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Patrick L. Massey
Michigan State University

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# 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 TIA increases post-designation mobility, but that the additional movement tilts toward more advantaged campuses rather than the highest-need schools. Preference estimates imply that the average designated teacher requires \$6,036 in additional annual compensation to accept a one-standard-deviation increase in school disadvantage, while the salary gradient created by TIA falls short of that benchmark at most designation levels. These results suggest that TIA's portable credential expands effective teachers' outside options, but that the SES-targeted pay gradient is too shallow to redirect that added mobility toward disadvantaged schools.

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

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

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†Michigan State University, pmassey@msu.edu

# 1 Introduction

Despite decades of policy interventions aimed at addressing educational inequality, substantial disparities in access to effective teachers remain. Teachers are regularly recognized as one of the most significant 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, students attending rural and low-income schools consistently have less access to high-quality educators compared to peers in more affluent areas. To address these inequities, policymakers have increasingly turned to financial incentives. Such incentives can take the form of loan forgiveness, retention bonuses, or performance-based pay. Existing research on these incentives often focuses on temporary bonuses, small-scale pilot programs, or district-level initiatives, resulting in findings that are context-dependent and difficult to generalize. Consequently, there is a lack of empirical evidence on whether a large-scale, long-term, and substantial incentive program can influence teacher mobility and sorting behavior.

In this study, I address this gap in the literature by examining how large and long-term financial incentives influence the labor-market decisions of teachers. Specifically, I analyze Texas's Teacher Incentive Allotment (TIA), a statewide, performance-based compensation program launched in 2019. This program is designed explicitly to reward highly effective teachers, particularly those who prioritize teaching at high-need schools. Under TIA, participating districts designate eligible teachers at one of three levels: Recognized, Exemplary, or Master. The annual incentive payments called allotments range from \$3,000 to \$32,000 depending on the designation level and school characteristics. Allotments increase as the share of low socioeconomic status (SES) students in a school increases or if a school is labeled as rural. This program feature creates substantial variation in financial incentives across schools not seen in incentive programs in the past.

I develop a random utility framework in which TIA acts through two channels. A compensation channel raises pay more steeply at disadvantaged schools. A credentialing channel reduces information frictions by making teacher quality publicly verifiable, expanding designated teachers' feasible set of schools. Both channels raise mobility, but they do not necessarily push sorting in the same direction. If teachers on average prefer more advantaged schools, credentialing widens access to those campuses while compensation pulls in the opposite direction. The net effect depends on whether the SES gradient in pay is large enough to offset teachers' average preference for advantaged settings.

Using restricted-use administrative data from the University of Houston Education Research Center (UHERC), I study how receiving a TIA designation affects teachers' career decisions. I use a difference-in-differences event study framework that exploits the staggered timing of teacher designations across districts to estimate the causal effects of designation on mobility and SES-related outcomes. I then estimate a mixed logit model of school choice on the post-designation sample of teachers, recovering preferences over salary and campus SES.

Two key findings come from these analyses. First, designation raises overall mobility, but the increase tilts toward more advantaged campuses. Designated teachers also become more likely to leave low-SES campuses, while exits from high-SES campuses do not change. Second, the average teacher's willingness to pay to avoid a one-standard-deviation increase in campus disadvantage is roughly \$6,000 per year. However, the additional TIA compensation generated by moving from a higher-SES to a lower-SES campus is smaller than this amount at the bottom two designation levels, and meets this amount at the highest designation level.

Taken together, the results indicate that the credentialing channel dominates the compensation channel for the majority of teachers under TIA's current structure. The implication is not that incentive pay cannot reshape teacher sorting, but that doing so requires SES gradients to be steeper than those TIA currently uses.

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 gives a descriptive analysis on the pre-TIA teacher labor market. Section 7 outlines 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

Lower-income, rural, and predominantly minority-serving schools are disproportionately staffed by less experienced and lower-performing teachers (Lankford et al., 2002; Hanushek et al., 2004; Boyd et al., 2005b; Isenberg et al., 2013; Goldhaber et al., 2015). One potential explanation lies in the rigid salary schedules used by U.S. public school districts, which differ from most labor markets in that they reduce a teacher's wage largely to a function of years of experience, with only limited differentiation by education level and subject area. Under these schedules, two teachers with the same experience and credentials earn essentially the same salary regardless of the school in which they work. This paper asks whether relaxing that rigidity can reshape the distribution of effective teachers across schools.

Economic theory suggests that if schools with less desirable working conditions could offer higher wages, they would be better positioned to attract and retain teachers (Rosen, 1986). Rigid salary schedules prevent this adjustment from occurring, and the empirical literature suggests the required differentials are large. Hanushek et al. (2004) estimate that central-city schools in Texas would need to offer salary increases of 25% to 43% for women and roughly 10% for men to reduce turnover to the level of the typical suburban school, and Imazeki (2005) similarly finds that salary increases of 15% to 20% may still be insufficient to reduce attrition out of urban districts to the levels observed in an average district. Using North Carolina data, Clotfelter et al. (2011) estimate that differentials of roughly 10% to 58% would be required to retain teachers with strong preservice qualifications, such as license test scores and the selectivity of their undergraduate institution, across schools differing in nonwhite student share by 10 to 50 percentage points, magnitudes the authors characterize as beyond the range of political feasibility. For comparison, top-tier TIA designations can add roughly $32,000 to a teacher's annual compensation at the most disadvantaged schools, on the order of 50% of a typical Texas teacher's base salary. This figure is the level of the allotment at high-poverty schools rather than the differential between high-poverty and low-poverty schools that Clotfelter et al. (2011) estimates describe.

Direct evidence on what happens when rigid salary schedules are relaxed comes from Wisconsin's 2011 Act 10, which ended collective bargaining over teacher pay and allowed districts to adopt flexible compensation. Biasi (2021) shows that districts adopting flexible pay raised compensation for higher-quality teachers, attracted high-quality teachers away from neighboring rigid-pay districts, and saw corresponding gains in student achievement. Biasi et al. (2025) build on this evidence with a structural equilibrium model in which districts compete for teachers through wage and hiring strategies and teachers differ in both preferences and comparative advantage. Their baseline counterfactual shows that wage flexibility improves teacher-district matching but can also widen inequality across districts, since more advantaged districts are better positioned to attract preferred teachers. They additionally consider a more targeted alternative: state-funded bonuses layered on top of flexible pay, designed to reward teachers whose effectiveness is especially valuable in a given district and to offer stronger incentives in districts serving more disadvantaged students. This bonus structure can improve equity without sacrificing efficiency, and in some specifications improves both simultaneously. TIA ties compensation to measured effectiveness and scales rewards with school disadvantage, making it a natural empirical setting in which to test the kind of targeted compensation policy that Biasi et al. (2025) highlight as efficiency- and equity-improving.

A separate barrier to the sorting of effective teachers is the absence of a credible, portable signal of teacher quality. Principals and colleagues within a school may develop reasonably accurate assessments of a teacher's effectiveness over time, but no standardized mechanism exists to translate these private assessments into a publicly verifiable credential that travels with the teacher across schools or districts. Jacob et al. (2018) document this transmission problem directly: principals making hiring decisions rely heavily on interviews, references, and other subjective signals that are noisy, idiosyncratic, and not standardized across districts. As a result, an effective teacher seeking to move faces a thin market for her quality. Even when a hiring principal would value that quality, she has no low-cost way to convey it.

The closest existing analog to a portable quality credential is National Board Certification (NBC). Cowan & Goldhaber (2018) study the combination of NBC with a bonus for teaching in challenging schools in Washington State and find reduced turnover among certified teachers at those schools, suggesting that a credential paired with a financial incentive can shift teacher labor market decisions. NBC, however, is voluntarily pursued, paid for by the teacher, and not embedded in the salary structure in most states, all of which limit its capacity to reshape sorting at scale. By contrast, the credentialing channel of TIA is empirically much less established than the compensation channel. This paper provides the first evidence on whether a portable, salary-linked, performance-based designation affects where teachers choose to teach.

Several studies examine what teachers value when choosing where to teach. Boyd et al. (2005a) use New York State data to show that teacher labor markets are geographically narrow and that teachers prefer to teach close to where they grew up and in areas demographically similar to their hometowns. These geographic preferences set a real boundary on what any compensation policy can achieve: pay incentives must be large enough to overcome both the disutility of less-preferred school characteristics and the cost of moving away from a teacher's preferred labor market. Controlling for proximity preferences, teachers in Texas (Hanushek et al., 2004), Florida (Feng & Sass, 2017), and New York (Boyd et al., 2005b) systematically move toward schools with higher-achieving, higher-income, and whiter student populations. Boyd et al. (2005b) further show that teachers with stronger academic credentials exit low-performing schools at higher rates than their lower-achieving peers at the same schools, indicating substantial heterogeneity in preferences over school characteristics. Clotfelter et al. (2011) similarly find that, particularly among teachers in their initial teaching spells, those with stronger preservice qualifications are both more responsive to the racial and socioeconomic composition of schools and less responsive to salary than their less-qualified peers. Beyond salary and student characteristics, teachers also value non-monetary factors such as school leadership (Boyd et al., 2011), class size, peers, and school culture (Feng & Sass, 2017). The effect of a monetary incentive on sorting therefore depends both on the size of the incentive and on the strength and heterogeneity of teachers' preferences over these school characteristics.

If schools serving disadvantaged students are able to offer differentiated compensation, those incentives can affect teachers' labor market choices. Clotfelter et al. (2008) studies a short-lived North Carolina policy aimed at retaining math, science, and special education teachers in high-poverty or academically struggling schools. The program provided an annual bonus of $1,800 and reduced turnover by 17%, indicating that even modest pay increases can affect labor market decisions. Denver Public Schools showed similar results through its ProComp program, which provided retention incentives at hard-to-staff schools (Fulbeck, 2014). Springer et al. (2016) show that $5,000 performance bonuses increased retention of effective teachers in Tennessee's lowest-performing schools. Dee & Wyckoff (2015) study DC's IMPACT program, which combined dismissal threats for low-rated teachers with substantial financial rewards for those rated Highly Effective, including annual bonuses of up to $25,000 and permanent base salary increases. Using a regression discontinuity design, they find that dismissal threats increased voluntary attrition among low-performers while incentives for highly effective teachers increased both retention and subsequent performance. Taken together, these studies show that pay incentives can affect whether teachers stay, but they are less informative about where teachers choose to teach.

A smaller set of studies examines incentives designed specifically to alter teacher mobility across schools. Glazerman et al. (2013) study the Talent Transfer Initiative, an RCT in which highly effective teachers were offered $20,000 over two years to transfer into academically struggling schools; the program successfully attracted high-effectiveness teachers into high-need schools. For a brief period, California offered the Governor's Teaching Fellowship, which provided $20,000 to high-achieving students preparing to become teachers if they committed to teaching in low-performing schools for four years, with prorated repayment if they exited early. Steele et al. (2010) show that receiving the fellowship increased the probability that these new teachers taught in low-performing schools relative to the counterfactual. These studies establish that incentives can redistribute teachers, but the programs they evaluate were temporary, externally funded, and targeted to narrow populations. TIA, by contrast, is a permanent statewide system that combines a portable quality designation, a financial premium scaled to school disadvantage, and a salary-embedded structure available to a broad set of teachers. That combination has not previously been evaluated.

The closest existing study of TIA is a working paper by Kirksey et al. (2024). Using a synthetic control approach on the first cohort of participating districts, they find a decrease in district-level turnover of roughly 5 percentage points and modest gains in student achievement. Because they cannot identify which teachers received TIA designations, their analysis speaks to district aggregates rather than teacher behavior. Conceptually, the closest prior work is Biasi et al. (2025), whose equilibrium model shows that targeted bonus systems can improve both efficiency and equity when they reward effectiveness and scale incentives with district disadvantage. This paper brings that insight to a policy environment in which such targeting is built directly into the compensation formula, and advances the literature in three ways. First, I use teacher-level designation data that directly identify TIA-designated teachers, allowing me to trace individual mobility decisions rather than district aggregates. Second, I examine not only whether designated teachers move but where they move along the school SES distribution, the margin most relevant to the policy's potential impact. Third, I estimate a discrete choice model of teacher school choice that recovers preferences over salary, school SES, and distance. Biasi et al. (2025) identify teacher preferences off district-level variation in Wisconsin's flexible-pay regime, variation that itself reflects strategic district wage-setting. TIA instead generates campus-level compensation variation tied to a state formula that scales designation pay with school demographics. This formula-based variation, which spans both within- and between-district comparisons, identifies preferences over school SES separately from district wage strategies and quantifies the compensation required to induce moves toward higher-poverty schools.

# 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 into 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 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)

[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.png`

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 gain measure rather than a level measure: it captures how far a student progressed from a starting point rather than 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 re-designation 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 $j$. 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 $j$, $\varepsilon_{ij}$ is an idiosyncratic match component, and $\alpha > 0$ is common to all teachers. The key parameter $\beta_i$ captures teacher $i$'s preference over SES: a negative $\beta_i$ indicates a preference for less disadvantaged schools, and a positive $\beta_i$ indicates a preference for serving more disadvantaged students. Consistent with prior literature, I assume $\beta_i$ 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. Teacher $i$ chooses the school that maximizes utility over her feasible set:

$$\max_{j \in A_i} U_{ij}$$

## 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

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

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 $\beta_i$. 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:

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

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 $\alpha m_k$[4]. A teacher who was mildly averse to disadvantaged placements ($\beta_i$ slightly below zero) may now have $\beta_i^* > 0$,

[3] The notable exception is Dallas ISD, the largest school district 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.

[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.

tipping her school choice toward higher-need schools. The teachers most affected are those near the $\beta_i = 0$ threshold, the marginal teachers for whom the additional compensation is decisive.

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

#### 4.3.3 Interaction Between Channels

The two channels have similar implications for overall mobility but different implications for sorting. The credentialing channel expands outside options by relaxing information frictions. The compensation channel changes the relative attractiveness of schools by raising pay more at disadvantaged campuses. Both channels can increase mobility, but they 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 current school $j$ to another school $j'$. She moves if:

$$\alpha(w_{ij'} - w_{ij}) + \beta_i(\bar{s}_{j'} - \bar{s}_j) + (\varepsilon_{ij'} - \varepsilon_{ij}) > 0 \tag{4}$$

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.

#### 4.4.1 Cross-District Sorting

A teacher considering a move to another district must weigh both the base salary and the TIA supplement. For the same teacher as before, abstracting from the idiosyncratic term, equation 4 becomes:

$$\alpha\big(\bar{w}_{d(j')}(exp_i, degree_i) + m_k\bar{s}_{j'} - \bar{w}_{d(j)}(exp_i, degree_i) - m_k\bar{s}_j\big) + \beta_i(\bar{s}_{j'} - \bar{s}_j) > 0$$

Collecting terms and dividing by $\bar{s}_{j'} - \bar{s}_j > 0$, the moving condition becomes:

$$\beta_i + \alpha m_k + \alpha\left(\frac{\Delta w}{\Delta s}\right) = \beta_i^* + \alpha\left(\frac{\Delta w}{\Delta s}\right) > 0 \tag{5}$$

where $\Delta w = \bar{w}_{d(j')} - \bar{w}_{d(j)}$ and $\Delta s = \bar{s}_{j'} - \bar{s}_j$. A designated teacher moves toward a more disadvantaged campus in another district if and only if her effective preference for disadvantage plus the cross-district base-pay gradient is positive.

### 4.4.2 Within-District Sorting

Within a district, the base salary schedule cancels: $\Delta w = 0$, so $\Delta w/\Delta s = 0$ and the moving condition in equation 5 reduces to:

$$\beta_i^* = \beta_i + \alpha m_k > 0$$

Sorting within a district is therefore determined entirely by the TIA demographic component. A designated teacher moves toward a more disadvantaged school if and only if her effective preference for disadvantage $\beta_i^*$ is positive. This holds for teachers who already prefer high-need schools ($\beta_i > 0$), but also for teachers who mildly prefer advantaged schools ($\beta_i < 0$ but $|\beta_i| < \alpha m_k$). Teachers with $\beta_i^* \leq 0$ do not move toward higher-need schools; the compensation channel is not enough to overcome their preferences.

Across both cases, the equity consequences of TIA depend on the size of the compensation gradient relative to teachers' underlying preferences. If the gradient is large relative to the average preference for more advantaged schools, TIA can redirect designated teachers toward higher-need campuses. If it is not, expanding mobility opportunities through credentialing increases movement without generating substantial sorting toward disadvantaged schools.

