Annenberg Institute
BROWN UNIVERSITY

EdWorkingPaper No. 26-1643

Performance Pay and Teacher Mobility: Statewide Evidence from Texas

Zachary Gooch
Florida State University

Texas's Teacher Incentive Allotment (TIA) provides performance-based funding to school districts, with larger awards at rural campuses and campuses serving more disadvantaged students. I study whether district adoption keeps teachers in their districts and at their schools. Using statewide administrative teacher records covering 2014--15 through 2025--26 and a staggered difference-in-differences design, I estimate an average increase in district retention of 0.95 percentage points starting in the year districts begin evaluating teachers under the program, equivalent to a 5.7 percent reduction in baseline district attrition. The estimated effect reaches 1.38 points once state funding begins. The gain comes from fewer teachers moving to other districts and fewer exiting the Texas public school system. Campus retention does not detectably change on average. At rural campuses, however, more teachers stay at the same school. At the most disadvantaged non-rural campuses, district retention and within-district transfers both rise, with no measurable change in campus retention. Adopting districts also hire fewer teachers from other districts, and I find little evidence that exposure to adopting districts lowers retention in comparison districts. Rural campuses are the only group with a detectable gain in campus retention, indicating that gains in district retention need not translate into greater staffing continuity at individual schools.

VERSION: November 2026

Suggested citation: Gooch, Zachary . (2026). Performance Pay and Teacher Mobility: Statewide Evidence from Texas. (EdWorkingPaper: 26-1643). Retrieved from Annenberg Institute at Brown University: https://doi.org/10.26300/dkfq-6f80

Performance Pay and Teacher Mobility: Statewide Evidence from Texas

Zachary Gooch*

Florida State University

Abstract

Texas's Teacher Incentive Allotment (TIA) provides performance-based funding to school districts, with larger awards at rural campuses and campuses serving more disadvantaged students. I study whether district adoption keeps teachers in their districts and at their schools. Using statewide administrative teacher records covering 2014–15 through 2025–26 and a staggered difference-in-differences design, I estimate an average increase in district retention of 0.95 percentage points starting in the year districts begin evaluating teachers under the program, equivalent to a 5.7 percent reduction in baseline district attrition. The estimated effect reaches 1.38 points once state funding begins. The gain comes from fewer teachers moving to other districts and fewer exiting the Texas public school system. Campus retention does not detectably change on average. At rural campuses, however, more teachers stay at the same school. At the most disadvantaged non-rural campuses, district retention and within-district transfers both rise, with no measurable change in campus retention. Adopting districts also hire fewer teachers from other districts, and I find little evidence that exposure to adopting districts lowers retention in comparison districts. Rural campuses are the only group with a detectable gain in campus retention, indicating that gains in district retention need not translate into greater staffing continuity at individual schools.

JEL codes: I21, I22, J33, J45

Keywords: performance pay, teacher retention, teacher mobility, difference-in-differences, education finance

*I am grateful to Luke Rodgers, Carl Kitchens, Shawn Kantor, Colin Adams, and Michael Darisse for helpful comments. I thank the Texas Education Agency for providing access to administrative data. All views and any remaining errors are my own.

1 Introduction

Teacher compensation is the largest line item in public education, yet little of it is explicitly allocated by teacher effectiveness or by the difficulty of staffing a given school (Biasi, 2021; Hoxby and Leigh, 2004; Clotfelter et al., 2008). Disadvantaged schools face higher turnover, and losing teachers can disrupt instruction and student achievement (Lankford et al., 2002; Boyd et al., 2011; Ronfeldt et al., 2013). Additional compensation may help teachers stay, but where they stay matters. A district can retain a teacher who moves to another of its schools. For a policy that directs funding toward hard-to-staff campuses, a gain in district retention therefore leaves an important question open. Do the schools the policy targets retain more of their own teachers?

Texas's Teacher Incentive Allotment (TIA) provides a setting in which to examine that question. Established in 2019, TIA provides performance-based funding through the state's school-finance formula. Teachers who earn a designation generate annual allotments for their employing districts, with larger amounts at rural campuses and campuses serving more disadvantaged students. Districts design and implement the evaluation systems that lead to designation, while an awarded designation stays with a teacher who changes schools or districts. These features give teachers reasons both to stay and to move. They also allow implementation to affect teachers who have yet to earn an award. By 2025–26, annual TIA awards exceeded $750 million, reaching more than 65,000 teachers across 809 participating school systems (Office of the Governor of Texas, 2026). I study how district implementation changes departures from Texas public schools, movement between employers, and retention at individual campuses.

I link statewide administrative teacher records across school years to distinguish staying at the same campus, moving within a district, moving to another district, and leaving Texas public schools. The analysis follows all instructional, non-charter campuses in adopting districts, including teachers who never receive a designation. Treatment begins in Data Capture, when districts start evaluating teachers under their local systems. I focus on districts that reach approval one year later, giving the treated campuses a common implementation sequence. Using the estimator of Sun and Abraham (2021), I compare their outcomes with those of campuses in never-participating districts. The design estimates the combined effect of evaluation, designation, and compensation on the teaching workforce. Participation is voluntary, and the causal interpretation requires that

retention in adopting and comparison districts would otherwise have followed parallel trends.

District retention rises by 0.95 percentage points from Data Capture forward and by 1.38 points during the payment years. Fewer teachers move to another district, and fewer leave Texas public schools. The response begins during Data Capture, before initial designations are awarded. Proposed designees must remain employed by the designating district through February of the school year following Data Capture, giving them a direct incentive to stay across the first transition I measure. Retention gains also appear among early-career teachers, who rarely hold designations. The timing therefore leaves room for both the immediate employment requirement and the prospect of future compensation to influence teachers' decisions.

The distinction between district and campus retention becomes clear in the schools the formula targets. At rural campuses, more teachers stay at the same school. During the payment years, the estimated gain represents a roughly 12 percent reduction in campus attrition relative to the pre-program baseline. At the most disadvantaged non-rural campuses, district retention rises alongside movement between schools within the district, with no detectable gain in campus retention. The district keeps more of its workforce, but the share returning to the school where they taught does not measurably increase. These patterns show why district retention alone cannot establish whether targeted compensation improves staffing continuity at the schools generating the awards.

The gain in district retention is accompanied by fewer hires from other districts. A model of annual teacher flows between district pairs likewise shows that both departures from and arrivals to adopting districts decline. This pattern is consistent with fewer departures creating fewer openings for outside hires, although I do not observe vacancies. I also examine whether teachers leaving comparison districts become more likely to choose TIA destinations and whether greater exposure to adopting districts reduces retention in those comparison districts. I find little evidence of either response. These exercises address a specific concern about the district comparisons: retention gains could be overstated if implementation reduces retention in the districts used as controls. They leave open changes in the composition of movers and any effect on entry into teaching shared across the state.

The estimated district-retention gain is similar across alternative estimators and remains positive when I broaden the sample to districts with longer implementation paths. Reweighting comparison campuses on baseline characteristics or pre-adoption retention histories also yields similar estimates.

The evidence on parallel trends warrants more caution. A temporary dip in retention two years before Data Capture is followed by a rebound, and the confidence interval includes zero under modest departures from parallel trends in the sensitivity analysis of Rambachan and Roth (2023). The estimates provide evidence on the staffing response of voluntary adopters, with that qualification on their causal interpretation.

The evidence on teacher performance pay in the United States is relatively mixed. Several programs find little evidence that teacher bonuses improve student achievement (Springer et al., 2010; Fryer, 2013). Broader reforms to evaluation and compensation have improved teacher performance and changed the composition of the workforce (Dee and Wyckoff, 2015; Biasi, 2021). Dallas provides a close institutional comparison. Its Teacher Excellence Initiative (TEI) replaced the traditional salary schedule with pay based on measured effectiveness. Following TEI came the Accelerating Campus Excellence (ACE) program which awarded compensation to attract teachers to the district's lowest performing schools. Morgan et al. (2023) find that ACE improved teacher quality and student achievement, but when stipends were largely withdrawn, teachers left and achievement declined. These findings make the location and durability of staffing gains central to evaluating performance pay programs. TIA brings these questions to a statewide system of voluntary district adopters, with recurring funding tied to teacher designations.

Recent studies examine TIA itself. Arechaga (2025) studies achievement and educational inequality, documenting turnover and sorting responses alongside gains in student performance. Massey (2026) examines individual designations and teachers' choices across schools, while Kirksey et al. (2024) study retention and achievement in the initial pilot districts. Here, the treatment is district-wide implementation and the outcomes cover the entire teaching workforce. Combining district and campus retention with hiring and flows between districts reveals where the staffing gains accrue and whether they are accompanied by losses in comparison districts. This distinction matters even when implementation and individual awards both reduce mobility. A program can help a district keep its teachers while producing different outcomes for the schools within it.

Section 2 describes TIA, and Sections 3 and 4 present the data and empirical design. Section 5 reports the retention, funding, heterogeneity, and hiring results. Section 6 examines teacher flows and exposure among comparison districts, and Section 7 assesses robustness. Section 8 discusses mechanisms, magnitudes, and policy implications. Section 9 concludes.

2 The Teacher Incentive Allotment

2.1 Program and fiscal structure

Texas established the Teacher Incentive Allotment (TIA) through House Bill 3 in 2019. Unlike temporary grant-funded incentive programs, TIA is established in statute and financed through the state's Foundation School Program. Teachers can earn one of three performance designations, Recognized, Exemplary, or Master, each of which generates an annual allotment for the employing district. The amount of the allotment depends on the teacher's designation level and the characteristics of the campus where the teacher works. Although the state determines the allotment amount, districts retain discretion over how the funds are distributed among teachers (Texas Education Agency, 2023).

2.2 District participation and validation

District participation in TIA is voluntary. To participate, a district develops a Local Designation System (LDS) specifying which teaching assignments and campuses are eligible, how teachers will be evaluated, and the criteria for awarding designations. Each system must incorporate classroom observations and measures of student growth, although districts have discretion over the specific measures and their relative weights. Student-growth measures may include value-added models, pre/post assessments, portfolios, or Student Learning Objectives. The Texas Education Agency establishes statewide performance benchmarks corresponding to approximately the top 33, 20, and 5 percent of teachers for Recognized, Exemplary, and Master designations, respectively. These benchmarks guide local evaluation systems but do not impose district-level designation quotas. Once the TEA accepts a district's application, the district enters a Data Capture Year, during which it evaluates teachers under its proposed system and submits the resulting data and proposed designations for validation. Texas Tech University conducts the statistical validation, while TEA makes the final approval and designation decisions. Districts that do not receive approval may revise their systems and resubmit in a subsequent year.

2.3 Designation timing, employment, and portability

An important feature of TIA is that teachers must remain employed by their designating district in an eligible teaching role on the last Friday in February of the school year following Data Capture to receive an initial designation. This requirement creates a potential retention incentive before teachers receive any additional compensation. Once awarded, designations remain valid for five years and are portable across Texas public school districts. A designated teacher who changes districts retains the designation, and the receiving district may generate the corresponding allotment. Consequently, districts can receive TIA funding without operating an approved Local Designation System if they employ teachers designated elsewhere. Thus, district participation, individual designation, and receipt of TIA funding are distinct, and the incentives facing teachers may change as they progress through the designation process.

2.4 Allotment formula and local spending discretion

The annual allotment depends on the teacher's designation level, the socioeconomic composition and rural status of the employing campus. During the study period, base allotments were $3,000, $6,000, and $12,000 for Recognized, Exemplary, and Master teachers, respectively. These amounts increase with campus need, producing annual allotments ranging from $3,000 to $9,000 for Recognized, $6,000 to $18,000 for Exemplary, and $12,000 to $32,000 for Master teachers. Thus, a Master-designated teacher could generate up to $20,000 more annually at one campus than at another solely because of differences in campus characteristics (Texas Education Agency, 2023).

2.5 Program scale

TIA has expanded rapidly since its introduction. Annual funding grew from approximately $290 million for nearly 25,000 designated teachers in 2023–24 to $481 million in 2024–25 (Texas Education Agency, 2024a, 2025). By 2025–26, the program awarded more than $750 million in funding associated with over 65,000 designated teachers, with 809 school systems participating statewide. Cumulative TIA awards have now exceeded $1.8 billion (Office of the Governor of Texas, 2026).

3 Data

3.1 Sources and sample

I construct a longitudinal panel of Texas public school teachers using the Texas Education Agency's Public Education Information Management System (PEIMS) staff records for school years 2014–15 through 2025–26. The records contain approximately 4.68 million teacher-year assignments, with scrambled employee identifiers that allow teachers to be followed across campuses, districts, and years. Mobility outcomes require observing teachers in subsequent school years, so the analysis covers origin years 2014–15 through 2024–25.

I supplement these records with administrative data on the Teacher Incentive Allotment. Annual district participation files identify which phase of adoption a district is currently in: nonparticipating, application accepted, or local designation system approved. Teacher-level designation records identify Recognized, Exemplary, and Master teachers and the allotments they generate for their employing districts. Campus-level files provide additional information on enrollment, student demographics, socioeconomic composition, rural status, and the allotment amounts associated with each designation level. The data were obtained through Public Information Requests to the Texas Education Agency and publicly available Texas Academic Performance Reports.

