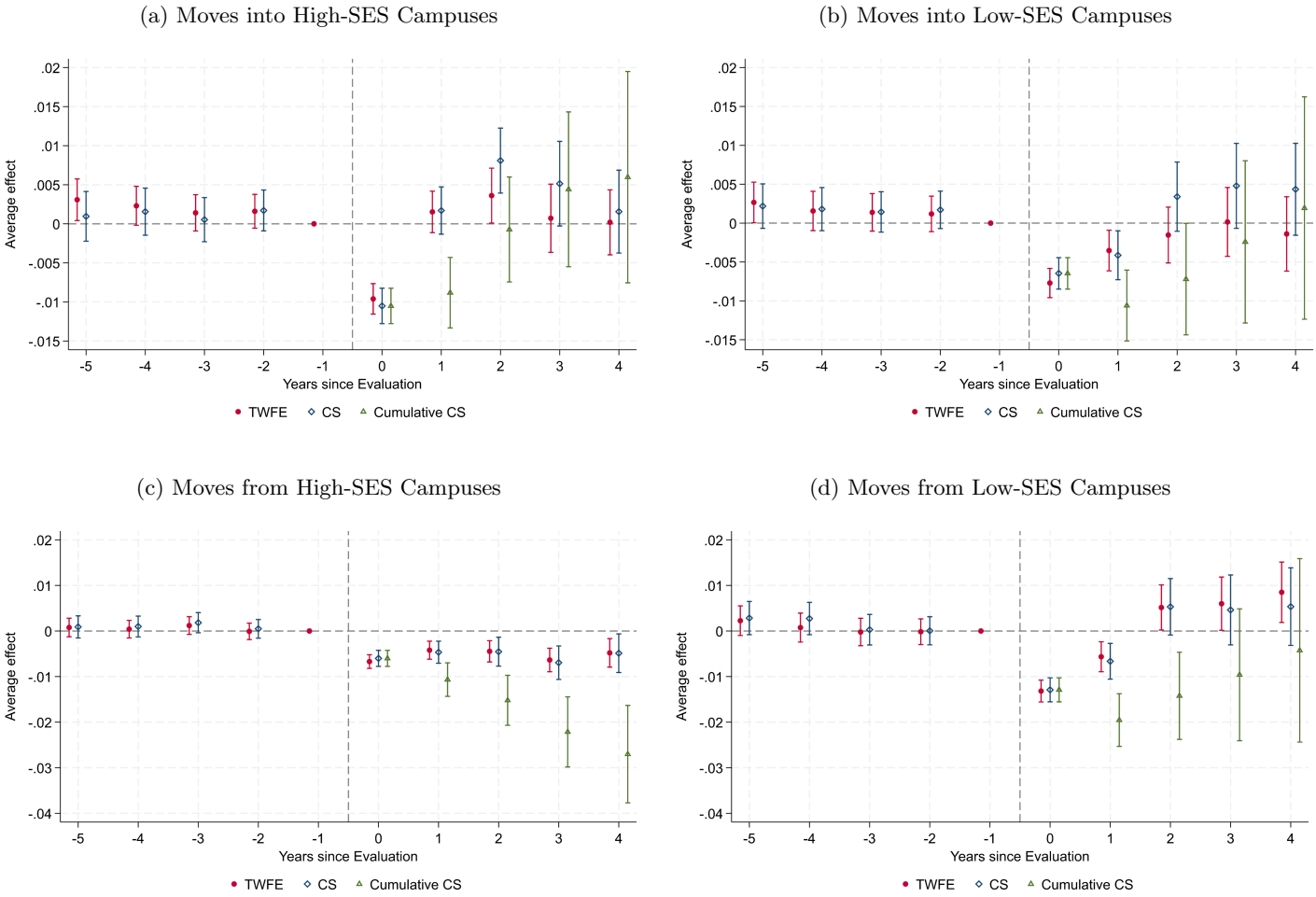
The predicted value is within 2% of the observed period 0 estimate of 0.304, indicating that the positive coefficient is almost entirely driven by suppressed mobility rather than a meaningful change in the SES of campuses treated teachers choose. Because of this, I treat the period 0 estimate as a mechanical result rather than a separate finding.

After the mechanical period 0 result, the estimated effects on ΔSES are negative in every post-designation period, ranging from -0.245 to -0.777 percentile points. The period 2 estimate is significant at the 1% level, while the estimates for periods 1, 3, and 4 are significant only at the 10% level. After designation, treated teachers move on average to more advantaged campuses relative to the comparison group. This measure is unconditional on moving and therefore attenuated by the mass of zeros from teachers who stay put; sorting among actual movers is larger than the unconditional coefficients suggest[6].

Taken together, Figure 6 does not raise campus mobility on net over the observed horizon and shifts designated teachers on average toward more advantaged campuses.

[6]Conditional on moving, the estimated effects on ΔSES range from -4.4 to -14.3 percentile points, implying substantial shifts toward more advantaged schools. Because ΔSES is mechanically zero for non-movers, the conditional estimates show the magnitude of destination changes among teachers who switch campuses. Mobility itself is a post-treatment outcome, however, so conditioning on moving may introduce selection bias if designation affects who moves. The conditional differences may therefore reflect both changes in movers' destinations and changes in the composition of movers across groups.

Figure 7: Estimated Effects of Designation on Moves Into and Out of the Tails of the SES Distribution



Notes: Time period $t = 0$ represents the school year in which a teacher is evaluated for designation. Error bars represent 95% confidence intervals. Red circles represent two-way fixed effects estimates. Blue diamonds represent Callaway & Sant'Anna (2021) estimates. Green triangles represent cumulative sums of the Callaway & Sant'Anna (2021) event-time estimates, such that the estimate at event time t equals $\sum_{\tau=0}^{t} \hat{\delta}_\tau$. The outcome in Panel A is an indicator for being observed at a high-SES campus in $t+1$ and a non-high-SES campus in t. The outcome in Panel B is an indicator for being observed at a low-SES campus in $t+1$ and a non-low-SES campus in t. The outcome in Panel C is an indicator for being observed at a non-high-SES campus in $t+1$ and a high-SES campus in t. The outcome in Panel D is an indicator for being observed at a non-low-SES campus in $t+1$ and a low-SES campus in t.

Having shown that designation shifts teachers on average toward more advantaged campuses, I next examine where in the SES distribution those moves occur. Figure 7 reports moves into and out of the top and bottom quartiles of the campus SES distribution, with campus type in columns and the direction of the move in rows. Panels within a row share a common vertical scale, so magnitudes can be read directly across campus types for a given direction of movement.

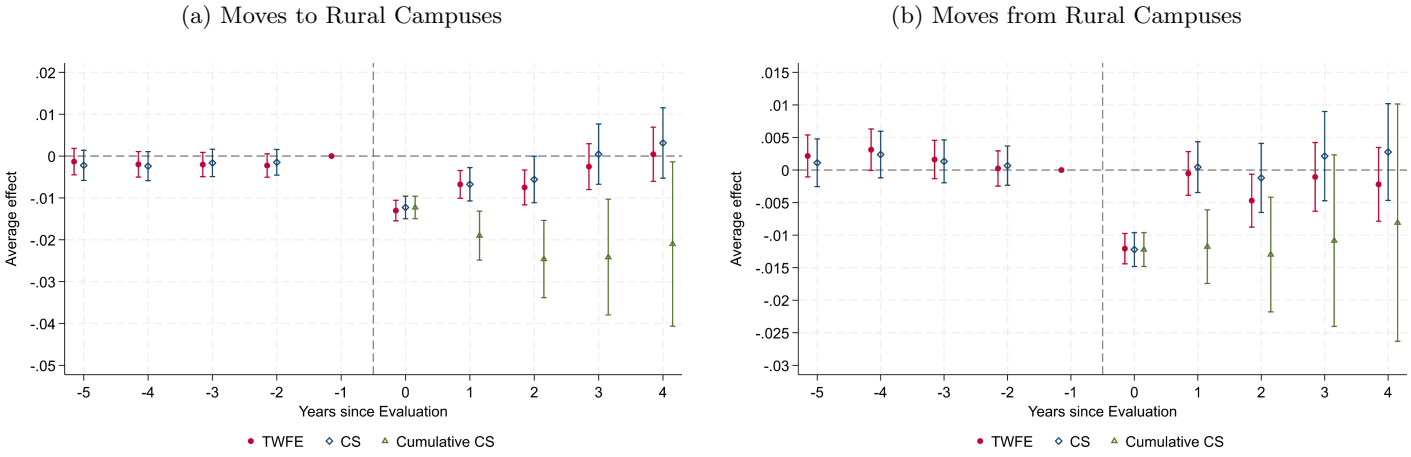
The clearest result in the figure is in Panel C. Designation reduces the probability of leaving a high-SES campus in every post-designation period by between 0.5 and 0.7 pp, and every estimate is statistically significant. The cumulative effect is -2.7 pp and statistically significant. Panel D shows no corresponding change at the other tail. After the period 0 decline, exits from low-SES campuses turn positive from period 2 onward, but the estimates are small and individually imprecise, and the cumulative effect is -0.4 pp and statistically indistinguishable from zero. Designation therefore holds teachers at advantaged campuses

without producing any offsetting change in the retention of teachers at disadvantaged ones.

The destination panels show a weaker version of the same asymmetry. Panel A indicates that moves into high-SES campuses rise following designation, with a significant estimate of 0.8 pp in period 2 and a marginally significant estimate of 0.5 pp in period 3, though the cumulative effect of 0.6 pp is not statistically significant. Panel B shows a smaller and less consistent pattern for moves into low-SES campuses, with a cumulative effect of 0.2 pp. Neither destination effect is large relative to the origin-side result in Panel C.

Read together with the negative post-treatment estimates on ΔSES, the four panels indicate that the sorting response to designation operates primarily through retention at already-advantaged campuses rather than through increased exit from disadvantaged ones. Designation makes teachers at high-SES campuses substantially more likely to stay, does not measurably change exit behavior at low-SES campuses, and modestly increases entry into high-SES campuses. The net direction of sorting is toward more advantaged schools.

Figure 8: Estimated Effects of Designation on Moves Into and Out of Rural Campuses



Notes: Time period $t = 0$ represents the school year in which a teacher is evaluated for designation. Error bars represent 95% confidence intervals. Red circles represent two-way fixed effects estimates. Blue diamonds represent Callaway & Sant'Anna (2021) estimates. Green triangles represent cumulative sums of the Callaway & Sant'Anna (2021) event-time estimates, such that the estimate at event time t equals $\sum_{\tau=0}^{t} \hat{\delta}_\tau$. The outcome in Panel A is an indicator for being observed at a rural campus in $t+1$ and a non-rural campus in t. The outcome in Panel B is an indicator for being observed at a non-rural campus in $t+1$ and a rural campus in t.

Figure 8 reports the corresponding results for rural campuses. Rural status enters the allotment formula separately from campus SES through a two-tier bump in the student point values, so these outcomes speak to a distinct margin of the policy.

Panel A shows that designation reduces the probability of moving into a rural campus. The estimates are negative and significant in periods 1 and 2, at -0.7 pp and -0.6 pp, before returning to zero, and the cumulative effect is -2.1 pp and statistically significant. Panel B shows no evidence of a change in exits from rural campuses; the post-designation estimates are small, none is statistically significant, and the cumulative effect of -0.8 pp is imprecise. Movement into rural schools is therefore not merely absent from the post-designation mobility response; it declines.

Finally, I consider the geographic scope of post-designation mobility. Within-district mobility is the one margin on which the cumulative effect is positive, at 2.3 pp, compared with -4.9 pp for mobility overall. The period 0 estimate for within-district moves is -0.002 and not statistically significant, consistent with the lock-in requirement binding only on moves that cross district lines. Cumulative within-county and within-

region mobility are −2.2 pp and −2.0 pp, respectively, neither statistically significant. Designation therefore shifts the composition of moves modestly toward within-district transitions while reducing the total number of moves. The corresponding figures are reported in Appendix Figure C.2.

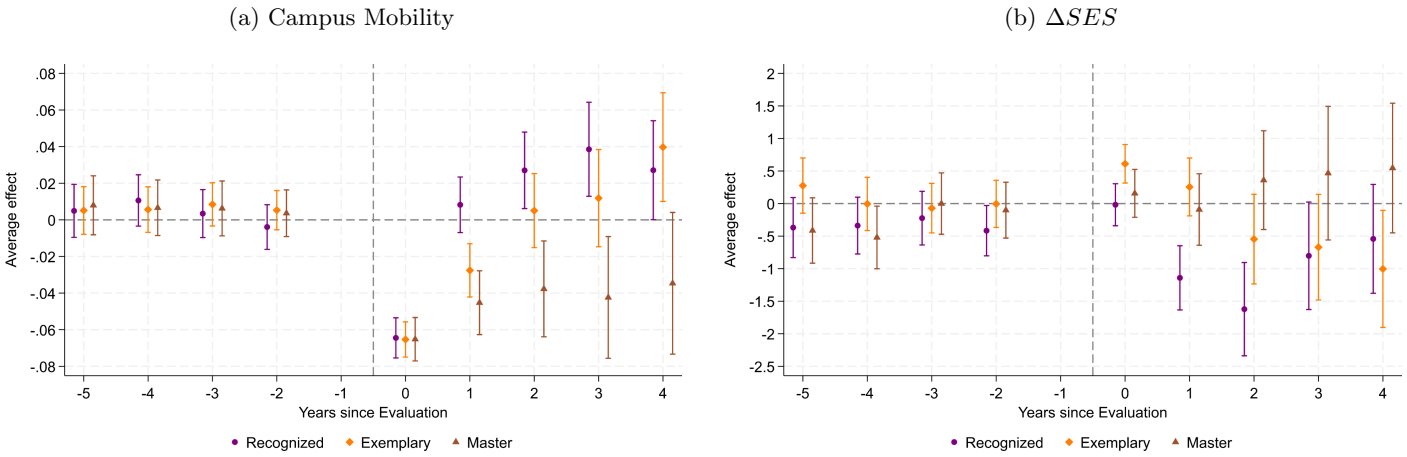
7.3 Heterogeneity

7.3.1 Designation Level

Now I consider whether the mobility and sorting responses to designation differ by designation level. To do so, I estimate the event-study specifications separately by teachers' highest observed designation: Recognized, Exemplary, or Master. These groups are mutually exclusive. Thus, a teacher whose highest observed designation is Master is included only in the Master sample, even if that teacher previously held a Recognized or Exemplary designation. This approach captures heterogeneity according to the highest level attained over the sample period.

This cut is of direct interest for the conceptual framework. Recall from Section 4 that the demographic supplement shifts a designated teacher's effective preference for disadvantage by αm_k, where m_k is the statutory scaling multiplier, equal to \$1,500, \$3,000, and \$5,000 per point of $\bar{s}_j$ at the Recognized, Exemplary, and Master levels, respectively. The framework therefore predicts that sorting toward advantaged campuses should weaken monotonically as m_k rises, and should reverse for teachers whose effective preference $\beta_i^* = \beta_i + \alpha m_k$ turns positive.

Figure 9: Estimated Effects of Designation on Mobility and ΔSES, by Designation Level



Notes: Time period $t = 0$ represents the school year in which a teacher is evaluated for designation. Error bars represent 95% confidence intervals. All results are estimated using Callaway & Sant'Anna (2021). Purple circle points represent Recognized level teachers. Orange diamond points represent Exemplary level teachers. Brown triangle points represent Master level teachers. The outcome in Panel A is an indicator for being observed at a different campus in $t + 1$ relative to period t. The outcome in Panel B is defined as ΔSES = destination percentile − origin percentile, where $\Delta SES < 0$ indicates a move to a more advantaged campus.

A clear pattern emerges from these estimates. The response to designation is ordered by designation level on every margin, and the ordering runs in the direction the framework predicts.

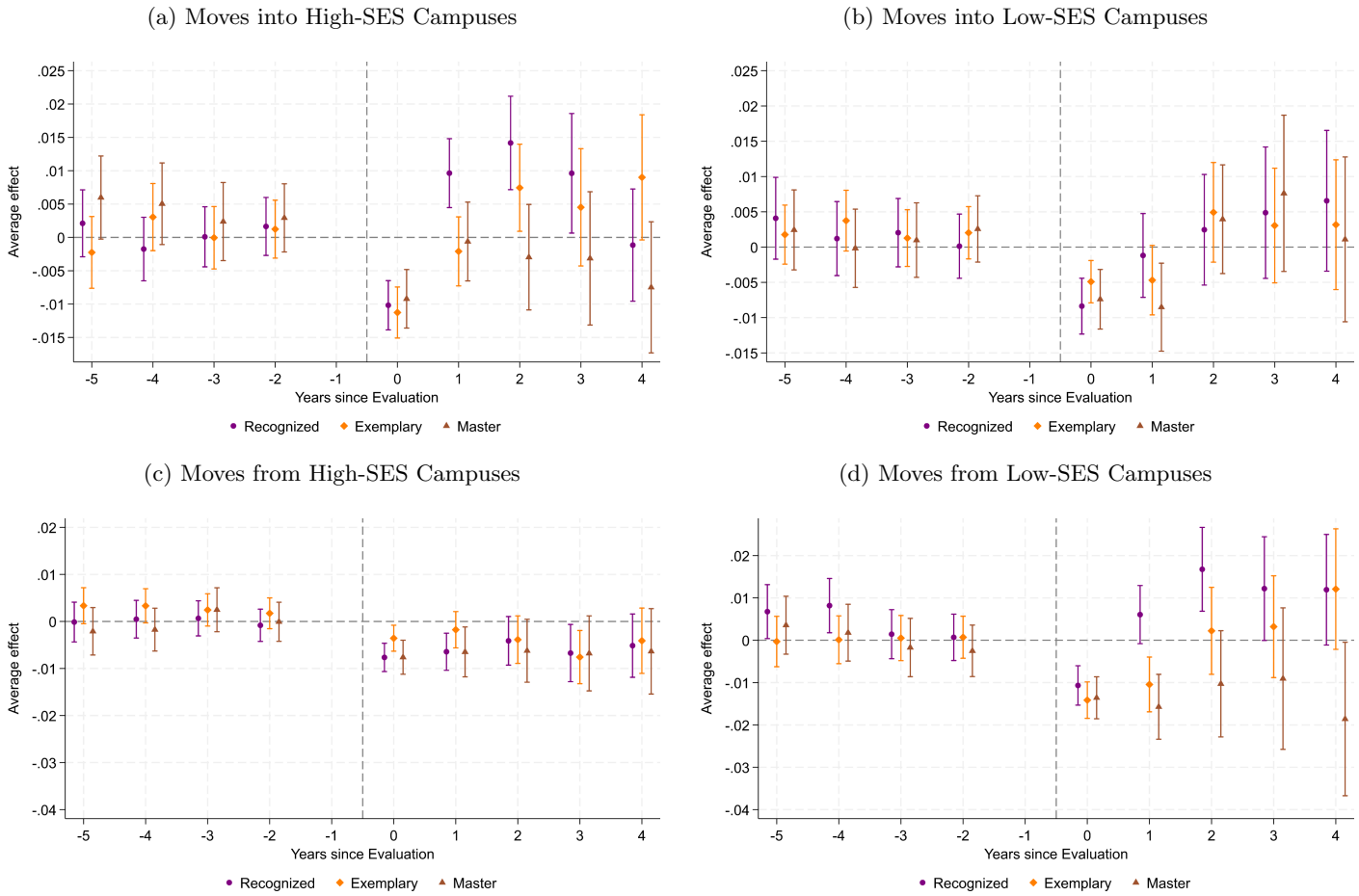
Panel A of Figure 9 shows that the mobility response is decreasing in designation level. Recognized teachers become more mobile after designation, with significant increases of 2.7 pp in period 2, 3.9 pp in period 3, and 2.7 pp in period 4. Exemplary teachers show a weaker and less consistent pattern, with mobility suppressed by 2.8 pp in period 1 before rising to 4.0 pp by period 4. Master teachers move in the

opposite direction: their mobility is lower in every post-designation period by between 3.5 and 4.5 pp, and every estimate is statistically significant. The net reduction in mobility documented in Panel A of Figure 6 is therefore concentrated at the higher designation levels.