# 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 significantly 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 with smaller districts needing 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 to the 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 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 includes 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

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 it induces 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 408,179 teacher-year observations[5], with 258,560 treated observations and 149,619 untreated observations.

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

| Year | New Teachers Designated | Total Teachers Designated |
|---|---|---|
| 2019 | 3,631 | 3,631 |
| 2020 | 602 | 4,233 |
| 2021 | 1,507 | 5,740 |
| 2022 | 6,680 | 12,420 |
| 2023 | 11,797 | 24,217 |
| 2024 | 15,239 | 39,456 |

*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 85.9% 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. Treated and control teachers also have comparable experience levels (10.7 and 9.8 years, respectively) and similar base salaries prior to treatment ($55,571 versus $54,512). The primary compensation difference is the TIA allotment itself, which averages approximately $10,800 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.

| | Treated | Control | Statewide |
|---|---|---|---|
| Female | 0.859 | 0.864 | 0.788 |
| | (0.348) | (0.343) | (0.408) |
| White | 0.459 | 0.480 | 0.585 |
| | (0.498) | (0.500) | (0.493) |
| Black | 0.118 | 0.079 | 0.114 |
| | (0.322) | (0.270) | (0.318) |
| Hispanic | 0.399 | 0.421 | 0.281 |
| | (0.490) | (0.494) | (0.450) |
| Years of Experience | 10.710 | 9.771 | 10.934 |
| | (8.222) | (7.773) | (9.211) |
| Base Annual Salary | 55,571 | 54,512 | 54,850 |
| | (11,197) | (10,872) | (10,202) |
| Total Annual Salary | 56,778 | 55,572 | 56,152 |
| | (12,080) | (11,458) | (10,887) |
| Allotment Amount | 10,798 | — | — |
| | (6,218) | — | — |
| N | 258,560 | 149,619 | 4,174,761 |

*Notes:* Standard deviations in parenthesis. Treated teachers are those designated in years 2019, 2020, 2021, 2022 and 2023. Control teachers are those designated in 2024. 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 campus by estimating heterogeneous effects by campus-level SES and rural classification.

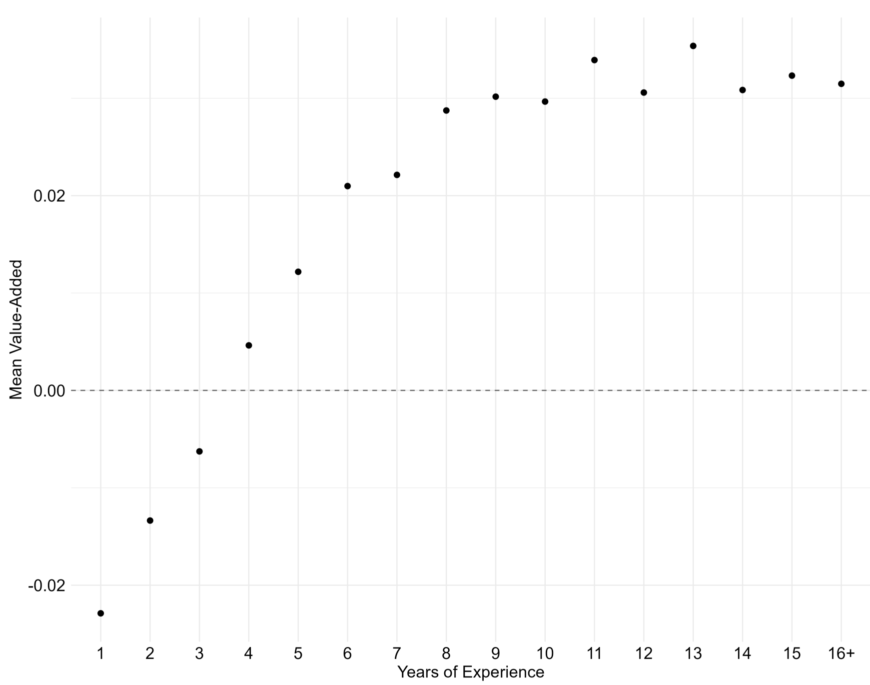
I use the Average Student Point Value (ASPV) to classify campuses by their SES status. With the data available I could back out the actual ASPV used in the allotment calculation, however this value includes an adjustment for the rural status of the school, which is not possible to account for with the 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 feeds 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 their origin campus is not high-SES and their 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 $\Delta SES$ = 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 (4–6 years), mid-career (7–9 years), and senior (10+ years).

Figure 1: Teacher Value-Added by Years of Experience

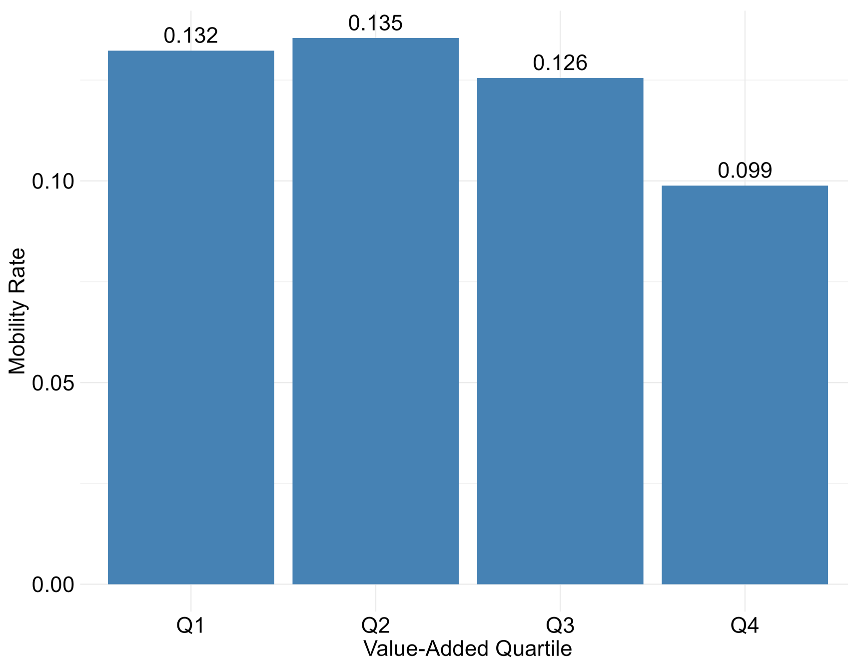



*Notes:* This figure reports mean teacher empirical Bayes shrunken value-added estimates by years of experience.

## 6.1 Baseline Mobility Rates

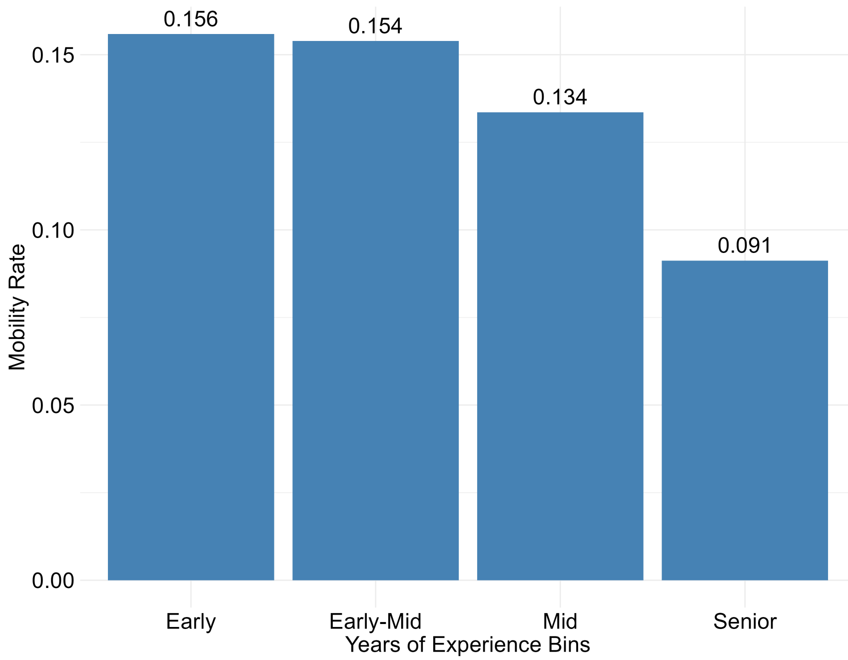
Figure 2 shows that higher value-added teachers are less mobile: teachers in the top quartile of the value-added distribution are approximately three percentage points less likely to move than those in the bottom quartile. The pattern is slightly more pronounced when teachers are partitioned by experience which I show in figure 3. Early-career teachers move at a rate of approximately 16% annually, declining to around 9% 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

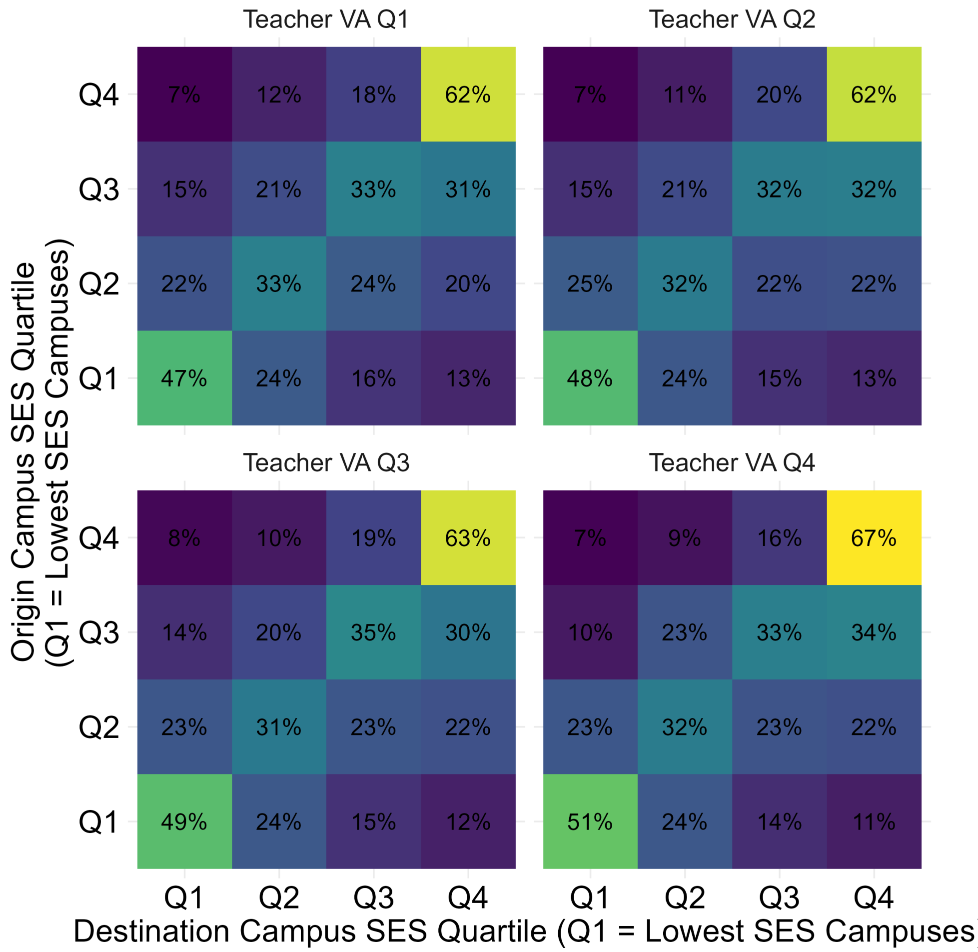



*Notes:* This figure reports pre-TIA teacher mobility rates by teacher experience bins. Mobility is defined as teaching in a different campus in the following academic year.

## 6.2 Sorting Patterns Across Campus SES

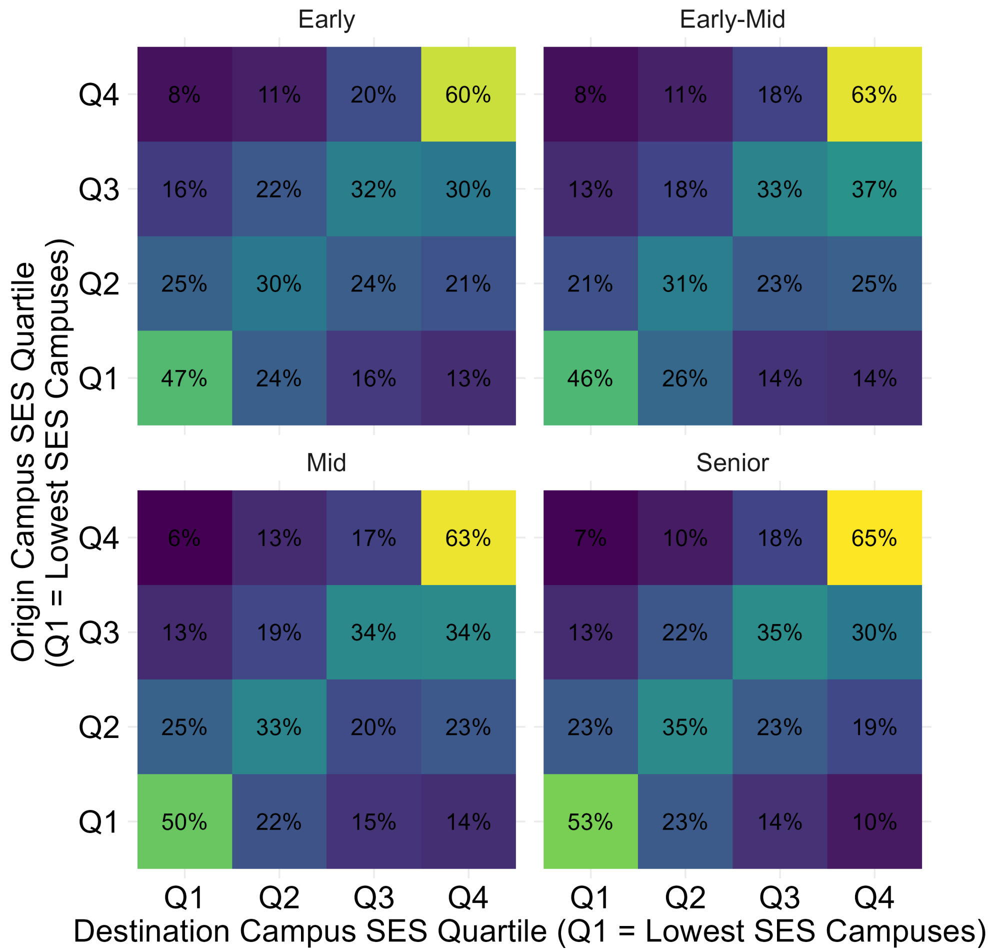
Figure 4 shows substantial persistence in the income distribution of schools teachers move between. Teachers who begin in lower-income campuses are disproportionately likely to move to other lower-income campuses, while teachers who begin in higher-income campuses are disproportionately likely to move 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 lowest-SES campuses and Q4 denotes the highest-SES 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 lowest-SES campuses and Q4 denotes the highest-SES 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's 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, .456 of entrants are early-career and .315 are senior; when a mid- or senior-career teacher leaves, those shares are .436 and .327, respectively. Taken together, the two tables suggest that replacement patterns in low-SES schools differ only slightly, when a teacher has a high value-added or is more experienced.

| Entrant Experience Category | Any Teacher Leaves | Mid or Senior Teacher Leaves |
|---|---|---|
| Early | .456 | .436 |
| Early-Mid | .132 | .134 |
| Mid | .098 | .103 |
| Senior | .315 | .327 |

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 nearly identical baseline mobility patterns, supporting the validity of the comparison group. About 9.9% of teacher-year observations involve a campus move in the following year for both groups. 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 $\Delta SES$ is $-0.487$ percentile points for treated teachers and $-0.706$ for controls. Because $\Delta SES$ 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 4.9 percentile points toward more advantaged campuses ($-0.487/0.099 \approx -4.92$), while control teachers shift 7.1 points ($-0.706/0.099 \approx -7.13$). 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 1.5% of teacher-year observations involve a move to a high-SES campus and 0.9 to 1.1% to a low-SES campus. Moves to and from rural campuses are similarly uncommon, ranging from 1.5 to 1.7% across both groups.