I use the linked records to identify teacher transitions, including retention at the same campus or district, movements between campuses and districts, and exits from the Texas public school system. I then aggregate these transitions to the campus-year level. Next, I exclude from the analysis all charter districts, multi-campus teachers, and all districts previously operating performance-pay systems including the 26-district initial pilot cohort of the TIA program.[1] Among these 26 is Dallas ISD, whose Teacher Excellence Initiative program served as the blueprint for TIA. After the exclusions, the panel consists of 7,299 campuses across 1,006 districts for a total of 72,613 campus-years.

[1]The initial pilot cohort was selected in part due to pre-existing strategic compensation systems. These districts are therefore excluded from analysis because their prior exposure to performance-based pay makes their treatment difficult to compare with that of later adopters.

3.2 Outcomes

The primary outcomes are four annual mobility rates, calculated for each campus based on the employment status of its teachers in the following school year. *District retention* measures the share of teachers employed at campus c in year t who remain in the same district in year $t+1$. *Campus retention* measures the share who remain at the same campus. *Exit* measures the share no longer observed teaching in Texas public schools and *out-of-district mobility* measures the share who move to another public school district. These outcomes distinguish different margins of teacher turnover. A teacher who leaves a campus may transfer to another campus within the district, move to another district, or leave the observed Texas public school workforce. District retention therefore includes both teachers who remain at their original campus and those who transfer within the district, while departures from the district consist of cross-district moves and exits. This distinction matters for interpreting the effects of TIA. Greater district retention need not imply greater campus retention, and a reduction in district departures can reflect either fewer exits or fewer moves between districts. I estimate these outcomes separately to identify the margins underlying any observed retention response. Cross-district mobility also provides the basis for examining potential spillovers on untreated districts in Section 6.

3.3 Treatment definition and adoption durations

Treatment is assigned at the district level and dated to the first school year in which the district is recorded with Application Accepted (AA) status, corresponding to the Data Capture stage in the administrative panel. All instructional, non-charter campuses in an adopting district are included, regardless of whether they are individually eligible to award designations under the local system. This definition captures the district-wide implementation of TIA rather than the effect of designation eligibility or receipt at a particular campus. Districts differ in the time between their first AA status and final Local Designation System (LDS) approval. Using the annual participation records, I calculate this interval and classify districts with observed durations of one, two, and three or more school years as AA1, AA2, and AA3+, respectively. Longer durations may reflect unsuccessful validation, revisions to the local system, or other implementation delays. Because the status files do not identify individual validation attempts, these categories measure observed administrative

duration rather than the number of attempts required for approval.

The primary analysis focuses on AA1 districts, yielding 4,850 campuses across 709 districts and 50,459 campus-years before estimator-specific exclusions. The headline Sun–Abraham regression uses 50,450 campus-years. For these districts, application acceptance, system approval, and the first state reimbursement follow a common sequence, allowing treatment effects to be compared at consistent stages of implementation. Longer-duration districts may remain in different stages of implementation for several years, complicating comparisons at a common event time. I therefore exclude AA2, AA3+, and application-only districts from both the primary treated and comparison groups. The comparison group consists of campuses in districts that never participate in TIA during the observation period. Although the AA1 restriction provides a more consistent implementation timeline, it also conditions the analysis on successful approval within one observed year. The resulting estimates therefore pertain to the teaching workforces of these adopting districts rather than all districts that apply for TIA. Section 7 examines sensitivity to this restriction by expanding the treated population to include longer-duration adopters and application-only districts.

3.4 Baseline balance and the vulnerability index

Table 1 shows that treated and control campuses are similar on baseline retention and mobility but differ in the populations they serve. AA1 treated campuses enroll a higher share of economically disadvantaged students, a higher share of Hispanic students, and are less likely to be rural. This pattern is consistent with the program's design, which directs the largest allotments to campuses serving disadvantaged populations. These differences are relevant because TIA's funding formula directs larger allotments to disadvantaged and rural campuses, and they motivate an examination of heterogeneous effects across these settings.

To summarize the campus characteristics targeted by the funding formula, I construct a predetermined vulnerability index using the recognized-designation allotment for each campus in 2019. Under the allotment schedule in effect during the study period, the recognized allotment equals \$3,000 plus \$1,500 times the campus average student point value, which incorporates socioeconomic disadvantage and a rural adjustment. I recover that value as

$$v_c = \frac{\text{Recognized Allotment}_{c,2019} - 3000}{1500}.$$

The resulting index is continuous, ranges from zero to four, and summarizes the campus's priority under the statutory funding formula. It is fixed at its pre-treatment value and does not depend on realized designations or allotment payments. The main heterogeneity analysis groups campuses into thirds of this formula-based index and then examines rural status and student economic disadvantage separately. Appendix B provides the corresponding targeting and comparison-group checks.

4 Empirical Strategy

4.1 Economic treatment sequence

The institutional process creates economically relevant exposure during Data Capture, following application acceptance. Teachers must be evaluated during this year to be considered for an initial designation. The district implements its system and administers the Teacher Buy-In Survey, so teachers learn about the evaluation and designation criteria. A proposed designee must also remain employed in an eligible teaching role through the last Friday in February of the following school year. Figure A4 maps this sequence. For AA1 districts, approval is exactly one coded school year after Application Accepted. I therefore set treatment onset to the observed Application Accepted/Data Capture year.

I treat TIA as a multistage intervention with a single treatment onset. The headline average treatment effect on the treated averages exposure from Data Capture forward. I also report an average for event years $k = 2$ through 4, which I call the payment phase. Districts must spend the first allotment by August 31 at the end of the approval year ($k = 1$), and the state reimburses that allotment in the September settle-up at the start of $k = 2$. This phase therefore dates state funding rather than the first payment to an individual teacher. The administrative staff records do not identify individual TIA payment dates. The phase estimates describe the implementation sequence without separately identifying the contribution of compensation, evaluation, information, or designation.

Event time is defined relative to the observed Application Accepted/Data Capture year. Thus, $k = 0$ is Data Capture, $k = 1$ is approval/designation, and $k = 2$ begins the payment phase defined above. The omitted $k = -1$ year is the local design/application year. Treating it as the reference requires that internal planning did not change campus-wide mobility before broad teacher

notification, evaluation, and eligibility began. Section 7 checks this choice by re-basing the event study to earlier pre-period years.

4.2 Staggered difference-in-differences estimators

Districts adopt TIA in different school years, creating a staggered difference-in-differences setting. It is now well understood that the conventional two-way fixed-effects estimator can be biased in this setting when treatment effects are heterogeneous across cohorts or over time. This is because already-treated units serve as controls for later-treated units and can receive negative weights (Sun and Abraham, 2021; Callaway and Sant'Anna, 2021). I therefore use estimators that accommodate heterogeneous treatment effects under staggered adoption.

My primary specification uses the interaction-weighted event-study estimator of Sun and Abraham (2021) (S&A). The estimator identifies cohort-specific effects by interacting adoption cohorts with relative-time indicators and then aggregates these effects across cohorts. For campus i in district d and school year t, I estimate

$$Y_{idt} = \alpha_i + \lambda_t + \sum_{e} \sum_{\ell \neq -1} \beta_{e,\ell} \, \mathbf{1}\{E_d = e\} \cdot \mathbf{1}\{t - E_d = \ell\} + X_i' \gamma_t + \varepsilon_{idt}, \tag{1}$$

where Y_{idt} is the campus-level mobility outcome, α_i and λ_t are campus and school-year fixed effects, and E_d denotes the district's first data capture year. $\beta_{e,\ell}$ capture cohort-specific effects at relative time $\ell = t - E_d$, with $\ell = -1$ serving as the omitted reference period. The vector X_i contains predetermined campus characteristics measured during 2015–2019, interacted with school-year indicators. Using predetermined rather than contemporaneous characteristics avoids conditioning on variables that may respond to treatment.

The primary specification uses instructional, non-charter campuses in districts that never enter TIA's local designation system as the comparison group. AA-only districts and longer-duration adopters (AA2 and AA3+) are excluded from the baseline estimation sample rather than used as controls. Standard errors are clustered at the district level, where treatment is assigned. Section 7 examines the sensitivity of the results to alternative estimators, comparison groups, and specifications.

4.3 Identifying assumptions

Identification relies on conditional parallel trends: absent TIA implementation, treated and comparison campuses would have experienced similar changes in teacher mobility, conditional on the included fixed effects and predetermined characteristics. It also requires that treatment does not affect mobility before the designated onset at $k = 0$. Because the omitted reference period ($k = -1$) coincides with district planning and application, anticipation is a potential concern if teachers respond before Data Capture begins.

I assess these assumptions using pre-treatment event-study estimates and additional sensitivity analyses. Although pre-treatment coefficients are generally stable, the event study exhibits a temporary decline in district retention at $k = -2$, accompanied by an increase in exits. This deviation raises concerns about the choice of reference period and potential departures from parallel trends. Section 7 examines these concerns through alternative reference periods, trend specifications, and formal sensitivity analyses.

A second concern is interference across districts. Changes in recruitment and mobility at participating districts may affect outcomes at nonparticipating districts, potentially violating the assumption that comparison campuses are unaffected by treatment elsewhere. Section 6 investigates this possibility using cross-district mobility flows and retention outcomes in nonparticipating districts exposed to nearby adopters.

Finally, the analysis is conducted at the campus level to maintain an absorbing treatment assignment. The staggered-adoption estimators used here assume that once a unit enters treatment, it remains treated. While campuses in adopting districts follow a common implementation path, individual teachers may move between treated and untreated districts, changing their exposure to TIA. Assigning treatment based on a teacher's current district would therefore violate the absorbing-treatment assumption. The campus-level design avoids this issue while allowing teacher mobility itself to serve as the outcome.

5 Results

5.1 Headline effects over the implementation sequence

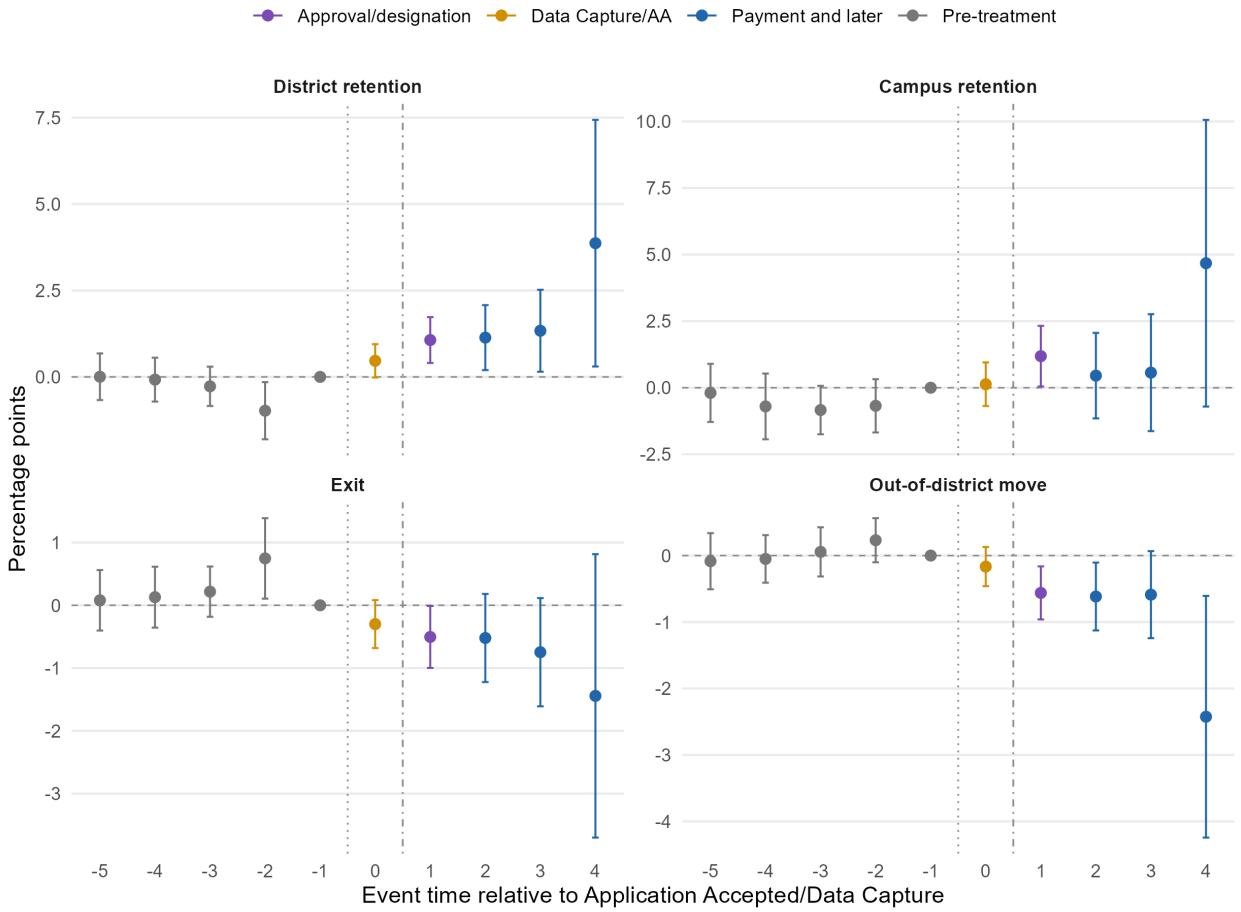
After a district begins evaluating teachers under TIA, more of its teachers stay in the district the following year. District retention rises by 0.95 percentage points on average (Table 2), a 5.7 percent reduction in attrition relative to the 2015–2018 baseline. Two changes account for the gain in nearly equal measure: fewer teachers leave Texas public schools and fewer move to another district. Whether teachers also stay at the same campus is less clear. The estimated increase in campus retention is 0.66 percentage points, but is statistically indistinguishable from zero. A teacher can stay in the district and still change schools. This raises the question of whether more moves between campuses explain the discrepancy between district and campus retention gains. I estimate that the share of teachers moving to another campus within the same district increases by 0.30 percentage points. However, this estimate is too imprecise to establish that these moves became more common.