Panel B shows the same ordering for sorting across campuses. For Recognized teachers, the post-treatment estimates for ΔSES are consistently negative and economically large, ranging from -0.542 to -1.622 percentile points, indicating movement toward more advantaged campuses after designation. Exemplary teachers experience negative post-treatment effects that are smaller and less uniform across event time, reaching -1.004 by period 4. For Master teachers the estimates are positive from period 2 onward, though imprecise. The aggregate shift toward more advantaged campuses documented in Panel B of Figure 6 is therefore concentrated among Recognized and Exemplary teachers, and is absent, if anything reversed, at the highest designation level[7].

[7]Conditional on moving, the ordering is similar, but the estimates are considerably noisier. For Recognized teachers the post-designation estimates range from -8.9 to -18.7 percentile points. For Exemplary teachers the estimates are positive in periods 1 and 2, although noisy and negative in periods 3 and 4, reaching -28.8. For Master teachers the estimates float above and below 0 and are extremely noisy. Similarly, these condition on moving, which is itself a post-treatment outcome. Panel A shows that designation changes mobility by different amounts at different designation levels, so the composition of movers differs across the three groups and across event time in ways the conditional estimates cannot separate from changes in destination.

Figure 10: Estimated Effects of Designation on Moves Into and Out of the Tails of the SES Distribution, by Designation Level



Notes: Time period $t = 0$ represents the school year in which a teacher is evaluated for designation. Error bars represent 95% confidence intervals. All results are estimated using Callaway & Sant'Anna (2021). Purple circle points represent Recognized level teachers. Orange diamond points represent Exemplary level teachers. Brown triangle points represent Master level teachers. The outcome in Panel A is an indicator for being observed at a high-SES campus in $t+1$ and a non-high-SES campus in t. The outcome in Panel B is an indicator for being observed at a low-SES campus in $t+1$ and a non-low-SES campus in t. The outcome in Panel C is an indicator for being observed at a non-high-SES campus in $t+1$ and a high-SES campus in t. The outcome in Panel D is an indicator for being observed at a non-low-SES campus in $t+1$ and a low-SES campus in t.

Figure 10 reports the same four transition outcomes as Figure 7, estimated separately by designation level.

Panel A shows the ordering on the destination side. The increase in moves to high-SES campuses is concentrated among Recognized teachers, with significant estimates of 1.0, 1.4, and 1.0 pp in periods 1 through 3. For Exemplary teachers only the period 2 and period 4 estimates are significant, at 0.7 and 0.9 pp, and for Master teachers every post-designation estimate is negative, and none is statistically significant. Panel B does not show a corresponding pattern. Moves into low-SES campuses are not ordered by designation level, and the only significant post-designation estimates are declines of 0.5 pp for Exemplary teachers and 0.9 pp for Master teachers, both in period 1.

The origin panels show the sharpest version of the pattern. Panel C indicates that exits from high-SES campuses fall at all three levels, with post-designation estimates between -0.2 and -0.8 pp and no visible ordering across levels, so the retention effect documented in Panel C of Figure 7 does not vary with the

SES gradient. Panel D is ordered sharply. Recognized teachers become significantly more likely to leave low-SES campuses in every post-designation period, by between 0.6 and 1.7 pp. Exemplary teachers show no consistent change, with a significant decline of 1.0 pp in period 1 and a significant increase of 1.2 pp in period 4. Master teachers become less likely to leave low-SES campuses in every post-designation period, significantly so in periods 1 and 4 at 1.6 and 1.9 pp. The sign of the exit response at disadvantaged campuses flips between the Exemplary and Master levels.

The rural and geographic outcomes follow the same ordering and are reported in Appendix Figure C.3. Movement into rural campuses declines only among Master teachers, significantly so in periods 1 through 3. Within-region mobility is strongly ordered, with Recognized teachers showing significant increases of 2.8, 3.6, and 2.2 pp in periods 2 through 4. Exemplary teachers experienced a significant decline of 2.2 pp in period 1, followed by a significant increase of 4.1 pp in period 4, and Master teachers experienced significant declines in every post-designation period, of between 3.0 and 3.8 pp. The full set of heterogeneity estimates is reported in Appendix Tables B.5, B.6, B.7, and B.8.

Overall, the heterogeneity results show that the labor-market response to designation is not uniform across designation levels. The increase in mobility and the associated shift toward more advantaged campuses are driven primarily by Recognized and, to a lesser extent, Exemplary teachers, while Master teachers become less mobile in every post-designation period. This pattern suggests that the credentialing channel is most important in the lower designation tiers, where the supplement attached to school disadvantage is smallest. There, designation appears to expand teachers' outside options without materially changing the relative attractiveness of campuses, so teachers act on the preference for more advantaged settings that the framework takes to hold on average. Recognized teachers become both more likely to move to high-SES campuses and more likely to leave low-SES campuses, which is what the credentialing channel predicts when β_i is negative on average.

By contrast, the reduced mobility among Master teachers is more consistent with compensation playing the dominant role. A Master teacher at a disadvantaged campus forgoes a large share of her allotment by moving to a more advantaged one, and the exit response follows: Master teachers become less likely to leave low-SES campuses, reversing the sign observed among Recognized teachers. In that case, Master teachers may be less responsive because the compensation attached to designation is large enough to offset the preference that would otherwise pull them toward more advantaged campuses. This also accounts for the aggregate pattern in Section 7.2, where a positive mobility response among Recognized teachers is outweighed by a larger and more persistent negative response among Master teachers.

The uniform decline in exits from high-SES campuses fits the same account. For a teacher already at an advantaged campus, the credential expands the set of campuses she can reach, but the campuses she would prefer are themselves high-SES, and lateral moves of that kind do not register in this outcome. The supplement, meanwhile, offers her little reason to move down the SES distribution. Neither channel pushes her out, and the effect is correspondingly similar across tiers. Taken together, these results suggest that designation operates through different channels at different points in the designation hierarchy and that the balance between them shifts as the SES-scaled supplement grows.

7.3.2 By Teacher Characteristics

I also estimate the event-study specifications separately by teacher experience, race and ethnicity, and sex. These are informative about how different groups respond to designation rather than tied to a prediction of the framework, since nothing here changes the price a teacher faces for teaching at a disadvantaged campus.

Each cut is reported in appendix figures following the layout above, one for mobility and ΔSES and one for the four transition outcomes.

Appendix Figure C.4 reports estimates by experience, using bins of 0 to 10, 11 to 20, and 21 years or more. Mobility is suppressed in period 1 in the two less experienced bins by 1.7 and 2.2 pp, and the period 0 decline appears in all three, so the lock-in and the slow recovery documented in Figure 6 are not confined to a particular career stage. The shift toward more advantaged campuses, by contrast, is concentrated among the least experienced teachers. For teachers with ten or fewer years of experience, the post-designation estimates on ΔSES reach -1.168 and -1.171 percentile points in periods 2 and 3, are both statistically significant, while the estimates for the two more experienced bins are smaller, and none is statistically significant in any period. Reduced exits from high-SES campuses appear in the two less experienced bins, with significant declines of between 0.4 and 1.0 pp in periods 1 through 3, and are smaller and insignificant among teachers with more than 20 years of experience. Moves into and out of low-SES campuses do not appear to be concentrated among any of the career stages.

Appendix Figure C.5 reports estimates by teacher race and ethnicity. The period 0 decline appears in all three groups, at 8.2, 6.5, and 4.5 pp for White, Black, and Hispanic teachers, respectively, and mobility remains significantly suppressed in period 1 for White and Hispanic teachers. The sorting response is not similar across groups. For White teachers the post-designation estimates on ΔSES are negative in every period, ranging from -0.434 to -1.428 percentile points and statistically significant throughout. For Hispanic teachers the post-designation estimates are close to zero, and none is statistically significant. Estimates for Black teachers are slightly negative but imprecise. The reduction in exits from high-SES campuses follows the same ordering. For White teachers every post-designation estimate is negative and statistically significant, at between 0.5 and 1.1 pp; for Black teachers the period 1 and period 2 estimates are significant; for Hispanic teachers no post-designation estimate is significant. The aggregate sorting result in Figure 6 therefore rests disproportionately on White teachers.

Appendix Figure C.6 reports estimates by teacher sex. Because the designated population is 86% female, the pooled estimates are close to the estimates for female teachers throughout. Mobility is significantly suppressed in period 1 for female teachers, by 2.2 pp, and significantly elevated in periods 3 and 4, by 1.7 and 2.3 pp; no post-designation estimate for male teachers is statistically significant. The post-designation estimates on ΔSES for female teachers are negative from period 2 onward, reaching -0.831 percentile points. However, for male teachers these estimates are slightly less negative, although very imprecise. Reduced exits from high-SES campuses appear for both groups, though only the female estimates are individually significant in every post-designation period, at between 0.4 and 0.8 pp, while for male teachers only the period 1 estimate is significant. Due to the male estimates being considerably less precise, I do not read the differences between the two as informative.

8 The Compensation-SES Tradeoff

8.1 Discrete Choice Model

A natural follow-up to these effects on mobility is how much teachers are trading off between SES and total compensation. To answer this question, I estimate a discrete choice model using the sample of eventually designated teachers from 2016 until 2024. Every year a teacher makes a discrete choice between (i) moving to a school within a 65-mile radius of her current school, (ii) staying at their current school, and (iii) leaving

teaching at Texas public schools, which I call the outside option. A decision in year t is measured using her employment in year $t+1$, so the final year of the panel supplies destination information rather than an additional choice observation. I exclude the decision year that is impacted by the lock-in mechanism to avoid using choices that could be mechanically impacted by designation and its retention incentives.

Let j_{it} denote teacher i's current school. Her choice set $\mathcal{C}_{it}$ includes her current school, eligible alternative schools operating in year $t+1$, and the outside option. I restrict alternative schools to those within 65 miles of her current school and exclude observations in which the realized move exceeds this radius. The radius captures approximately 90% of observed moves and focuses the analysis on relatively local school choices. I measure distance using the euclidean distance between her current school j and alternative j'. Because I do not observe teachers' residences, this measure captures the distance between workplaces rather than commuting distance or time.

Because of how teacher certifications work in Texas, I further limit each teacher's choice set using the grade span of the campus where she is currently teaching. For most subjects, teachers are certified for Early Childhood through 6th grade, 4th through 8th grade, or 7th through 12th grade. For subjects such as music, physical education, and art, teachers are certified for Early Childhood through 12th grade. Specifically, if a teacher is currently teaching at an elementary school, the only campuses in her choice set are other elementary schools and middle schools. If a teacher is currently teaching at a high school, the only campuses in her choice set are other high schools and middle schools. If a teacher is currently teaching at a middle school, then elementary, middle, and high schools are all included in her choice set. Imposing these restrictions removes an additional 2% of movers.

Even with these restrictions, the choice sets can still be computationally infeasible to estimate. To solve this, I use a stratified sampling approach where I sample 50 schools from the remaining schools in the choice set, ensuring there are schools from within- and across-districts in the sampled choice set. To correct for this sampling, I use the approach suggested by McFadden (1977) in the case of a conditional logit, which has been shown by Guevara & Ben-Akiva (2013) and Dekker et al. (2025) to extend to the mixed logit setting. I include a term $\ln(q_{ij})$ where $q_{ij} = Pr(j \in \mathcal{C}_i^*)$ where $\mathcal{C}_i^*$ is the sampled choice set. The coefficient of this term is fixed to -1.

I do not directly observe each district's official salary schedule. However, because Texas districts tend to follow fairly rigid salary structures, I recover district salary schedules from observed teacher salaries. I collapse the data to district-year-experience-degree cells and assign each cell the median observed base salary, pooling teachers with 25 or more years of experience into a single category. This gives me an estimate of what a district pays, for example, a teacher with four years of experience and a master's degree in a given year. I use cells with at least five teachers as observed salary steps. I fill gaps between them by linearly interpolating salaries.

For cells that remain missing, I use the district's observed salaries together with statewide salary patterns to predict the missing values. When I observe other salaries within the same district-year-degree schedule, I use those observations to estimate the district's pay level for teachers with that degree. I then use the statewide relationship between experience and salary for teachers with the same degree in the same year to fill the missing steps. If I do not observe any salaries for a particular degree in a district and year, I instead use the district's overall pay level and statewide salary differences by experience and degree. I estimate both models using log median base salaries, weighting each cell by its number of teachers. Cells that remain missing are excluded from the available alternatives. I assign each teacher the estimated salary she would receive in the following year, adding one year of experience, capped at 25, and holding her degree constant.

Let teacher i's indirect utility for moving to campus j' from j be represented by:

$$u_{ij't} = A_{it} + \alpha_i w_{ij't} + \beta_i \bar{s}_{j't} + \delta Dist_{jj't} + \gamma_i \mathbf{1}\{j_{it} \neq j'\} + \eta \mathbf{1}\{d(j_{it}) \neq d(j')\} + \varepsilon_{ij't} \tag{8}$$

where $w_{ij't}$ is the total wage, in thousands of 2024 dollars, offered by school j', in period t. This mirrors equation 3 from the conceptual framework, in which her new salary is based on the base pay she would receive plus any TIA allotment. Specifically, $w_{ij't} = \bar{w}_{d(j')}(exp_{it}, cred_{it}) + B_k + m_k \bar{s}_{j't}$. $\bar{s}_{j't}$ is the ASPV that both measures the SES of a school and directly impacts the salary offered. I normalize $\bar{s}_{j'}$ across schools so that it is interpretable as one-standard-deviation (1SD) changes. $Dist_{jj'}$ is the Euclidean distance between her current school j and alternative school j', which is zero for her current school. $\mathbf{1}\{j_{it} \neq j'\}$ equals one for any alternative other than her current school, and $\mathbf{1}\{d(j_{it}) \neq d(j')\}$ equals one for any alternative outside of her current district. These two terms capture the moving costs from the conceptual framework. I include distance as an additional control. Finally, teacher i exhibits idiosyncratic match preferences $\varepsilon_{ij't}$, which I assume follow a Type 1 Extreme Value Distribution, and I normalize the outside option to have a utility of 0.

To allow a teacher's unobserved attachment to teaching to be correlated with the campuses available to her, I follow Mundlak (1978) and Chamberlain (1982) and let A_{it} depend on the average attributes of the campuses in her choice set, along with her observed characteristics and year fixed effects.

$$A_{it} = \mu_A + \psi X_{it} + \phi \bar{Z}_{it} + \tau_t$$

where X_{it} contains indicators for whether teacher i is Black, White, or Hispanic and indicators for whether she has 0–3, 4–10, or 11–20 years of experience, with more than 20 years as the omitted category, $\bar{Z}_{it}$ contains the averages of $w_{ij't}$, $\bar{s}_{j't}$, and $Dist_{jj't}$ across the campuses in $\mathcal{C}_i$ in year t, computed before sampling alternatives, and τ_t are year fixed effects.

I allow for teachers to have heterogenous preferences over wage, school SES, and switching schools. More formally I let:

$$\begin{aligned}
\alpha_i &= \exp(\mu_\alpha + \sigma_\alpha v_{i1}) \\
\beta_i &= \mu_\beta + \sigma_\beta v_{i2} \\
\gamma_i &= -\exp(\mu_\gamma + \sigma_{\gamma\beta} v_{i2} + \sigma_\gamma v_{i3})
\end{aligned}$$

where each v_{ik} follows a standard normal distribution fixed across a teacher's observed choices. The disadvantage coefficient is normally distributed and unrestricted in sign. The compensation coefficient is lognormal and strictly positive, while the school-switching coefficient is negative lognormal and strictly negative. The common draw v_{i2} allows preferences over disadvantage to be correlated with school-switching costs. The compensation coefficient is independent of both coefficients under this specification.

From this I estimate the distribution of teachers' willingness-to-pay, which is represented by $WTP = (-\beta_i/\alpha_i) \times 1,000$. This is interpreted as the additional amount a teacher requires to teach at a school that has a 1SD increase in $\bar{s}_j$. Because $\bar{s}_j$ is increasing in disadvantage, a 1SD increase corresponds to a school that is 1SD more disadvantaged.

By using choices made before designation, I implicitly assume that designation does not change teachers' preferences, so that α_i and β_i are the same before and after a teacher is designated. The pre-designation choices are informative because pay does not vary across campuses within a district before designation, so

within-district choices in those years reflect preferences over campus attributes alone. After designation, the allotment adds a within-district pay gradient that scales with $\bar{s}_j$, and part of the separation between the wage and SES coefficients comes from comparing choices across the two periods.

8.2 Limitations

There are several limitations that affect the interpretation of these estimates.

First, the coefficient β_i should be interpreted as a preference over the full bundle of campus attributes correlated with $\bar{s}_j$, not as a preference for student socioeconomic status per se. Disadvantaged campuses differ from advantaged campuses along many dimensions that are both observed and unobserved in the data. To the extent that these correlated attributes affect teacher utility, they load onto β_i alongside any preference over student demographics. The estimated WTP therefore reflects the compensation required to teach at a school with a 1SD increase in $\bar{s}_j$ together with the bundle of working conditions typical of such schools, not the compensation associated with student composition alone. This distinction matters for policy: a financial incentive calibrated to the estimated WTP compensates teachers for the full bundle, but a bonus instrument such as the TIA allotment cannot directly remedy non-pecuniary working conditions.

Second, the choice set construction assumes that any school within a 65-mile radius belongs to a teacher's consideration set. In practice, teachers face vacancy constraints (the alternative campus must have an open position), informational constraints (teachers are unlikely to know about all alternatives equally), and personal constraints (family ties, spouse careers) that further narrow the set of campuses they actually consider. Treating the geographic set as the choice set therefore overstates the breadth of options each teacher evaluates, which may distort the estimated preference parameters in ways that depend on the structure of unobserved consideration.