Finally, when teachers do move, those moves tend to be geographically local. About 5% of treated and 4.5% of control teacher-year observations involve a within-district move, 7% and 6.7% within county, and 8.0% and 7.9% 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 | 0.099 | 0.099 |
| | (0.298) | (0.298) |
| $\Delta SES$ | -0.487 | -0.706 |
| | (9.467) | (10.004) |
| Move to High-SES Campus | 0.011 | 0.015 |
| | (0.106) | (0.120) |
| Move to Low-SES Campus | 0.011 | 0.009 |
| | (0.106) | (0.097) |
| Move to Rural Campus | 0.016 | 0.015 |
| | (0.125) | (0.121) |
| Move from High-SES Campus | 0.006 | 0.008 |
| | (0.080) | (0.087) |
| Move from Low-SES Campus | 0.018 | 0.017 |
| | (0.132) | (0.130) |
| Move from Rural Campus | 0.017 | 0.017 |
| | (0.130) | (0.130) |
| Move within School District | 0.050 | 0.045 |
| | (0.218) | (0.206) |
| Move within County | 0.070 | 0.067 |
| | (0.255) | (0.250) |
| Move within TEA Region | 0.080 | 0.079 |
| | (0.271) | (0.270) |
| N | 183,528 | 128,941 |

Standard Deviations in parenthesis. 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:

$$y_{it} = \alpha_i + \lambda_t + \sum_{\tau=-5}^{4} \delta_\tau \, 1\{t - (D_i - 1) = \tau\} + X_{it}\beta + \varepsilon_{it} \tag{6}$$

where $y_{it}$ denotes a binary outcome for teacher $i$ in year $t$, which is associated with the teacher's placement in period $t+1$. The vector $X_{it}$ controls for the teacher's years of experience. I include teacher fixed effects ($\alpha_i$) and year fixed effects ($\lambda_t$), 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 $\delta_\tau$ 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, we should 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 potentially increasing 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 $\delta_{-1}$ so that treatment effects are measured relative to the year prior to evaluation.

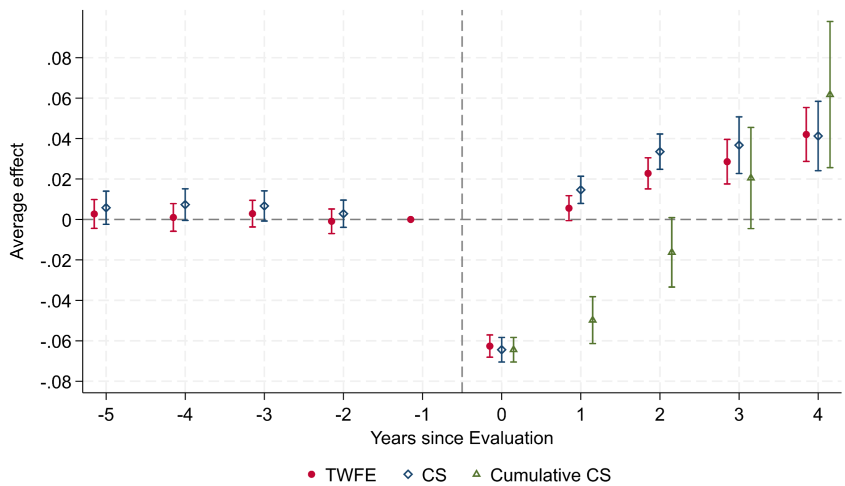
## 7.2 Results

I present a series of event-study figures organized into four sets of outcomes. The first set examines the effect of designation on overall teacher mobility and on the net change in campus SES, measured by $\Delta SES$. The second set focuses on destination-campus sorting, estimating whether designation affects the likelihood that teachers move to low-SES, high-SES, or rural campuses. The third set looks to a teacher's origin-campus, and examines whether post-designation mobility is disproportionately concentrated among teachers leaving low-SES, high-SES, or rural campuses. The fourth and last set considers the geographic scope of mobility, looking at whether designation changes the likelihood of moving within district. 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 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 below, providing additional evidence that the result reflects the institutional rule rather than a broader change in willingness to move.

Beginning in period 1, mobility increases. Designated teachers are 1.5 pp more likely to change campuses in period 1, with the effect rising to 3.4 pp in period 2, 3.7 pp in period 3, and 4.1 pp in period 4. Against a baseline annual mobility rate of roughly 10%, these are substantial increases. One possibility is that the lock-in period simply postpones moves rather than generating new ones. The cumulative estimates suggest otherwise. By period 3, the post-designation increase in mobility fully offsets the period 0 decline. By period 4, cumulative mobility is 6.2 pp and statistically significant, implying a net increase in mobility over the full post-designation period. Taken together, these results suggest that designation ultimately increases teacher mobility once the temporary constraint is lifted.

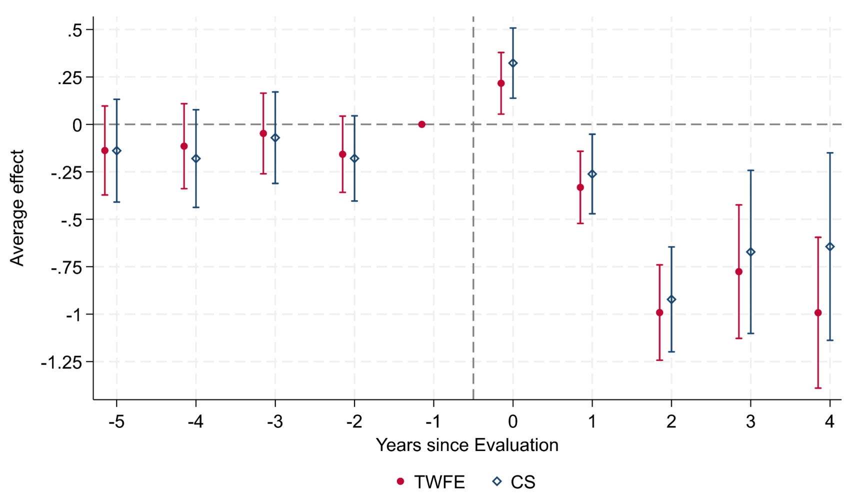
Figure 6: Estimated Effects on Moving Campuses




*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. Error bars represent 95% confidence intervals. Red circle points represent two-way fixed effects estimates. Blue diamond points represent Callaway & Sant'Anna (2021) estimates. Green triangle points 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 is an indicator for being observed at a different campus in $t + 1$ relative to period $t$.

Figure 7 reports the estimated effects of designation on $\Delta SES$, the net change in campus SES percentile between origin and destination. The period 0 estimate is positive and statistically significant. On its face, this would suggest treated teachers move to more disadvantaged campuses. That reading is misleading. The period 0 estimate is primarily a mechanical consequence of the sharp mobility decline during the lock-in year shown in Figure 6.

Figure 7: Estimated Effects on $\Delta SES$




*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. Error bars represent 95% confidence intervals. The outcome is defined as $\Delta SES =$ destination percentile $-$ origin percentile, where $\Delta SES < 0$ indicates a move to a more advantaged campus. Red circle points represent two-way fixed effects estimates. Blue diamond points represent Callaway & Sant'Anna (2021) estimates.

Because $\Delta SES$ 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 $E[\Delta SES \mid Moving] \approx -4.92$. 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 $\Delta SES$ 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:

$$\delta_0^{\Delta SES} \approx \delta_0^{move} m_T = -0.064 \times -4.92 \approx 0.315$$

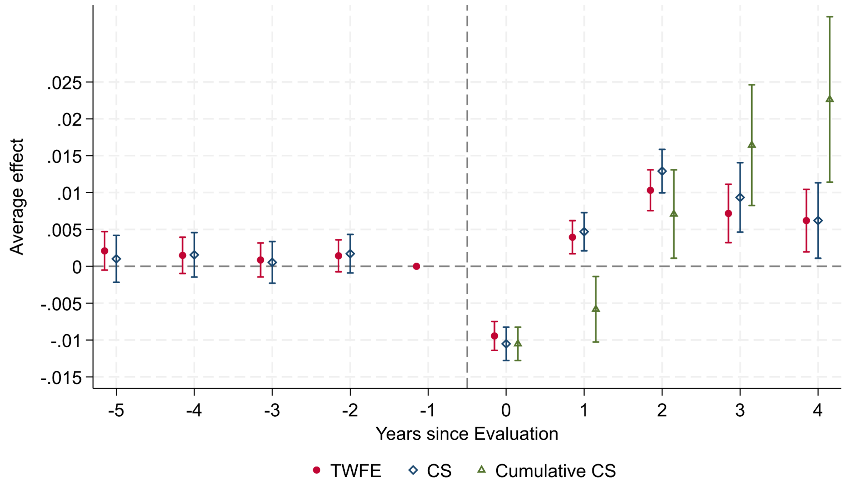
This accounts for roughly 98% of the observed period 0 estimate (0.315/0.323), indicating that the positive coefficient is almost entirely driven by suppressed mobility rather than a meaningful change in the types of campuses treated teachers choose. Because of this I treat the period 0 estimate as a mechanical result rather than a separate finding.

After the mechanical period 0 result, the estimated effects on $\Delta SES$ turn negative and significant, ranging from $-0.262$ to $-0.922$ percentile points. 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].

Having shown that designation increases mobility and, on net, shifts teachers toward more advantaged campuses relative to the control group, I next examine the destinations of those moves. Figures 8 and 9 show the effects of designation on moving to high-SES and low-SES campuses, respectively. For a teacher to be classified as moving to a low-SES (high-SES) campus, their origin campus must be non low-SES (non high-SES). Following the mechanical decline in mobility in period 0, both outcomes increase, indicating that designation raises movement toward both ends of the SES distribution. However, the increase is slightly larger for moves to high-SES campuses than for moves to low-SES campuses. The cumulative post-designation effect is 2.3 pp for moves to high-SES campuses, compared with 1.3 pp for moves to low-SES campuses. Taken together with the negative post-treatment estimates for $\Delta SES$, these results indicate that although designation increases movement toward both extremes of the school SES distribution, the average shift is tilted toward more advantaged destinations. In other words, designated teachers are more likely to sort into both relatively advantaged and relatively disadvantaged campuses, but the net direction of sorting is toward higher-SES schools.

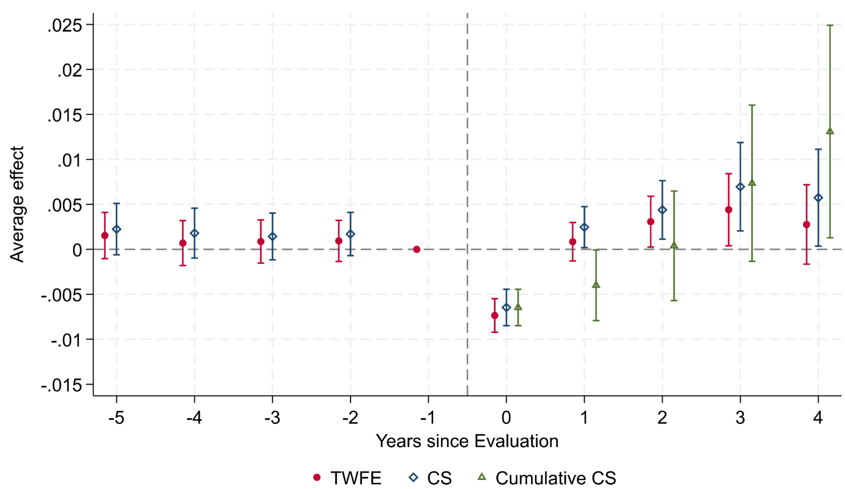
[6] Conditional on moving, the estimated effects on $\Delta SES$ range from $-4.2$ to $-13.1$ percentile points, implying substantial shifts toward more advantaged schools. Because $\Delta SES$ is mechanically zero for non-movers, the conditional estimates show the magnitude of destination changes among 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 8: Estimated Effects on Moving to High-SES Campuses




*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. Error bars represent 95% confidence intervals. Red circle points represent two-way fixed effects estimates. Blue diamond points represent Callaway & Sant'Anna (2021) estimates. Green triangle points 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 is an indicator for being observed at a high-SES campus in $t + 1$ and a non high-SES campus in $t$.

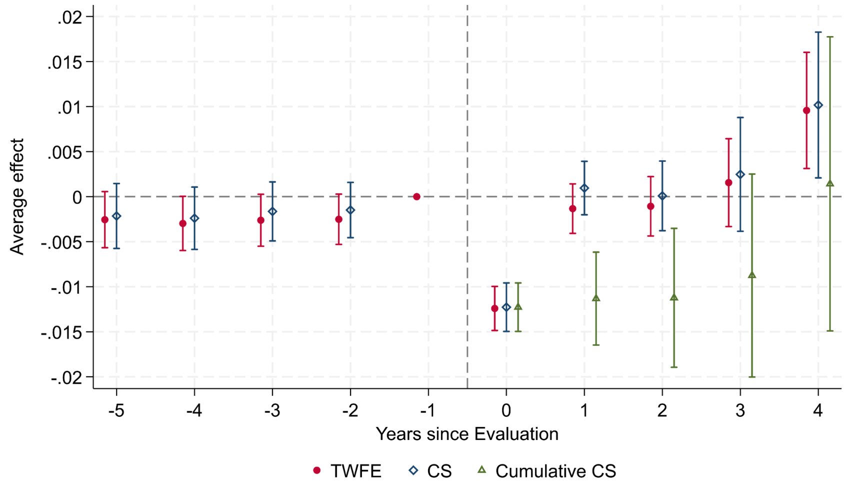
Figure 9: Estimated Effects on Moving to Low-SES Campuses




*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. Error bars represent 95% confidence intervals. Red circle points represent two-way fixed effects estimates. Blue diamond points represent Callaway & Sant'Anna (2021) estimates. Green triangle points 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 is an indicator for being observed at a low-SES campus in $t + 1$ and a non low-SES campus in $t$.

Figure 10 reports the effects on moving to a rural campus. The pattern here is weaker. Point estimates turn positive in later periods, but the cumulative post-designation effect is small and statistically indistinguishable from zero. Movement into rural schools is not a major driver of the post-designation mobility increase.

Figure 10: Estimated Effects on Moving to Rural Campuses

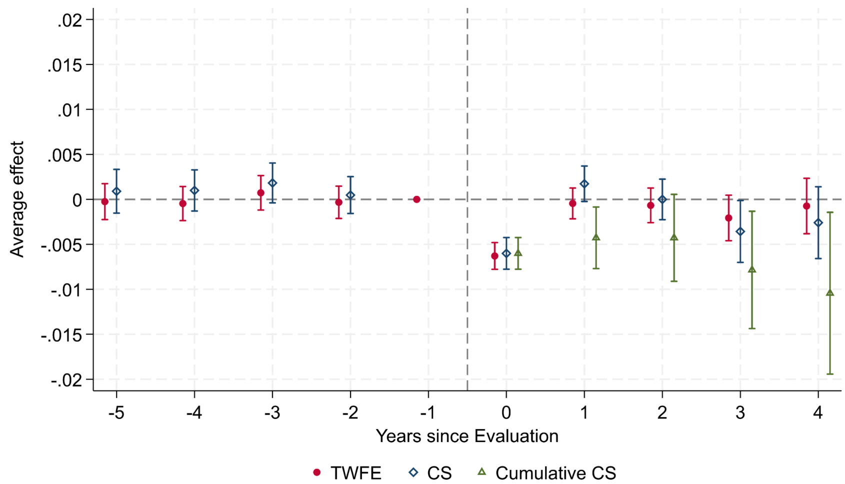



*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. Error bars represent 95% confidence intervals. Red circle points represent two-way fixed effects estimates. Blue diamond points represent Callaway & Sant'Anna (2021) estimates. Green triangle points 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 is an indicator for being observed at a rural campus in $t + 1$ and a non rural campus in $t$.

I next turn to teachers' origin campuses in order to identify where these changes in mobility are coming from. Figures 11, 12, and 13 report the effects of designation on the probability of leaving high-SES, low-SES, and rural campuses, respectively. These outcomes are likewise transition-based; for example, moving from a low-SES campus is defined for teachers whose origin campus is low-SES and whose destination campus is not low-SES.

Figure 11 shows little evidence of increased exits from high-SES campuses following designation. The post-treatment estimates are small and mixed in sign, and the cumulative effect is negative, at $-1.0$ pp, and statistically significant. This suggests that the overall increase in mobility is not being driven by teachers leaving already advantaged schools. If anything, the results imply that designated teachers at high-SES campuses become slightly less likely, relative to the control group, to leave these settings over the post-designation period.