Districts began evaluating teachers two coded school years before the first state reimbursement, so the timing of the retention response is of particular interest. Figure 1 shows a small estimated gain in district retention in the evaluation year. By the time the state approves the district's system the following year, the gain is about one percentage point. It then increases to around 1.4 percentage points during the payment years, again divided roughly evenly between fewer exits and fewer moves to other districts. Only the earliest adoption cohort contributes to the final year shown in the figure, and its estimate has a wide confidence interval accordingly. The timing suggests that teachers begin responding during Data Capture, although the event study cannot isolate how much of the later gain is due to compensation itself. The estimated gain in district retention is similar across several estimators for staggered adoption as Section 7 shows.

5.2 The first stage: designation and generated funding

Texas awarded more than $750 million through TIA in 2025–2026, roughly $11,500 per teacher who generated an allotment. At campuses in adopting districts, designations and the funding they generate begin to rise in the approval year, before the first state reimbursement (Figure 2). During the payment years, implementation raises the share of teachers holding a designation by about 23

Figure 1: Teacher retention and mobility over implementation

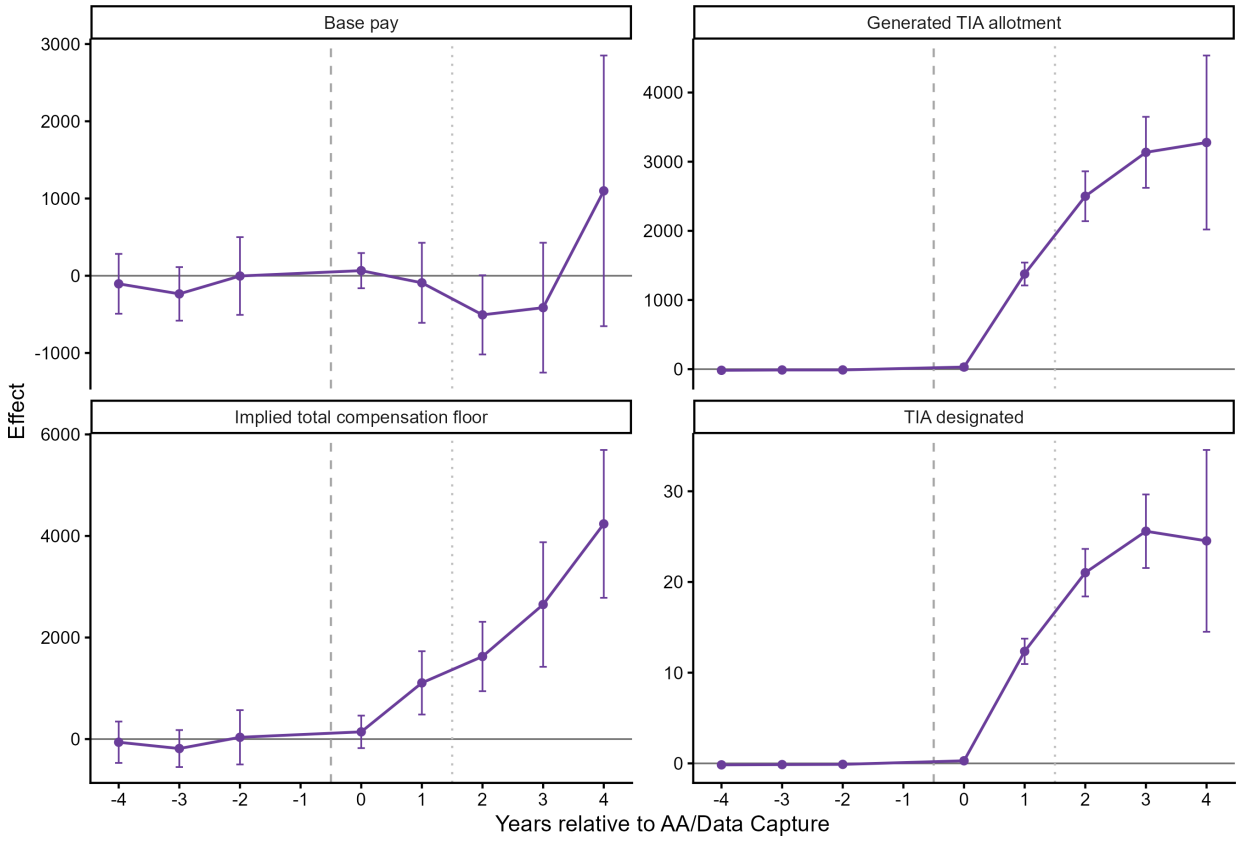


Notes: Sun–Abraham estimates relative to $k = -1$. Data Capture begins at $k = 0$, approval is $k = 1$, and the payment phase is $k \geq 2$. Bars show district-clustered 95 percent confidence intervals.

percentage points. The additional allotment generated for the district amounts to $2,760 per teacher when averaged across the campus's full teaching staff (Table 3).

How districts plan to distribute these funds varies. In their applications, 82 of the 380 districts in the analysis set aside part of each allotment for non-designated teachers at the campus. Among those districts, the share set aside averages 16 percent. The remaining districts specify no such set-aside. The $2,760 per teacher therefore measures the scale of the allotment across the campus workforce. To estimate what it implies for compensation, I add the statutory minimum of 90 percent of each allotment to reported campus payroll. During the payment years, this compensation floor rises by $2,133 per teacher.

Figure 2: Designation, generated funding, and compensation



Notes: Sun–Abraham estimates relative to $k = -1$. Dollar outcomes are campus averages over all teachers. The compensation floor adds 90 percent of generated allotments to payroll. Bars show district-clustered 95 percent confidence intervals.

5.3 Retention at the campuses TIA targets

TIA gives districts funding to "retain their best teachers" and encourages teachers to "work in high-needs schools."[2] The per-teacher formula reinforces the latter of those goals. A teacher with the same designation generates a larger allotment at a rural campus or one serving more economically disadvantaged students. Therefore, it's of interest whether more teachers stay at the schools where the formula is most generous. I group treated campuses into thirds by the allotment they would receive for employing a designated teacher in 2019–2020, before any district in the analysis began treatment. A Recognized teacher would have generated about $3,640 per year at the average campus in the lowest third, $5,340 in the middle third, and $7,070 in the highest. Within each third, I compare campuses in adopting districts with campuses in never-participating districts, so all three

[2]See Texas Education Agency (2024b).

estimates use a common benchmark. [3]

During the payment years, I find little evidence that TIA changes retention or mobility at the least vulnerable campuses (Table 4). All five estimates in the lowest third are smaller than half a percentage point in absolute value, and none is statistically distinguishable from zero at the ten percent level. The estimated gain in district retention rises to 1.15 percentage points in the middle third and 1.96 points in the highest. This ordering holds with either a common pool of never-participating campuses or controls drawn from the same formula tercile, although it does not hold over the full implementation period.

At campuses in the highest third, the district-retention gain is accompanied by a shift in where teachers move. During the payment years, moves to other districts fall by 1.18 percentage points and exits from Texas public schools fall by an estimated 0.78 points, although this estimate is imprecise. Together, these declines nearly match the 2.14-point increase in moves to another campus within the district. I find no detectable change in campus retention. One interpretation is that TIA changes the destination of some teachers who would have left their campus anyway. A teacher who might otherwise have left the district instead takes a position at another of its schools.

Which schools receive the additional within-district transfers? During the payment years, moves between campuses serving similar shares of economically disadvantaged students account for 1.04 of the 2.14 additional transfers per 100 teachers. Transfers to campuses where the disadvantaged share is at least five percentage points lower rise by 0.88 per 100 teachers, compared with 0.06 for transfers to campuses where it is at least five points higher.[4] The five-point comparison suggests a tilt toward less disadvantaged destinations, though the difference is imprecise (0.82 moves per 100 teachers, SE 0.45). For teachers starting at schools that already serve high shares of disadvantaged students, that pattern may partly reflect which other schools in the district have openings.

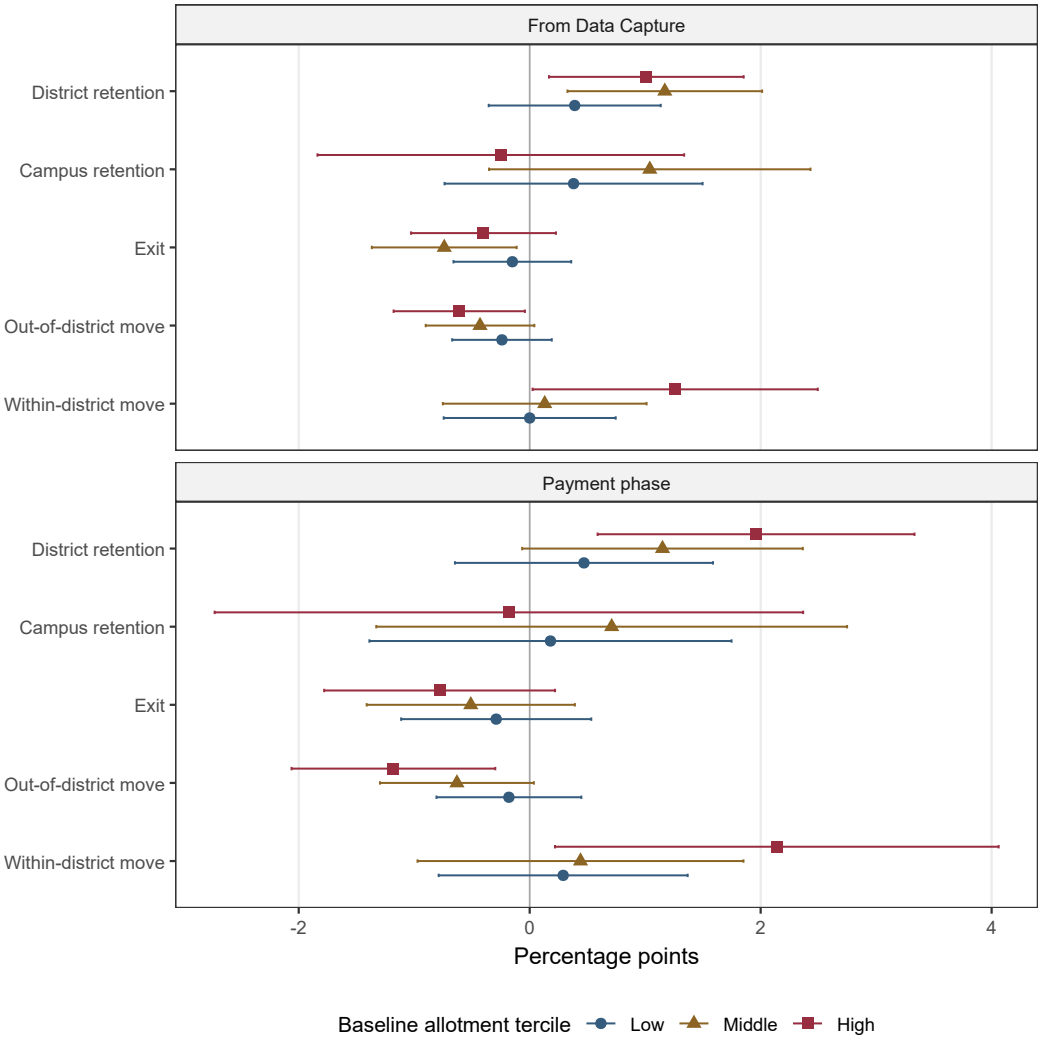
At rural campuses, the retention gain extends to the school itself. Rural campuses make up about 45 percent of the highest formula tercile, and separating them from non-rural campuses reveals a distinction that the formula ranking alone obscures (Appendix Table A2). Among non-rural campuses in the most disadvantaged third, district retention rises by 1.47 percentage points from

[3]The results are robust to using a control group composed of similarly disadvantaged non-adopters.

[4]I use each campus's average economically disadvantaged share in 2015–2019 where available and its first observed share otherwise. This classifies 98.3 percent of payment-year transfers from treated campuses in the highest tercile. With a ten-point band, transfers between similarly disadvantaged campuses account for 1.54 additional moves per 100 teachers, and the difference between moves toward less and more disadvantaged campuses is 0.68 (SE 0.31).

Data Capture forward, with no detectable gain in campus retention. At rural campuses, retention at the same school rises by 1.91 percentage points from Data Capture forward and by 2.94 points during the payment years. Before TIA, annual campus attrition in adopting districts averaged 24.4 percent at rural campuses, compared with 20.4 percent at non-rural campuses. Relative to that rural baseline, the payment-year gain represents a 12 percent reduction in campus attrition.