Third, I model each teacher-year as a static choice and therefore abstract from the dynamic nature of mobility decisions. A dynamic specification would capture several forces that the current model does not separately identify. Changing campuses likely involves one-time transition costs, including household disruption, search and information costs, the loss of campus-specific human capital, and an adjustment period. In the present model these costs enter through the switching-cost terms γ_i and η and the distance term δ, and because the model is static, they are weighed against a single year of wages and working conditions rather than against the stream of future utility a move secures. A dynamic model would also better represent how teachers evaluate compensation: because Texas salary schedules are district-specific and step-based, current wage is only a noisy proxy for the present-value compensation a forward-looking teacher considers when deciding whether to switch districts. Likewise, because TIA designations persist and are re-evaluated over multi-year cycles, teachers likely consider continuation values tied to future designation outcomes that a static model does not price. The willingness-to-pay estimates should therefore be interpreted as current-period compensating differentials associated with $\bar{s}_j$, rather than as the compensation required to induce sustained mobility. A fully dynamic treatment is left to future work.

Lastly, I construct $(w_{ij't})$ for both a teacher's current school and counterfactual schools from estimated district salary schedules. I assign each district-year-experience-degree cell the median observed base salary, linearly interpolate missing experience steps, and predict remaining cells from the district's observed pay level and statewide experience profiles. These estimates may differ from the wages teachers would actually earn. The resulting measurement error is unlikely to be classical because its magnitude may vary with district size and the number of teachers observed in each salary cell. I therefore cannot assign a direction to the bias in the wage coefficient or the implied willingness to pay, $(-\beta_i/\alpha_i)$. In Section 9, I assess the

sensitivity of the estimates by restricting to district-year-experience-degree cells with more observations and eliminating any imputed cells.

8.3 Results

The event-study results in Section 7 show that designation does not raise mobility on net and that designated teachers shift toward more advantaged campuses, with the shift ordered by designation level. This section asks whether that pattern reflects the compensation channel, the credentialing channel, or both. I report estimates from the discrete choice model described above in Section 8.1, recover the salary gradient designated teachers face, and compare the framework's predictions with the heterogeneity in sorting shown in Section 7.3.1.

Table 8 reports estimates from the discrete choice model for teachers who receive a TIA designation during the observation period.

Table 8: Teacher Preference Estimates

	Estimate	Standard error
Constant (μ_A)	6.686	0.232
Distance (δ)	−0.082	0.001
District switching (η)	−1.940	0.022
School preferences and heterogeneity		
School SES (μ_β)	−0.114	0.007
St. dev. school SES RC (σ_β)	0.097	0.011
Log wage coefficient (μ_α)	−4.024	0.048
St. dev. log wage coefficient (σ_α)	0.410	0.038
Log school switching cost (μ_γ)	1.846	0.003
SES loading in log switching cost ($\sigma_{\gamma\beta}$)	0.225	0.007
Independent loading in log switching cost (σ_γ)	−0.024	0.056
Teacher characteristics		
Black	0.175	0.087
White	0.055	0.077
Hispanic	0.250	0.080
Experience 0–3	−0.656	0.061
Experience 4–10	−0.342	0.052
Experience 11–20	0.418	0.052
Choice-set averages		
Wage mean	−0.075	0.003
School SES mean	1.161	0.058
Distance mean	0.024	0.002
Number of teachers	37,441	
Number of teacher-year observations	210,319	
Log-likelihood	−156,103.728	
AIC	312,257.455	

Notes: Standard errors are robust. Estimation by maximum simulated likelihood using the BGW algorithm with 300 Sobol draws with inter-individual variation, implemented in Apollo (Hess & Palma, 2019). The random coefficients (RC) are $\beta_i = \mu_\beta + \sigma_\beta v_{i1}$, $\gamma_i = -\exp(\mu_\gamma + \sigma_{\gamma\beta} v_{i1} + \sigma_\gamma v_{i2})$, and $\alpha_i = \exp(\mu_\alpha + \sigma_\alpha v_{i3})$, where the v_{ik} are independent standard normal draws held fixed across each teacher's choices. Wage parameters describe $\ln(\alpha_i)$; switching parameters describe $\ln(-\gamma_i)$. The parameter σ_γ is an independent loading, not the total standard deviation of log switching costs, which is $\sqrt{\sigma_{\gamma\beta}^2 + \sigma_\gamma^2}$. Choice-set averages are calculated before sampling and exclude the outside option. Teacher characteristics, choice-set averages, and year indicators enter the common school intercept A_{it}. The omitted year is 2016. Compensation is measured in thousands of 2024 dollars, distance in miles, and school SES in standard deviations, with higher values indicating greater disadvantage.

The estimates indicate that teachers prefer more advantaged schools on average, with a mean SES coef-

ficient of −0.114 and a standard deviation of 0.097, suggesting substantial heterogeneity in preferences over school disadvantage. Distance and district switching also reduce utility, consistent with teachers preferring schools closer to their current workplace and within their current district. The positive shared loading between SES preferences and log switching costs indicates that teachers with weaker aversion to disadvantage tend to face larger school switching costs.

To look at the distribution of willingness to pay, I simulate draws from the estimated population distributions of the SES and salary coefficients. For each draw m I let $WTP_m = 1000 * (-\beta_m/\alpha_m)$, which estimates the annual salary increase, that leaves a teacher indifferent to a one standard deviation increase in campus disadvantage. Figure 11 plots the resulting distribution. The median teacher requires $5,980 and the interquartile range goes from $2,450 to $10,424. Because the sign of α_m is strictly positive, any negative values come from β_m. The share of teachers with a negative willingness to pay is about 12%. These teachers would be made worse off if they moved to a school that was more advantaged, holding all else equal.

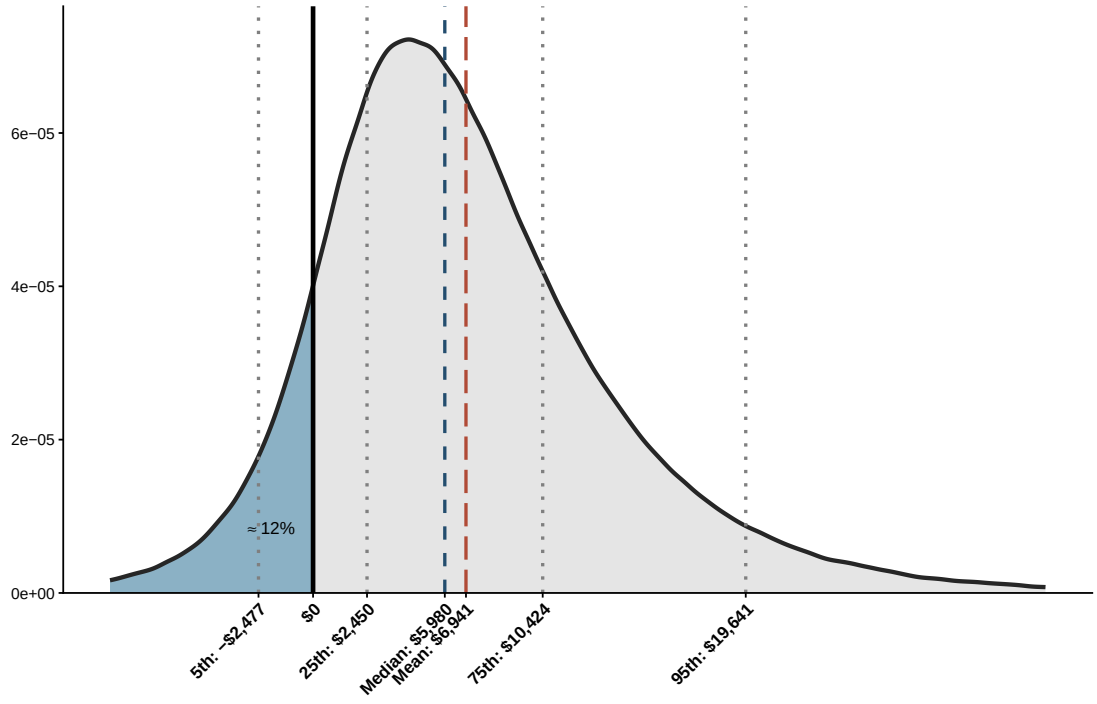


Figure 11: Distribution of Willingness to Pay to Avoid Campus Disadvantage

Notes: The figure plots the population distribution of willingness to pay (WTP) to avoid a one standard deviation increase in campus disadvantage, implied by the mixed logit estimates. WTP equals $-\beta_{SES}/\alpha$, where β_{SES} is the coefficient on normalized campus SES (higher values indicate more disadvantaged campuses) and α is the salary coefficient, and is expressed in dollars of annual salary. The SES coefficient is normally distributed, and the salary coefficient is lognormally distributed, independently of each other. The density is estimated from 1,000,000 draws from the estimated population distributions of the two coefficients rather than from teacher-specific conditional distributions. The dashed and long-dashed lines mark the median and mean; dotted lines mark the 5th, 25th, 75th, and 95th percentiles. Horizontal-axis labels give the value of each statistic. The shaded region marks negative WTP, corresponding to a positive SES coefficient, and the label gives its population share. The horizontal axis shows the 0.5th through 99.5th percentiles; all statistics use the full distribution.

The average willingness to pay is about $6,940 per year per standard deviation of school disadvantage, holding compensation constant. In my data, a one standard deviation increase corresponds to approximately a 20 percentage point increase in the share of economically disadvantaged students. Expressed per 10 percentage points, my estimate is about $3,470, or 12.3 times the $282 estimated by Johnston (2025). The

estimates measure different objects. Johnston independently varies salaries and school attributes in a teacher choice experiment, allowing him to estimate stated willingness to pay for a change in student composition while holding the other presented attributes fixed. My estimate comes from teachers' observed school choices. It therefore reflects preferences for the bundle of school conditions associated with disadvantage, rather than the effect of changing the share of economically disadvantaged students alone.

8.4 Comparing the Salary Gradient to Willingness to Pay

To compare WTP to the realized salary gradient teachers actually face, I estimate

$$w_{ijt+1} = \theta_{it} + \sum_{k=1}^{3} \pi_k \bar{s}_j 1[k_{it} = k] + e_{ijt}$$

on the constructed choice-set data, where the unit of observation is at the teacher × year × alternative level and w_{ijt+1} is the constructed wage teacher i would receive at campus j in the following year. The inclusion of teacher-year fixed effects absorbs all teacher and year characteristics that are constant across alternatives within a choice set and so $\hat{\pi}_k$ is identified entirely from variation in constructed wages across alternatives in a given year. $\hat{\pi}_k$ is therefore the average realized salary gradient for teachers at designation level k.

The estimates are $\hat{\pi}_{Recognized} = 2,713$, $\hat{\pi}_{Exemplary} = 3,646$, and $\hat{\pi}_{Master} = 5,285$. Recall from the framework that a designated teacher moves to a more disadvantaged campus if

$$\beta_i^* \Delta s + \alpha_i \Delta w > c_{jj'}$$

where $\beta_i^* = \beta_i + \alpha m_k$ and $c_{jj'} = c_S + c_D$; within a district, $\Delta w = 0$ and $c_{jj'} = c_S$. For a move to a more disadvantaged campus, $\Delta s > 0$, so dividing by $\alpha \Delta s$ preserves the inequality. Substituting $\hat{\pi}_k$ for $m_k + \Delta w / \Delta s$, a teacher facing the average gradient moves toward a more disadvantaged campus if

$$-\beta_i / \alpha_i < \hat{\pi}_k - \frac{c_{jj'}}{\alpha_i \Delta s}$$

that is, if her willingness to pay to avoid disadvantage is smaller than the salary gradient net of the moving cost per SD of disadvantage gained. Because the moving cost is positive, $-\beta_i/\alpha_i < \hat{\pi}_k$ is a necessary but not sufficient condition for the move: a teacher whose willingness to pay exceeds the gradient does not move toward more disadvantaged campuses regardless of the cost of moving, while a teacher whose willingness to pay falls below it moves only if the gap also covers the cost.

The same comparison governs retention. A teacher at the more disadvantaged campus j' stays as long as her gain from moving to j, $-(\beta_i^* \Delta s + \alpha \Delta w)$, does not exceed the cost of moving. This is the lower bound of the retention range in equation 6:

$$\beta_i^* \Delta s + \alpha_i \Delta w \geq -c_{jj'}$$

Applying the same substitution, she stays if

$$-\beta_i / \alpha_i \leq \hat{\pi}_k + \frac{c_{jj'}}{\alpha_i \Delta s}$$

so a gradient above her willingness to pay is sufficient for her to stay, whatever the cost of moving. For the average teacher, with WTP of \$6,941, the comparison that matters in both cases is whether $\hat{\pi}_k >$ \$6,941.

Table 9: Salary Gradients and Willingness to Pay

	Recognized	Exemplary	Master
Salary gradient $\hat{\pi}_k$	2,713	3,646	5,285
	(14.18)	(15.73)	(24.40)
Mean WTP	6,941	6,941	6,941
Median WTP	5,980	5,980	5,980
Share of mean WTP covered	39.1%	52.5%	76.1%
Share of teachers with WTP $\leq \hat{\pi}_k$	26.8%	33.3%	45.1%

Notes: Salary gradients and WTP are expressed in 2024 dollars per standard deviation increase in school disadvantage. Standard errors for the salary gradients, clustered by teacher, are in parentheses. Mean and median WTP summarize the simulated population distribution of $WTP_i = -1{,}000\beta_i/\alpha_i$. The share of mean WTP covered equals $100\hat{\pi}_k/\overline{WTP}$. The final row reports the approximate share of the estimated WTP distribution below each salary gradient, holding distance and switching status fixed.

Table 9 shows that the salary gradient falls short of mean WTP at every designation level. Even at the Master level, the gradient covers about 75% of mean WTP and lies below the WTP of more than 50% of designated teachers. For most designated teachers, then, the supplement is too small to offset their preference for more advantaged campuses. The framework implies that in this case any expansion of outside options through the credentialing channel tilts sorting toward more advantaged campuses rather than away from them. The event-study estimates are consistent with this. Mobility falls in the lock-in year and turns positive in later post-designation periods, and ΔSES is negative in every post-designation period, so designation shifts teachers toward more advantaged campuses.

The framework also implies that the compensation channel should show up more in retention than in moves toward disadvantaged campuses. A gradient above a teacher's WTP is necessary for a move toward a more disadvantaged campus but sufficient for her to stay at one. The moving cost works against the move in the first case and in favor of staying in the second. A teacher whose WTP exceeds the gradient can therefore still be retained at a disadvantaged campus if the gap is smaller than her moving cost per SD of disadvantage, but she cannot be drawn to one. The estimates follow this asymmetry: designation does little to increase moves into low-SES campuses at any level, while exits from low-SES campuses vary with the size of the gradient.

Panel B of Figure 9 in Section 7.3.1 follows this ordering. The share of teachers whose WTP lies below the gradient rises from 27% at the Recognized level to 45% at the Master level. The share of mean WTP the gradient covers rises from 39% to 76% percent. Recognized teachers become more mobile and more likely to leave low-SES campuses after designation. Master teachers become less mobile and less likely to leave low-SES campuses in every post-designation period, and Exemplary teachers fall between the two. The Master result does not require the gradient to exceed WTP. Under the retention condition, a teacher at a disadvantaged campus stays as long as the gap between her WTP and the gradient is no larger than her moving cost per SD of disadvantage. At the Master level, where that gap is smallest, the supplement plausibly closes enough of it to hold teachers at disadvantaged campuses who would otherwise leave.

9 Robustness Checks

9.1 Event-Study

I perform several robustness checks to assess whether my results are driven by factors other than designation itself. For brevity, I provide the event-study figures in the Appendix.

9.1.1 Choice of DiD Estimator

The main estimates I report come from the Callaway & Sant'Anna (2021) estimator, but the choice of DiD estimator is not innocuous. To show that my results are not driven by this choice, I re-estimate the event studies using the extended two-way fixed effects (ETWFE) estimator of Wooldridge (2025), the interaction-weighted estimator of Sun & Abraham (2021), and the imputation estimator of Borusyak et al. (2024), holding the sample, comparison group, and event-time window fixed. Because the transition outcomes are binary with low base rates, I also estimate ETWFE by Poisson quasi-maximum likelihood (Wooldridge, 2023), with cohort and period fixed effects in place of teacher fixed effects, and report average marginal effects. I estimate the Poisson specification only for the binary outcomes, since ΔSES takes negative values. The point estimates track the baseline closely, which I take as evidence that my results are not driven by the choice of estimator.

9.1.2 Using Not-Yet Designated Teachers

In my main results I use the 2024 cohort of treated teachers as my comparison group. The majority of these teachers are treated because they work in the last observed set of enrolling districts. As shown in Table 2 these districts differ along observable characteristics; they could also differ on unobservable characteristics that predict mobility. I therefore reestimate the main event studies using the not-yet designated set of teachers so that each $ATT(g,t)$ is identified against every cohort untreated at that period rather than the 2024 cohort alone. The results track the main estimates closely, and I interpret this as the main results not being driven by the composition of the final cohort.

9.1.3 Using First Wave of Designees

Designation is available only to teachers in adopting districts, and once a district adopts, TIA becomes a visible feature of employment there. Teachers designated in later rounds may therefore have entered the district after adoption or remained in it with knowledge that designation and the associated allotment were available. Their presence in the district is itself a choice made under the program, and it is made on the same preferences over compensation and campus characteristics that the mobility estimates are meant to recover. I restrict the sample to teachers designated in the first year their district was eligible to designate. For these teachers, placement in an adopting district predates TIA, so designation is not conditioned on having already sorted toward the program. The estimates are similar to the baseline, which suggests the main results are not driven by teachers who sorted into adopting districts under the program.

9.1.4 Threats from COVID-19

One concern is that year fixed effects may not fully account for the overlap between the timing of designation and the COVID-19 pandemic. Although year fixed effects absorb shocks common to all teachers in a given year, pandemic-related bias could still arise if treated teachers were affected differently from control teachers.

To address this concern, I re-estimate equation 7 after excluding the 2019 and 2020 school years. Because the CS estimates rely on cohort-specific timing, dropping these years removes the early treatment cohorts from the estimation sample entirely. I therefore present the estimates from Borusyak et al. (2024) for this exercise. These estimates closely track the full-sample results and suggest that the main findings are not driven by pandemic-related shocks.

9.1.5 Matching Analysis

Another concern is that the 2024 cohort may be an imperfect counterfactual because it is composed of future designated teachers rather than truly never-treated teachers. If later cohorts differ systematically from earlier ones, or if teachers and districts adjust behavior in anticipation of designation, then the baseline estimates may partly reflect cohort composition rather than the effect of designation itself. To address this concern, I re-estimate the event study on a matched sample. I construct the matched sample separately by designation cohort, assigning each teacher to the cohort of the first year in which the teacher receives a designation. For each cohort from 2019 through 2024, the treated group consists of designated teachers with non-missing pre-designation value-added, using the most recent available pre-designation measure for that cohort. The donor pool consists of never-treated teachers with the same value-added availability drawn from never-treated districts. To avoid reusing the same comparison teachers across cohorts, I remove teachers selected into earlier matched cohorts from later donor pools. Within each cohort, I implement one-to-one nearest-neighbor matching without replacement using Mahalanobis distance. The matching variables include pre-designation value-added, campus enrollment, district enrollment, and the campus share of economically disadvantaged students. I also require exact matches on experience bin and subject area. I then stack the cohort-specific matched samples into a single panel and re-estimate equation 7 on the resulting matched sample. I provide an analogous table 3 for the matched sample in Appendix Table B.1.