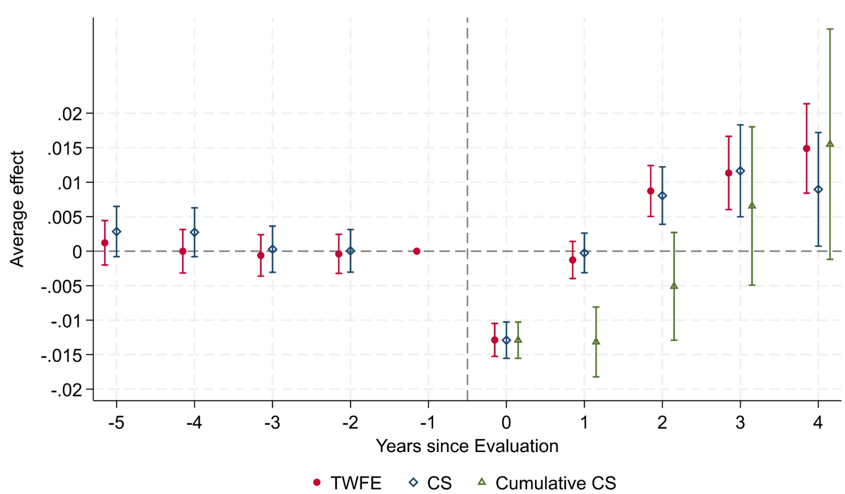
Figure 11: Estimated Effects on Moving From High-SES Campuses




*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. Error bars represent 95% confidence intervals. Red circle points represent two-way fixed effects estimates. Blue diamond points represent Callaway & Sant'Anna (2021) estimates. Green triangle points 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 is an indicator for being observed at a non high-SES campus in $t + 1$ and a high-SES campus in $t$.

By contrast, Figure 12 shows that post-designation mobility rises among teachers leaving low-SES campuses. After the period 0 decline, the point estimates become positive and statistically significant in later periods, and the cumulative post-designation effect reaches 1.6 pp, although only marginally significant. This pattern indicates that designated teachers are increasingly likely to exit low-SES schools once the temporary lock-in constraint is lifted. This result is consistent with designation expanding outside options for teachers initially employed in more disadvantaged school settings.

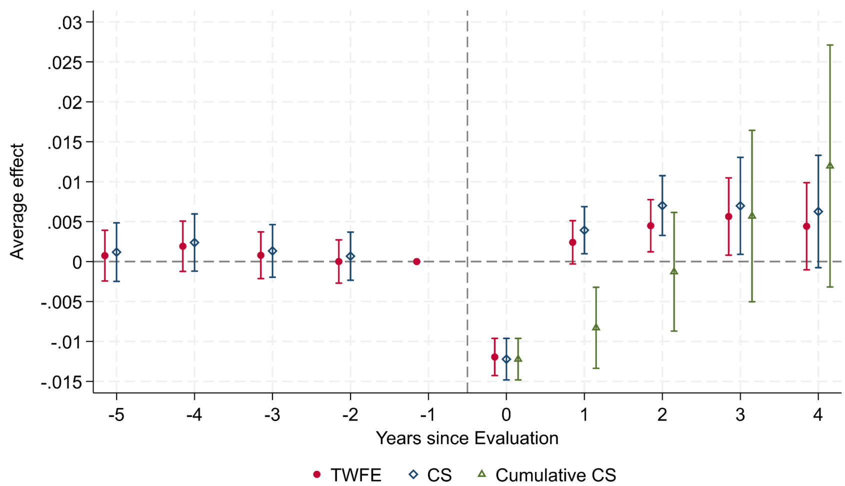
Figure 12: Estimated Effects on Moving From Low-SES Campuses




*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. Error bars represent 95% confidence intervals. Red circle points represent two-way fixed effects estimates. Blue diamond points represent Callaway & Sant'Anna (2021) estimates. Green triangle points 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 is an indicator for being observed at a non low-SES campus in $t + 1$ and a low-SES campus in $t$.

Figure 13 presents estimates for moving from rural campuses. The post-treatment coefficients are generally positive, and several are statistically significant, indicating that designation increases the likelihood that teachers leave rural campuses in the years after designation. However, because the cumulative post-designation effect is not statistically distinguishable from zero, this pattern is weaker than the corresponding evidence for exits from low-SES campuses. Taken together, the origin-campus results suggest that the increase in post-designation mobility is not coming from exits out of high-SES campuses and is more consistent with increased movement among teachers initially employed in less advantaged settings, particularly low-SES campuses.

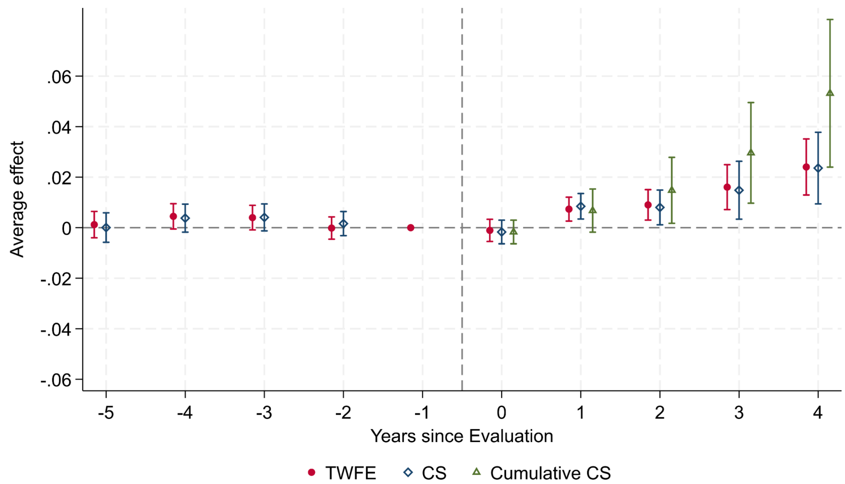
Figure 13: Estimated Effects on Moving From Rural Campuses




*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. Error bars represent 95% confidence intervals. Red circle points represent two-way fixed effects estimates. Blue diamond points represent Callaway & Sant'Anna (2021) estimates. Green triangle points 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 is an indicator for being observed at a non rural campus in $t + 1$ and a rural campus in $t$.

Lastly, I consider the geographic scope of post-designation mobility. Figure 14 indicates that a large share of post-designation mobility occurs within districts. This is consistent with pre-treatment patterns, under which roughly half of all teacher moves are already within-district transitions. The evidence therefore suggests that designation increases overall mobility without materially changing its geographic composition. Rather than shifting teachers toward broader geographic moves, designation appears to simply raise the overall rate of movement while maintaining the existing within-district moves. For completeness, the corresponding figures for within-county and within-region mobility are reported in Appendix Figures C.2 and C.3.

Figure 14: Estimated Effects on Moving Within School District




*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. Error bars represent 95% confidence intervals. Red circle points represent two-way fixed effects estimates. Blue diamond points represent Callaway & Sant'Anna (2021) estimates. Green triangle points 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 is an indicator for being observed at a campus in district $j$ in $t+1$ and a different campus in district $j$ in $t$.

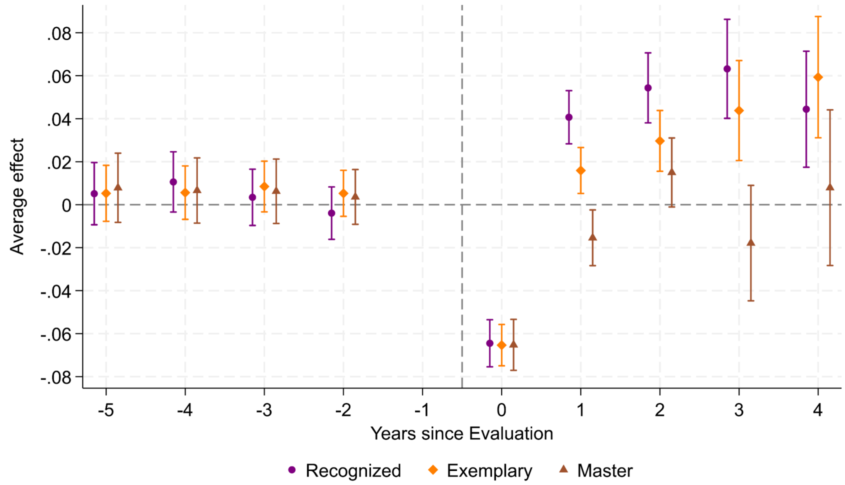
## 7.3 Heterogeneity

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.

A clear pattern emerges from these estimates. The post-designation mobility response is concentrated among teachers receiving Recognized and Exemplary designations, while the corresponding response for Master teachers is substantially weaker.

Figure 15 shows that the increase in overall campus mobility is largest for Recognized teachers and somewhat smaller, though still substantial, for Exemplary teachers. For both groups, mobility rises after the lock-in year and remains elevated through the post-treatment periods. By contrast, Master teachers exhibit little evidence of a sustained increase in campus-to-campus mobility. This suggests that the aggregate mobility effects documented above are driven primarily by teachers in the lower two tiers of the designation hierarchy.

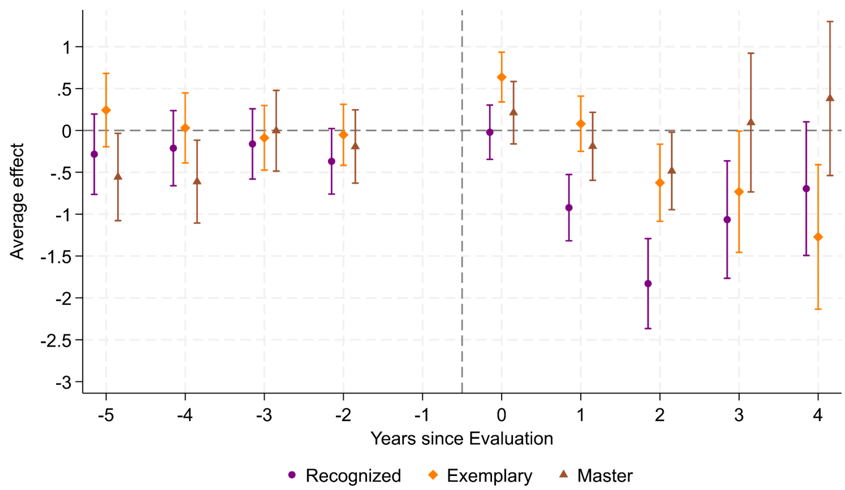
Figure 15: Estimated Effects on Moving Campuses by Designation Level




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

Figure 16 shows a similar pattern for sorting across campuses. For Recognized teachers, the post-treatment estimates for $\Delta SES$ are consistently negative and economically large, indicating movement toward more advantaged campuses after designation. Exemplary teachers also experience negative post-treatment effects, although these are generally smaller and somewhat less uniform across event time. For Master teachers, the estimates are weaker and do not display the same persistent post-treatment decline. Thus, the aggregate shift toward more advantaged campuses appears to be concentrated among Recognized and Exemplary teachers rather than among those receiving the highest designation.

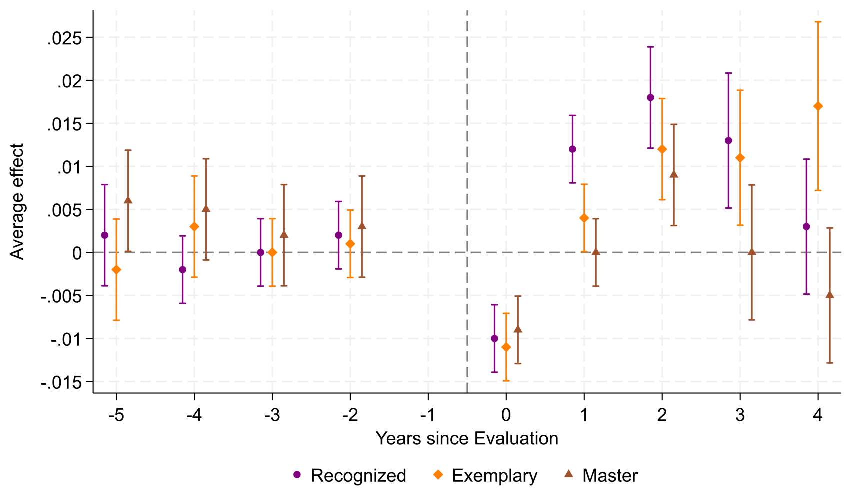
Figure 16: Estimated Effects on $\Delta SES$ by Designation Level




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

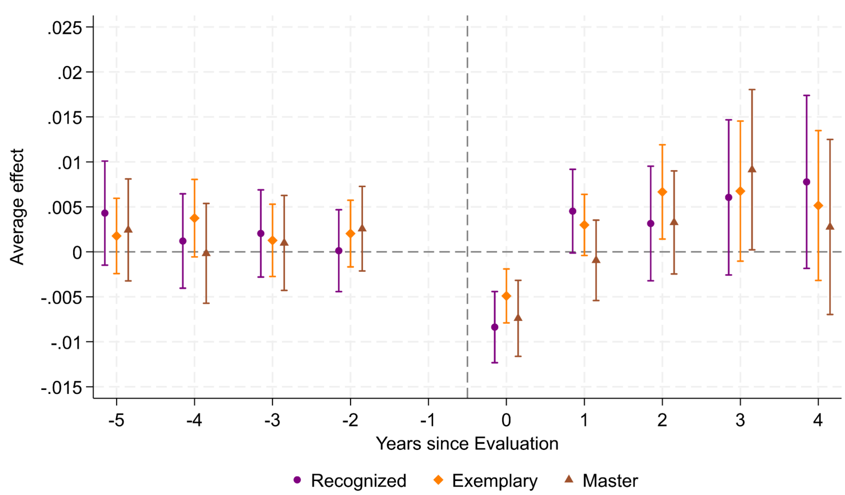
The destination-specific results reinforce this interpretation. Figures 17 and 18 show that for Recognized and Exemplary teachers, the clearest destination-side response is an increase in moves to high-SES campuses. Although there is also some increase in movement to low-SES campuses, the pattern is smaller and less consistent, implying that the net destination response is tilted toward more advantaged schools. Figure 19 shows limited evidence of heterogeneity in moves to rural campuses.

Figure 17: Estimated Effects on Moving to High-SES Campuses by Designation Level



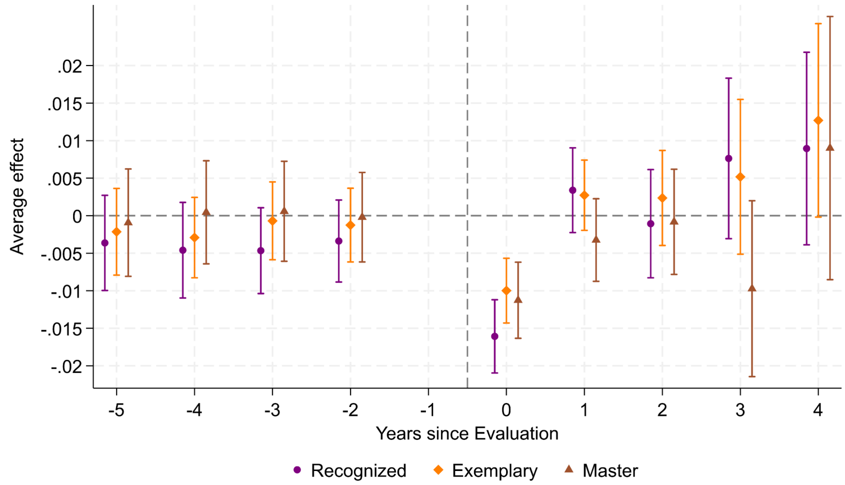

*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. All results are estimated using Callaway & Sant'Anna (2021). Error bars represent 95% confidence intervals. Purple circle points represent Recognized level teachers. Orange diamond points represent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

Figure 18: Estimated Effects on Moving to Low-SES Campuses by Designation Level




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

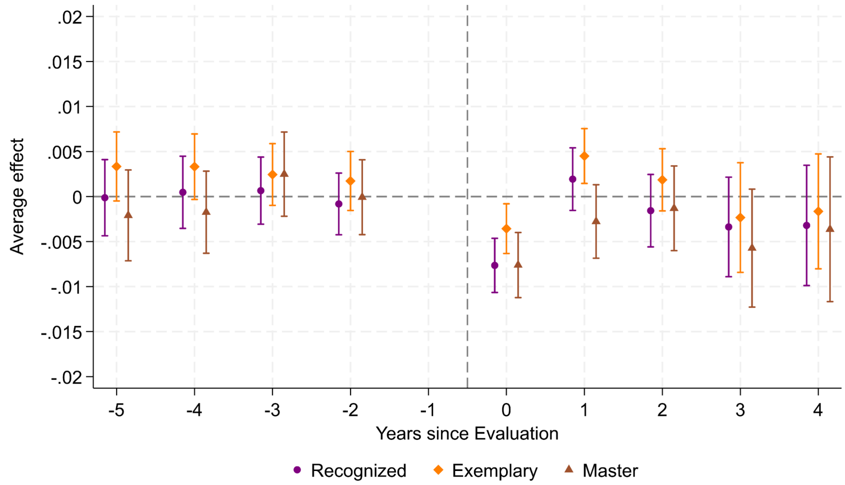
Figure 19: Estimated Effects on Moving to Rural Campuses by Designation Level




*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. All results are estimated using Callaway & Sant'Anna (2021). Error bars represent 95% confidence intervals. Red circle points represent Recognized level teachers. Blue diamond points represent Exemplary level teachers. Green triangle points represent Master level teachers. The outcome is an indicator for being observed at a rural campus in $t+1$ and a non rural campus in $t$.