Figure 3: Retention and mobility by baseline allotment value



Notes: Estimates from Table 4. All terciles use the common pool of never-participating controls. Bars show district-clustered 95 percent confidence intervals.

5.4 Hiring margins

The retention results describe who stays. Table 5 describes who arrives. I use teachers' prior employment records to identify four sources of campus arrivals: teachers new to Texas public schools, teachers returning after an absence, and teachers moving either within or across district lines. During the payment years, arrivals fall by 1.61 per 100 teachers, about 7 percent of the 2019 treated mean. The decline is smaller and less precisely estimated when averaged from Data Capture forward. Fewer hires from other districts account for roughly 70 percent of the payment-year decline. I find no detectable change in arrivals from the other three sources. For every arrival rate in Panel A, joint tests of the pre-treatment leads from $k = -5$ through -2 do not reject. The decline in outside hiring is consistent with adopting districts retaining more teachers and having fewer openings to fill, although I do not observe vacancies directly. Section 6 examines these flows between district pairs.

Does the slowdown in hiring change the experience mix of new arrivals? During the payment years, first-year hires fall by 0.74 per 100 teachers and hires with five or more years of experience fall by 0.80 per 100 teachers. The two declines are similar in magnitude and are statistically significant at the ten and five percent levels, respectively. Neither group's share of arrivals changes detectably. I also find no detectable change in mean experience, either among all arrivals or among hires from other districts, where the decline in hiring is concentrated. Adopting campuses hire fewer teachers at both ends of the experience range, with little evidence that they shift hiring from novices toward experienced recruits.

TIA adoption keeps more teachers in their district, while the gain in continuity at individual schools depends on the setting.[5] At rural campuses, more teachers stay at the same school. At the most disadvantaged non-rural campuses, more teachers stay in the district, with no detectable gain in campus retention and some evidence of more transfers within the district. Across adopting districts, fewer teachers leave for other districts, and fewer teachers arrive from them. These flows show how the staffing response connects adopting districts to surrounding employers. If adoption also changes retention in those other districts, it may change the benchmark against which the retention gain is measured. Section 6 examines both questions.

[5] Appendix A reports retention and mobility estimates by teacher experience.

6 Teacher Flows and Control-Group Spillovers

6.1 Staffing adjustments across district boundaries

The decline in cross-district departures and hires suggests that implementation changes movement between employers. To understand that adjustment, I examine flows between district pairs and retention in the districts serving as controls.

Suppose a teacher would have stayed in her district but moves to a neighboring district for the opportunity to earn a TIA designation. The adopting district records a new hire, and the district she leaves records a departure. If her former district serves as a control, its retention rate is now lower than it would have been in the absence of its neighbor's adoption. The estimated retention gain at adopting districts would then be too large. This concern applies both to districts that never adopt and to districts serving as controls before their own adoption.

The hiring results show that adopting districts bring in fewer teachers from other districts on average. That aggregate decline could still conceal increased hiring from particular districts, especially those that have historically supplied many of an adopter's hires. I therefore examine where teachers move and what happens to the districts they leave. First, I use annual flows between district pairs to ask whether teachers leaving a control district become more likely to choose a destination after it adopts TIA. I then examine retention in the control districts themselves: as more of their usual destinations adopt, do fewer teachers stay?

6.2 Teacher flows between districts

I follow teachers who change districts from one school year to the next. For each pair of districts, I count the teachers who work in origin district o in year t and in destination district d in year $t+1$. A move between the same two districts in the opposite direction is a separate flow. I first ask how these flows change when either the sending or the receiving district adopts TIA. I estimate

$$E[\text{Flow}_{odt}] = \exp\left(\alpha_{od} + \lambda_t + \beta_O \,\text{TIA}_{ot} + \beta_D \,\text{TIA}_{d,t+1}\right), \tag{2}$$

where α_{od} absorbs persistent differences across district pairs and λ_t absorbs shocks common to all districts in a given year. The timing follows the teacher: origin treatment is measured in the year

the teacher leaves, and destination treatment in the year the teacher arrives. Both sides of the pair are drawn from the AA1 and never-participating districts in the main analysis. I use Poisson pseudo-maximum likelihood to accommodate zero annual flows and cluster standard errors by origin and destination district (Santos Silva and Tenreyro, 2006; Beine et al., 2016).

Departures from adopting districts fall by 7.7 percent, and arrivals to adopting districts fall by 8.3 percent (Table 6, Panel A). The two estimates put the campus results in a common frame: after implementation, fewer teachers cross an adopting district's boundary in either direction. The overall decline in arrivals could, however, conceal a shift in where those arrivals come from.

I therefore turn to teachers leaving districts that serve as controls. Among teachers leaving a given district in a given year, do more go to destinations that have implemented TIA? I restrict the sample to never-participating origins and to AA1 origins before their own adoption. Origin-by-year fixed effects hold constant the total outflow from each origin in each year, while district-pair fixed effects account for the fact that some destinations have historically attracted more of that origin's teachers. I allow the full set of eligible destinations in this comparison.[6] Teachers leaving control districts become less likely to choose a destination once it implements TIA. These flows fall by 7.6 percent. Poaching would predict the opposite sign. The decline is consistent with adopting districts having fewer positions to fill, although I do not observe vacancies directly. The estimate remains negative when I require the origin to remain untreated through the teacher's arrival year and when I omit destination adoption cohorts one at a time.

Teachers who change districts often stay close to the district they leave. Among the 105,769 moves from districts serving as controls, 51 percent are to a district whose geographic center lies within 25 miles of the origin's.[7] Could the overall decline in flows to adopting districts mask increased hiring by nearby adopters? Allowing the response to vary with distance, I estimate that flows to adopting districts fall by 6.2 percent at 25 miles and by 5.6 percent at 100 miles. These estimates come from the same model, and the difference is not statistically distinguishable from zero. Even at short distances, I find little evidence that adopting districts attract a larger share of teachers leaving control districts. The flow estimates describe where teachers go once they leave. To assess

[6] For AA1 destinations, exposure begins in Data Capture. For other districts that obtain system approval, exposure begins in the approval year.

[7] I use Census boundary data to measure straight-line distance between the geographic centers of the sending and receiving districts.

whether adoption elsewhere affects the comparison group itself, I next ask whether teachers in more exposed control districts become less likely to stay.

6.3 Retention in the control group

A teacher who would have left her district anyway can change destinations without affecting her district's retention rate. The concern for the main estimates is that TIA elsewhere induces a teacher to leave who otherwise would have stayed. I therefore ask whether teachers become less likely to stay in their district as more of their outside employment options are in districts that participate in TIA.

I measure exposure to TIA in two ways. The first uses the destinations of teachers who moved between districts before TIA. For each origin district, I calculate the share of its 2014–2018 cross-district moves that went to each destination and hold those shares fixed. Adoption by a district that historically hired many of the origin's teachers therefore increases its exposure more than adoption by a district that rarely hired from it. The second measure uses geography: among teachers working elsewhere in the same commuting zone, what share work in participating districts? I relate each measure to campus-level district retention using campus and year fixed effects. Districts contribute observations only while they serve as controls.

Neither measure indicates that retention falls as exposure rises (Panel B of Table 6). Across campuses serving as controls, a 10-percentage-point increase in exposure is associated with a 0.04 percentage point increase in district retention using historical moves and a 0.08 percentage point increase using commuting zones. Neither estimate is statistically distinguishable from zero.

Never-participating districts are particularly informative because their campuses supply the comparison group for the headline estimate. Over the comparisons underlying that estimate, exposure at these campuses rose by an average of 22 percentage points using historical moves and 21 points using commuting zones.[8] At those increases in exposure, neither point estimate implies a decline in control retention. At the lower endpoints of the respective 95 percent confidence intervals, however, the implied declines are 0.37 and 0.38 percentage points, each about two-fifths of the estimated 0.95-point retention gain.

Taken together, the flow and retention estimates give little indication that adopting districts

[8]For each treated cohort and post-treatment year, I calculate the change in exposure since the cohort's design/application year among never-participating campuses observed in both years. I average these changes using the treated campus-year weights.

retain more teachers at the expense of the comparison group. The precision of the retention estimates nevertheless leaves room for spillovers large enough to matter for the headline result.

7 Robustness

The headline estimate implies that district retention rises by 0.95 percentage points after implementation. Table 7 shows how that estimate changes with the estimator, the assumptions about untreated retention trends, the districts included in the analysis, and the weight given to each campus. The appendix reports the corresponding estimates for the other mobility outcomes.

Estimator, aggregation, and inference. Across the estimators in Table 7, the estimated gain in district retention ranges from 0.89 to 1.31 percentage points. The comparison includes two-way fixed effects, Sun and Abraham (2021), doubly robust Callaway and Sant'Anna (2021) with either never-treated or not-yet treated controls, Gardner et al. (2024), and Borusyak et al. (2024). Precision varies across specifications, but every 95 percent confidence interval includes the main estimate.

How often would the same design detect a retention effect in districts that never adopted TIA? Across 300 placebo assignments among never-participating districts, the estimated district-retention effect averages 0.01 percentage points. The estimates have a standard deviation of 0.52 points, close to their average district-clustered standard error of 0.50 points. Using each replication's own standard error, 4.7 percent reject at the five percent level. In 16 of the 300 assignments, the placebo estimate is at least as large in absolute value as the 0.95-point headline estimate.

Parallel trends. In the four years leading up to the design/application year ($k = -1$), did retention in adopting districts already follow a different path? For each of the four outcomes, a joint test of the lead coefficients from $k = -5$ through -2 does not reject at the five percent level ($p = 0.162$ for district retention, Table A6). One year stands out. At $k = -2$, district retention in adopting districts was 0.98 percentage points lower relative to comparison districts than in the reference year, and exit was 0.75 points higher. The retention dip is concentrated in the 2022–2024 adoption cohorts. Using the average of the three years before the dip as the reference raises the estimated district-retention gain from 0.95 to 1.22 percentage points.

Allowing each district its own linear trend, estimated from untreated observations, gives a

district-retention gain of 1.36 percentage points. The estimates remain positive, though less precise, under cohort-specific quadratic trends and when I combine district trends with commuting-zone-by-year fixed effects. I also use synthetic difference-in-differences (Arkhangelsky et al., 2021) to construct a comparison from campuses' pre-adoption retention histories. Because the estimator requires a balanced panel and a common adoption date, I restrict the sample to campuses observed in all eleven years and compare each adoption cohort separately with never-participating campuses. I average the cohort estimates in proportion to their treated campus-years. The estimated gain is 1.03 percentage points, compared with 0.83 points under Sun–Abraham on the same campuses (Table A11). The standard error is 0.30 points across 399 bootstrap draws that resample whole districts within adoption groups and re-estimate the weights.

Following Rambachan and Roth (2023), I then ask how much the retention gap could have drifted after adoption, even without TIA, before the 95 percent confidence interval includes zero. The gap ends the pre-period close to where it began. Its largest annual change is the 0.98-point rebound after the dip at $k = -2$. Using that rebound to put the bounds in percentage points, the interval excludes zero when the gap is allowed to drift by about 0.12 points per year ($\bar{M} = 0.125$) and first includes zero at about 0.15 points per year ($\bar{M} = 0.15$). For the payment years alone, the interval first includes zero at about 0.12 points per year.

Adopter sample and treatment timing. The main analysis follows districts that reached system approval one year after entering Data Capture, so that each event year corresponds to the same stage of implementation across districts. Campuses in districts that took longer to reach approval served larger shares of economically disadvantaged students before TIA (Table A4). Including slower adopters therefore changes both the mix of campuses and the number of years districts spend in Data Capture. Across the 460 approved districts that entered Data Capture in 2020 or later, the estimated gain in district retention is 0.80 percentage points, compared with 0.95 in the main sample. Including districts whose applications were accepted but had not reached approval by the end of the study gives an estimate of 0.86 points (Appendix Table A5). In a separate check, adding the pilot districts raises the estimate to 1.27 points. Those districts already operated performance-pay systems before TIA, which is why I exclude them from the main analysis.

To compare the timing of these retention gains with the achievement analysis of Arechaga (2025),

I also date treatment to system approval. Among one-year adopters, the estimated district-retention gain after approval is 0.74 percentage points relative to the Data Capture year and 1.17 points relative to the design/application year (Table 7). Both estimates cover the same post-approval years. The smaller estimate uses a baseline in which implementation was already underway.

The first two cohorts entered Data Capture during the pandemic. To assess whether those cohorts account for the headline result, I estimate the same specification separately for early and later adopters. The estimated district-retention gain is 0.86 percentage points for the 2020–2021 cohorts and 0.88 points for the 2022–2024 cohorts (Table A7). Across all five cohorts, omitting one cohort at a time leaves the pooled estimate between 0.84 and 1.04 points (Figure A3).

Sample construction and weights. Restricting the analysis to campuses observed in all eleven years gives a district-retention gain of 0.83 percentage points (Table A9). The main analysis also excludes teachers observed at several campuses the following year because I cannot identify a single destination campus. When all of those assignments are in the teacher's original district, district retention is still observable. Counting those teachers as retained gives an estimate of 0.96 points.