The CS estimates on the matched sample are larger than the full-sample results, though the dynamic pattern is largely preserved. The estimates are also noisier, as expected given the smaller estimation sample. Because matching requires a pre-designation value-added measure, the matched sample is restricted to teachers in tested grades and subjects, a population whose mobility need not respond to designation in the same way as the full sample. I therefore read the matched estimates as evidence on the dynamic pattern rather than on magnitudes.

9.1.6 Balanced Panel

An additional concern is that the estimates of the event study may be driven by changing sample composition rather than treatment effects. In staggered-treatment settings, coefficients at longer leads and lags are typically identified from smaller and different sets of cohorts, so comparisons across event time may partly reflect differences in cohorts rather than genuine dynamics. To address this concern, I re-estimate the event-study using a balanced event-time window from -3 to 3, rather than the broader baseline window of -5 to 4. Restricting the analysis in this way ensures that each reported coefficient is estimated from cohorts observed over the full balanced range of event time, providing a cleaner assessment of both pre-trends and post-designation dynamics.

The balanced-window CS estimates are nearly identical to the full-sample results across all mobility outcomes. Pre-trends appear somewhat cleaner in the balanced sample, with no significant pre-period coefficients on the primary mobility outcomes. These results indicate that the dynamics documented in the main specification are not artifacts of changing cohort composition across event time.

9.2 Discrete Choice Model

9.2.1 Sensitivity to Salary Imputation

The baseline specification uses the salary schedule recovered from district-year-experience-degree cell medians, with missing cells filled by linear interpolation and prediction from district pay levels and statewide experience profiles. To test whether the estimates are sensitive to this imputation, I re-estimate the mixed logit on choice sets restricted to what I call valid salary cells: district-year-experience-degree cells with at least N directly observed teacher salaries, with no imputation. I run this for $N \geq 5$, $N \geq 10$, and $N \geq 15$. The restriction works on both sides of the choice problem. A teacher is dropped if her own current cell is invalid, and a campus is removed from her choice set if the salary cell she would occupy there next year is invalid.

Table 10 reports the results next to the baseline.

Table 10: Teacher Preference Estimates: Robustness to Salary Cell Definition

	Baseline (1)	$N \geq 5$ (2)	$N \geq 10$ (3)	$N \geq 15$ (4)
Constant (μ_A)	6.686			
	(0.232)			
Distance (δ)	−0.082			
	(0.001)			
District switching (η)	−1.940			
	(0.022)			
School preferences and heterogeneity				
School SES (μ_β)	−0.114			
	(0.007)			
St. dev. school SES RC (σ_β)	0.097			
	(0.011)			
Log wage coefficient (μ_α)	−4.024			
	(0.048)			
St. dev. log wage coefficient (σ_α)	0.410			
	(0.038)			
Log school switching cost (μ_γ)	1.846			
	(0.003)			
SES loading in log switching cost ($\sigma_{\gamma\beta}$)	0.225			
	(0.007)			
Independent loading in log switching cost (σ_γ)	−0.024			
	(0.056)			
Teacher characteristics				
Black	0.175			
	(0.087)			
White	0.055			
	(0.077)			
Hispanic	0.250			
	(0.080)			
Experience 0–3	−0.656			
	(0.061)			
Experience 4–10	−0.342			
	(0.052)			
Experience 11–20	0.418			
	(0.052)			
Choice-set averages				
Wage mean	−0.075			
	(0.003)			
School SES mean	1.161			
	(0.058)			
Distance mean	0.024			
	(0.002)			
Number of teachers	37,441			
Number of teacher-year observations	210,319			
Log-likelihood	−156,103.728			
AIC	312,257.455			

Notes: Robust standard errors in parentheses. Column (1) reports the baseline specification using the full salary schedule with imputed cells. Columns (2)–(4) restrict choice sets to valid salary cells, defined as district-year-experience-degree cells containing at least 5, 10, and 15 directly observed teacher salaries, respectively, with no imputation. Estimation by maximum simulated likelihood using the BGW algorithm with 300 Sobol draws with inter-individual variation, implemented in Apollo (Hess & Palma, 2019). The random coefficients (RC) are $\beta_i = \mu_\beta + \sigma_\beta v_{i1}$, $\gamma_i = -\exp(\mu_\gamma + \sigma_{\gamma\beta} v_{i1} + \sigma_\gamma v_{i2})$, and $\alpha_i = \exp(\mu_\alpha + \sigma_\alpha v_{i3})$, where the v_{ik} are independent standard normal draws held fixed across each teacher's choices. Wage parameters describe $\ln(\alpha_i)$; switching parameters describe $\ln(-\gamma_i)$. The parameter σ_γ is an independent loading, not the total standard deviation of log switching costs, which is $\sqrt{\sigma_{\gamma\beta}^2 + \sigma_\gamma^2}$. Choice-set averages are calculated before sampling and exclude the outside option. Teacher characteristics, choice-set averages, and year indicators enter the common school intercept A_{it}. The omitted year is 2016. Compensation is measured in thousands of 2024 dollars, distance in miles, and school SES in standard deviations, with higher values indicating greater disadvantage.

9.2.2 Sensitivity to Radius Cutoff

To construct the choice sets used to estimate the baseline specification, I use a 65-mile radius from a teacher's origin school. This cutoff was generated to capture 90% of all moves during the sample period. However, teachers' actual geographic preferences are unobserved, and the estimated preferences could be sensitive to this restriction. To test this, I reestimate the model using cutoffs that capture 85% and 95% of all moves, which corresponds to 30 miles and 165 miles, respectively. These specifications test whether the results are robust to both a narrower choice set containing more geographically proximate alternatives and a broader choice set that allows teachers to consider more distant schools.

Table 11 reports the results next to the baseline.

Table 11: Teacher Preference Estimates: Robustness to Choice-Set Radius

	Baseline (1)	30-mile radius (2)	165-mile radius (3)
Constant (μ_A)	6.686		
	(0.232)		
Distance (δ)	−0.082		
	(0.001)		
District switching (η)	−1.940		
	(0.022)		
School preferences and heterogeneity			
School SES (μ_β)	−0.114		
	(0.007)		
St. dev. school SES RC (σ_β)	0.097		
	(0.011)		
Log wage coefficient (μ_α)	−4.024		
	(0.048)		
St. dev. log wage coefficient (σ_α)	0.410		
	(0.038)		
Log school switching cost (μ_γ)	1.846		
	(0.003)		
SES loading in log switching cost ($\sigma_{\gamma\beta}$)	0.225		
	(0.007)		
Independent loading in log switching cost (σ_γ)	−0.024		
	(0.056)		
Teacher characteristics			
Black	0.175		
	(0.087)		
White	0.055		
	(0.077)		
Hispanic	0.250		
	(0.080)		
Experience 0–3	−0.656		
	(0.061)		
Experience 4–10	−0.342		
	(0.052)		
Experience 11–20	0.418		
	(0.052)		
Choice-set averages			
Wage mean	−0.075		
	(0.003)		
School SES mean	1.161		
	(0.058)		
Distance mean	0.024		
	(0.002)		
Number of teachers	37,441		
Number of teacher-year observations	210,319		
Log-likelihood	−156,103.728		
AIC	312,257.455		

Notes: Robust standard errors in parentheses. Column (1) reports the baseline specification using a 65-mile choice-set radius. Columns (2) and (3) use 30-mile and 165-mile radii, respectively. Estimation by maximum simulated likelihood using the BGW algorithm with 300 Sobol draws with inter-individual variation, implemented in Apollo (Hess & Palma, 2019). The random coefficients (RC) are $\beta_i = \mu_\beta + \sigma_\beta v_{i1}$, $\gamma_i = -\exp(\mu_\gamma + \sigma_{\gamma\beta} v_{i1} + \sigma_\gamma v_{i2})$, and $\alpha_i = \exp(\mu_\alpha + \sigma_\alpha v_{i3})$, where the v_{ik} are independent standard normal draws held fixed across each teacher's choices. Wage parameters describe $\ln(\alpha_i)$; switching parameters describe $\ln(-\gamma_i)$. The parameter σ_γ is an independent loading, not the total standard deviation of log switching costs, which is $\sqrt{\sigma_{\gamma\beta}^2 + \sigma_\gamma^2}$. Choice-set averages are calculated before sampling and exclude the outside option. Teacher characteristics, choice-set averages, and year indicators enter the common school intercept A_{it}. The omitted year is 2016. Compensation is measured in thousands of 2024 dollars, distance in miles, and school SES in standard deviations, with higher values indicating greater disadvantage.

10 Conclusion

Employers can use both monetary and non-monetary compensation to offset unfavorable working conditions (Rosen, 1986). However, a salary schedule only allows schools with less desirable attributes to compete on the non-monetary dimension. Asymmetric information about productivity adds a second layer of friction: a teacher's current employer knows more about their effectiveness than prospective employers, and this informational advantage can help less desirable schools retain effective teachers. Texas' Teacher Incentive Allotment (TIA) affects both frictions. Its portable designation makes effectiveness visible to the public, creating a credentialing channel that expands designated teachers' outside options. Its allotment increases with campus disadvantage, creating a compensation channel that increases the return to working in disadvantaged schools. Effective teachers in disadvantaged schools face two channels pushing in opposite directions.

The most immediate effect on mobility following evaluation is mechanical. Moves are cut sharply because teachers have to stay in their district to obtain their first allotment. In later years there are gradual increases in mobility. Therefore, the reduction is not permanent. Designated teachers also sort to more advantaged schools, and departures from advantaged schools diminish at all designation levels with no apparent ordering. Taken together, these estimates suggest the net movement of effective teachers to advantaged schools.

These effects vary considerably by designation tier. At the Recognized level, where the premium is the lowest, designated teachers become more mobile, moving to advantaged campuses and leaving disadvantaged ones more frequently. This pattern is consistent with the credentialing channel outweighing the compensation channel, just as high-value-added teachers moved toward higher-performing schools once hiring principals could observe their value-added (Bates, 2020). As the premium grows, the response weakens. At the Master level, designated teachers become less mobile and less likely to leave disadvantaged campuses, indicating that the compensation channel becomes strong enough to offset the credential's expanded outside options. This ordering is consistent with the choice estimates. The Recognized, Exemplary, and Master salary gradients offset the estimated disadvantage penalty for 26.8, 33.3, and 45.1 percent of teachers, respectively.

No designation tier appears to move into disadvantaged campuses consistently, so the program's contribution to staffing in those schools is based on retention, as demonstrated by the Master tier. Because underprivileged campuses don't seem to replace high-value-added teachers with equally effective ones, the retention margin is important. The descriptive evidence from the pre-TIA data suggests that the distribution of value-added for teachers entering disadvantaged campuses is fairly similar whether any teacher leaves or a high-value-added teacher leaves. The Master results therefore indicate a benefit for disadvantaged schools even without increased recruitment.

The policy implication is that credentialing effective teachers without adequate compensation for disadvantage may make them more easily recruited by more advantaged schools. This balance may be shifted toward the compensation channel by steeper SES scaling at the lower designation tiers. Given the limited evidence of increased recruitment to disadvantaged campuses, the most compelling argument for this change is its potential to retain effective teachers who are already working in those schools.

References

Bates, M. (2020, April). Public and Private Employer Learning: Evidence from the Adoption of Teacher Value Added. *Journal of Labor Economics*, *38*(2), 375–420. Retrieved 2026-08-24, from https://www.journals.uchicago.edu/doi/abs/10.1086/705881 doi: 10.1086/705881

Bates, M., Dinerstein, M., Johnston, A. C., & Sorkin, I. (2025, April). Teacher Labor Market Policy and the Theory of the Second Best. *The Quarterly Journal of Economics*, *140*(2), 1417–1469. Retrieved 2026-08-23, from https://academic.oup.com/qje/article/140/2/1417/7914177 doi: 10.1093/qje/qjae042

Biasi, B. (2021, August). The Labor Market for Teachers under Different Pay Schemes. *American Economic Journal: Economic Policy*, *13*(3), 63–102. Retrieved 2026-04-27, from https://pubs.aeaweb.org/doi/10.1257/pol.20200295 doi: 10.1257/pol.20200295

Biasi, B., Fu, C., & Stromme, J. (2025, December). Equilibrium in the Market for Public School Teachers: District Wage Strategies and Teacher Comparative Advantage.

Borusyak, K., Jaravel, X., & Spiess, J. (2024, November). Revisiting Event-Study Designs: Robust and Efficient Estimation. *The Review of Economic Studies*, *91*(6), 3253–3285. Retrieved from https://doi.org/10.1093/restud/rdae007 doi: 10.1093/restud/rdae007

Boyd, D., Grossman, P., Ing, M., Lankford, H., Loeb, S., & Wyckoff, J. (2011). The Influence of School Administrators on Teacher Retention Decisions. *American Educational Research Journal*, *48*(2), 303–333. Retrieved from https://www.jstor.org/stable/27975291

Boyd, D., Lankford, H., Loeb, S., & Wyckoff, J. (2005a). The draw of home: How teachers' preferences for proximity disadvantage urban schools. *Journal of Policy Analysis and Management*, *24*(1), 113–132. Retrieved from https://onlinelibrary.wiley.com/doi/10.1002/pam.20072 doi: 10.1002/pam.20072

Boyd, D., Lankford, H., Loeb, S., & Wyckoff, J. (2005b, May). Explaining the Short Careers of High-Achieving Teachers in Schools with Low-Performing Students. *American Economic Review*, *95*(2), 166–171. Retrieved from https://www.aeaweb.org/articles?id=10.1257/000282805774669628 doi: 10.1257/000282805774669628

Boyd, D., Lankford, H., Loeb, S., & Wyckoff, J. (2013, January). Analyzing the Determinants of the Matching of Public School Teachers to Jobs: Disentangling the Preferences of Teachers and Employers. *Journal of Labor Economics*, *31*(1), 83–117. Retrieved 2026-08-23, from https://www.journals.uchicago.edu/doi/10.1086/666725 doi: 10.1086/666725

Callaway, B., & Sant'Anna, P. H. C. (2021, December). Difference-in-Differences with multiple time periods. *Journal of Econometrics*, *225*(2), 200–230. Retrieved from https://www.sciencedirect.com/science/article/pii/S0304407620303948 doi: 10.1016/j.jeconom.2020.12.001

Chamberlain, G. (1982, January). Multivariate regression models for panel data. *Journal of Econometrics*, *18*(1), 5–46. Retrieved 2026-10-16, from

https://www.sciencedirect.com/science/article/pii/030440768290094X doi: 10.1016/0304-4076(82)90094-X

Chetty, R., Friedman, J. N., & Rockoff, J. E. (2014a, September). Measuring the Impacts of Teachers I: Evaluating Bias in Teacher Value-Added Estimates. *American Economic Review*, *104*(9), 2593–2632. Retrieved from https://pubs.aeaweb.org/doi/10.1257/aer.104.9.2593 doi: 10.1257/aer.104.9.2593

Chetty, R., Friedman, J. N., & Rockoff, J. E. (2014b, September). Measuring the Impacts of Teachers II: Teacher Value-Added and Student Outcomes in Adulthood. *American Economic Review*, *104*(9), 2633–2679. Retrieved from https://www.aeaweb.org/articles?id=10.1257/aer.104.9.2633 doi: 10.1257/aer.104.9.2633

Clotfelter, C., Glennie, E., Ladd, H., & Vigdor, J. (2008, June). Would higher salaries keep teachers in high-poverty schools? Evidence from a policy intervention in North Carolina. *Journal of Public Economics*, *92*(5-6), 1352–1370. Retrieved from https://linkinghub.elsevier.com/retrieve/pii/S0047272707001065 doi: 10.1016/j.jpubeco.2007.07.003

Clotfelter, C., Ladd, H. F., & Vigdor, J. L. (2011, July). Teacher Mobility, School Segregation, and Pay-Based Policies to Level the Playing Field. *Education Finance and Policy*, *6*(3), 399–438. Retrieved from https://doi.org/10.1162/EDFP_a_00040 doi: 10.1162/EDFP_a_00040

Cowan, J., & Goldhaber, D. (2018, August). Do bonuses affect teacher staffing and student achievement in high poverty schools? Evidence from an incentive for national board certified teachers in Washington State. *Economics of Education Review*, *65*, 138–152. doi: 10.1016/j.econedurev.2018.06.010

de Chaisemartin, C., & D'Haultfœuille, X. (2020, September). Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review*, *110*(9), 2964–2996. Retrieved from https://www.aeaweb.org/articles?id=10.1257/aer.20181169 doi: 10.1257/aer.20181169

Dee, T. S., & Wyckoff, J. (2015). Incentives, Selection, and Teacher Performance: Evidence from IMPACT. *Journal of Policy Analysis and Management*, *34*(2), 267–297. Retrieved from https://onlinelibrary.wiley.com/doi/abs/10.1002/pam.21818 (_eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1002/pam.21818) doi: 10.1002/pam.21818

Dekker, T., Bansal, P., & Huo, J. (2025, February). Revisiting McFadden's correction factor for sampling of alternatives in multinomial logit and mixed multinomial logit models. *Transportation Research Part B: Methodological*, *192*, 103129. Retrieved from https://www.sciencedirect.com/science/article/pii/S0191261524002534 doi: 10.1016/j.trb.2024.103129

Feng, L., & Sass, T. R. (2017, July). Teacher Quality and Teacher Mobility. *Education Finance and Policy*, *12*(3), 396–418. Retrieved from https://doi.org/10.1162/EDFP_a_00214 doi: 10.1162/EDFP_a_00214

Fulbeck, E. S. (2014, March). Teacher Mobility and Financial Incentives: A Descriptive Analysis of Denver's ProComp. *Educational Evaluation and Policy Analysis*, *36*(1), 67–82. Retrieved from https://doi.org/10.3102/0162373713503185 doi: 10.3102/0162373713503185

Glazerman, S., Protik, A., Teh, B.-r., Bruch, J., & Max, J. (2013, November). *Transfer Incentives for High-Performing Teachers: Final Results from a Multisite Randomized Experiment. NCEE 2014-4003* (Tech. Rep.). National Center for Education Evaluation and Regional Assistance. Retrieved from https://eric.ed.gov/?id=ED544269 (ERIC Number: ED544269)

Goldhaber, D., Lavery, L., & Theobald, R. (2015, June). Uneven Playing Field? Assessing the Teacher Quality Gap Between Advantaged and Disadvantaged Students. *Educational Researcher*, *44*(5), 293–307. Retrieved from https://doi.org/10.3102/0013189X15592622 doi: 10.3102/0013189X15592622

Goodman-Bacon, A. (2021, December). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, *225*(2), 254–277. Retrieved from https://www.sciencedirect.com/science/article/pii/S0304407621001445 doi: 10.1016/j.jeconom.2021.03.014

Guevara, C. A., & Ben-Akiva, M. E. (2013, December). Sampling of alternatives in Logit Mixture models. *Transportation Research Part B: Methodological*, *58*, 185–198. Retrieved from https://www.sciencedirect.com/science/article/pii/S0191261513001471 doi: 10.1016/j.trb.2013.08.011