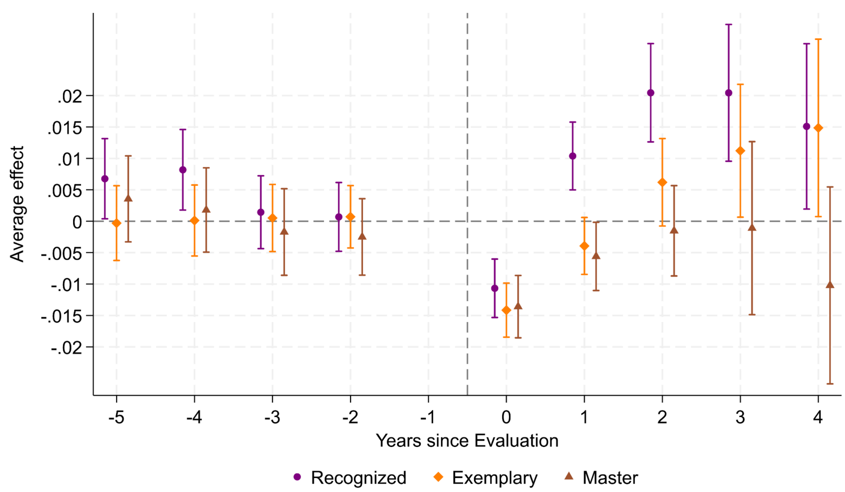
The origin-side results point to a similar conclusion. Figures 20, 21, and 22 indicate that the post-designation increase in mobility for Recognized teachers is concentrated more in exits from low-SES campuses than in exits from high-SES campuses. Exemplary teachers show a qualitatively similar, though weaker, pattern. Master teachers, by contrast, show little evidence of increased exits from low-SES campuses following designation. Taken together, the origin- and destination-side results suggest that the aggregate re-sorting effect is driven disproportionately by movement out of less advantaged schools and into more advantaged ones among teachers in the lower designation tiers.

Figure 20: Estimated Effects on Moving from High-SES Campuses by Designation Level



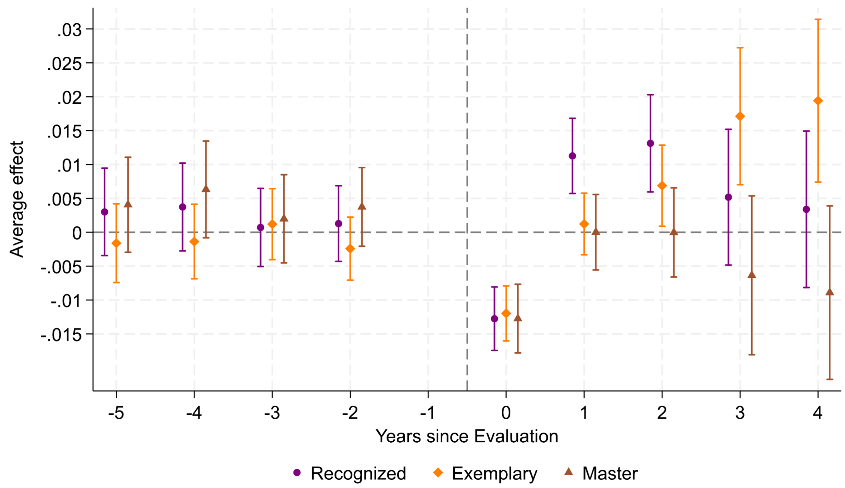

*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. All results are estimated using Callaway & Sant'Anna (2021). Error bars represent 95% confidence intervals. Purple circle points represent Recognized level teachers. Orange diamond points represent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

Figure 21: Estimated Effects on Moving from Low-SES Campuses by Designation Level




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

Figure 22: Estimated Effects on Moving from Rural Campuses by Designation Level




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

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 the response for Master teachers is much more limited. This pattern suggests that the credentialing channel may be most important in the lower designation tiers, where receiving a designation appears to meaningfully expand teachers' outside options and facilitate movement to more advantaged schools. By contrast, the weaker mobility response among Master teachers is more consistent with compensation playing the dominant role, or at least offsetting any additional credentialing value of the designation. In that case, Master teachers may be less responsive not because designation has no value, but because the compensation attached to the highest tier is sufficient to induce relative indifference between moving and staying. Taken together, these results suggest that designation operates through different channels at different points in the designation hierarchy.

# 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 designated teachers after they have been designated. Every year a teacher makes a discrete choice between (i) moving to a school within a 30 mile radius of their current school, (ii) staying at their current school, (iii) leaving teaching at Texas public schools which I will call the outside option. The 30 mile radius captures 90% of all moves, and it seems reasonable to believe that teachers moving larger distances are moving for reasons other than salary. A limitation of the data is that I do not observe a teacher's residence, and therefore cannot observe commute time. Formally let teacher $i$'s choice set $\mathcal{C}_i$ be composed of teacher $i$'s current school in period $t$, the set of schools within 30 miles of her current school, and the outside option.

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.

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

$$u_{ij'} = \alpha w_{ij'} + \beta_i \bar{s}_{j'} + \delta Dist_{ij'} + \varepsilon_{ij'} \tag{7}$$

where $w_{ij'}$ is the total wage offered in thousands by school $j'$, which 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, $w_{ij'} = \bar{w}_{d(j')}(exp_i, cred_i) + B_k + m_k \bar{s}_{j'}$. $\bar{s}_{j'}$ 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. I allow for teachers to have heterogeneous preferences over $\bar{s}_{j'}$, I assume their preferences follow a normal distribution, $\beta_i \sim \mathcal{N}(\bar{\beta}, \sigma_\beta)$. $Dist_{ij'} = d(j, j')$ is the euclidean distance between her current campus $j$ and alternative campus $j'$. Due to the limitation of not observing a teachers home residence, embedded within $\delta$ is both the cost of additional distance in miles but also the cost of switching campuses in general because $d(j, j) = 0$ and $d(j, j') > 0$ for all $j \neq j'$. Finally, teacher $i$ exhibits idiosyncratic match preferences $\varepsilon_{ij'}$, which I assume follow a Type 1 Extreme Value Distribution and I normalize the outside option to have a utility of 0.

Even with the restrictions on the choice set, they can still be computationally infeasible to estimate. In order to solve this issue I do 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.

It is important to note that I do not directly observe each district's official salary schedule. However, because Texas districts tend to follow fairly rigid salary structures, I am able to recover district salary schedules from observed teacher salaries. Specifically, I collapse the data to district-year-experience-degree cells and assign each cell the median observed base salary. This allows me to estimate, for example, the salary a district pays a teacher with four years of experience and a master's degree in a given year. In larger districts, this approach reproduces posted salary schedules very closely. In smaller districts, however, some cells are unobserved because no teacher in the data has that exact combination of experience and degree. To address these gaps, I fill missing cells using the salary from the nearest observed experience step within the same district-year-degree schedule, and when necessary I use broader district-degree or district-level medians as fallback values. This procedure yields a complete salary schedule for each district-year.

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

## 8.2 Limitations

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

First, the coefficient $\beta_i$ 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 $\beta_i$ 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 30-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 an independent 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, but in the present framework these costs are absorbed into the distance coefficient $\delta$ and treated as recurring per-period disutility rather than as costs paid only when a move occurs. 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, the wage variable $w_{ij'}$ is constructed from district salary schedules imputed from cell-level medians of observed base salaries, with nearest-step and district-level fallbacks for unobserved cells. This procedure introduces measurement error in $w_{ij'}$, which biases the wage coefficient $\alpha$ toward zero and inflates the implied magnitude of $-\beta_i/\alpha$. The bias is likely largest in small districts where sparse cells require more aggressive imputation. In addition, wages at a teacher's current campus are observed directly while wages at counterfactual alternatives are imputed, introducing asymmetric measurement error across alternatives. In section 9 I discuss a robustness check in which I restrict to district-year-experience-degree cells with at least 3 observations.

## 8.3 Results

### 8.3.1 Mixed Logit Estimates and Willingness to Pay

The event-study difference-in-differences results in Section 7.2 show that designation increases mobility and that, on net, designated teachers move toward more advantaged campuses. This section asks whether that pattern reflects the compensation channel, the credentialing channel, or both. I present estimates from the discrete choice model described in Section 8.3.1, recover the salary gradient designated teachers face, and show that the framework's predictions match the heterogeneity in sorting shown in Section 8.3.2.

Table 8 reports estimates from the discrete choice model on the sample of all designated teachers in post-designation years.

Table 8: Mixed Logit Estimates of Teacher School Choice

| | (1) |
|---|---|
| *Fixed parameters* | |
| Wage ($\alpha$) | 0.027 |
| | ($< 0.001$) |
| Distance ($\delta$) | $-1.473$ |
| | (0.018) |
| *SES preference distribution:* $\beta_i \sim N(\bar{\beta}, \sigma_\beta^2)$ | |
| Mean ($\bar{\beta}$) | $-0.165$ |
| | (0.013) |
| Std. dev. ($\sigma_\beta$) | 0.976 |
| | (0.022) |
| *Implied willingness to pay (per 1 SD* $\uparrow$ *in* $\bar{s}_j$*)* | |
| WTP | \$6,036 |
| | (485) |
| Number of individuals | 23,619 |
| Number of observations | 45,725 |
| Log-likelihood | $-96,287$ |
| AIC | 192,583 |

*Notes:* Robust standard errors in parentheses, WTP standard error estimated via Delta Method. Mixed logit estimates with SES score as a normally distributed random coefficient. Estimation by maximum simulated likelihood using the BFGS algorithm with 1,000 Sobol draws with inter-individual variation, implemented in Apollo (Hess & Palma, 2019). The sample includes all designated teachers in post-designation years. WTP is computed as $-\bar{\beta}/\alpha \times 1{,}000$ and represents the annual willingness-to-pay (in dollars) for a 1SD increase in school disadvantage $\bar{s}_j$.

The marginal utility of wages is positive and the disutility of distance is negative and large. The large disutility on distance is consistent with the fact that distance also incorporates switching costs. The mean SES preference is negative confirming the framework's maintained assumption that teachers on average prefer less-disadvantaged schools. The estimated dispersion in preferences is also substantial: the standard deviation of the random SES coefficient is large relative to the mean, and the implied share of teachers with a positive SES coefficient .is about 0.43, suggesting a non-trivial share of teachers are willing to teach at more disadvantaged schools.

The willingness to pay estimate is \$6,036 per year per standard deviation of school disadvantage. This is the average teacher's willingness to pay to avoid a 1SD increase in school disadvantage, holding compensation constant, and it provides the benchmark against which the salary gradient should be compared.

#### 8.3.2 Comparing the Salary Gradient to Willingness to Pay

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

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

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. Therefore $\hat{\pi}_k$ recovers $E\left[m_k + \frac{\Delta w}{\Delta s}\right]$ for teachers at designation level $k$.

The estimates are $\hat{\pi}_{Recognized} = 2,973$, $\hat{\pi}_{Exemplary} = 4,028$, and $\hat{\pi}_{Master} = 5,674$. Recall from the framework the moving condition for moving to a more disadvantaged campus is $\beta_i + \alpha\left(m_k + \frac{\Delta w}{\Delta s}\right) > 0$ after substituting in $\hat{\pi} = m_k + \frac{\Delta w}{\Delta s}$ the moving condition becomes $\beta_i + \alpha\hat{\pi}_k > 0$. Rearranging this to be in willingness to pay terms, teacher $i$ moves toward more disadvantaged campuses if and only if

$$-\beta_i/\alpha < \hat{\pi}_k$$

or in words, if and only if her willingness to pay to avoid disadvantage is smaller than the salary gradient she faces. For the average teacher with $-\bar{\beta}/\alpha = \$6,036$, the condition becomes $\hat{\pi}_k > \$6,036$.

Table 9: Framework prediction level

| | Recognized | Exemplary | Master |
|---|---|---|---|
| Salary gradient $\hat{\pi}_k$ | 2,973 | 4,028 | 5,674 |
| | (14.72) | (17.04) | (28.98) |
| Average WTP, $-\bar{\beta}/\alpha$ | 6,036 | 6,036 | 6,036 |
| | (485) | (485) | (485) |
| Net incentive $\hat{\pi}_k-$ WTP | $-3,063$ | $-2,008$ | $-362$ |
| $p$-value ($H_0 : \hat{\pi}_k \geq$ WTP) | $< 0.001$ | $< 0.001$ | 0.224 |

*Notes:* All units are in dollars per SD increase in $\bar{s}_j$ (\$/SD). Standard errors in parentheses. WTP standard error is from the delta method applied to the mixed logit estimates. The $p$-value tests the one-sided hypothesis that the salary gradient is at least as large as the average WTP, against the alternative that it falls short. Test statistics use SE($\hat{\pi}_k$ − WTP) ≈ SE(WTP), since SE($\hat{\pi}_k$) is small relative to SE(WTP).

For Recognized teachers the salary gradient closes only 49% of the gap to average WTP, leaving the typical Recognized teacher's WTP well above the gradient and a clear motivation to sort toward more advantaged schools. For Exemplary teachers the gradient closes 67% of the gap, predicting a much weaker sorting incentive. For Master teachers the gradient essentially closes the gap, predicting that the average Master teacher should be near indifferent to school SES on net.

The framework's predictions are best highlighted by looking at Figure 16 in section 7.3. I show that the effect of designation on $\Delta SES$ is largest in magnitude for Recognized teachers, smaller for Exemplary and smallest for Master, with the Master level fluctuating around 0. This ordering matches the predictions that observed sorting should weaken as the salary gradient closes the gap to WTP. The compensation channel does not redirect Master level teachers toward more disadvantaged campuses, because the salary gradient falls slightly short of the average WTP, even at the highest designation level.

# 9 Robustness Checks

## 9.1 Event-Study

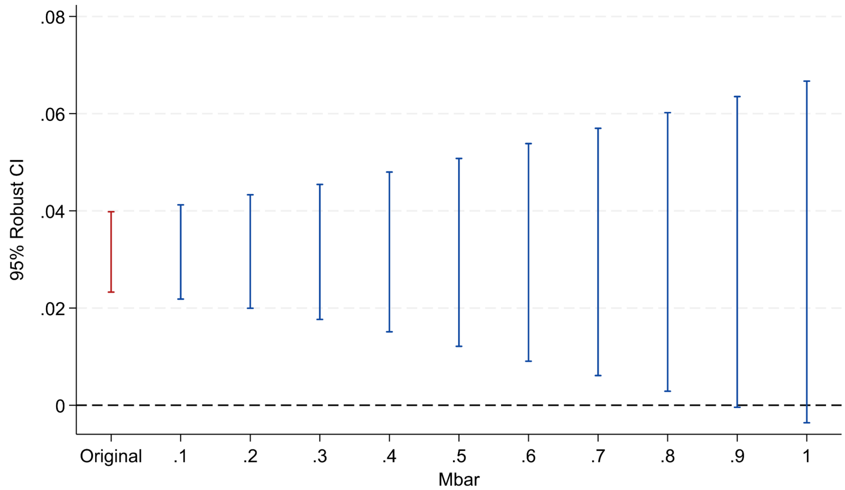
I perform several robustness checks to assess whether my results are driven by factors other than designation itself. For brevity, I present the relevant figures for campus mobility, and provide the full event-study tables in the Appendix.

### 9.1.1 Honest DiD

I test the sensitivity of my results to violations of parallel trends using the honest DiD framework from Rambachan & Roth (2023). I test these violations against the simple average of the event-study estimates from periods 1 through 4 $\left(\frac{1}{4}\sum_{\tau=1}^{4}\hat{\delta}_{\tau}\right)$. I test these using the relative magnitudes restriction, which bounds the largest post-treatment violation of parallel trends at $M$ times the largest violation observed in the pre-treatment period. $M = 0$ implies exact parallel trends and $M = 1$ allows for post-treatment deviations as large as the worst pre-treatment deviation.