Weighting campuses by teacher headcount gives larger campuses more influence. The estimated district-retention gain falls from 0.95 percentage points to 0.70 with 2019 headcounts and 0.73 with current-year headcounts. Both estimates remain significant at the one percent level (Table A8). Rural campuses, where the estimated retention gains are larger, receive less weight: among treated campus-years with recorded rural status, they account for 20 percent of observations from adoption forward but 13 percent of teachers. The 2019 weights also exclude 197 campuses without a positive baseline headcount. Finally, I use propensity-score weighting to give more influence to never-participating campuses that resemble adopting campuses in pre-program enrollment and student composition. The estimated district-retention gain is 0.89 percentage points, close to the headline estimate of 0.95 (Table A10).

8 Discussion

8.1 Why might teachers stay?

TIA gives teachers a reason to stay in an adopting district before they receive an initial designation. To receive an initial local designation based on performance during Data Capture, a teacher must be employed in an eligible teaching role in the designating district on the last Friday in February of the following school year, the approval year (Section 2). That condition spans the transition measured by the Data Capture retention outcome: a teacher who leaves the district between those two school years gives up the pending designation. The estimated district-retention gain is 0.47 percentage points for that transition and 1.07 points for the transition out of the approval year, by which time the employment condition for that award has been met. Once awarded, a designation is portable. A teacher can then change districts without giving up the award, an option relevant to the mobility documented by Massey (2026).

The timing of the retention gain also differs by teacher experience (Appendix A). Among teachers with at least four years of experience, estimated retention gains are larger during the payment phase, when designations cover 20 to 23 percent of their teacher-years. This contrast is easiest to interpret for teachers with four to nine years of experience, since the two more experienced groups also exhibit differences in retention before adoption. For teachers with zero to three years of experience, by contrast, the estimated gain is already 1.13 percentage points over Data Capture and approval, close to the 1.04-point estimate during the payment years. Even then, these teachers hold designations in only 6.2 percent of teacher-years. Although the records identify awards rather than all proposed designees, the low designation rate makes it difficult to attribute the early-career response entirely to teachers staying to secure a pending award. The prospect of qualifying in a later year could also matter, as could changes in evaluation that reach teachers before they are considered for designation.

8.2 How large is the retention response?

The closest comparison comes from Texas itself. Using variation in district salary schedules, Hendricks (2014) estimates that a 1 percent increase in base pay reduces the share of teachers leaving their district by 0.16 percentage points. TIA generates an additional $2,760 per teacher in the payment years, averaged across the campus's full teaching staff. For a comparison based on

the funds available for compensation, I use the statutory minimum of 90 percent of that allotment, about $2,480 per teacher or 4.4 percent of average teacher pay at treated campuses in 2019. A uniform raise of that size would imply a decline in district turnover of about 0.70 percentage points using Hendricks's estimate. Using the $2,133 increase in the implied compensation floor, which also incorporates reported payroll, gives a benchmark of 0.60 points (Table 3). Both lie within the 95 percent confidence interval for TIA's estimated payment-phase gain of 1.38 points (SE 0.47).

A targeted bonus offers a second comparison. In North Carolina, Clotfelter et al. (2008) find that an annual $1,800 bonus reduced school turnover by 17 percent among eligible math, science, and special education teachers in high-poverty or low-performing secondary schools. At rural Texas campuses, the payment-phase estimate represents a roughly 12 percent reduction in campus attrition relative to the pre-program baseline. Both estimates measure departures from the school, although the Texas estimate covers the full teaching staff and the North Carolina estimate covers teachers eligible for the bonus.

8.3 Cost and incidence

During the payment years, TIA generates about $276,000 in additional annual funding per 100 teachers at a treated campus. On the same scale, an estimated 1.38 additional teachers remain in the district for the following school year. Dividing the two gives about $200,000 in annual allotment funding per additional teacher retained for one year. Both estimates give campuses equal weight. The ratio is sensitive to the estimated retention gain: holding funding fixed and using the endpoints of the conventional 95 percent confidence interval for retention gives roughly $120,000 to $600,000 per additional teacher retained. This range illustrates uncertainty in the denominator alone. The causal interpretation also depends on the parallel-trends assumption examined in Section 7. The calculation assigns the full allotment to retention, although the program also compensates teachers who would have stayed and may affect other outcomes, including student achievement (Arechaga, 2025).

Who receives the funding is a separate question. A designation generates an allotment for the teacher's district, and at least 90 percent must support teacher compensation at the campus. District applications describe how districts intend to divide that compensation between designated teachers and their colleagues. The designation records show how much funding each teacher generates, but

the staff files omit individual TIA payments. I can therefore trace the resources directed to a campus and describe the district's intended allocation without observing each teacher's realized pay increase. I find no statistically detectable change in reported base pay.

The distinction between where the money goes and where teachers stay matters for a program that directs larger allotments to rural and economically disadvantaged schools. At rural campuses, more teachers stay at the school that generated the allotment. At the most disadvantaged non-rural campuses, the gain appears in district retention alongside an increase in transfers between schools in the same district. In that setting, the district keeps more of its teaching workforce, even though the share returning to the school where they taught does not measurably rise.

8.4 What do the estimates generalize to?

The main estimates describe districts that chose to adopt TIA and reached system approval one year after entering Data Capture. Broadening the sample to include slower approved adopters, whose campuses serve more disadvantaged students on average, lowers the estimated district-retention gain from 0.95 to 0.80 percentage points. The result thus extends to a broader group of voluntary adopters. How districts that never applied would respond remains open. The estimates also offer limited guidance about a stand-alone bonus because Texas districts begin evaluating teachers before initial designations and compensation.

Finally, a district can retain more teachers without increasing the statewide supply of teachers. The records show who stays, who changes districts, and who arrives at an adopting campus. They offer less guidance about whether higher expected earnings induce someone to enroll in teacher preparation and enter the profession several years later. A statewide before-and-after comparison would face an additional difficulty: the same 2019 legislation that created TIA also raised the state minimum salary schedule. Separating those incentives would require variation beyond the district adoption timing studied here.

9 Conclusion

Texas's Teacher Incentive Allotment directs funding toward particular schools through systems implemented by districts. I estimate that implementation increases district retention by 0.95

percentage points from Data Capture forward and 1.38 points during the payment years. Fewer teachers leave for another district or exit Texas public schools. The response begins while districts are evaluating teachers for designation, before initial designations are awarded, suggesting that the implementation process and the prospect of future compensation both deserve attention alongside the payments themselves.

The staffing implications differ across schools. At rural campuses, more teachers stay at the same school. At the most disadvantaged non-rural campuses, more stay in the district while transfers between its schools increase, with no detectable gain in campus retention. Adopting districts also hire fewer teachers from elsewhere, with little evidence of a change in the experience composition of arrivals. Both departures and arrivals across district boundaries decline, and I find little evidence that exposure to adopting districts reduces retention in the comparison districts.

These estimates describe voluntary adopters that reached approval one year after entering Data Capture. The district-retention gain remains similar across estimators and broader adopter samples, although its statistical significance is sensitive to departures from parallel trends. The design identifies the combined response to evaluation, designation, and compensation. It leaves open how much each component contributes and whether the program expands the statewide supply of teachers.

TIA therefore illustrates why the location of retention matters. A district can keep more of its workforce through transfers among its schools, even when I find no detectable increase in the share staying at the schools generating the largest allotments. At rural campuses, the gains extend to continuity at the school itself. Evaluating a program intended to staff high-needs schools requires attention to both outcomes.

References

Arechaga, S. (2025). Performance pay and educational inequality: Evidence from Texas. Job Market Paper, University of California, Berkeley, November 27, 2025.

Arkhangelsky, D., Athey, S., Hirshberg, D. A., Imbens, G. W., and Wager, S. (2021). Synthetic difference-in-differences. *American Economic Review*, 111(12):4088–4118.

Beine, M., Bertoli, S., and Fernández-Huertas Moraga, J. (2016). A practitioner's guide to gravity models of migration. *The World Economy*, 39(4):496–516.

Biasi, B. (2021). The labor market for teachers under different pay schemes. *American Economic Journal: Economic Policy*, 13(2):39–75.

Borusyak, K., Jaravel, X., and Spiess, J. (2024). Revisiting event-study designs: Robust and efficient estimation. *The Review of Economic Studies*, 91(6):3253–3285.

Boyd, D., Grossman, P., Ing, M., Lankford, H., Loeb, S., and Wyckoff, J. (2011). The influence of school administrators on teacher retention decisions. *American Educational Research Journal*, 48(2):303–333.

Callaway, B. and Sant'Anna, P. H. C. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.

Clotfelter, C. T., Glennie, E., Ladd, H. F., and Vigdor, J. L. (2008). Would higher salaries keep teachers in high-poverty schools? Evidence from a policy intervention in North Carolina. *Journal of Public Economics*, 92(5–6):1352–1370.

Dee, T. S. and Wyckoff, J. (2015). Incentives, selection, and teacher performance: Evidence from IMPACT. *Journal of Policy Analysis and Management*, 34(2):267–297.

Fryer, R. G. (2013). Teacher incentives and student achievement: Evidence from New York City public schools. *Journal of Labor Economics*, 31(2):373–407.

Gardner, J., Thakral, N., Tô, L. T., and Yap, L. (2024). Two-stage differences in differences. Working Paper.

Hendricks, M. D. (2014). Does it pay to pay teachers more? Evidence from Texas. *Journal of Public Economics*, 109:50–63.

Hoxby, C. M. and Leigh, A. (2004). Pulled away or pushed out? explaining the decline of teacher aptitude in the United States. *American Economic Review*, 94(2):236–240.

Kirksey, J. J., Lansford, T., Crevar, A. R., and Mansell, K. E. (2024). From incentive to impact: The Texas Teacher Incentive Allotment's path to improved retention and achievement. Working Paper 02-001, Center for Innovative Research in Change, Leadership, and Education, Texas Tech University.

Lankford, H., Loeb, S., and Wyckoff, J. (2002). Teacher sorting and the plight of urban schools: A descriptive analysis. *Educational Evaluation and Policy Analysis*, 24(1):37–62.

Massey, P. L. (2026). Teacher sorting and preferences over school disadvantage: Evidence from performance pay in Texas. EdWorkingPaper 26-1512, Annenberg Institute at Brown University.

Morgan, A. J., Nguyen, M., Hanushek, E. A., Ost, B., and Rivkin, S. G. (2023). Attracting and retaining highly effective educators in hard-to-staff schools. NBER Working Paper 31051, National Bureau of Economic Research.

National Center for Education Statistics (2022). Percentage distribution of public school teachers, by total years of teaching experience: School year 2020–21. National Teacher and Principal Survey, Table 3.

Office of the Governor of Texas (2026). Governor Abbott announces $750 million in Teacher Incentive Allotment awards. Press release, June 11.

Rambachan, A. and Roth, J. (2023). A more credible approach to parallel trends. *Review of Economic Studies*, 90(5):2555–2591.

Ronfeldt, M., Loeb, S., and Wyckoff, J. (2013). How teacher turnover harms student achievement. *American Educational Research Journal*, 50(1):4–36.

Santos Silva, J. and Tenreyro, S. (2006). The log of gravity. *Review of Economics and Statistics*, 88(4):641–658.

Springer, M. G., Ballou, D., Hamilton, L., Le, V.-N., Lockwood, J. R., McCaffrey, D. F., and Stecher, B. M. (2010). *Teacher pay for performance: Experimental evidence from the Project on Incentives in Teaching*. Vanderbilt University, National Center on Performance Incentives.

Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199.

Texas Education Agency (2023). Teacher Incentive Allotment guidebook, 2023–24. Version 2.2.

Texas Education Agency (2024a). TEA awards over $290 million as part of the Teacher Incentive Allotment. News release, June 11.

Texas Education Agency (2024b). Teacher Incentive Allotment annual report, 2023–24. Annual report.

Texas Education Agency (2025). 2024–25 Texas public school statistics: Pocket edition.

Tables

Table 1: Baseline Campus Characteristics (2019), AA1 Treated vs. Control

	AA1 treated		Control	
Characteristic	Mean	SD	Mean	SD
Outcomes (pp)				
District retention rate	85.5	8.7	86.3	9.1
Campus retention rate	81.5	12.2	83.2	11.4
Exit rate	9.2	5.8	9.0	6.7
Out-of-district move rate	5.2	5.6	4.7	5.5
Teacher workforce				
Avg. total pay ($)	56,975	7,979	56,522	5,558
Avg. base pay ($)	55,730	7,390	55,194	5,388
Campus / student composition				
Pct. econ. disadvantaged	61.1	25.0	50.4	25.2
Pct. Hispanic students	53.3	28.7	39.6	28.4
Pct. Black students	9.8	12.6	7.2	8.9
Total enrollment	711	524	615	514
Pct. rural	22.5	41.7	38.9	48.8
Campuses	3,151		1,508	

Notes: Campus means and standard deviations in 2019, before adoption. Student composition and enrollment use frozen 2015–2019 averages. AA1 campuses belong to districts approved one year after Data Capture; controls never participate. Outcomes and shares are percentage points, and pay is in dollars.