Hanushek, E. A., Kain, J. F., & Rivkin, S. G. (2004). Why Public Schools Lose Teachers. *The Journal of Human Resources*, *39*(2), 326–354. Retrieved from https://www.jstor.org/stable/3559017 doi: 10.2307/3559017

Hanushek, E. A., Luo, J., Morgan, A., Nguyen, M., Ost, B., Rivkin, S., & Shakeel, A. (2026, May). The effects of comprehensive pay reform on achievement in urban schools. *Journal of Urban Economics*, *153*, 103857. Retrieved from https://www.sciencedirect.com/science/article/pii/S0094119026000288 doi: 10.1016/j.jue.2026.103857

Hess, S., & Palma, D. (2019, September). *Apollo*: A flexible, powerful and customisable freeware package for choice model estimation and application. *Journal of Choice Modelling*, *32*, 100170. Retrieved 2026-04-29, from https://www.sciencedirect.com/science/article/pii/S1755534519300703 doi: 10.1016/j.jocm.2019.100170

Imazeki, J. (2005, August). Teacher salaries and teacher attrition. *Economics of Education Review*, *24*(4), 431–449. Retrieved from https://www.sciencedirect.com/science/article/pii/S0272775704001189 doi: 10.1016/j.econedurev.2004.07.014

Ingersoll, R. M. (2001, January). Teacher Turnover and Teacher Shortages: An Organizational Analysis. *American Educational Research Journal*, *38*(3), 499–534. Retrieved 2026-08-24, from https://doi.org/10.3102/00028312038003499 doi: 10.3102/00028312038003499

Isenberg, E., Max, J., Gleason, P., Potamites, L., Santillano, R., Hock, H., & Hansen, M. (2013, November). *Access to Effective Teaching for Disadvantaged Students. NCEE 2014-4001* (Tech. Rep.). National Center for Education Evaluation and Regional Assistance. Retrieved from https://eric.ed.gov/?id=ED544345 (ERIC Number: ED544345)

Jackson, C. K. (2018, October). What Do Test Scores Miss? The Importance of Teacher Effects on Non–Test Score Outcomes. *Journal of Political Economy*, *126*(5), 2072–2107. Retrieved from https://www.journals.uchicago.edu/doi/abs/10.1086/699018 doi: 10.1086/699018

Jacob, B. A., Rockoff, J. E., Taylor, E. S., Lindy, B., & Rosen, R. (2018, October). Teacher applicant hiring and teacher performance: Evidence from DC public schools. *Journal of Public Economics*, *166*, 81–97. Retrieved from https://www.sciencedirect.com/science/article/pii/S0047272718301555 doi: 10.1016/j.jpubeco.2018.08.011

Johnston, A. C. (2025, July). Preferences, Selection, and the Structure of Teacher Pay. *American Economic Journal: Applied Economics*, *17*(3), 310–346. Retrieved 2026-08-23, from https://pubs.aeaweb.org/doi/10.1257/app.20210763 doi: 10.1257/app.20210763

Kirksey, J., Lansford, T., Crevar, A., & Mansell, K. (2024). From Incentive to Impact: The Texas Teacher Incentive Allotment's Path to Improved Retention and Achievement. Retrieved from https://hdl.handle.net/2346/100291

Lankford, H., Loeb, S., & Wyckoff, J. (2002, March). Teacher Sorting and the Plight of Urban Schools: A Descriptive Analysis. *Educational Evaluation and Policy Analysis*, *24*(1), 37–62. Retrieved from https://doi.org/10.3102/01623737024001037 doi: 10.3102/01623737024001037

McFadden, D. (1977). Modeling the Choice of Residential Location.

Mundlak, Y. (1978). On the Pooling of Time Series and Cross Section Data. *Econometrica*, *46*(1), 69–85. Retrieved 2026-10-16, from https://www.jstor.org/stable/1913646 doi: 10.2307/1913646

Papay, J. P., & Kraft, M. A. (2015, October). Productivity returns to experience in the teacher labor market: Methodological challenges and new evidence on long-term career improvement. *Journal of Public Economics*, *130*, 105–119. Retrieved from https://linkinghub.elsevier.com/retrieve/pii/S0047272715000304 doi: 10.1016/j.jpubeco.2015.02.008

Rockoff, J. E. (2004, May). The Impact of Individual Teachers on Student Achievement: Evidence from Panel Data. *The American Economic Review*, *94*(2), 247–252. Retrieved from https://research.ebsco.com/linkprocessor/plink?id=bea72a73-88c8-3c14-a13d-ccd8344ffd1f

Rosen, S. (1986). Chapter 12 The theory of equalizing differences. In *Handbook of Labor Economics* (Vol. 1, pp. 641–692). Elsevier. Retrieved from https://linkinghub.elsevier.com/retrieve/pii/S1573446386010155 doi: 10.1016/S1573-4463(86)01015-5

Springer, M. G., Swain, W. A., & Rodriguez, L. A. (2016, June). Effective Teacher Retention Bonuses: Evidence From Tennessee. *Educational Evaluation and Policy Analysis*, *38*(2), 199–221. Retrieved from https://doi.org/10.3102/0162373715609687 doi: 10.3102/0162373715609687

Steele, J. L., Murnane, R. J., & Willett, J. B. (2010). Do Financial Incentives Help Low-Performing Schools Attract and Keep Academically Talented Teachers? Evidence from California. *Journal of Policy Analysis and Management*, *29*(3), 451–478. Retrieved from https://www.jstor.org/stable/40802084

Sun, L., & Abraham, S. (2021, December). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, *225*(2), 175–199. Retrieved 2026-08-12, from https://www.sciencedirect.com/science/article/pii/S030440762030378X doi: 10.1016/j.jeconom.2020.09.006

Svitek, B. P. (2019, June). Gov. Greg Abbott signs $11.6 billion school finance measure into law. *The Texas Tribune*.

Texas Education Agency. (2024, June). TEA Awards Over $290 Million as Part of The Teacher Incentive Allotment. Retrieved from https://tea.texas.gov/about-tea/news-and-multimedia/news-releases/news-2024/tea-awards-over-290-million-as-part-of-the-teacher-incentive-allotment

Wooldridge, J. M. (2023, September). Simple approaches to nonlinear difference-in-differences with panel data. *The Econometrics Journal*, *26*(3), C31–C66. Retrieved 2026-08-12, from https://academic.oup.com/ectj/article/26/3/C31/7250479 doi: 10.1093/ectj/utad016

Wooldridge, J. M. (2025, November). Two-way fixed effects, the two-way mundlak regression, and difference-in-differences estimators. *Empirical Economics*, *69*(5), 2545–2587. Retrieved 2026-08-12, from https://doi.org/10.1007/s00181-025-02807-z doi: 10.1007/s00181-025-02807-z

Appendix A Construction of Teacher Value-Added Estimates

I estimate teacher value-added using a modified leave-year-out empirical Bayes procedure adapted from Jackson (2018). Following that framework, I first residualize student achievement with respect to predetermined student characteristics and peer composition, and I then use residual variation across teachers to construct out-of-sample measures of teacher effectiveness. This approach is conceptually similar to Jackson (2018), although my first-stage control set is tailored to my data.

I begin by standardizing test scores within year-by-subject-by-grade cells so that scores have mean zero and unit variance within each testing cohort. I exclude classrooms with 5 or fewer tested students. For both mathematics and reading, I measure prior achievement using two lagged test-score controls: the student's lagged score in both math and reading. A lag is treated as valid when the lag score in year t is observed in year $t-1$; because statewide testing was disrupted during the pandemic and no test scores exist for the 2019 school year, I allow 2018 scores to serve as the lag for 2020 scores.

For each subject, I estimate a residualized achievement model of the form:

$$Y_{icgt} = f(B_{it}, G_{it}) + \beta X_{it} + \gamma \bar{X}_{-i,ct} + \delta \bar{X}_{-i,sgt} + u_{icgt}$$

where Y_{icgt} is the standardized score of student i in classroom c in grade g in year t. B_{it} denotes baseline achievement, G_{it} denotes grade, X_{it} includes individual demographic controls, $\bar{X}_{-i,ct}$ denotes leave-one-out classroom means, and $\bar{X}_{-i,sgt}$ denotes leave-one-out school-by-grade-by-year means. The individual controls include the age of the student as of September 1st during that year, economic disadvantage status, race and ethnicity indicators, and at-risk status. The peer controls include leave-one-out averages of prior achievement and demographic composition at both the classroom and school-grade-year levels, as well as classroom size. The function $f(B_{it}, G_{it})$ is a flexible grade-specific cubic in baseline achievement so that the relationship between current achievement and prior achievement may be nonlinear and may vary across grades.

I interpret the residual u_{icgt} as the sum of three components:

$$u_{icgt} = \theta_j + \eta_{ct} + \varepsilon_{icgt}$$

where θ_j is the teacher component, η_{ct} is a classroom-year shock, and ε_{icgt} is idiosyncratic student-level noise. I then aggregate residuals to the classroom-year level:

$$v_{ct} = \frac{1}{n_{ct}} \sum_{i \in c,t} u_{icgt}$$

where n_{ct} is the number of tested students in classroom c in year t.

Following Jackson (2018), I use the covariance structure of these residuals to estimate the variance components needed for empirical Bayes shrinkage. I estimate the variance of the teacher component, σ^2_θ, using the covariance of classroom-year mean residuals across years for the same teacher. I estimate student-level noise, σ^2_ε, from within-classroom residual variation. I then recover the variance of classroom-year shocks, σ^2_η, as the remaining residual variance after accounting for the teacher and student-level components.

Using these variance components, I construct two out-of-sample teacher value-added measures for each teacher-year. The first is a jackknife leave-year-out measure based on all classroom-year observations for

that teacher outside year t, which uses the fullest available information and is therefore more precisely estimated. The second is a prior-years-only measure based only on classroom-year observations from years prior to year t, which addresses concerns that TIA may affect teacher performance and thereby contaminate value-added measures that incorporate post-t outcomes. For each version, I compute both an unweighted raw mean of classroom-year residual means and a precision-weighted mean.

$$\widetilde{VA}_{i,-t} = \frac{\sum_r h_r v_r}{\sum_r h_r} \qquad h_r = \frac{1}{\sigma_\eta^2 + \sigma_\varepsilon^2/n_r}$$

where r indexes the relevant classroom-year observations for teacher i, v_r is the classroom-year mean residual, and n_r is the number of tested students in that classroom-year. Thus, classroom-year observations measured with greater precision receive more weight.
I then shrink the precision-weighted estimate toward zero using its estimated reliability:

$$\lambda_{i,-t} = \frac{\sigma_\theta^2}{\sigma_\theta^2 + 1/\sum_r h_r} \qquad \widehat{VA}^{EB}_{i,-t} = \lambda_{i,-t}\widetilde{VA}_{i,-t}.$$

This empirical Bayes adjustment shrinks noisier teacher estimates more strongly toward zero and leaves more precise estimates closer to their weighted means. I retain both the raw and empirical-Bayes-shrunken estimates for each teacher-by-subject-by-year cell, for both the prior-years and jackknife versions. In the analysis, my preferred measure is the empirical-Bayes-shrunken estimate using the jackknife version, as it provides more opportunities for matches. In testing between using prior years only and jackknife versions, there were no qualitative differences.

Appendix B Tables

Year	New Teachers Designated	Total Teachers Designated
2019	695	695
2020	200	895
2021	243	1,138
2022	1,858	2,996
2023	2,651	5,647
2024	3,263	8,910

Notes: Counts reflect teachers newly designated in each year and the running cumulative total. Sample comes from teachers who can be matched to a pre-designation value-added score.

Table B.1: New and Cumulative TIA Designations

	Move Campus	ΔSES	Move to Low-SES Campus	Move to High-SES Campus	Move to Rural Campus
δ_{-5}	0.006	-0.113	0.002	0.001	-0.002
	(0.004)	(0.133)	(0.001)	(0.002)	(0.002)
δ_{-4}	0.007*	-0.203	0.002	0.002	-0.002
	(0.004)	(0.128)	(0.001)	(0.002)	(0.002)
δ_{-3}	0.007*	-0.079	0.001	0.001	-0.002
	(0.004)	(0.121)	(0.001)	(0.001)	(0.002)
δ_{-2}	0.003	-0.155	0.002	0.002	-0.001
	(0.003)	(0.114)	(0.001)	(0.001)	(0.002)
δ_0	-0.064***	0.304***	-0.006***	-0.011***	-0.012***
	(0.003)	(0.094)	(0.001)	(0.001)	(0.001)
δ_1	-0.020***	-0.245*	-0.004***	0.002	-0.007***
	(0.005)	(0.139)	(0.002)	(0.002)	(0.002)
δ_2	0.005	-0.777***	0.003	0.008***	-0.006**
	(0.007)	(0.215)	(0.002)	(0.002)	(0.003)
δ_3	0.012	-0.488*	0.005*	0.005*	0.000
	(0.008)	(0.258)	(0.003)	(0.003)	(0.004)
δ_4	0.019**	-0.446*	0.004	0.002	0.003
	(0.009)	(0.266)	(0.003)	(0.003)	(0.004)
$\sum_{i=0}^{4} \delta_i$	-0.049**	–	0.002	0.006	-0.021**
	(0.022)	–	(0.007)	(0.007)	(0.010)
N	314,904	309,447	314,904	314,904	314,904

Table B.2: Callaway-Sant'Anna Estimates for Overall Mobility and Destination School SES

Notes: Teacher-level clustered standard errors in parentheses. Omitted period is $t = -1$. The sample size decreases for ΔSES because it is conditional on staying in teaching. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	Move from Low-SES Campus	Move from High-SES Campus	Move from Rural Campus
δ_{-5}	0.003	0.001	0.001
	(0.002)	(0.001)	(0.002)
δ_{-4}	0.003	0.001	0.002
	(0.002)	(0.001)	(0.002)
δ_{-3}	0.000	0.002	0.001
	(0.002)	(0.001)	(0.002)
δ_{-2}	0.000	0.000	0.001
	(0.002)	(0.001)	(0.002)
δ_0	-0.013***	-0.006***	-0.012***
	(0.001)	(0.001)	(0.001)
δ_1	-0.007***	-0.005***	0.000
	(0.002)	(0.001)	(0.002)
δ_2	0.005*	-0.005***	-0.001
	(0.003)	(0.002)	(0.003)
δ_3	0.005	-0.007***	0.002
	(0.004)	(0.002)	(0.003)
δ_4	0.005	-0.005**	0.003
	(0.004)	(0.002)	(0.004)
$\sum_{i=0}^{4} \delta_i$	-0.004	-0.027***	-0.008
	(0.010)	(0.005)	(0.009)
N	314,904	314,904	314,904

Table B.3: Callaway-Sant'Anna Estimates for Origin School Context

Notes: Teacher-level clustered standard errors in parentheses. Omitted period is $t = -1$. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	Move within District	Move within County	Move within TEA Region
δ_{-5}	0.000	0.005	0.004
	(0.003)	(0.004)	(0.004)
δ_{-4}	0.004	0.005	0.006*
	(0.003)	(0.003)	(0.004)
δ_{-3}	0.004	0.004	0.002
	(0.003)	(0.003)	(0.003)
δ_{-2}	0.002	0.001	0.001
	(0.002)	(0.003)	(0.003)
δ_0	-0.002	-0.029***	-0.042***
	(0.002)	(0.003)	(0.003)
δ_1	-0.002	-0.012***	-0.014***
	(0.003)	(0.004)	(0.004)
δ_2	0.002	0.001	0.007
	(0.005)	(0.006)	(0.006)
δ_3	0.010	0.007	0.012
	(0.007)	(0.008)	(0.008)
δ_4	0.014*	0.011	0.017*
	(0.008)	(0.008)	(0.009)
$\sum_{i=0}^{4} \delta_i$	0.023	-0.022	-0.020
	(0.018)	(0.020)	(0.021)
N	314,904	314,904	314,904

Table B.4: Callaway-Sant'Anna Estimates for Geographic Scope of Teacher Mobility

Notes: Teacher-level clustered standard errors in parentheses. Omitted period is $t = -1$. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	Move Campus			ΔSES		
	Recognized	Exemplary	Master	Recognized	Exemplary	Master
δ_{-5}	0.005	0.005	0.008	-0.369	0.276	-0.413
	(0.007)	(0.007)	(0.008)	(0.235)	(0.217)	(0.256)
δ_{-4}	0.011	0.006	0.007	-0.338	-0.005	-0.521**
	(0.007)	(0.006)	(0.008)	(0.223)	(0.209)	(0.245)
δ_{-3}	0.003	0.008	0.006	-0.224	-0.071	-0.000
	(0.007)	(0.006)	(0.008)	(0.210)	(0.194)	(0.240)
δ_{-2}	-0.004	0.005	0.004	-0.416**	-0.005	-0.101
	(0.006)	(0.005)	(0.007)	(0.197)	(0.185)	(0.219)
δ_0	-0.064***	-0.065***	-0.065***	-0.018	0.611***	0.157
	(0.006)	(0.005)	(0.006)	(0.165)	(0.151)	(0.188)
δ_1	0.008	-0.028***	-0.045***	-1.141***	0.255	-0.091
	(0.008)	(0.007)	(0.009)	(0.252)	(0.227)	(0.280)
δ_2	0.027**	0.005	-0.038***	-1.622***	-0.546	0.360
	(0.011)	(0.010)	(0.013)	(0.366)	(0.351)	(0.387)
δ_3	0.039***	0.012	-0.042**	-0.802*	-0.670	0.467
	(0.013)	(0.014)	(0.017)	(0.421)	(0.414)	(0.523)
δ_4	0.027**	0.040***	-0.035*	-0.542	-1.004**	0.546
	(0.014)	(0.015)	(0.020)	(0.427)	(0.459)	(0.508)
N	102,509	130,456	81,939	100,283	128,324	80,840

Table B.5: Heterogeneity in Teacher Mobility and Destination School SES by Designation Level

Notes: Estimates are reported separately for mutually exclusive subsamples based on each teacher's highest observed designation. Thus, the Recognized sample includes only teachers whose highest observed designation is Recognized, the Exemplary sample includes only teachers whose highest observed designation is Exemplary, and the Master sample includes only teachers whose highest observed designation is Master. Teacher-level clustered standard errors in parentheses. Omitted period is $t = -1$. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	Move to Low-SES Campus			Move to High-SES Campus			Move to Rural Campus		
	Recognized	Exemplary	Master	Recognized	Exemplary	Master	Recognized	Exemplary	Master
δ_{-5}	0.004	0.002	0.002	0.002	-0.002	0.006*	-0.004	-0.002	-0.001
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
δ_{-4}	0.001	0.004*	-0.000	-0.002	0.003	0.005	-0.005	-0.003	0.000
	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
δ_{-3}	0.002	0.001	0.001	0.000	-0.000	0.002	-0.005	-0.001	0.001
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
δ_{-2}	0.000	0.002	0.003	0.002	0.001	0.003	-0.003	-0.001	-0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
δ_0	-0.008***	-0.005***	-0.007***	-0.010***	-0.011***	-0.009***	-0.016***	-0.010***	-0.011***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
δ_1	-0.001	-0.005*	-0.009***	0.010***	-0.002	-0.001	-0.006	-0.005	-0.011***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
δ_2	0.002	0.005	0.004	0.014***	0.007**	-0.003	-0.004	-0.005	-0.011*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)	(0.005)	(0.006)
δ_3	0.005	0.003	0.008	0.010**	0.005	-0.003	0.006	0.002	-0.014*
	(0.005)	(0.004)	(0.006)	(0.005)	(0.004)	(0.005)	(0.006)	(0.006)	(0.007)
δ_4	0.007	0.003	0.001	-0.001	0.009*	-0.007	0.004	0.005	-0.002
	(0.005)	(0.005)	(0.006)	(0.004)	(0.005)	(0.005)	(0.007)	(0.007)	(0.010)
N	102,509	130,456	81,939	102,509	130,456	81,939	102,509	130,456	81,939