Figure 23 presents the 95% confidence intervals across $M \in [0, 1]$ at .1 intervals. The average post treatment estimates remains significant up to $.9M$. The implied breakdown value suggests that post-treatment violations would need to be almost as large as the worst pre-period violation before the average becomes statistically insignificant. I interpret this as evidence that my estimated effects are robust to modest violations of the parallel-trends assumption.

Figure 23: Honest DiD Results for Moving Campuses

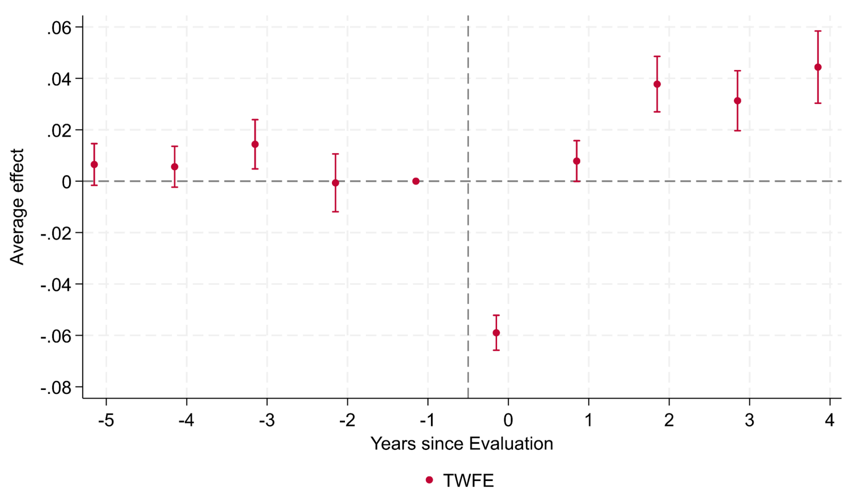



*Notes:* Results from Honest DiD testing the relative magnitude restriction.

#### 9.1.2 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 6 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 only the TWFE estimates for this exercise. I present these findings in Tables B.9, B.10, and B.11. These estimates closely track the full-sample results and suggest that the main findings are not driven by pandemic-related shocks.

Figure 24: Estimated Effects on Moving Campuses, 2019 and 2020 School Years Removed



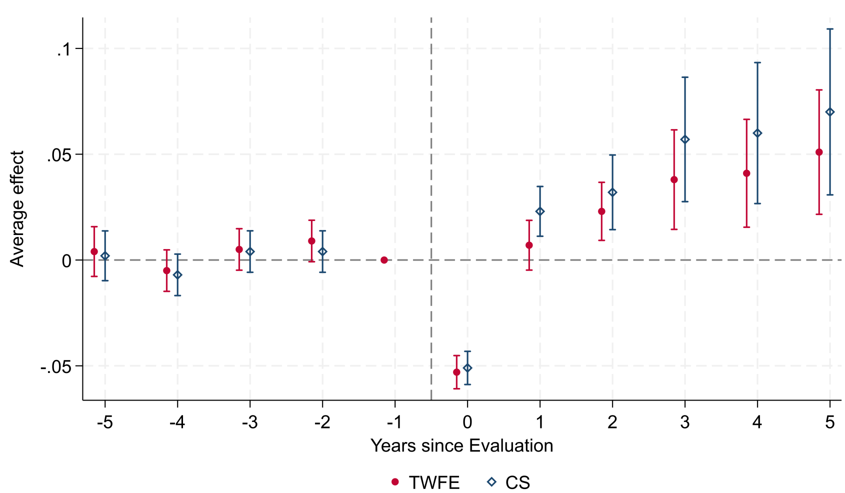

*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. Error bars represent 95% confidence intervals. Red circle points represent two-way fixed effects estimates. The outcome is an indicator for being observed at a different campus in $t + 1$ relative to period $t$.

### 9.1.3 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 and impose a standardized caliper of 0.1 on value-added. I then stack the cohort-specific matched samples into a single panel and re-estimate equation 6 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 in Tables B.15, B.16, and B.17 are similar to the full-sample results, though noisier and insignificant during the post-treatment period. This should be expected given the significantly smaller estimation sample. The mechanical $t = 0$ dip in cross-campus mobility, the post lock-in rebound, and the concentration of upward moves at the top of the SES distribution all persist. I treat these as suggestive evidence that my results are not driven by the choice of using the 2024 cohort as the comparison group.

Figure 25: Estimated Effects on Moving Campuses, Matched Sample



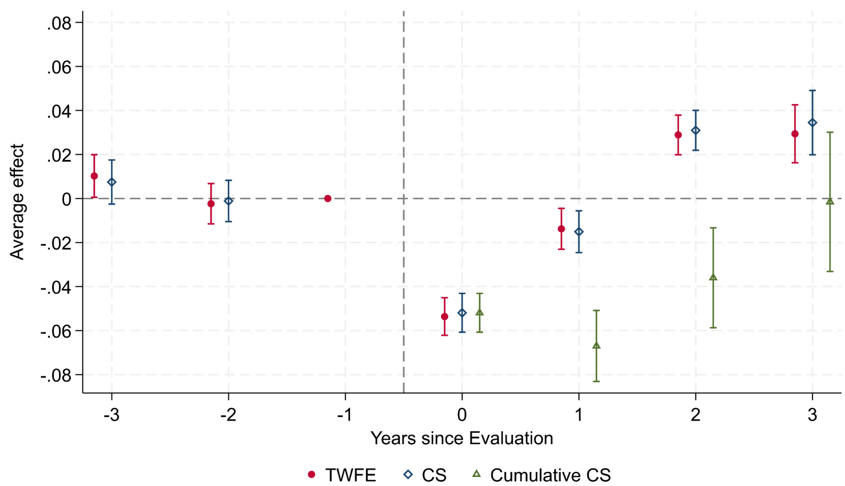

*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. Error bars represent 95% confidence intervals. Red circle points represent two-way fixed effects estimates. Blue diamond points represent Callaway & Sant'Anna (2021) estimates. The outcome is an indicator for being observed at a different campus in $t + 1$ relative to period $t$

### 9.1.4 Balanced Panel

A third 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 in Tables B.12, B.13, and B.14 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.

Figure 26: Estimated Effects on Moving Campuses, Balanced Sample




*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. Error bars represent 95% confidence intervals. Red circle points represent two-way fixed effects estimates. Blue diamond points represent Callaway & Sant'Anna (2021) estimates. The outcome is an indicator for being observed at a different campus in $t + 1$ relative to period $t$.

## 9.2 Discrete Choice Model

The baseline specification I presented earlier uses the salary schedule recovered from district-year-experience-degree cell medians, with missing cells filled by nearest-step imputation and district-level fallbacks. This procedure introduces two sources of measurement error on the right-hand side. First, observed cells with few teachers yield noisier estimates of the underlying salary schedule. Second, cells that are empty in the data are imputed from neighboring cells, which introduces error that is mechanically correlated with district size. Small districts have more empty cells and therefore more imputed values, and district size is correlated with SES to an extent. Similar to OLS, introduction of this kind of measurement error could attenuate $\alpha$ which would inflate the implied WTP.

To check whether this matters, 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 $N \geq 3$,$N \geq 5$, and $N \geq 10$. 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.

The sample drops are modest relative to the share of cells removed. At $N \geq 3$ the sample falls from 23,619 to 22,418 individuals while around half of all potential salary cells are excluded. Tighter thresholds drop more cells but still a small share of teachers, falling to 19,439 at $N \geq 10$. This is because small-cell observations are concentrated in small districts: the typical teacher is in a well-populated cell, but the typical cell is not. Tighter thresholds also push the effective sample toward larger districts with more complete salary schedules, which differ from the full sample on the SES margin.

Table 10 reports the results next to the baseline. The WTP estimate falls modestly at tighter thresholds, from \$6,036 in the baseline to \$5,294 at $N \geq 10$, a 12% decline.

Table 10: Mixed Logit Estimates: Robustness to Salary Cell Definition

| | Baseline<br>(1) | $N \geq 3$<br>(2) | $N \geq 5$<br>(3) | $N \geq 10$<br>(4) |
|---|---|---|---|---|
| *Fixed parameters* | | | | |
| Wage ($\alpha$) | 0.027 | 0.028 | 0.028 | 0.027 |
| | (< 0.001) | (< 0.001) | (< 0.001) | (< 0.001) |
| Distance ($\delta$) | −1.473 | −1.484 | −1.496 | −1.509 |
| | (0.018) | (0.018) | (0.019) | (0.019) |
| *SES preference distribution:* $\beta_i \sim N(\bar{\beta},\, \sigma^2_\beta)$ | | | | |
| Mean ($\bar{\beta}$) | −0.165 | −0.163 | −0.153 | −0.142 |
| | (0.013) | (0.014) | (0.014) | (0.014) |
| Std. dev. ($\sigma_\beta$) | 0.976 | 0.955 | 0.964 | 0.950 |
| | (0.022) | (0.023) | (0.023) | (0.024) |
| *Implied willingness to pay (per 1 SD* $\uparrow$ *in* $\bar{s}_j$*)* | | | | |
| WTP | \$6,036 | \$5,870 | \$5,526 | \$5,239 |
| | (485) | (483) | (495) | (521) |
| Number of individuals | 23,619 | 22,418 | 21,369 | 19,439 |
| Number of observations | 45,725 | 43,485 | 41,735 | 38,258 |
| Log-likelihood | $-96,287$ | $-89,798$ | $-86,383$ | $-78,908$ |
| AIC | 192,583 | 179,604 | 172,675 | 157,824 |

*Notes:* Robust standard errors in parentheses, WTP standard error estimated via Delta Method. Mixed logit estimates with SES score as a normally distributed random coefficient. 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 $N$ directly observed teacher salaries (no imputation), for $N \geq 3$, $N \geq 5$, and $N \geq 10$ respectively. Estimation by maximum simulated likelihood using the BFGS algorithm with 1,000 Sobol draws with inter-individual variation, implemented in Apollo (Hess & Palma, 2019). The sample includes all designated teachers in post-designation years. WTP is computed as $-\bar{\beta}/\alpha \times 1,000$ and represents the annual willingness-to-pay (in dollars) for a 1SD increase in school disadvantage $\bar{s}_j$.

I do not read this as evidence that the baseline is biased by measurement error. The more plausible explanation is selection. Tighter thresholds push the effective sample toward larger districts, which in Texas include the big urban districts containing many of the highest-SES campuses. Teachers in these districts have systematically less aversion to disadvantaged schools than the full sample of designated teachers, both because they are already working in higher-SES districts and because the within-district choice set looks different. The drift in $\bar{\beta}$ is consistent with this selection, not with a correction of measurement-error bias on $\alpha$.

## 10 Conclusion

In this paper I study how the Texas Teacher Incentive Allotment affects teacher mobility and sorting across schools. TIA combines a portable performance-based designation with a salary supplement that rises in more disadvantaged schools, making it a useful setting for studying whether targeted compensation can improve the distribution of effective teachers.

I find that designation increases teacher mobility after the temporary lock-in year, but the additional movement is not concentrated toward the highest-need campuses. On net, designated teachers move toward more advantaged schools, with the strongest effects among Recognized and Exemplary teachers. The discrete choice estimates help explain this pattern. The average designated teacher requires roughly \$6,000 in additional annual compensation to accept a 1SD increase in school disadvantage, while the salary gradient created by TIA falls short of that amount at the Recognized and Exemplary levels, but nearly meets it at the Master level.

These results suggest that, under its current parameters, the credentialing effect of TIA is stronger than its compensation effect. The program makes effective teachers more mobile and gives them a more credible signal of quality in the labor market, but the additional pay attached to disadvantaged schools is not large enough to offset teachers' average preference for more advantaged settings. As a result, TIA changes sorting, but not in the direction the policy is intended to.

The framework also points to a clear policy implication. Closing the gap between the salary gradient and teachers' willingness to pay would require steeper SES scaling, especially at the Recognized tier where most designations occur. For that tier, the results suggest the SES scaling would need to increase substantially, roughly doubling to offset the average teacher's preference for more advantaged schools. This suggests that the main issue is not the portability of the designation itself, but that the compensation schedule is too shallow to direct the added mobility toward higher-need campuses. While in this paper I have focused on teachers' responses to TIA, an important part of the story is how schools and districts respond. This analysis is a partial equilibrium result and modeling the district side, and characterizing a general equilibrium is left to future work.

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# 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 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 $\theta_j$ is the teacher component, $\eta_{ct}$ is a classroom-year shock, and $\varepsilon_{icgt}$ 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 $n_c t$ 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, $\sigma^2_\theta$, using the covariance of classroom-year mean residuals across years for the same teacher. I estimate student-level noise, $\sigma^2_\varepsilon$, from within-classroom residual variation. I then recover the variance of classroom-year shocks, $\sigma^2_\eta$, 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:

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

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 | 786 | 786 |
| 2020 | 214 | 1,000 |
| 2021 | 267 | 1,267 |
| 2022 | 2,005 | 3,272 |
| 2023 | 2,881 | 6,153 |
| 2024 | 3,660 | 9,813 |

*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 | $\Delta SES$ | Move to Low-SES Campus | Move to High-SES Campus | Move to Rural Campus |
|---|---|---|---|---|---|
| $\delta_{-5}$ | 0.006 | -0.139 | 0.002 | 0.001 | -0.002 |
| | (0.004) | (0.138) | (0.001) | (0.002) | (0.002) |
| $\delta_{-4}$ | 0.007* | -0.180 | 0.002 | 0.002 | -0.002 |
| | (0.004) | (0.131) | (0.001) | (0.002) | (0.002) |
| $\delta_{-3}$ | 0.007* | -0.070 | 0.001 | 0.001 | -0.002 |
| | (0.004) | (0.123) | (0.001) | (0.001) | (0.002) |
| $\delta_{-2}$ | 0.003 | -0.180 | 0.002 | 0.002 | -0.001 |
| | (0.003) | (0.115) | (0.001) | (0.001) | (0.002) |
| $\delta_0$ | -0.064*** | 0.323*** | -0.006*** | -0.011*** | -0.012*** |
| | (0.003) | (0.094) | (0.001) | (0.001) | (0.001) |
| $\delta_1$ | 0.015*** | -0.262** | 0.002** | 0.005*** | 0.001 |
| | (0.003) | (0.107) | (0.001) | (0.001) | (0.002) |
| $\delta_2$ | 0.034*** | -0.922*** | 0.004*** | 0.013*** | 0.000 |
| | (0.004) | (0.141) | (0.002) | (0.002) | (0.002) |
| $\delta_3$ | 0.037*** | -0.672*** | 0.007*** | 0.009*** | 0.002 |
| | (0.007) | (0.219) | (0.003) | (0.002) | (0.003) |
| $\delta_4$ | 0.041*** | -0.644** | 0.006** | 0.006** | 0.010** |
| | (0.009) | (0.252) | (0.003) | (0.003) | (0.004) |
| $\sum_{i=0}^{4} \delta_i$ | 0.062*** | – | 0.013** | 0.023*** | 0.001 |
| | (0.018) | – | (0.006) | (0.006) | (0.008) |
| N | 347,665 | 347,665 | 347,665 | 347,665 | 347,665 |

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 $t = -1$. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

| | Move from Low-SES Campus | Move from High-SES Campus | Move from Rural Campus |
|---|---|---|---|
| $\delta_{-5}$ | 0.003 | 0.001 | 0.001 |
| | (0.002) | (0.001) | (0.002) |
| $\delta_{-4}$ | 0.003 | 0.001 | 0.002 |
| | (0.002) | (0.001) | (0.002) |
| $\delta_{-3}$ | 0.000 | 0.002 | 0.001 |
| | (0.002) | (0.001) | (0.002) |
| $\delta_{-2}$ | 0.000 | 0.000 | 0.001 |
| | (0.002) | (0.001) | (0.002) |
| $\delta_0$ | -0.013*** | -0.006*** | -0.012*** |
| | (0.001) | (0.001) | (0.001) |
| $\delta_1$ | -0.000 | 0.002* | 0.004*** |
| | (0.001) | (0.001) | (0.002) |
| $\delta_2$ | 0.008*** | 0.000 | 0.007*** |
| | (0.002) | (0.001) | (0.002) |
| $\delta_3$ | 0.012*** | -0.004** | 0.007** |
| | (0.003) | (0.002) | (0.003) |
| $\delta_4$ | 0.009** | -0.003 | 0.006* |
| | (0.004) | (0.002) | (0.004) |
| $\sum_{i=0}^{4} \delta_i$ | 0.016* | -0.010** | 0.012 |
| | (0.009) | (0.005) | (0.008) |
| N | 347,665 | 347,665 | 347,665 |