Table 2: Teacher retention and mobility over implementation

Outcome	From Data Capture	Payment phase
District retention	+0.95***	+1.38***
	(0.29)	(0.47)
Campus retention	+0.66	+0.76
	(0.52)	(0.81)
Exit from Texas public schools	-0.48**	-0.66*
	(0.22)	(0.34)
Move to another district	-0.47***	-0.72***
	(0.17)	(0.26)
Move within the district	+0.30	+0.61
	(0.36)	(0.58)

Notes: Sun–Abraham estimates in percentage points, averaging $k = 0$–4 and $k = 2$–4 by treated campus-years. The reference is $k = -1$. LDS approval occurs at $k = 1$, and the first state reimbursement occurs at $k = 2$. Models include campus and year effects and baseline-covariate-by-year controls. All use the AA1 and never-participating sample. District-clustered standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: Designation and funding during the TIA payment years

Outcome	Estimate	SE
Share of teachers designated (percentage points)	22.76	1.57
Generated allotment per teacher ($)	2,760	201
Implied compensation floor per teacher ($)	2,133	401
Base pay per teacher ($)	−372	296

Notes: Sun–Abraham averages over $k = 2$–4, relative to $k = -1$, with never-participating controls. Dollar outcomes average over all campus teachers. The compensation floor adds 90 percent of generated allotments to payroll. The generated allotment is district funding, not observed individual pay. District-clustered standard errors.

Table 4: Teacher retention and mobility by baseline allotment value

	From Data Capture			Payment years		
Outcome	Low	Middle	High	Low	Middle	High
District retention	0.39	1.17***	1.01**	0.47	1.15*	1.96***
	(0.38)	(0.43)	(0.43)	(0.57)	(0.62)	(0.70)
Campus retention	0.38	1.04	−0.25	0.18	0.71	−0.18
	(0.57)	(0.71)	(0.81)	(0.80)	(1.04)	(1.30)
Exit	−0.15	−0.74**	−0.40	−0.29	−0.51	−0.78
	(0.26)	(0.32)	(0.32)	(0.42)	(0.46)	(0.51)
Out-of-district move	−0.24	−0.43*	−0.61**	−0.18	−0.63*	−1.18***
	(0.22)	(0.24)	(0.29)	(0.32)	(0.34)	(0.45)
Within-district move	0.00	0.13	1.26**	0.29	0.44	2.14**
	(0.38)	(0.45)	(0.63)	(0.55)	(0.72)	(0.98)

Notes: Sun–Abraham estimates in percentage points, relative to $k = -1$. Terciles rank treated campuses by the Recognized allotment on the 2019–20 map; all use common never-participating controls. Columns average $k = 0$–4 or $k = 2$–4 by treated campus-years. District-clustered standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Hiring margins at treated campuses

Arrival measure	2019 treated mean	From Data Capture/AA	Payment phase
Panel A. Arrivals per 100 teachers			
All arrivals	22.4	-0.58	-1.61***
		(0.40)	(0.53)
New to Texas public schools	8.7	-0.17	-0.37
		(0.21)	(0.34)
Returning after an absence	2.6	+0.02	-0.22
		(0.13)	(0.17)
From another campus in the district	4.4	+0.06	+0.10
		(0.18)	(0.26)
From another district	6.7	-0.48**	-1.12***
		(0.19)	(0.28)
In first year of teaching	6.8	-0.49*	-0.74*
		(0.28)	(0.39)
With five or more years of experience	10.1	-0.18	-0.80**
		(0.27)	(0.40)
Panel B. Composition of arrivals			
Share in first year of teaching (pp)	30.4	-1.44	-0.90
		(1.21)	(1.80)
Share with five or more years (pp)	44.7	+0.42	-0.43
		(0.97)	(1.54)
Mean experience of arrivals (years)	6.5	+0.02	-0.08
		(0.15)	(0.24)
Mean experience of cross-district arrivals (years)	7.5	-0.23	-0.28
		(0.20)	(0.31)

Notes: Sun–Abraham estimates relative to $k = -1$, with never-participating controls. Arrival rates use the campus panel's valid prior-year-link denominator. Composition measures condition on positive arrivals. Experience above 60 years is excluded. Columns average $k = 0$–4 and $k = 2$–4. Joint tests of the four leads do not reject for any Panel A rate. District-clustered standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Teacher flows and control-group exposure

Panel A. PPML estimates of district-to-district teacher flows

Margin or specification	Log coefficient	SE	Change (percent)	Observations
Treated origin: departures	-0.080**	0.032	-7.7	278,938
Treated destination: arrivals	-0.086**	0.039	-8.3	278,938
Control origins to treated destinations	-0.079***	0.025	-7.6	304,374

Panel B. District-retention response to a 10-pp increase in exposure

Control sample	Historical-moves effect	SE	Commuting-zone effect	SE
Never and not yet treated	+0.04	0.09	+0.08	0.08
Never treated	+0.06	0.11	+0.00	0.10
Not yet treated	+0.08	0.14	+0.19	0.12
Not yet treated, excluding 2024	+0.26	0.17	+0.27*	0.15
2024 cohort before treatment	-0.28	0.29	+0.04	0.21

Notes: Panel A uses directed district-pair counts. The first two rows include pair and year effects; the third restricts origins to control years and includes pair and origin-by-year effects. Origin treatment is dated at departure and destination treatment at arrival. Standard errors are clustered by origin and destination. Panel B includes campus and year effects; entries are retention changes in percentage points per 10-point exposure increase, with district-clustered standard errors. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: District retention across robustness checks

Specification	Estimate (pp)	SE
Estimator		
Sun–Abraham, never-treated controls (headline)	$+0.95^{***}$	(0.29)
Callaway–Sant'Anna, doubly robust, never-treated	$+0.89$	(0.60)
Callaway–Sant'Anna, doubly robust, not-yet-treated	$+1.06^{**}$	(0.42)
Two-way fixed effects, static	$+1.21^{***}$	(0.25)
Gardner two-stage, untreated-only first stage	$+1.31^{***}$	(0.28)
Borusyak–Jaravel–Spiess imputation	$+1.31^{***}$	(0.28)
Parallel trends		
Reference period $k = -5$ through -3	$+1.22^{***}$	(0.32)
Untreated-only district linear trends	$+1.36^{***}$	(0.47)
Synthetic DiD, balanced campus panel	$+1.03^{***}$	(0.30)
Cohort-specific quadratic untreated paths	$+0.55$	(1.02)
Commuting-zone-by-year effects and district trends	$+1.07$	(0.68)
Selection into adoption and timing		
Onset at approval, Data Capture year as reference	$+0.74^{**}$	(0.32)
Onset at approval, design year as reference	$+1.17^{***}$	(0.35)
Leave one adoption cohort out (range over five deletions)	$+0.84$ to $+1.04$	–
Cohorts 2022–2024 only	$+0.88^{**}$	(0.38)
Cohorts 2020–2021 only	$+0.86^{*}$	(0.45)
All approved adopters regardless of implementation duration	$+0.80^{***}$	(0.28)
Adding pilot-cohort districts with pre-TIA performance pay	$+1.27^{***}$	(0.31)
Fake treatment among never-adopting districts (300 draws)	$+0.01$	sd 0.52
Sample construction and weights		
Campuses observed in all eleven years	$+0.83^{***}$	(0.29)
Same-district multi-campus teachers counted as retained	$+0.96^{***}$	(0.28)
Weighted by 2019 teacher count	$+0.70^{***}$	(0.27)
Weighted by current teacher count	$+0.73^{***}$	(0.27)
Controls reweighted to the treated covariate distribution	$+0.89^{***}$	(0.30)

Notes: District-retention effects in percentage points. Unless indicated, estimates use AA1 adopters, never-participating controls, and Data Capture onset. Standard errors are district-clustered; the gravity models are reported separately. Synthetic DiD uses 399 district bootstrap draws (Table A11). The placebo row reports the mean and standard deviation across 300 assignments. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A Experience-band heterogeneity

Table A1 and Figure A1 report S&A estimates for four experience groups, 0–3, 4–9, 10–14, and 15 or more years, that align with the experience categories commonly reported by the National Center for Education Statistics (National Center for Education Statistics, 2022). Experience is measured in the origin year, so a teacher moves across groups as her career progresses. Experience is observed for every retained teacher-year. Designations concentrate among experienced teachers: in the payment stage, 6.2 percent of teacher-years in the 0–3 band carry a designation, against 20.2, 23.2, and 23.4 percent in the 4–9, 10–14, and 15+ bands.

The average district-retention effects from Data Capture forward are 1.10, 0.95, 1.14, and 1.16 percentage points across the four groups. The decomposition shows that the youngest group's gain is split almost evenly between fewer exits and fewer moves to other districts. For teachers with 4–9 and 10–14 years, reduced cross-district movement is the larger district-retention margin; for teachers with 15 or more years, reduced exit is larger. The accounting is exact within each band: the district-retention effect equals the reduction in exits plus the reduction in out-of-district moves, and campus retention equals district retention minus within-district moves.

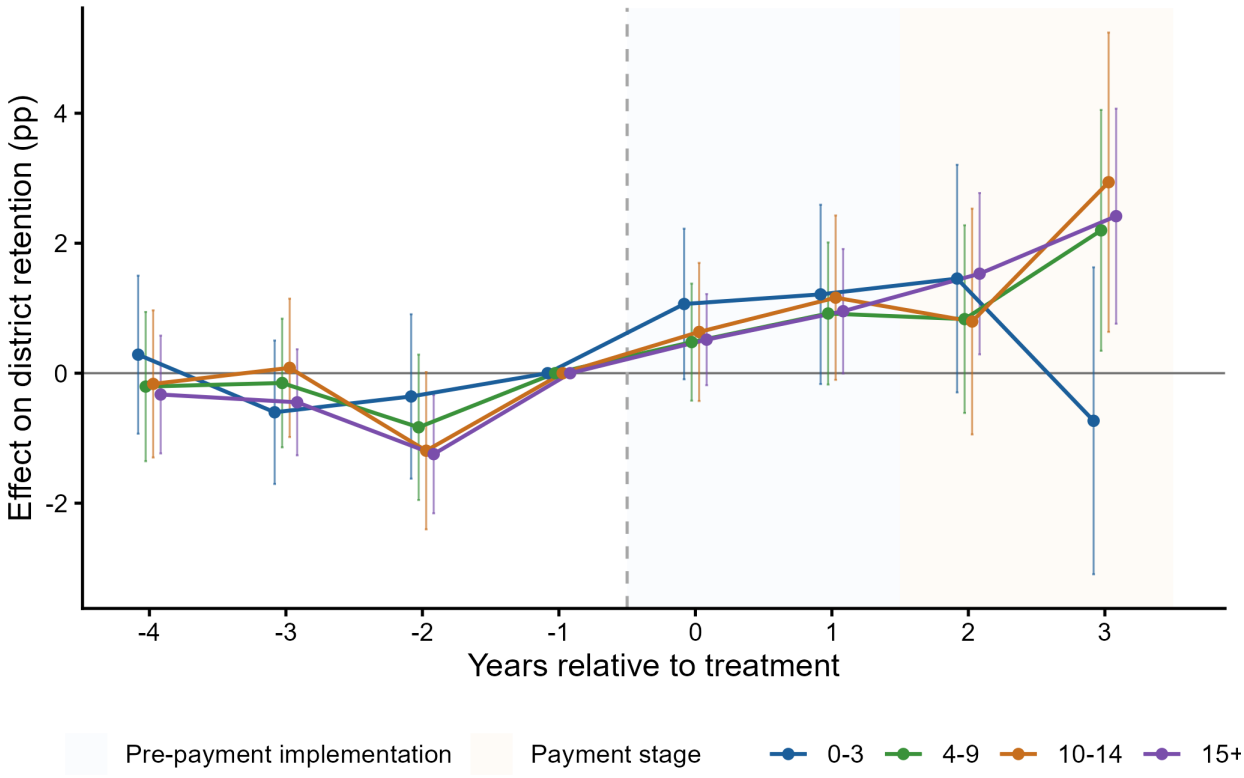
The dynamic estimates separate the phases. For teachers with 0–3 years of experience, the average over Data Capture and approval is 1.13 points and the payment-phase average is 1.04. In the other groups the estimates rise from 0.67, 0.87, and 0.71 points over Data Capture and approval to 1.48, 1.67, and 2.02 points during the payment phase. These phase differences are descriptive, not a separate causal estimate of cash, and the pre-trend tests reject for the 10–14 and 15+ groups. I therefore do not claim a monotone life-cycle gradient. The more limited finding is that early-career retention improves over Data Capture and approval, while later-career estimates are larger in the payment phase.

Table A1: Retention effects and mobility margins by teacher experience

Outcome or margin	0–3 yr	4–9 yr	10–14 yr	15+ yr
Panel A. Average from Data Capture forward				
District retention	+1.10*	+0.95**	+1.14*	+1.16***
	(0.59)	(0.48)	(0.58)	(0.38)
Fewer exits	+0.51	+0.32	+0.36	+0.71**
	(0.49)	(0.37)	(0.44)	(0.34)
Fewer moves to another district	+0.59	+0.64*	+0.79**	+0.45**
	(0.38)	(0.33)	(0.37)	(0.19)
Within-district moves	+0.28	+0.77*	+0.01	+0.19
	(0.43)	(0.42)	(0.48)	(0.37)
Campus retention	+0.82	+0.18	+1.14	+0.97*
	(0.74)	(0.69)	(0.78)	(0.58)
Panel B. District retention by implementation phase				
Pre-payment implementation ($k = 0, 1$)	+1.13**	+0.67	+0.87*	+0.71*
	(0.54)	(0.43)	(0.50)	(0.36)
Payment stage ($k \geq 2$)	+1.04	+1.48**	+1.67*	+2.02***
	(0.87)	(0.72)	(0.92)	(0.60)

Notes: Sun–Abraham estimates in percentage points, with never-participating controls. Experience is measured in the origin year; bands do not follow fixed teacher cohorts. Phase averages use $k = 0, 1$ and $k = 2$–4. Joint district-retention lead-test p-values are 0.51, 0.36, 0.02, and 0.03 across bands. District-clustered standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A1: District retention by teacher experience



Notes: Sun–Abraham estimates relative to $k = -1$. Data Capture begins at $k = 0$, approval is $k = 1$, and the payment phase is $k \geq 2$. Bars show district-clustered 95 percent confidence intervals.