Table B.6: Heterogeneity in Destination School Context by Designation Level

Notes: Estimates are reported separately for mutually exclusive subsamples based on each teacher's highest observed designation. Thus, the Recognized sample includes only teachers whose highest observed designation is Recognized, the Exemplary sample includes only teachers whose highest observed designation is Exemplary, and the Master sample includes only teachers whose highest observed designation is Master. Teacher-level clustered standard errors in parentheses. Omitted period is $t = -1$. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	Move from High-SES Campus			Move from Low-SES Campus			Move from Rural Campus		
	Recognized	Exemplary	Master	Recognized	Exemplary	Master	Recognized	Exemplary	Master
δ_{-5}	-0.000	0.003*	-0.002	0.007**	-0.000	0.004	0.003	-0.002	0.004
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
δ_{-4}	0.000	0.003*	-0.002	0.008**	0.000	0.002	0.004	-0.001	0.006*
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
δ_{-3}	0.001	0.002	0.002	0.001	0.001	-0.002	0.001	0.001	0.002
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)
δ_{-2}	-0.001	0.002	-0.000	0.001	0.001	-0.002	0.001	-0.002	0.004
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)
δ_0	-0.008***	-0.004**	-0.008***	-0.011***	-0.014***	-0.014***	-0.013***	-0.012***	-0.013***
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
δ_1	-0.006***	-0.002	-0.006**	0.006*	-0.010***	-0.016***	0.005	0.000	-0.004
	(0.002)	(0.002)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)
δ_2	-0.004	-0.004	-0.006*	0.017***	0.002	-0.010	0.002	0.003	-0.015***
	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)	(0.006)	(0.005)	(0.004)	(0.006)
δ_3	-0.007**	-0.008***	-0.007*	0.012*	0.003	-0.009	-0.002	0.013**	-0.010
	(0.003)	(0.003)	(0.004)	(0.006)	(0.006)	(0.009)	(0.006)	(0.006)	(0.007)
δ_4	-0.005	-0.004	-0.006	0.012*	0.012*	-0.019**	0.000	0.017**	-0.016**
	(0.003)	(0.004)	(0.005)	(0.007)	(0.007)	(0.009)	(0.006)	(0.007)	(0.007)
N	102,509	130,456	81,939	102,509	130,456	81,939	102,509	130,456	81,939

Table B.7: Heterogeneity in Origin School Context by Designation Level

Notes: Estimates are reported separately for mutually exclusive subsamples based on each teacher's highest observed designation. Thus, the Recognized sample includes only teachers whose highest observed designation is Recognized, the Exemplary sample includes only teachers whose highest observed designation is Exemplary, and the Master sample includes only teachers whose highest observed designation is Master. Teacher-level clustered standard errors in parentheses. Omitted period is $t = -1$. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

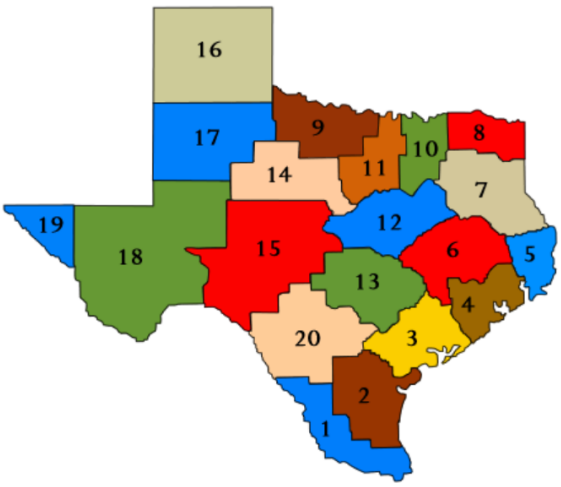
	Move within District			Move within County			Move within TEA Region		
	Recognized	Exemplary	Master	Recognized	Exemplary	Master	Recognized	Exemplary	Master
δ_{-5}	-0.001	0.006	-0.007	0.007	0.007	0.000	0.006	0.003	0.007
	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.008)
δ_{-4}	0.005	0.008*	-0.004	0.010*	0.007	-0.002	0.008	0.006	0.007
	(0.005)	(0.004)	(0.006)	(0.006)	(0.005)	(0.007)	(0.007)	(0.006)	(0.007)
δ_{-3}	0.000	0.010**	-0.001	0.003	0.006	0.003	-0.000	0.003	0.002
	(0.005)	(0.004)	(0.006)	(0.006)	(0.005)	(0.007)	(0.006)	(0.005)	(0.007)
δ_{-2}	-0.001	0.007*	-0.004	-0.003	0.006	-0.002	-0.002	0.005	-0.004
	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)
δ_0	0.002	-0.000	-0.009**	-0.027***	-0.027***	-0.036***	-0.039***	-0.042***	-0.048***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)
δ_1	0.012*	-0.008	-0.011	0.010	-0.019***	-0.029***	0.009	-0.022***	-0.031***
	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.008)	(0.007)	(0.007)	(0.008)
δ_2	0.016*	0.003	-0.021**	0.018*	0.003	-0.033***	0.028***	0.006	-0.030**
	(0.008)	(0.008)	(0.011)	(0.009)	(0.009)	(0.012)	(0.010)	(0.009)	(0.012)
δ_3	0.035***	0.007	-0.032**	0.033***	0.006	-0.043***	0.036***	0.012	-0.038**
	(0.011)	(0.011)	(0.014)	(0.012)	(0.012)	(0.016)	(0.012)	(0.013)	(0.016)
δ_4	0.016	0.027**	-0.011	0.013	0.034**	-0.032*	0.022*	0.041***	-0.034*
	(0.011)	(0.013)	(0.017)	(0.013)	(0.014)	(0.019)	(0.013)	(0.015)	(0.019)
N	102,509	130,456	81,939	102,509	130,456	81,939	102,509	130,456	81,939

Table B.8: Heterogeneity in the Geographic Scope of Teacher Mobility by Designation Level

Notes: Estimates are reported separately for mutually exclusive subsamples based on each teacher's highest observed designation. Thus, the Recognized sample includes only teachers whose highest observed designation is Recognized, the Exemplary sample includes only teachers whose highest observed designation is Exemplary, and the Master sample includes only teachers whose highest observed designation is Master. Teacher-level clustered standard errors in parentheses. Omitted period is $t = -1$. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix C Figures

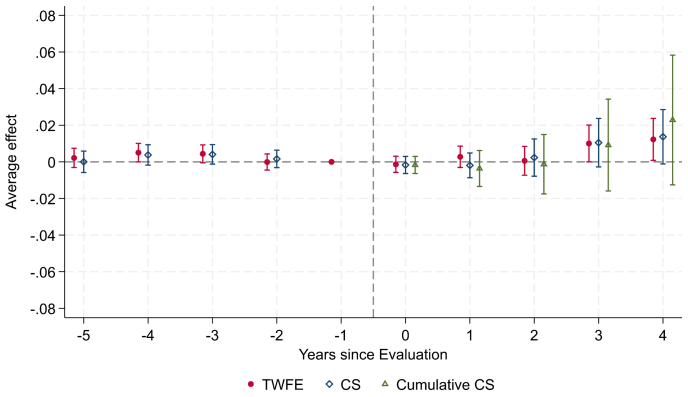
Figure C.1: TEA Region Map



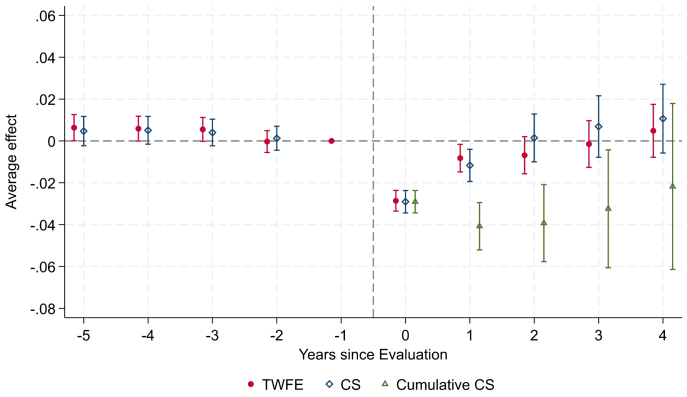
Source: `https://tea.texas.gov/about-tea/other-services/education-service-centers/education-service-centers-map`

Figure C.2: Estimated Effects of Designation on Geographic Mobility

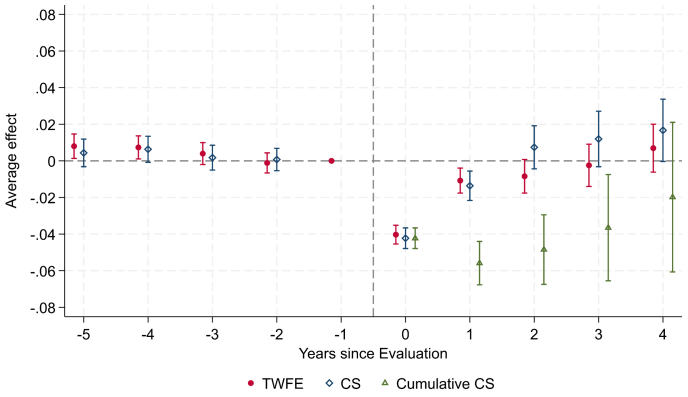
(a) Moves within Same School District



(b) Moves within Same County



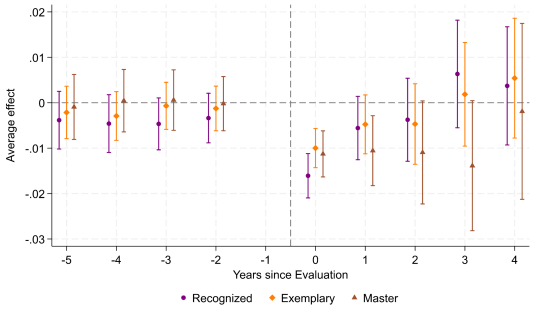
(c) Moves within TEA Region



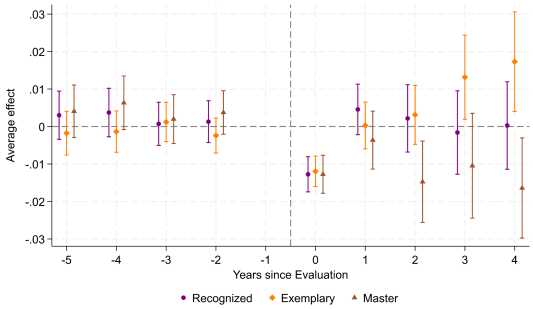
Notes: Time period $t = 0$ represents the school year in which a teacher is evaluated for designation. Error bars represent 95% confidence intervals. Red circles represent two-way fixed effects estimates. Blue diamonds represent Callaway & Sant'Anna (2021) estimates. Green triangles represent cumulative sums of the Callaway & Sant'Anna (2021) event-time estimates, such that the estimate at event time t equals $\sum_{\tau=0}^{t} \hat{\delta}_\tau$. The outcome in Panel A is an indicator for being observed at a campus in school district j in $t+1$ and a different campus in school district j in t. The outcome in Panel B is an indicator for being observed at a campus in county j in $t+1$ and a different campus in county j in t. The outcome in Panel C is an indicator for being observed at a campus in TEA region j in $t+1$ and a different campus in TEA region j in t.

Figure C.3: Estimated Effects of Designation on Rural and Geographic Mobility, by Designation Level

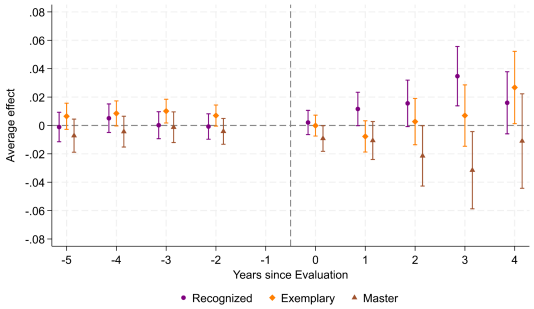
(a) Moves to Rural Campuses



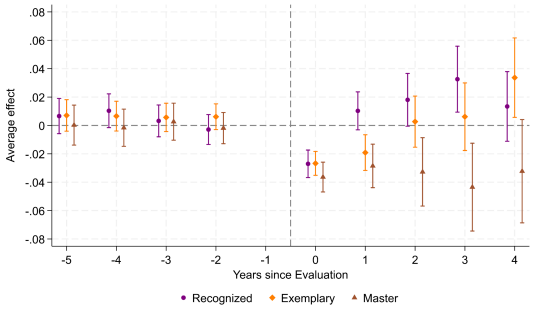
(b) Moves from Rural Campuses



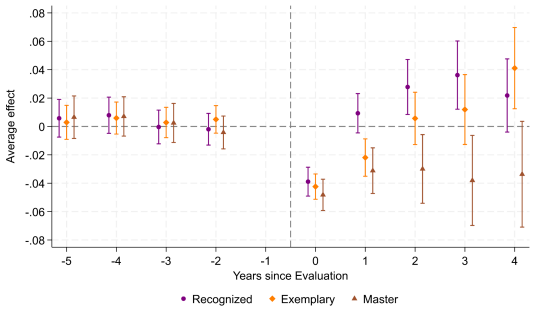
(c) Moves within Same School District



(d) Moves within Same County



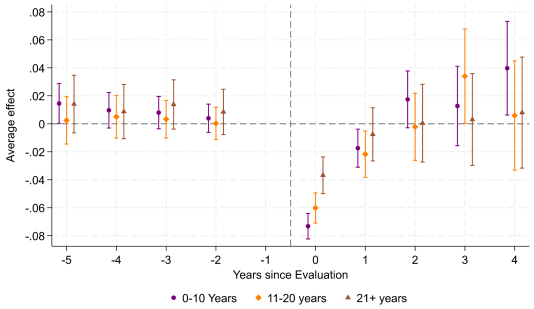
(e) Moves within TEA Region



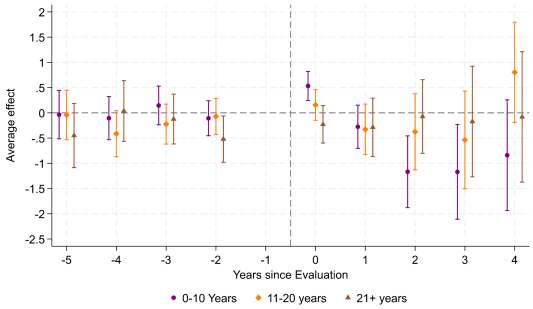
Notes: Time period $t = 0$ represents the school year in which a teacher is evaluated for designation. Error bars represent 95% confidence intervals. All results are estimated using Callaway & Sant'Anna (2021). Purple circle points represent Recognized level teachers. Orange diamond points represent Exemplary level teachers. Brown triangle points represent Master level teachers. The outcome in Panel A is an indicator for being observed at a rural campus in $t + 1$ and a non-rural campus in t. The outcome in Panel B is an indicator for being observed at a non-rural campus in $t + 1$ and a rural campus in t. The outcome in Panel C is an indicator for being observed at a campus in school district j in $t + 1$ and a different campus in school district j in t. The outcome in Panel D is an indicator for being observed at a campus in county j in $t + 1$ and a different campus in county j in t. The outcome in Panel E is an indicator for being observed at a campus in TEA region j in $t + 1$ and a different campus in TEA region j in t.

Figure C.4: Estimated Effects of Designation, by Years of Experience

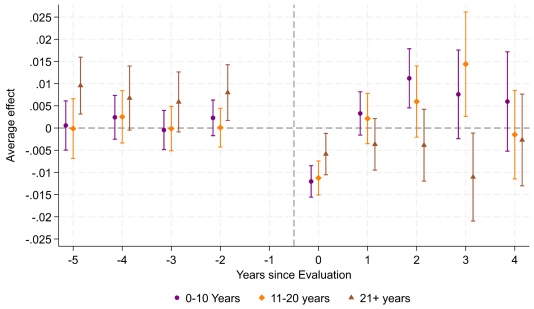
(a) Campus Mobility



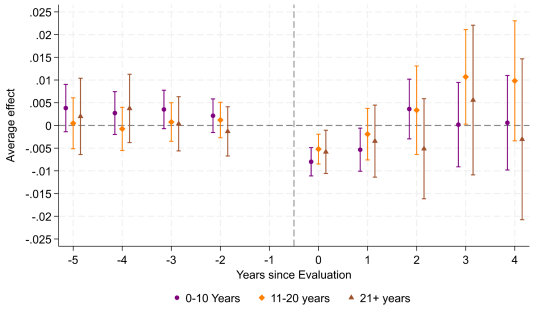
(b) ΔSES



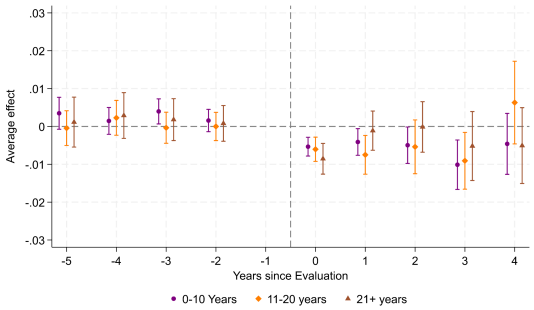
(c) Moves into High-SES Campuses



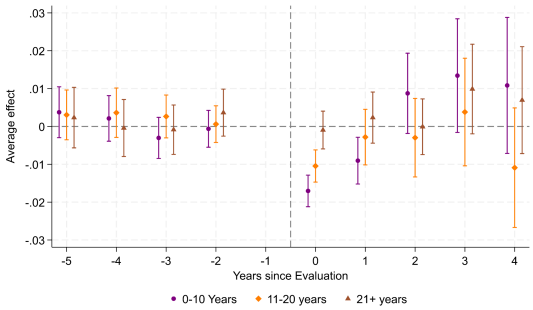
(d) Moves into Low-SES Campuses



(e) Moves from High-SES Campuses



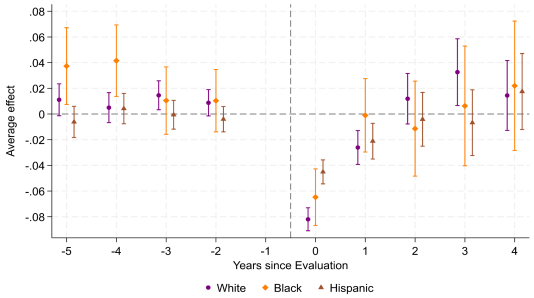
(f) Moves from Low-SES Campuses



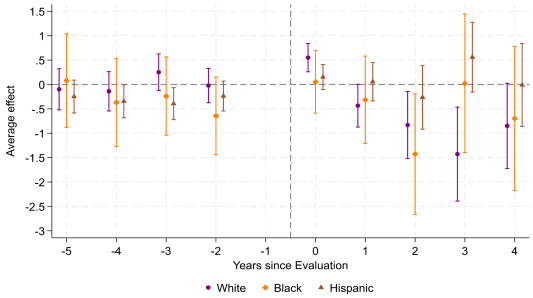
Notes: Time period $t = 0$ represents the school year in which a teacher is evaluated for designation. Error bars represent 95% confidence intervals. All results are estimated using Callaway & Sant'Anna (2021). Purple circle points represent teachers with 0 to 10 years of experience. Orange diamond points represent teachers with 11 to 20 years of experience. Brown triangle points represent teachers with 21 or more years of experience. The outcome in Panel A is an indicator for being observed at a different campus in $t + 1$ relative to period t. The outcome in Panel B is defined as ΔSES = destination percentile − origin percentile, where $\Delta SES < 0$ indicates a move to a more advantaged campus. The outcome in Panel C is an indicator for being observed at a high-SES campus in $t + 1$ and a non-high-SES campus in t. The outcome in Panel D is an indicator for being observed at a low-SES campus in $t+1$ and a non-low-SES campus in t. The outcome in Panel E is an indicator for being observed at a non-high-SES campus in $t + 1$ and a high-SES campus in t. The outcome in Panel F is an indicator for being observed at a non-low-SES campus in $t + 1$ and a low-SES campus in t.