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 |
|---|---|---|---|
| $\delta_{-5}$ | 0.000 | 0.005 | 0.004 |
| | (0.003) | (0.004) | (0.004) |
| $\delta_{-4}$ | 0.004 | 0.005 | 0.006* |
| | (0.003) | (0.003) | (0.004) |
| $\delta_{-3}$ | 0.004 | 0.004 | 0.002 |
| | (0.003) | (0.003) | (0.003) |
| $\delta_{-2}$ | 0.002 | 0.001 | 0.001 |
| | (0.002) | (0.003) | (0.003) |
| $\delta_0$ | -0.002 | -0.029*** | -0.042*** |
| | (0.002) | (0.003) | (0.003) |
| $\delta_1$ | 0.008*** | 0.008*** | 0.011*** |
| | (0.003) | (0.003) | (0.003) |
| $\delta_2$ | 0.008** | 0.018*** | 0.028*** |
| | (0.004) | (0.004) | (0.004) |
| $\delta_3$ | 0.015** | 0.022*** | 0.034*** |
| | (0.006) | (0.007) | (0.007) |
| $\delta_4$ | 0.024*** | 0.027*** | 0.035*** |
| | (0.007) | (0.008) | (0.008) |
| $\sum_{i=0}^{4} \delta_i$ | 0.053*** | 0.046*** | 0.065*** |
| | (0.015) | (0.017) | (0.017) |
| N | 347,665 | 347,665 | 347,665 |

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 | | | $\Delta SES$ | | |
|---|---|---|---|---|---|---|
| | Recognized | Exemplary | Master | Recognized | Exemplary | Master |
| $\delta_{-5}$ | 0.005 | 0.005 | 0.008 | -0.284 | 0.243 | -0.556** |
| | (0.007) | (0.007) | (0.008) | (0.245) | (0.224) | (0.266) |
| $\delta_{-4}$ | 0.011 | 0.006 | 0.007 | -0.212 | 0.030 | -0.611** |
| | (0.007) | (0.006) | (0.008) | (0.229) | (0.213) | (0.252) |
| $\delta_{-3}$ | 0.003 | 0.008 | 0.006 | -0.161 | -0.088 | -0.004 |
| | (0.007) | (0.006) | (0.008) | (0.214) | (0.197) | (0.246) |
| $\delta_{-2}$ | -0.004 | 0.005 | 0.004 | -0.369* | -0.052 | -0.192 |
| | (0.006) | (0.005) | (0.007) | (0.200) | (0.186) | (0.223) |
| $\delta_0$ | -0.064*** | -0.065*** | -0.065*** | -0.021 | 0.637*** | 0.212 |
| | (0.006) | (0.005) | (0.006) | (0.166) | (0.152) | (0.190) |
| $\delta_1$ | 0.041*** | 0.016*** | -0.015** | -0.922*** | 0.080 | -0.190 |
| | (0.006) | (0.005) | (0.007) | (0.202) | (0.168) | (0.207) |
| $\delta_2$ | 0.054*** | 0.030*** | 0.015* | -1.829*** | -0.625*** | -0.484** |
| | (0.008) | (0.007) | (0.008) | (0.274) | (0.235) | (0.236) |
| $\delta_3$ | 0.063*** | 0.044*** | -0.018 | -1.065*** | -0.733** | 0.094 |
| | (0.012) | (0.012) | (0.014) | (0.358) | (0.370) | (0.423) |
| $\delta_4$ | 0.044*** | 0.059*** | 0.008 | -0.695* | -1.272*** | 0.381 |
| | (0.014) | (0.014) | (0.018) | (0.407) | (0.440) | (0.469) |
| N | 112,800 | 144,466 | 90,399 | 112,800 | 144,466 | 90,399 |

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 |
| $\delta_{-5}$ | 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) |
| $\delta_{-4}$ | 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) |
| $\delta_{-3}$ | 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) |
| $\delta_{-2}$ | 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) |
| $\delta_0$ | -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) |
| $\delta_1$ | 0.005* | 0.003* | -0.001 | 0.012*** | 0.004** | -0.000 | 0.003 | 0.003 | -0.003 |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.003) | (0.002) | (0.003) |
| $\delta_2$ | 0.003 | 0.007** | 0.003 | 0.018*** | 0.012*** | 0.009*** | -0.001 | 0.002 | -0.001 |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.004) | (0.003) | (0.004) |
| $\delta_3$ | 0.006 | 0.007* | 0.009** | 0.013*** | 0.011*** | 0.000 | 0.008 | 0.005 | -0.010 |
| | (0.004) | (0.004) | (0.005) | (0.004) | (0.004) | (0.004) | (0.005) | (0.005) | (0.006) |
| $\delta_4$ | 0.008 | 0.005 | 0.003 | 0.003 | 0.017*** | 0.006 | 0.009 | 0.013* | 0.009 |
| | (0.005) | (0.004) | (0.005) | (0.004) | (0.005) | (0.005) | (0.007) | (0.007) | (0.009) |
| N | 112,800 | 144,466 | 90,399 | 112,800 | 144,466 | 90,399 | 112,800 | 144,466 | 90,399 |

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 |
| $\delta_{-5}$ | -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) |
| $\delta_{-4}$ | 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) |
| $\delta_{-3}$ | 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) |
| $\delta_{-2}$ | -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) |
| $\delta_0$ | -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) |
| $\delta_1$ | 0.002 | 0.005*** | -0.003 | 0.010*** | -0.004* | -0.005* | 0.011*** | 0.001 | 0.000 |
| | (0.002) | (0.002) | (0.002) | (0.003) | (0.002) | (0.003) | (0.003) | (0.002) | (0.003) |
| $\delta_2$ | -0.002 | 0.002 | -0.001 | 0.020*** | 0.006* | -0.002 | 0.013*** | 0.007** | -0.000 |
| | (0.002) | (0.002) | (0.002) | (0.004) | (0.004) | (0.004) | (0.004) | (0.003) | (0.003) |
| $\delta_3$ | -0.003 | -0.002 | -0.006* | 0.020*** | 0.011** | -0.001 | 0.005 | 0.017*** | -0.006 |
| | (0.003) | (0.003) | (0.003) | (0.006) | (0.005) | (0.007) | (0.005) | (0.005) | (0.006) |
| $\delta_4$ | -0.003 | -0.002 | -0.004 | 0.015** | 0.015** | -0.010 | 0.003 | 0.019*** | -0.009 |
| | (0.003) | (0.003) | (0.004) | (0.007) | (0.007) | (0.008) | (0.006) | (0.006) | (0.007) |
| N | 112,800 | 144,466 | 90,399 | 112,800 | 144,466 | 90,399 | 112,800 | 144,466 | 90,399 |

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 |
| $\delta_{-5}$ | 0.006 | 0.006 | -0.007 | 0.007 | 0.007 | 0.000 | 0.006 | 0.003 | 0.006 |
| | (0.005) | (0.005) | (0.006) | (0.006) | (0.005) | (0.007) | (0.007) | (0.006) | (0.008) |
| $\delta_{-4}$ | 0.008* | -0.004 | 0.004 | 0.010* | 0.007 | -0.002 | 0.008 | 0.006 | 0.007 |
| | (0.004) | (0.006) | (0.006) | (0.006) | (0.005) | (0.007) | (0.007) | (0.006) | (0.007) |
| $\delta_{-3}$ | 0.010** | -0.001 | -0.000 | 0.003 | 0.006 | 0.003 | -0.000 | 0.003 | 0.002 |
| | (0.004) | (0.006) | (0.006) | (0.006) | (0.005) | (0.007) | (0.006) | (0.005) | (0.007) |
| $\delta_{-2}$ | 0.007* | -0.004 | -0.002 | -0.003 | 0.006 | -0.002 | -0.002 | 0.005 | -0.004 |
| | (0.004) | (0.005) | (0.006) | (0.005) | (0.005) | (0.006) | (0.006) | (0.005) | (0.006) |
| $\delta_{0}$ | -0.000 | -0.009** | -0.039*** | -0.027*** | -0.027*** | -0.036*** | -0.039*** | -0.042*** | -0.048*** |
| | (0.004) | (0.005) | (0.005) | (0.005) | (0.004) | (0.005) | (0.005) | (0.005) | (0.006) |
| $\delta_{1}$ | 0.012*** | -0.003 | 0.031*** | 0.024*** | 0.011** | -0.010* | 0.031*** | 0.012** | 0.011* |
| | (0.004) | (0.005) | (0.006) | (0.005) | (0.005) | (0.006) | (0.006) | (0.005) | (0.006) |
| $\delta_{2}$ | 0.003 | 0.005 | 0.048*** | 0.036*** | 0.012* | 0.010 | 0.048*** | 0.021*** | 0.014* |
| | (0.006) | (0.007) | (0.008) | (0.007) | (0.006) | (0.007) | (0.008) | (0.007) | (0.008) |
| $\delta_{3}$ | 0.018* | -0.025** | 0.058*** | 0.049*** | 0.025** | -0.028** | 0.058*** | 0.038*** | -0.010 |
| | (0.009) | (0.011) | (0.011) | (0.011) | (0.011) | (0.013) | (0.011) | (0.011) | (0.013) |
| $\delta_{4}$ | 0.029** | 0.017 | 0.035*** | 0.026** | 0.045*** | 0.000 | 0.035*** | 0.055*** | 0.005 |
| | (0.012) | (0.016) | (0.013) | (0.012) | (0.013) | (0.017) | (0.013) | (0.014) | (0.018) |
| N | 112,800 | 144,466 | 90,399 | 112,800 | 144,466 | 90,399 | 112,800 | 144,466 | 90,399 |

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 $t = -1$. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

| | Move Campus | Δ SES | Move to Low-SES Campus | Move to High-SES Campus | Move to Rural Campus |
|---|---|---|---|---|---|
| $\delta_{-5}$ | 0.006 | -0.059 | 0.004*** | 0.002 | -0.001 |
| | (0.004) | (0.135) | (0.001) | (0.002) | (0.002) |
| $\delta_{-4}$ | 0.006 | -0.008 | 0.003** | 0.001 | -0.001 |
| | (0.004) | (0.132) | (0.001) | (0.002) | (0.002) |
| $\delta_{-3}$ | 0.014*** | 0.262 | 0.006*** | -0.001 | 0.001 |
| | (0.005) | (0.159) | (0.002) | (0.002) | (0.002) |
| $\delta_{-2}$ | -0.001 | 0.189 | 0.007*** | -0.001 | 0.003 |
| | (0.006) | (0.189) | (0.002) | (0.002) | (0.002) |
| $\delta_{0}$ | -0.059*** | 0.327*** | -0.005*** | -0.011*** | -0.011*** |
| | (0.003) | (0.106) | (0.001) | (0.001) | (0.002) |
| $\delta_{1}$ | 0.008* | -0.283** | 0.003** | 0.003* | 0.001 |
| | (0.004) | (0.129) | (0.001) | (0.002) | (0.002) |
| $\delta_{2}$ | 0.038*** | -0.854*** | 0.008*** | 0.011*** | 0.005** |
| | (0.006) | (0.182) | (0.002) | (0.002) | (0.002) |
| $\delta_{3}$ | 0.031*** | -0.640*** | 0.007*** | 0.006*** | 0.002 |
| | (0.006) | (0.189) | (0.002) | (0.002) | (0.003) |
| $\delta_{4}$ | 0.044*** | -0.838*** | 0.005** | 0.005** | 0.010*** |
| | (0.007) | (0.213) | (0.002) | (0.002) | (0.003) |
| N | 225,646 | 225,646 | 225,646 | 225,646 | 225,646 |

Table B.9: Two-Way Fixed Effects Estimates for Teacher Mobility and Destination School Context, Excluding 2020

*Notes:* Estimates are based on the sample excluding the 2020 school year. Teacher-level clustered standard errors in parentheses. Omitted period is $t = -1$. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

| | Move from Low-SES Campus | Move from High-SES Campus | Move from Rural Campus |
|---|---|---|---|
| $\delta_{-5}$ | 0.001 | 0.002 | 0.000 |
| | (0.001) | (0.002) | (0.002) |
| $\delta_{-4}$ | 0.001 | 0.001 | 0.002 |
| | (0.001) | (0.002) | (0.002) |
| $\delta_{-3}$ | 0.004*** | 0.002 | 0.001 |
| | (0.001) | (0.002) | (0.002) |
| $\delta_{-2}$ | 0.000 | -0.000 | -0.001 |
| | (0.002) | (0.003) | (0.003) |
| $\delta_{0}$ | -0.005*** | -0.012*** | -0.012*** |
| | (0.001) | (0.002) | (0.001) |
| $\delta_{1}$ | 0.001 | -0.000 | 0.005 |
| | (0.001) | (0.002) | (0.004) |
| $\delta_{2}$ | 0.002 | 0.010*** | 0.006*** |
| | (0.001) | (0.003) | (0.002) |
| $\delta_{3}$ | -0.001 | 0.012*** | 0.006** |
| | (0.001) | (0.003) | (0.003) |
| $\delta_{4}$ | 0.000 | 0.016*** | 0.005 |
| | (0.002) | (0.003) | (0.003) |
| N | 225,646 | 225,646 | 225,646 |

Table B.10: Two-Way Fixed Effects Estimates for Origin School Context, Excluding 2020

*Notes:* Estimates are based on the sample excluding the 2020 school year. 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 |
|---|---|---|---|
| $\delta_{-5}$ | 0.004 | 0.005 | 0.004 |
| | (0.003) | (0.004) | (0.004) |
| $\delta_{-4}$ | 0.008** | 0.006* | 0.005 |
| | (0.003) | (0.003) | (0.004) |
| $\delta_{-3}$ | 0.010*** | 0.011** | 0.007 |
| | (0.004) | (0.004) | (0.004) |
| $\delta_{-2}$ | 0.001 | -0.003 | -0.005 |
| | (0.004) | (0.005) | (0.005) |
| $\delta_{0}$ | 0.002 | -0.025*** | 0.038*** |
| | (0.003) | (0.003) | (0.003) |
| $\delta_{1}$ | 0.007** | 0.005 | 0.005 |
| | (0.003) | (0.004) | (0.004) |
| $\delta_{2}$ | 0.014*** | 0.023*** | 0.029*** |
| | (0.004) | (0.005) | (0.005) |
| $\delta_{3}$ | 0.019*** | 0.021*** | 0.025*** |
| | (0.005) | (0.005) | (0.006) |
| $\delta_{4}$ | 0.027*** | 0.029*** | 0.036*** |
| | (0.006) | (0.007) | (0.007) |
| N | 225,646 | 225,646 | 225,646 |

Table B.11: Two-Way Fixed Effects Estimates for Geographic Scope of Teacher Mobility, Excluding 2020

*Notes:* Estimates are based on the sample excluding the 2020 school year. Teacher-level clustered standard errors in parentheses. Omitted period is $t = -1$. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

| | Move schools | Δ SES | Move to Low-SES Campus | Move to High-SES Campus | Move to Rural Campus |
|---|---|---|---|---|---|
| $\delta_{-3}$ | 0.007 | 0.087 | 0.002 | 0.001 | -0.003 |
| | (0.005) | (0.157) | (0.002) | (0.002) | (0.002) |
| $\delta_{-2}$ | -0.001 | -0.012 | 0.001 | 0.001 | -0.003 |
| | (0.005) | (0.151) | (0.002) | (0.001) | (0.002) |
| $\delta_0$ | -0.052*** | 0.099 | -0.006*** | -0.004*** | -0.012*** |
| | (0.004) | (0.131) | (0.002) | (0.001) | (0.002) |
| $\delta_1$ | -0.015*** | -0.263* | -0.003* | 0.003* | -0.006** |
| | (0.005) | (0.143) | (0.002) | (0.002) | (0.002) |
| $\delta_2$ | 0.031*** | -0.774*** | 0.005*** | 0.011*** | -0.000 |
| | (0.005) | (0.140) | (0.002) | (0.001) | (0.002) |
| $\delta_3$ | 0.034*** | -0.528** | 0.007*** | 0.010*** | 0.002 |
| | (0.007) | (0.226) | (0.003) | (0.002) | (0.003) |
| N | 220,818 | 220,818 | 220,818 | 220,818 | 220,818 |

Table B.12: Callaway-Sant'Anna Estimates for Teacher Mobility and Destination School Context, Balanced Sample