B Targeting and within-district mobility

Table A2 separates rural campuses from non-rural campuses grouped by baseline economic disadvantage. Table A3 compares common and tercile-specific controls for the formula analysis. Figure A2 shows the timing of within-district moves at campuses in the highest formula tercile.

Table A2: Heterogeneity by the program's targeting channels

Panel A. Rural and non-rural campuses

	Rural		Non-rural	
Outcome	From Data Capture	Payment phase	From Data Capture	Payment phase
District retention	+1.29**	+2.46**	+0.85**	+0.99*
	(0.57)	(1.00)	(0.34)	(0.51)
Campus retention	+1.91***	+2.94**	+0.25	-0.05
	(0.73)	(1.20)	(0.65)	(0.97)
Within-district move	-0.63	-0.47	+0.59	+1.04
	(0.43)	(0.67)	(0.46)	(0.72)
Exit rate	-0.92**	-1.48**	-0.25	-0.29
	(0.44)	(0.74)	(0.24)	(0.37)
Out-of-district move	-0.36	-0.98*	-0.60***	-0.70**
	(0.36)	(0.58)	(0.19)	(0.28)

Panel B. Non-rural campuses, by economic disadvantage

Outcome	Least disadvantaged	Middle	Most disadvantaged
District retention	+0.34	+0.50	+1.47**
	(0.43)	(0.55)	(0.66)
Campus retention	+0.93	-0.42	-0.51
	(0.70)	(0.95)	(1.37)
Within-district move	-0.59	+0.93	+1.98*
	(0.51)	(0.58)	(1.07)
Out-of-district move	-0.43*	-0.51	-0.90**
	(0.25)	(0.31)	(0.44)

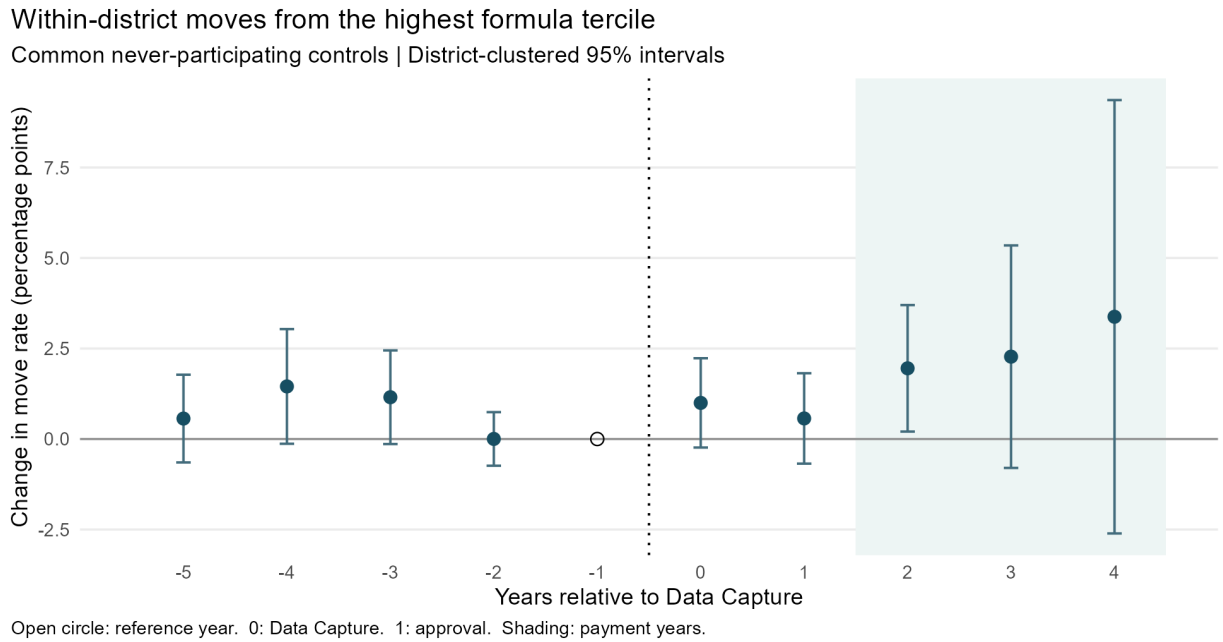
Notes: Sun–Abraham estimates in percentage points. Each stratum uses never-participating controls in the same stratum. Rural status comes from TIA maps. Non-rural tercile cutoffs are 51.9 and 79.6 percent economically disadvantaged, using 2015–2019 averages. Panel B averages from Data Capture forward. The least-disadvantaged non-rural group fails the joint lead test ($p = 0.011$). District-clustered standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A3: Formula-tercile estimates under alternative control groups

	Common controls			Same-tercile controls		
Outcome	Low	Middle	High	Low	Middle	High
District retention	+0.39	+1.17***	+1.01**	+0.31	+1.40***	+1.03**
	(0.38)	(0.43)	(0.43)	(0.41)	(0.50)	(0.49)
Campus retention	+0.38	+1.04	-0.25	+0.55	+1.45*	-0.18
	(0.57)	(0.71)	(0.81)	(0.69)	(0.77)	(0.91)
Exit from Texas public schools	-0.15	-0.74**	-0.40	+0.08	-0.88**	-0.46
	(0.26)	(0.32)	(0.32)	(0.29)	(0.35)	(0.37)
Move to another district	-0.24	-0.43*	-0.61**	-0.39*	-0.52*	-0.57*
	(0.22)	(0.24)	(0.29)	(0.23)	(0.29)	(0.33)
Move within the district	+0.00	+0.13	+1.26**	-0.25	-0.05	+1.21*
	(0.38)	(0.45)	(0.63)	(0.48)	(0.46)	(0.69)

Notes: Sun–Abraham effects from Data Capture forward, in percentage points. Terciles use baseline Recognized allotments. The first three columns share all never-participating controls; the last three use controls in the same tercile. District-clustered standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A2: Within-district mobility at campuses in the highest formula tercile



Notes: Sun–Abraham estimates relative to $k = -1$, using the common pool of never-participating controls. Bars show district-clustered 95 percent confidence intervals.

C Sample composition and robustness

The tables document the adopter groups and the robustness checks summarized in Section 7. Appendix Table A11 provides the sample and inference details for synthetic DiD.

Table A4: Baseline characteristics by implementation duration

	AA1	AA2	AA3+	Not-yet	Never
Panel A: Student composition					
Economically disadvantaged (%)	61.7	70.7	75.6	54.9	50.8
	(24.8)	(21.2)	(19.7)	(23.6)	(25.2)
Hispanic students (%)	53.4	55.2	59.7	39.6	39.6
	(28.7)	(25.6)	(36.1)	(24.6)	(28.5)
Black students (%)	9.9	18.1	12.1	11.4	7.1
	(12.8)	(18.1)	(18.6)	(14.4)	(9.0)
White students (%)	30.7	20.4	25.7	40.5	46.6
	(25.0)	(21.7)	(28.9)	(25.4)	(27.0)
Limited English proficient (%)	18.3	23.9	24.7	13.7	13.6
	(19.1)	(19.1)	(20.6)	(14.6)	(15.9)
Special education (%)	10.1	9.1	8.9	10.1	10.2
	(4.2)	(4.4)	(4.7)	(3.3)	(4.5)
Panel B: Campus characteristics					
Rural campus (%)	22.3	16.2	38.8	37.2	38.1
	(41.6)	(36.9)	(48.9)	(48.4)	(48.6)
Campus enrollment	697	775	599	622	605
	(519)	(586)	(407)	(478)	(511)
Campuses	3,478	742	150	1,200	1,661
Districts	380	58	26	210	329

Notes: Campus means of frozen 2015–2019 characteristics, with standard deviations in parentheses. AA1, AA2, and AA3+ indicate one, two, and three or more years from application acceptance to approval. Not-yet districts have accepted applications but no observed approval. The five columns partition 7,231 campuses in 1,003 districts. The panel described in Section 3 (7,299 campuses, 1,006 districts) differs in three ways: it also contains two districts (104 campuses) whose application was accepted in 2019 and that implemented in one year but fall outside the headline AA1 group, which begins with the 2020 cohort; it also contains three districts (8 campuses) that dissolved before TIA; and it excludes two slower adopters (44 campuses) whose application was accepted before 2019 and that are counted in the AA3+ column. The AA2 column likewise includes two districts (24 campuses) accepted in 2019. The headline estimation sample further requires complete frozen covariates and at least one pre-period year, leaving 3,288 AA1 and 1,562 never-participating campuses (4,850 campuses in 709 districts).

Table A5: Treated-population scope: one-year adopters versus all adopters

Treated population	Treated districts	District retention	Campus retention	Exit	Out-of-district
One-year adopters (headline)	380	+0.95***	+0.66	-0.48**	-0.47***
		(0.29)	(0.52)	(0.22)	(0.17)
All approved adopters, any duration	460	+0.80***	+0.53	-0.34	-0.46***
		(0.28)	(0.48)	(0.21)	(0.17)
All approved and application-only districts	670	+0.86***	+0.72	-0.31	-0.56***
		(0.28)	(0.46)	(0.20)	(0.17)

Notes: Sun–Abraham estimates in percentage points, dated to first AA year, with never-participating controls. All rows require AA in 2020 or later and exclude pre-TIA performance-pay districts. The 460 approved adopters exclude four early entrants included in Table A4. District-clustered standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A6: Pre-trends, reference-period sensitivity, calibrated bounds, and district-trend checks

	Joint lead	Lead $k = -2$	ATT, ref.	HonestDiD $\bar{M}$		District trend
Outcome	p-value	(pp)	$k = -5..-3$ (pp)	From AA	Payment	(pp)
District retention	0.162	-0.98	+1.22***	0.125	0.100	+1.36***
		(0.42)	(0.32)			(0.47)
Campus retention	0.392	-0.68	+1.45***	–	–	+1.15*
		(0.51)	(0.49)			(0.64)
Exit rate	0.221	+0.75	-0.81***	0.025	–	-0.67
		(0.33)	(0.21)			(0.44)
Out-of-district move	0.487	+0.23	-0.41**	0.175	0.200	-0.70***
		(0.17)	(0.21)			(0.24)

Notes: Lead tests cover $k = -5$ through -2. The alternative reference averages $k = -5$ through -3. HonestDiD columns show the largest tested $\bar{M}$ excluding zero on a 0.025 grid; a dash means zero is included already at $\bar{M} = 0$. District trends are learned from untreated observations, with 199 district bootstrap draws. Standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A7: Pandemic-cohort sensitivity

	Baseline	Cohorts 2022–2024		Cohorts 2020–2021
Outcome	All cohorts	All years	Drop 2020–21	All years
District retention	+0.95***	+0.88**	+0.62	+0.86*
	(0.29)	(0.38)	(0.39)	(0.45)
Campus retention	+0.66	+0.31	+0.42	+0.62
	(0.52)	(0.65)	(0.60)	(0.84)
Exit rate	-0.48**	-0.45	-0.39	-0.38
	(0.22)	(0.29)	(0.27)	(0.33)
Out-of-district move	-0.47***	-0.43*	-0.24	-0.48**
	(0.17)	(0.23)	(0.24)	(0.24)

Notes: Sun–Abraham phase averages with never-participating controls. The later-cohort year-deletion specification also changes the reference to available years among $k = -5$ through -3, because the 2022 cohort loses $k = -1$. Thus it changes both calendar-year support and the baseline. District-clustered standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A8: Weighting checks under Application Accepted/Data Capture onset

Outcome	Unweighted (baseline) ATT (pp)	Baseline-teacher wt. ATT (pp)	Contemp.-teacher wt. ATT (pp)
District retention	+0.95***	+0.70***	+0.73***
	(0.29)	(0.27)	(0.27)
Campus retention	+0.66	+0.49	+0.52
	(0.52)	(0.46)	(0.45)
Exit rate	-0.48**	-0.35*	-0.37*
	(0.22)	(0.20)	(0.19)
Out-of-district move	-0.47***	-0.35**	-0.36**
	(0.17)	(0.16)	(0.16)

Notes: Sun–Abraham estimates from Data Capture forward. Weights use 2019 or current-year teacher headcounts. The 2019 specification drops 197 campuses without a positive baseline count, leaving 4,653 campuses in 707 districts. District-clustered standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A9: Sample-definition and timing checks, all outcomes