Figure C.5: Estimated Effects of Designation, by Race and Ethnicity

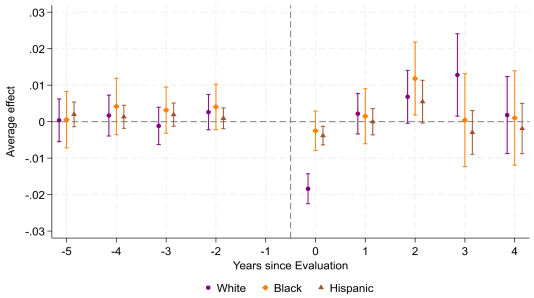
(a) Campus Mobility



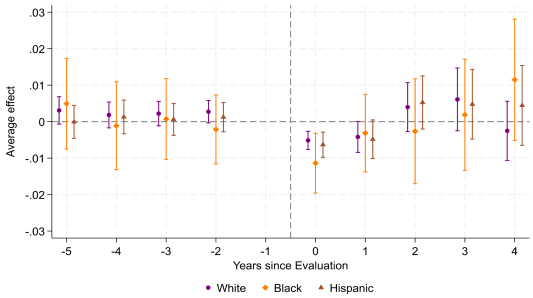
(b) ΔSES



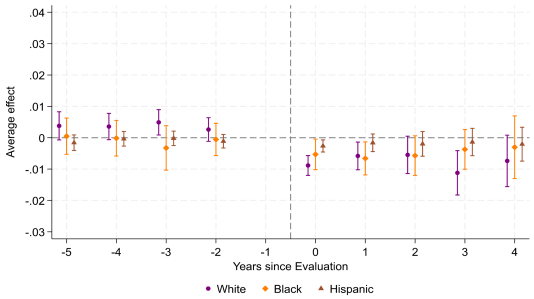
(c) Moves into High-SES Campuses



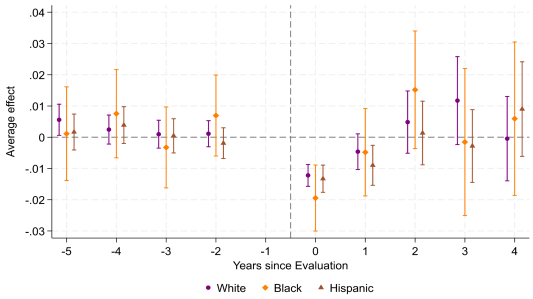
(d) Moves into Low-SES Campuses



(e) Moves from High-SES Campuses



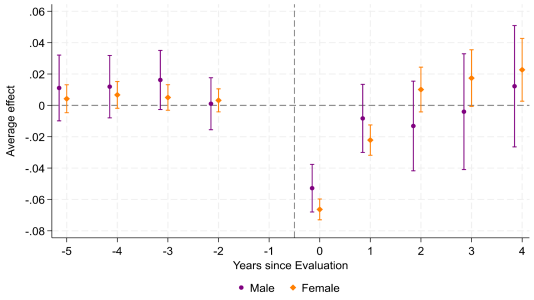
(f) Moves from Low-SES Campuses



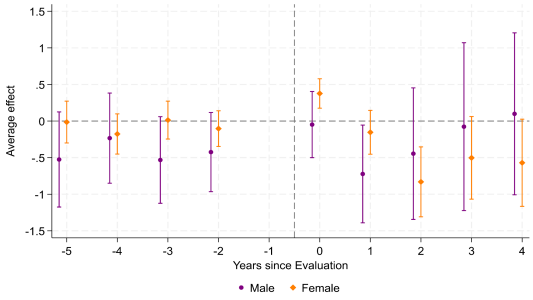
Notes: Time period $t = 0$ represents the school year in which a teacher is evaluated for designation. Error bars represent 95% confidence intervals. All results are estimated using Callaway & Sant'Anna (2021). Purple circle points represent White teachers. Orange diamond points represent Black teachers. Brown triangle points represent Hispanic teachers. The outcome in Panel A is an indicator for being observed at a different campus in $t + 1$ relative to period t. The outcome in Panel B is defined as ΔSES = destination percentile − origin percentile, where $\Delta SES < 0$ indicates a move to a more advantaged campus. The outcome in Panel C is an indicator for being observed at a high-SES campus in $t + 1$ and a non-high-SES campus in t. The outcome in Panel D is an indicator for being observed at a low-SES campus in $t + 1$ and a non-low-SES campus in t. The outcome in Panel E is an indicator for being observed at a non-high-SES campus in $t + 1$ and a high-SES campus in t. The outcome in Panel F is an indicator for being observed at a non-low-SES campus in $t + 1$ and a low-SES campus in t.

Figure C.6: Estimated Effects of Designation, by Sex

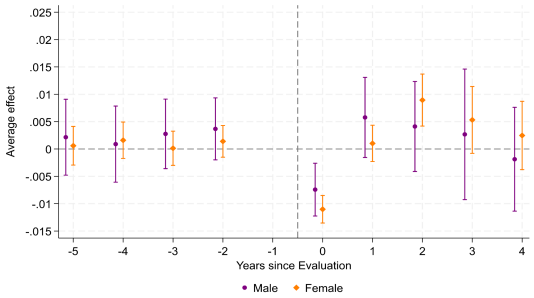
(a) Campus Mobility



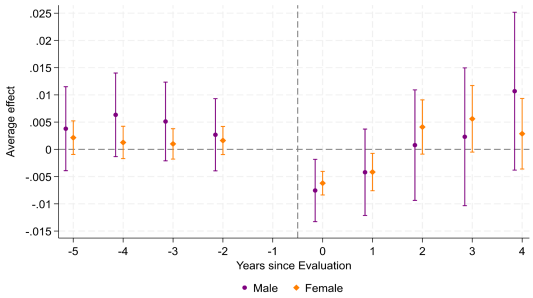
(b) ΔSES



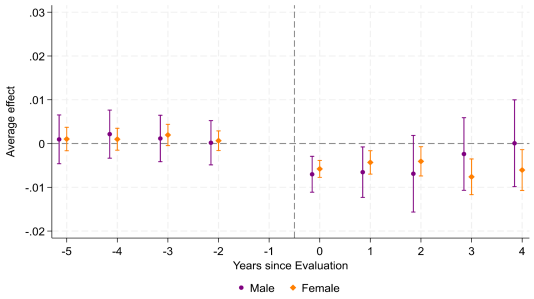
(c) Moves into High-SES Campuses



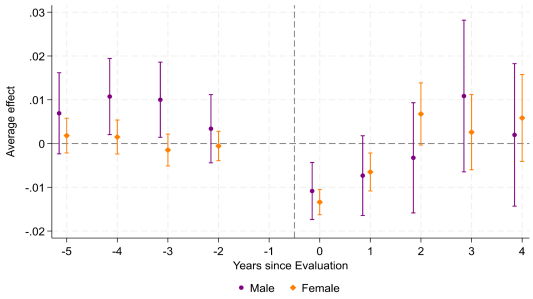
(d) Moves into Low-SES Campuses



(e) Moves from High-SES Campuses



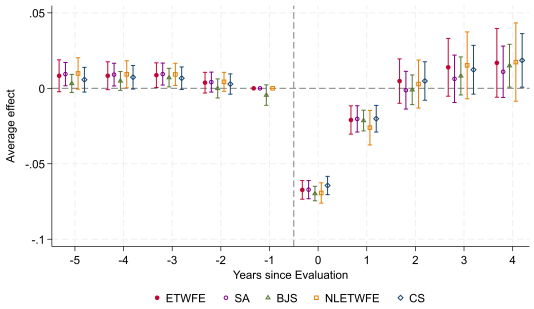
(f) Moves from Low-SES Campuses



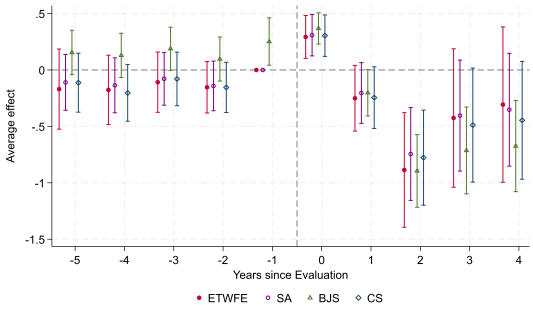
Notes: Time period $t = 0$ represents the school year in which a teacher is evaluated for designation. Error bars represent 95% confidence intervals. All results are estimated using Callaway & Sant'Anna (2021). Purple circle points represent Male teachers. Orange diamond points represent Female teachers. The outcome in Panel A is an indicator for being observed at a different campus in $t + 1$ relative to period t. The outcome in Panel B is defined as ΔSES = destination percentile − origin percentile, where $\Delta SES < 0$ indicates a move to a more advantaged campus. The outcome in Panel C is an indicator for being observed at a high-SES campus in $t + 1$ and a non-high-SES campus in t. The outcome in Panel D is an indicator for being observed at a low-SES campus in $t + 1$ and a non-low-SES campus in t. The outcome in Panel E is an indicator for being observed at a non-high-SES campus in $t + 1$ and a high-SES campus in t. The outcome in Panel F is an indicator for being observed at a non-low-SES campus in $t + 1$ and a low-SES campus in t.

Figure C.7: Estimated Effects of Designation with Multiple Estimators

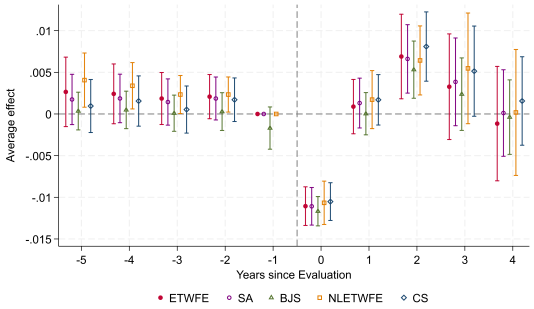
(a) Campus Mobility



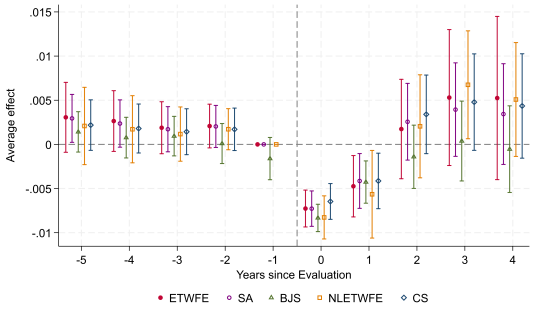
(b) ΔSES



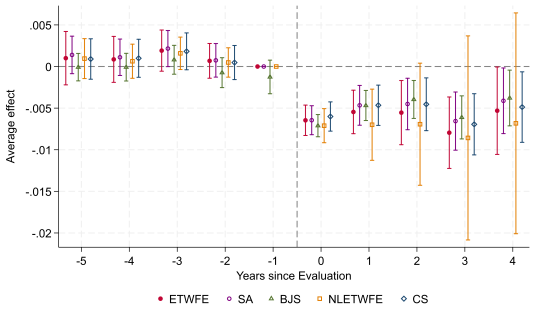
(c) Moves into High-SES Campuses



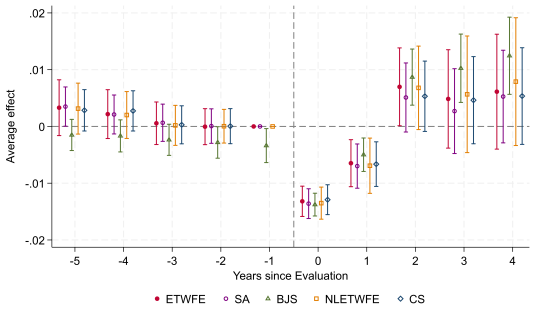
(d) Moves into Low-SES Campuses



(e) Moves from High-SES Campuses



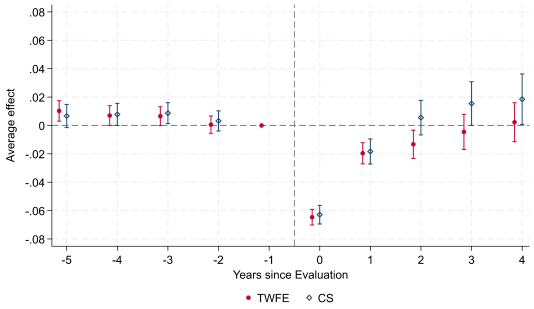
(f) Moves from Low-SES Campuses



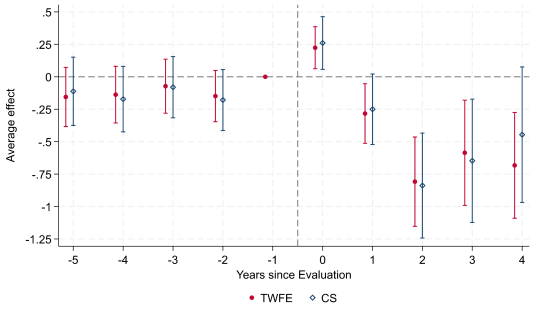
Notes: Time period $t = 0$ represents the school year in which a teacher is evaluated for designation. Error bars represent 95% confidence intervals. Red circle points represent estimates using Wooldridge (2025). Purple circle points represent estimates using Sun & Abraham (2021). Green triangle points represent estimates using Borusyak et al. (2024). Yellow square points represent non-linear estimates using Wooldridge (2023). Blue diamond points represent estimates using the Callaway & Sant'Anna (2021). The outcome in Panel A is an indicator for being observed at a different campus in $t+1$ relative to period t. The outcome in Panel B is defined as ΔSES = destination percentile − origin percentile, where $\Delta SES < 0$ indicates a move to a more advantaged campus. The outcome in Panel C is an indicator for being observed at a high-SES campus in $t+1$ and a non-high-SES campus in t. The outcome in Panel D is an indicator for being observed at a low-SES campus in $t+1$ and a non-low-SES campus in t. The outcome in Panel E is an indicator for being observed at a non-high-SES campus in $t+1$ and a high-SES campus in t. The outcome in Panel F is an indicator for being observed at a non-low-SES campus in $t+1$ and a low-SES campus in t.

Figure C.8: Estimated Effects of Designation using Not-Yet Designated Teachers

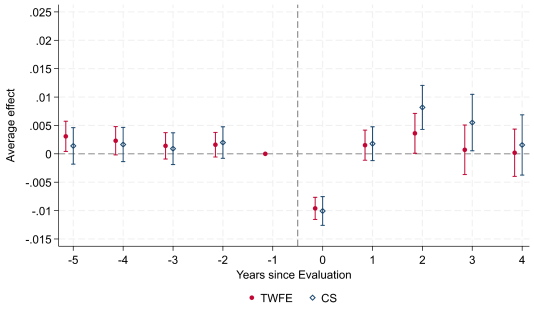
(a) Campus Mobility



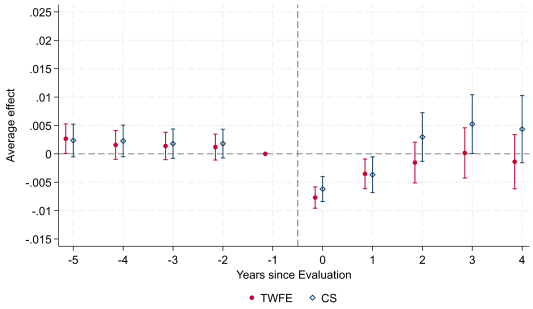
(b) ΔSES



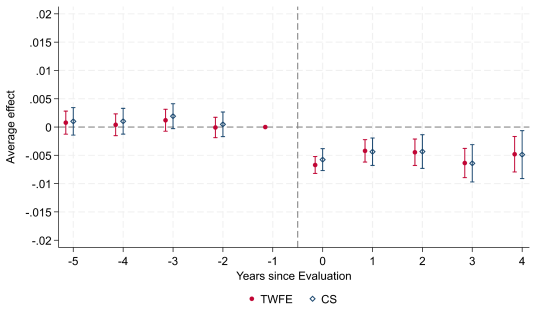
(c) Moves into High-SES Campuses



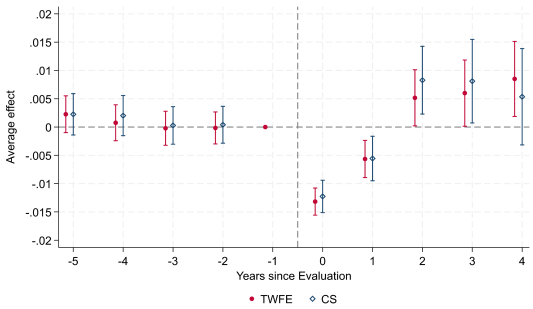
(d) Moves into Low-SES Campuses



(e) Moves from High-SES Campuses



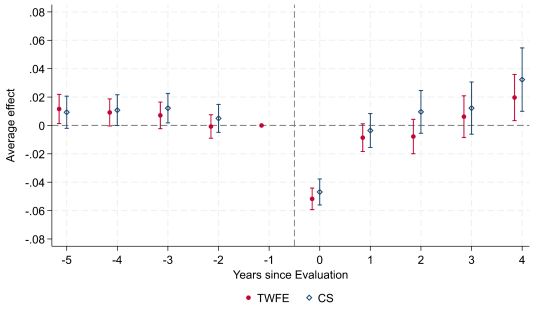
(f) Moves from Low-SES Campuses



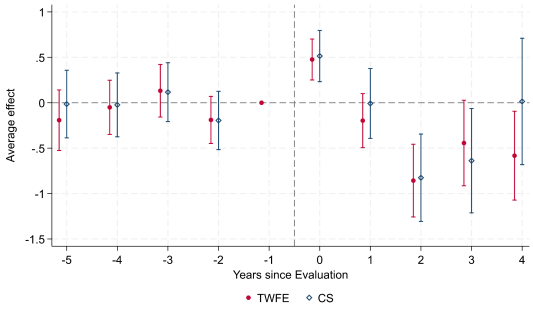
Notes: Time period $t = 0$ represents the school year in which a teacher is evaluated for designation. Error bars represent 95% confidence intervals. Red circles represent two-way fixed effects estimates. Blue diamonds represent Callaway & Sant'Anna (2021) estimates. The outcome in Panel A is an indicator for being observed at a different campus in $t + 1$ relative to period t. The outcome in Panel B is defined as ΔSES = destination percentile − origin percentile, where $\Delta SES < 0$ indicates a move to a more advantaged campus. The outcome in Panel C is an indicator for being observed at a high-SES campus in $t + 1$ and a non-high-SES campus in t. The outcome in Panel D is an indicator for being observed at a low-SES campus in $t + 1$ and a non-low-SES campus in t. The outcome in Panel E is an indicator for being observed at a non-high-SES campus in $t + 1$ and a high-SES campus in t. The outcome in Panel F is an indicator for being observed at a non-low-SES campus in $t + 1$ and a low-SES campus in t.