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

| | Move from Low-SES Campus | Move from High-SES Campus | Move from Rural Campus |
|---|---|---|---|
| $\delta_{-3}$ | 0.002 | -0.001 | -0.001 |
| | (0.001) | (0.002) | (0.002) |
| $\delta_{-2}$ | 0.000 | -0.001 | -0.000 |
| | (0.001) | (0.002) | (0.002) |
| $\delta_0$ | -0.007*** | -0.013*** | -0.011*** |
| | (0.001) | (0.002) | (0.002) |
| $\delta_1$ | -0.004*** | -0.006*** | 0.002 |
| | (0.001) | (0.002) | (0.002) |
| $\delta_2$ | 0.000 | 0.007*** | 0.006*** |
| | (0.001) | (0.002) | (0.002) |
| $\delta_3$ | -0.003 | 0.008** | 0.006* |
| | (0.002) | (0.003) | (0.003) |
| N | 220,818 | 220,818 | 220,818 |

Table B.13: Callaway-Sant'Anna Estimates for Origin School Context, Balanced Sample

*Notes:* Estimates are based on the balanced sample. 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 |
|---|---|---|---|
| $\delta_{-3}$ | 0.001 | 0.002 | 0.002 |
| | (0.004) | (0.004) | (0.005) |
| $\delta_{-2}$ | -0.006* | -0.007* | -0.006 |
| | (0.004) | (0.004) | (0.004) |
| $\delta_0$ | -0.006* | -0.027*** | -0.033*** |
| | (0.004) | (0.004) | (0.004) |
| $\delta_1$ | 0.001 | -0.008* | -0.009** |
| | (0.004) | (0.004) | (0.004) |
| $\delta_2$ | 0.008** | 0.017*** | 0.026*** |
| | (0.004) | (0.004) | (0.004) |
| $\delta_3$ | 0.016*** | 0.022*** | 0.033*** |
| | (0.006) | (0.007) | (0.007) |
| N | 220,818 | 220,818 | 220,818 |

Table B.14: Callaway-Sant'Anna Estimates for Geographic Scope of Teacher Mobility, Balanced Sample

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

| | Move Campus | $\Delta$ SES | Move to Low-SES Campus | Move to High-SES Campus | Move to Rural Campus |
|---|---|---|---|---|---|
| $\delta_{-5}$ | 0.002 | 0.157 | 0.002 | -0.003 | -0.003 |
| | (0.006) | (0.196) | (0.002) | (0.003) | (0.003) |
| $\delta_{-4}$ | -0.007 | 0.053 | -0.001 | -0.002 | -0.002 |
| | (0.005) | (0.184) | (0.002) | (0.002) | (0.002) |
| $\delta_{-3}$ | 0.004 | -0.130 | -0.001 | -0.001 | -0.002 |
| | (0.005) | (0.173) | (0.002) | (0.002) | (0.002) |
| $\delta_{-2}$ | 0.004 | -0.172 | -0.001 | -0.001 | -0.001 |
| | (0.005) | (0.171) | (0.002) | (0.002) | (0.002) |
| $\delta_0$ | -0.051*** | 0.080 | -0.008*** | -0.009*** | -0.013*** |
| | (0.004) | (0.127) | (0.001) | (0.002) | (0.002) |
| $\delta_1$ | 0.023*** | -0.925*** | 0.001 | 0.010*** | -0.001 |
| | (0.006) | (0.198) | (0.002) | (0.002) | (0.003) |
| $\delta_2$ | 0.032*** | -1.494*** | 0.004 | 0.014*** | -0.001 |
| | (0.009) | (0.272) | (0.003) | (0.003) | (0.004) |
| $\delta_3$ | 0.057*** | -1.079** | 0.014** | 0.017*** | 0.006 |
| | (0.015) | (0.441) | (0.006) | (0.005) | (0.007) |
| $\delta_4$ | 0.060*** | -0.901** | 0.011** | 0.014*** | 0.028*** |
| | (0.017) | (0.435) | (0.005) | (0.005) | (0.009) |
| $\delta_5$ | 0.070*** | -2.120*** | 0.008 | 0.018*** | 0.006 |
| | (0.020) | (0.555) | (0.007) | (0.006) | (0.010) |
| N | 180,382 | 180,382 | 180,382 | 180,382 | 180,382 |

Table B.15: Callaway-Sant'Anna Estimates for Teacher Mobility and Destination School Context, Matched Sample

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

| | Move from Low-SES Campus | Move from High-SES Campus | Move from Rural Campus |
|---|---|---|---|
| $\delta_{-5}$ | -0.004* | -0.002 | 0.000 |
| | (0.003) | (0.002) | (0.003) |
| $\delta_{-4}$ | -0.002 | -0.000 | -0.001 |
| | (0.002) | (0.002) | (0.002) |
| $\delta_{-3}$ | -0.000 | -0.001 | 0.002 |
| | (0.002) | (0.002) | (0.002) |
| $\delta_{-2}$ | 0.003 | 0.001 | 0.003 |
| | (0.002) | (0.002) | (0.002) |
| $\delta_0$ | -0.009*** | -0.006*** | -0.012*** |
| | (0.002) | (0.001) | (0.002) |
| $\delta_1$ | 0.005* | -0.000 | 0.003 |
| | (0.003) | (0.002) | (0.003) |
| $\delta_2$ | 0.016*** | -0.004* | 0.005 |
| | (0.004) | (0.002) | (0.004) |
| $\delta_3$ | 0.027*** | 0.002 | 0.006 |
| | (0.007) | (0.003) | (0.006) |
| $\delta_4$ | 0.022*** | -0.001 | 0.011 |
| | (0.007) | (0.002) | (0.007) |
| $\delta_5$ | 0.033*** | -0.004 | 0.021*** |
| | (0.009) | (0.003) | (0.008) |
| N | 180,382 | 180,382 | 180,382 |

Table B.16: Callaway-Sant'Anna Estimates for Origin School Context, Matched Sample

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

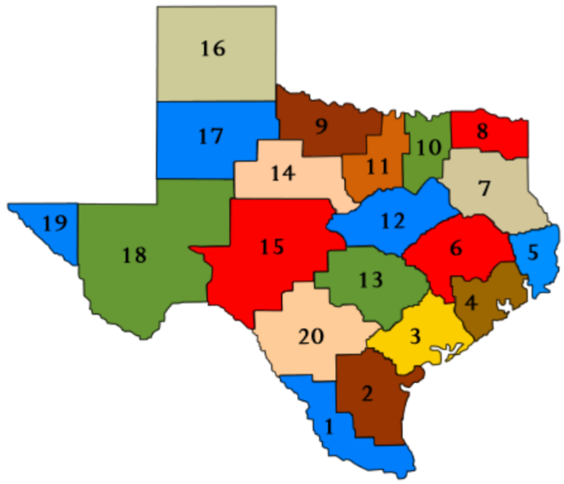
| | Move within District | Move within County | Move within TEA Region |
|---|---|---|---|
| $\delta_{-5}$ | 0.010** | 0.008* | 0.006 |
| | (0.004) | (0.005) | (0.005) |
| $\delta_{-4}$ | 0.006 | 0.001 | -0.002 |
| | (0.004) | (0.005) | (0.005) |
| $\delta_{-3}$ | 0.010*** | 0.006 | 0.003 |
| | (0.004) | (0.004) | (0.005) |
| $\delta_{-2}$ | 0.009*** | 0.011** | 0.007 |
| | (0.004) | (0.004) | (0.005) |
| $\delta_0$ | 0.006* | -0.021*** | -0.032*** |
| | (0.003) | (0.004) | (0.004) |
| $\delta_1$ | 0.026*** | 0.021*** | 0.025*** |
| | (0.005) | (0.005) | (0.006) |
| $\delta_2$ | 0.022*** | 0.025*** | 0.030*** |
| | (0.007) | (0.008) | (0.008) |
| $\delta_3$ | 0.018 | 0.035*** | 0.044*** |
| | (0.012) | (0.013) | (0.014) |
| $\delta_4$ | 0.034** | 0.048*** | 0.060*** |
| | (0.015) | (0.016) | (0.016) |
| $\delta_5$ | 0.018 | 0.051*** | 0.065*** |
| | (0.016) | (0.018) | (0.019) |
| N | 180,382 | 180,382 | 180,382 |

Table B.17: Callaway-Sant'Anna Estimates for Geographic Scope of Teacher Mobility, Matched Sample

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

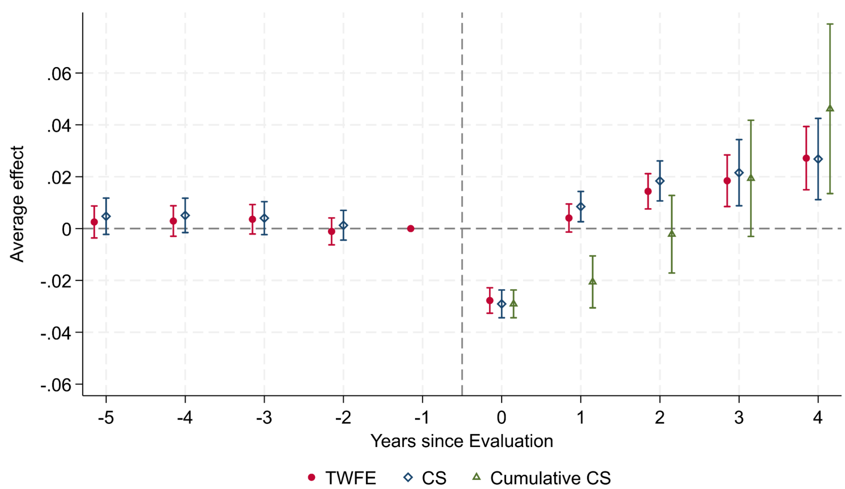
# Appendix C Figures

Figure C.1: TEA Region Map



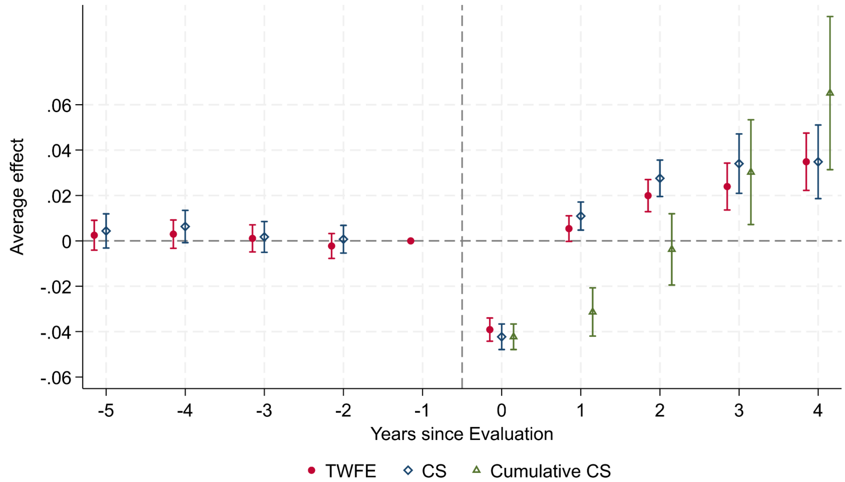

Source: `https://tea.texas.gov/about-tea/other-services/education-service-centers/education-service-centers-map`

Figure C.2: Estimated Effects on Moving Within County



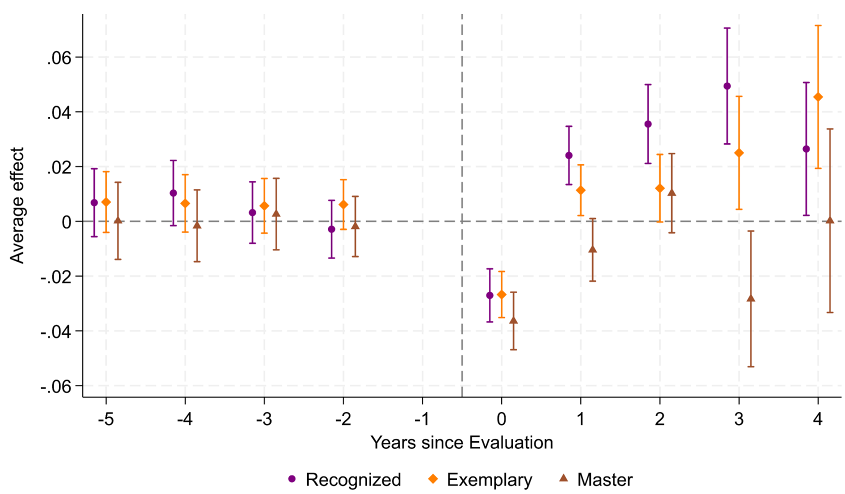

*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. Error bars represent 95% confidence intervals. Red circle points represent two-way fixed effects estimates. Blue diamond points represent Callaway & Sant'Anna (2021) estimates. Green triangle points 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 is an indicator for being observed at a campus in county $j$ in $t+1$ and a different campus in county $j$ in $t$.

Figure C.3: Estimated Effects on Moving Within TEA Region



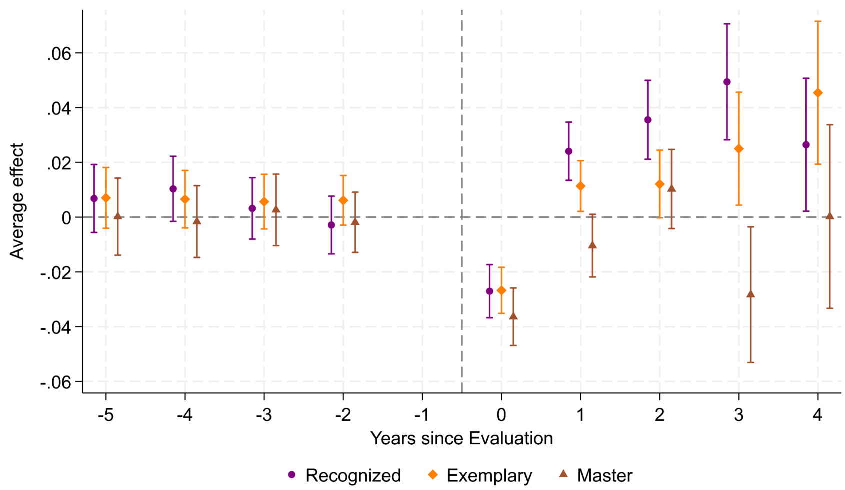

*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. Error bars represent 95% confidence intervals. Red circle points represent two-way fixed effects estimates. Blue diamond points represent Callaway & Sant'Anna (2021) estimates. Green triangle points 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 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 on Moving Within District By Designation Level



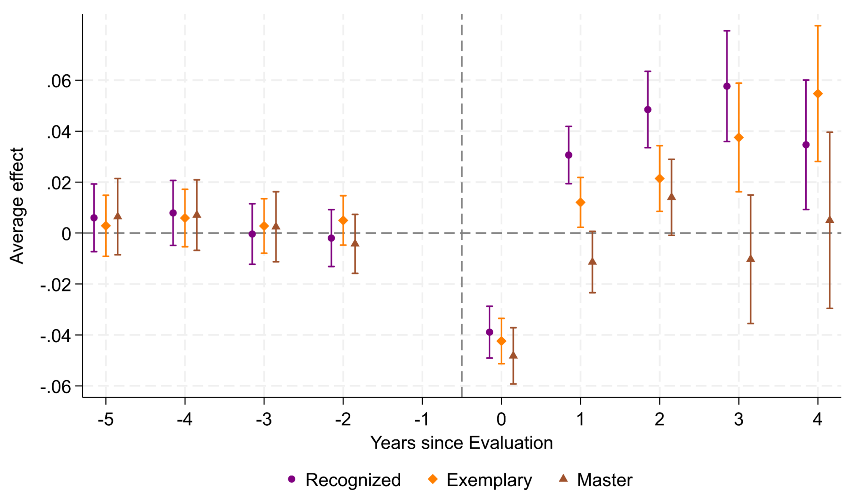

*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. All results are estimated using Callaway & Sant'Anna (2021). Error bars represent 95% confidence intervals. Purple circle points represent Recognized level teachers. Orange diamond points represent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

Figure C.5: Estimated Effects on Moving Within County By Designation Level




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

Figure C.6: Estimated Effects on Moving Within TEA Region By Designation Level




*Notes:* Time period $t = 0$ represents the school year in which a teacher is being evaluated for designation. All results are estimated using Callaway & Sant'Anna (2021). Error bars represent 95% confidence intervals. Purple circle points represent Recognized level teachers. Orange diamond points represent Exemplary level teachers. Brown triangle points represent Master level teachers. The outcome 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 $t$.