Specification	District retention	Campus retention	Exit	Out-of-district
Onset at approval, Data Capture reference	+0.74**	+0.77	-0.37*	-0.37*
	(0.32)	(0.50)	(0.21)	(0.20)
Onset at approval, design-year reference	+1.17***	+1.21*	-0.55**	-0.62***
	(0.35)	(0.64)	(0.26)	(0.19)
Campuses observed in all eleven years	+0.83***	+0.66	-0.43*	-0.41**
	(0.29)	(0.40)	(0.22)	(0.17)
Pilot-cohort districts added	+1.27***	+1.18*	-0.76***	-0.51**
	(0.31)	(0.60)	(0.18)	(0.20)
Same-district multi-campus counted as retained	+0.96***	–	-0.47**	-0.49***
	(0.28)		(0.21)	(0.17)

Notes: Sun–Abraham estimates in percentage points with never-participating controls. Approval-onset references are specified in the rows. The multi-campus check counts teachers assigned only within their original district as retained; campus retention is undefined for these cases. District-clustered standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A10: Propensity reweighting under Application Accepted/Data Capture onset

Outcome	Baseline ATT (pp)	PS-reweighted ATT (pp)
District retention	+0.95***	+0.89***
	(0.29)	(0.30)
Campus retention	+0.66	+0.30
	(0.52)	(0.54)
Exit rate	-0.48**	-0.44**
	(0.22)	(0.21)
Out-of-district move	-0.47***	-0.44**
	(0.17)	(0.18)

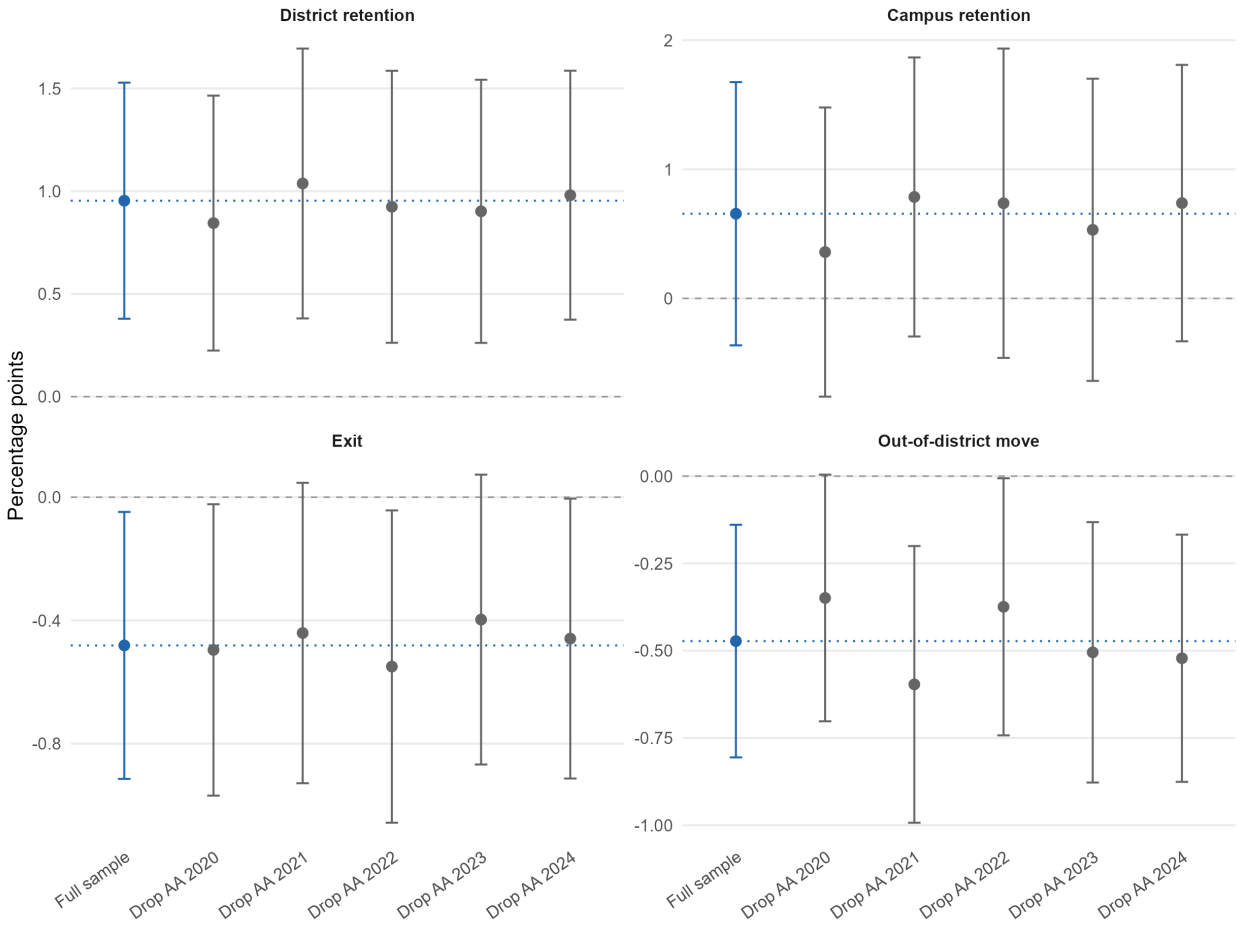
Notes: Sun–Abraham averages from Data Capture forward. Control campuses receive ATT propensity-score weights from a logit using baseline enrollment and student composition; weights are capped at the 99th percentile. Treated campuses retain unit weight. District-clustered standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A11: Synthetic difference-in-differences and the balanced-panel benchmark

Estimator	District retention (pp)	SE
Synthetic DiD	+1.03***	(0.30)
Sun–Abraham, same campuses	+0.83***	(0.29)
Campuses	4,171	
Districts	698	
Origin years	2014–2024	
Bootstrap draws	399	

Notes: SDID fits each cohort against never-participating campuses, then averages by treated campus-years. Unit and time weights use retention histories without covariates. The district bootstrap resamples within cohorts, sharing the resampled donor pool across cohort fits and re-estimating all weights. The Sun–Abraham benchmark retains baseline-covariate-by-year controls. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A3: Sensitivity to omitting an adoption cohort

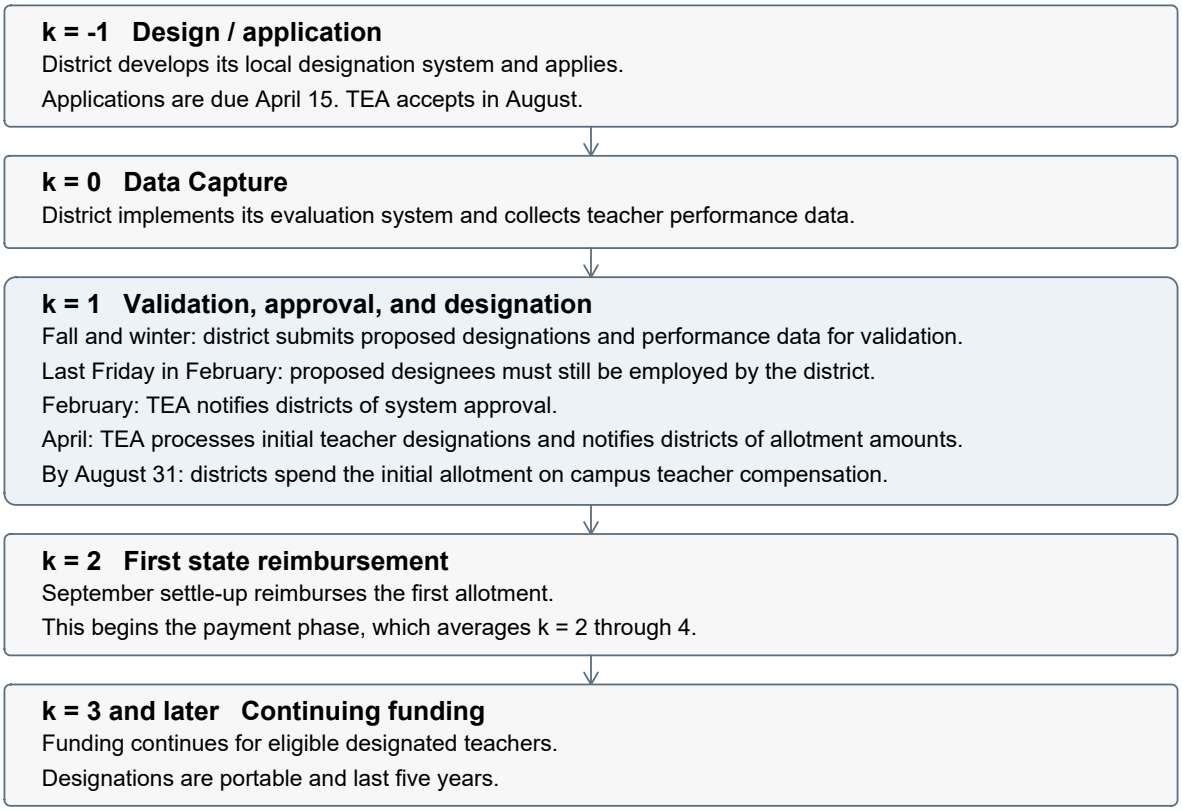


Notes: Sun–Abraham estimates from Data Capture forward. Each panel compares the full sample with estimates omitting one AA cohort. Bars show district-clustered 95 percent confidence intervals.

D Institutional timing and district flows

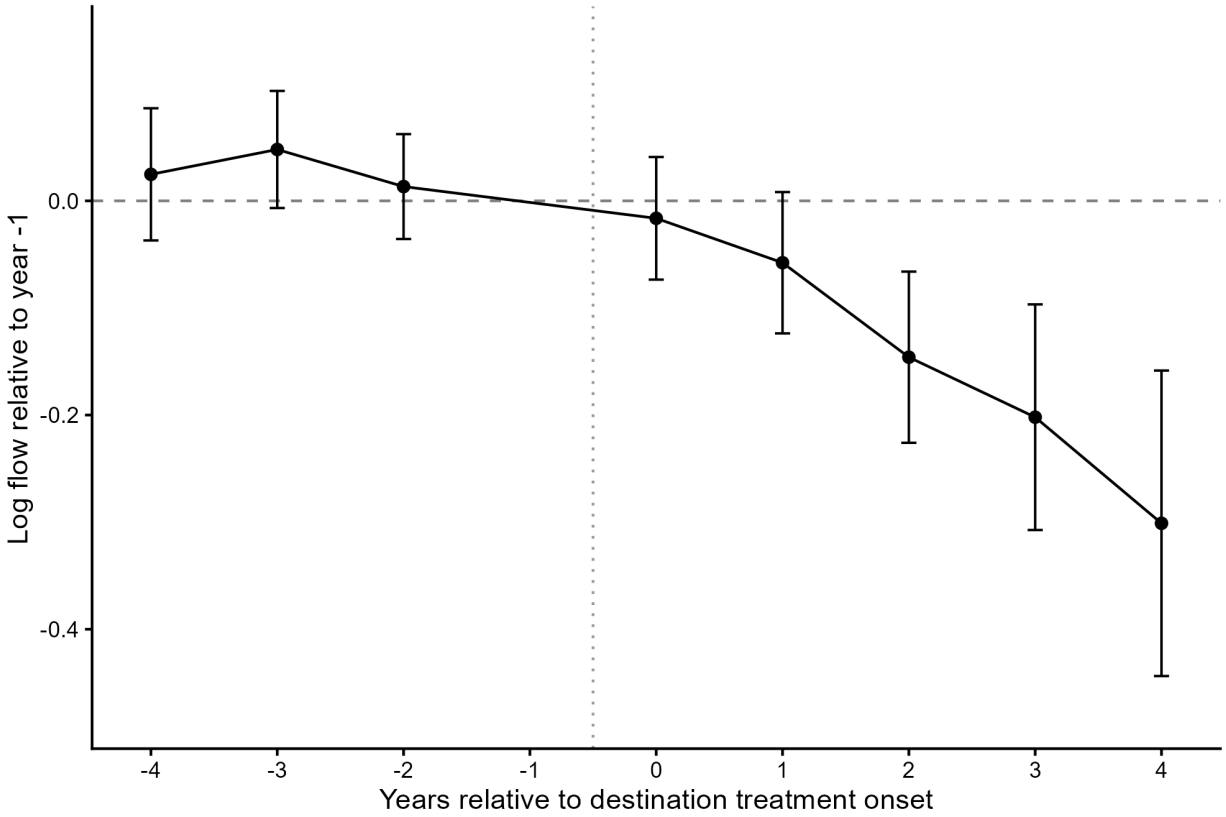
Figure A4 distinguishes the designation, compensation, and reimbursement dates. Figure A5 supplements the district-flow estimates in Section 6.

Figure A4: Implementation sequence for a one-year adopter



Notes: Event time is measured from the Application Accepted/Data Capture year. AA1 districts reach approval one school year after Data Capture. The state reimburses the first allotment in the September settle-up at the beginning of $k = 2$, after the district has spent it on campus teacher compensation by August 31 of $k = 1$. Individual TIA payment dates are not observed. Source: Texas Education Agency (2023).

Figure A5: Flows from control districts to adopting destinations



Notes: Destination event-time PPML estimates with district-pair and origin-by-year fixed effects. The reference is $k = -1$; endpoints pool $k \leq -4$ and $k \geq 4$. Bars show 95 percent confidence intervals clustered by origin and destination.