Figure C.9: Estimated Effects of Designation using First Wave Designees

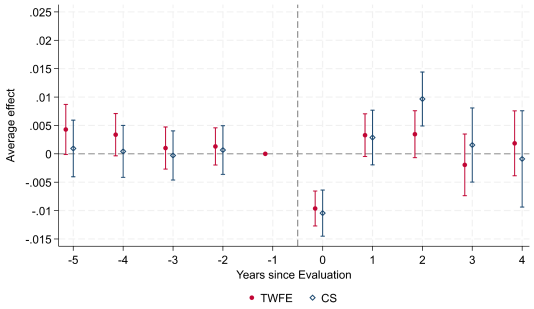
(a) Campus Mobility



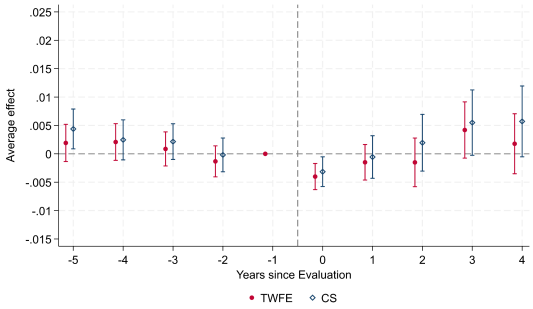
(b) ΔSES



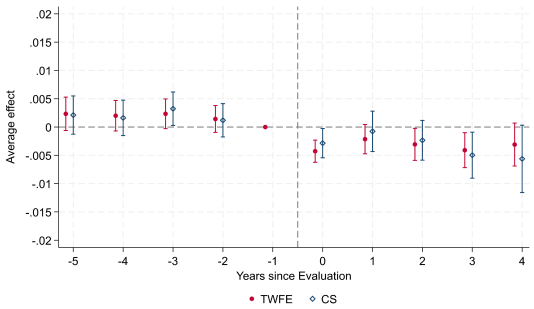
(c) Moves into High-SES Campuses



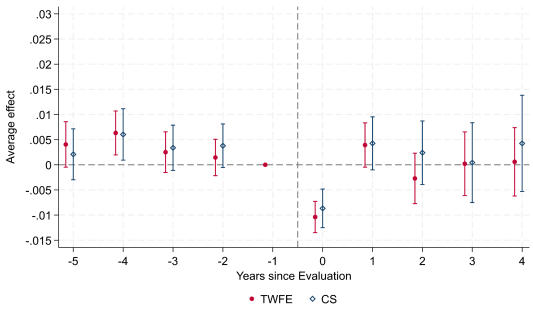
(d) Moves into Low-SES Campuses



(e) Moves from High-SES Campuses



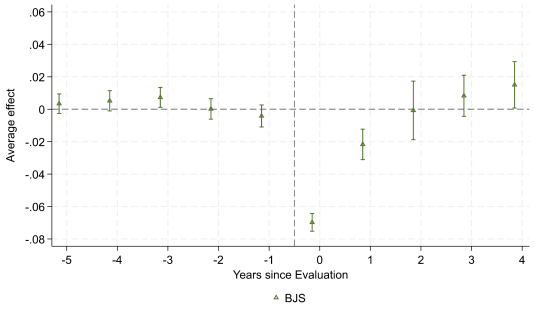
(f) Moves from Low-SES Campuses



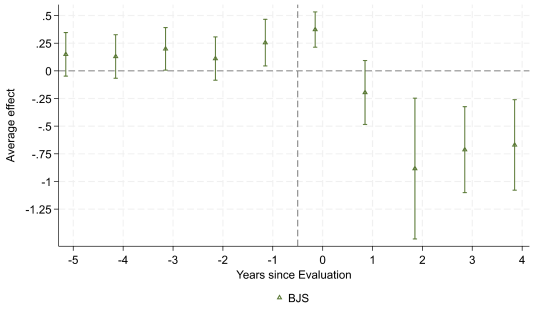
Notes: Time period $t = 0$ represents the school year in which a teacher is evaluated for designation. Error bars represent 95% confidence intervals. Red circles represent two-way fixed effects estimates. Blue diamonds represent Callaway & Sant'Anna (2021) estimates. The outcome in Panel A is an indicator for being observed at a different campus in $t + 1$ relative to period t. The outcome in Panel B is defined as ΔSES = destination percentile − origin percentile, where $\Delta SES < 0$ indicates a move to a more advantaged campus. The outcome in Panel C is an indicator for being observed at a high-SES campus in $t + 1$ and a non-high-SES campus in t. The outcome in Panel D is an indicator for being observed at a low-SES campus in $t + 1$ and a non-low-SES campus in t. The outcome in Panel E is an indicator for being observed at a non-high-SES campus in $t + 1$ and a high-SES campus in t. The outcome in Panel F is an indicator for being observed at a non-low-SES campus in $t + 1$ and a low-SES campus in t.

Figure C.10: Estimated Effects of Designation with 2019 and 2020 School Years Removed

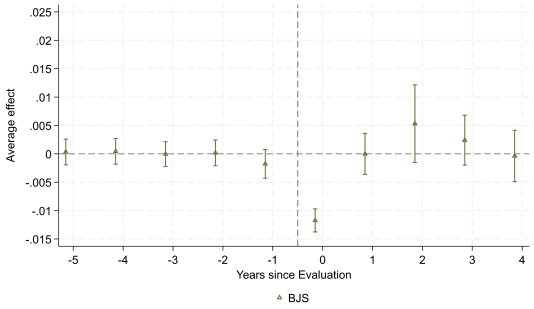
(a) Campus Mobility



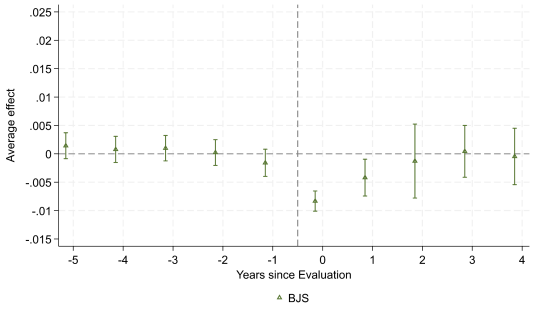
(b) ΔSES



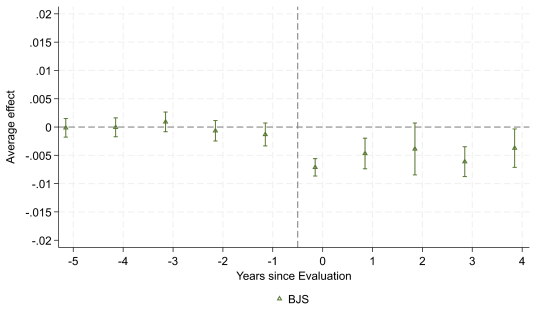
(c) Moves into High-SES Campuses



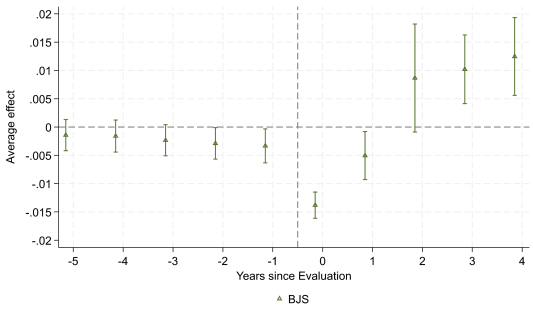
(d) Moves into Low-SES Campuses



(e) Moves from High-SES Campuses



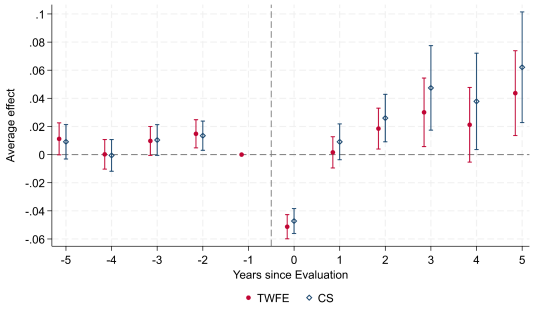
(f) Moves from Low-SES Campuses



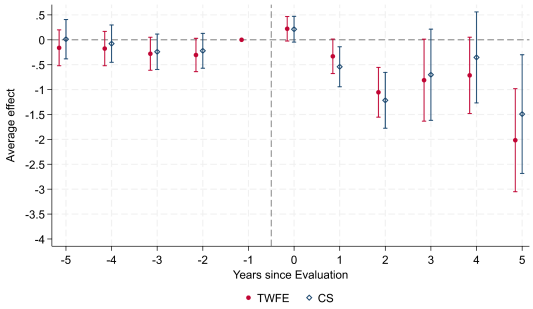
Notes: Time period $t = 0$ represents the school year in which a teacher is evaluated for designation. Error bars represent 95% confidence intervals. Green triangle points represent Borusyak et al. (2024) estimates. The outcome in Panel A is an indicator for being observed at a different campus in $t + 1$ relative to period t. The outcome in Panel B is defined as ΔSES = destination percentile − origin percentile, where $\Delta SES < 0$ indicates a move to a more advantaged campus. The outcome in Panel C is an indicator for being observed at a high-SES campus in $t + 1$ and a non-high-SES campus in t. The outcome in Panel D is an indicator for being observed at a low-SES campus in $t + 1$ and a non-low-SES campus in t. The outcome in Panel E is an indicator for being observed at a non-high-SES campus in $t+1$ and a high-SES campus in t. The outcome in Panel F is an indicator for being observed at a non-low-SES campus in $t + 1$ and a low-SES campus in t.

Figure C.11: Estimated Effects of Designation using Matched Sample

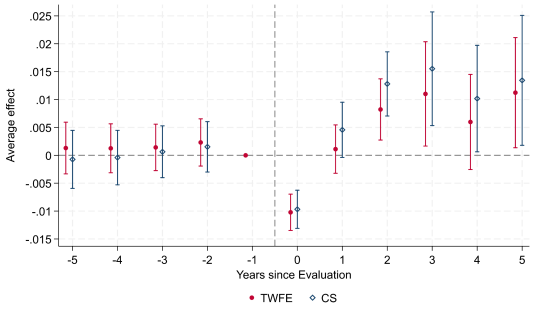
(a) Campus Mobility



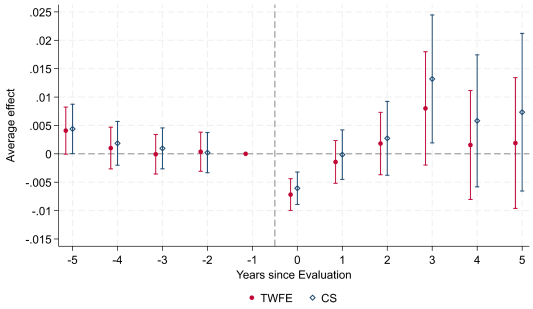
(b) ΔSES



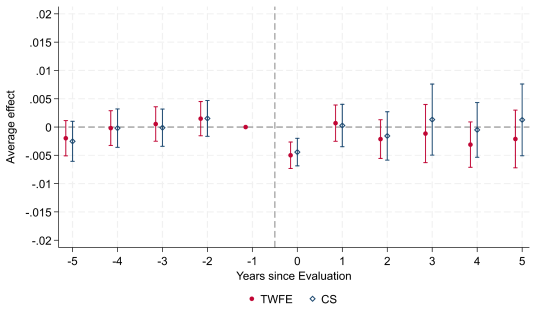
(c) Moves into High-SES Campuses



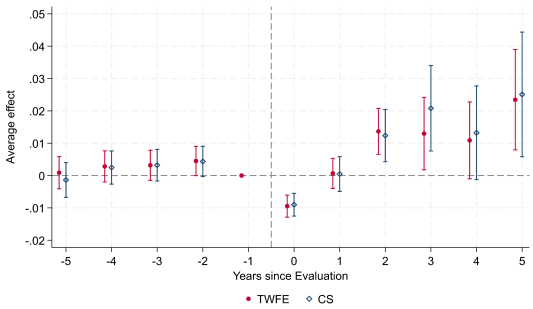
(d) Moves into Low-SES Campuses



(e) Moves from High-SES Campuses



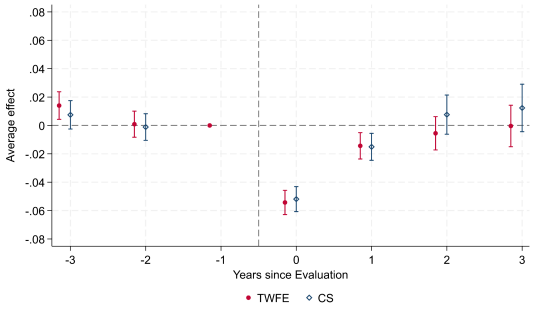
(f) Moves from Low-SES Campuses



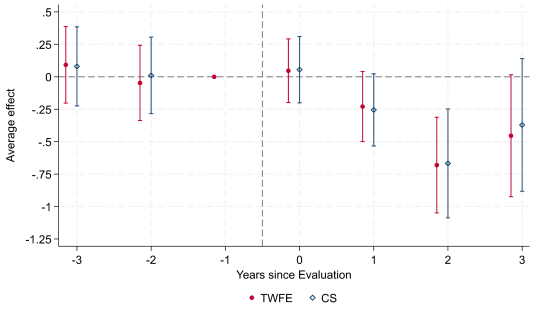
Notes: Time period $t = 0$ represents the school year in which a teacher is evaluated for designation. Error bars represent 95% confidence intervals. Red circles represent two-way fixed effects estimates. Blue diamonds represent Callaway & Sant'Anna (2021) estimates. The outcome in Panel A is an indicator for being observed at a different campus in $t + 1$ relative to period t. The outcome in Panel B is defined as ΔSES = destination percentile − origin percentile, where $\Delta SES < 0$ indicates a move to a more advantaged campus. The outcome in Panel C is an indicator for being observed at a high-SES campus in $t + 1$ and a non-high-SES campus in t. The outcome in Panel D is an indicator for being observed at a low-SES campus in $t + 1$ and a non-low-SES campus in t. The outcome in Panel E is an indicator for being observed at a non-high-SES campus in $t + 1$ and a high-SES campus in t. The outcome in Panel F is an indicator for being observed at a non-low-SES campus in $t + 1$ and a low-SES campus in t.

Figure C.12: Estimated Effects of Designation using Balanced Sample

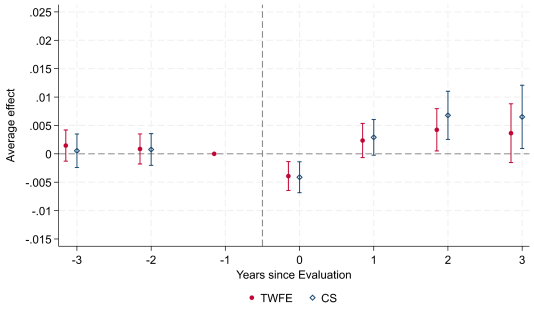
(a) Campus Mobility



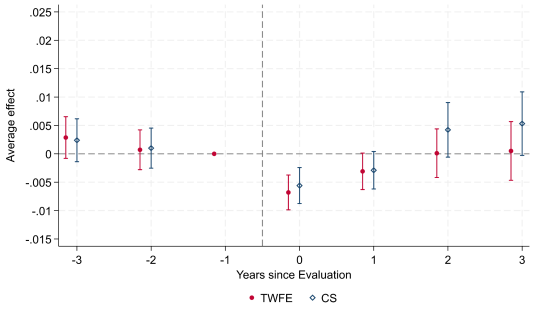
(b) ΔSES



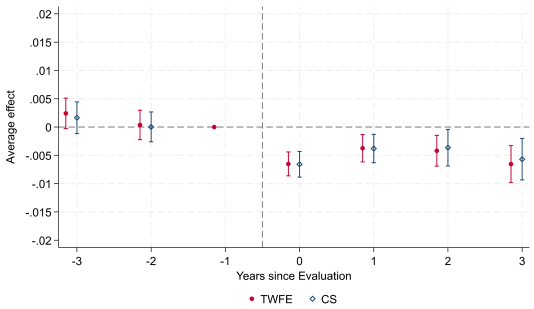
(c) Moves into High-SES Campuses



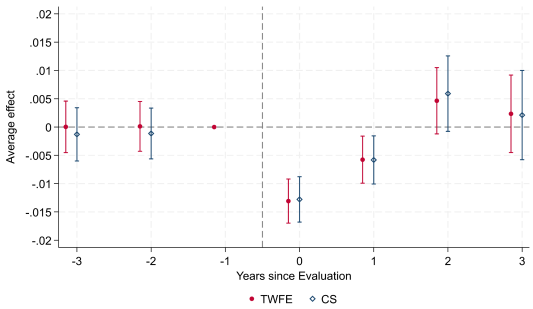
(d) Moves into Low-SES Campuses



(e) Moves from High-SES Campuses



(f) Moves from Low-SES Campuses



Notes: Time period $t = 0$ represents the school year in which a teacher is evaluated for designation. Error bars represent 95% confidence intervals. Red circles represent two-way fixed effects estimates. Blue diamonds represent Callaway & Sant'Anna (2021) estimates. The outcome in Panel A is an indicator for being observed at a different campus in $t + 1$ relative to period t. The outcome in Panel B is defined as ΔSES = destination percentile − origin percentile, where $\Delta SES < 0$ indicates a move to a more advantaged campus. The outcome in Panel C is an indicator for being observed at a high-SES campus in $t + 1$ and a non-high-SES campus in t. The outcome in Panel D is an indicator for being observed at a low-SES campus in $t + 1$ and a non-low-SES campus in t. The outcome in Panel E is an indicator for being observed at a non-high-SES campus in $t + 1$ and a high-SES campus in t. The outcome in Panel F is an indicator for being observed at a non-low-SES campus in $t + 1$ and a low-SES campus in t.