Targeted Performance Pay in Disadvantaged Schools

Effects on Achievement and Educational Inequality

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Abstract

Performance-based incentives in the public sector often raise concerns that they may exacerbate inequality by disproportionately benefiting already advantaged localities. I study the *Teacher Incentive Allotment* program (TIA), a statewide performance-pay reform introduced in Texas in 2019 that provides substantial bonuses to highly effective teachers, with larger payments in schools serving more disadvantaged students. Exploiting the staggered rollout of TIA across nearly 440 districts and 3,000 schools, I estimate its effects on student achievement and the distribution of these effects across schools. TIA increased standardized test scores by 0.065 standard deviations in mathematics and 0.057 standard deviations in reading at the average school. Achievement gains were substantially larger in schools with higher vulnerability scores, reducing the achievement gap associated with school socioeconomic disadvantage by approximately 6 to 10%. These results show that a large-scale performance-pay program can increase average achievement while disproportionately benefiting disadvantaged schools, providing evidence on how the design of public-sector incentives can shape both productivity and inequality.

I Introduction

The delivery of high-quality public services remains a central challenge in public administration. Outputs are often difficult to measure, oversight mechanisms are weak, and rigid pay structures limit incentives for effort (Bertrand, Burgess, Chawla, and Xu 2020). Education exemplifies some of these broader challenges. Teachers play a central role in student learning (Chetty, Friedman, and Rockoff 2014a, 2014b), yet many U.S. schools, especially those serving disadvantaged populations, struggle to retain and motivate effective educators (Morgan et al. 2026; Nguyen, Pham, Crouch, and Springer 2020; García and Weiss 2019).

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Despite the importance of these challenges, little is known about how to attract and motivate talented workers in vulnerable or remote places. Texas's recent policy reforms address this issue directly. In 2019, the state launched the *Teacher Incentive Allotment* (TIA), a statewide initiative that provides substantial performance-based bonuses to highly effective teachers. The program seeks to improve teacher performance and retention, particularly in high-need and rural schools, by linking bonus amounts to school vulnerability levels: teachers in more disadvantaged settings receive higher awards, aligning financial incentives with the state's equity and performance goals. The Teacher Incentive Allotment Program thus offers a valuable setting to study the economics of public service delivery, specifically, how financial incentives shape student achievement, teacher labor decisions, and the distribution of educational opportunity across schools.

The rollout of TIA offers a unique empirical opportunity: the program has been implemented gradually across 439 independent school districts (out of ∼1,000 school districts in Texas) and over 3,000 schools. I exploit this staggered adoption in a quasi-experimental, event-study design. Rich administrative data, including school-by-grade-by-year test scores and teacher-level employment records, let me track the program's effects across schools with different levels of vulnerability indicators. This event study framework allows for credible causal inference on TIA's effects by using later adopters and non-adopters as counterfactuals. I find that performance incentives improved student achievement. Schools with an average vulnerability score (a value that reflects students' socioeconomic status and the school's rurality status) increased standardized test scores by about 0.065 standard deviations (s.d.) in math and 0.057 s.d. in reading.

High-powered incentives often risk widening inequalities: when rewards depend on performance, better-resourced schools and more advantaged students are typically positioned to gain the most. The Teacher Incentive Allotment (TIA) was designed with this challenge in mind, by channeling additional resources to high-vulnerability schools, the program was explicitly designed to reduce inequality in student achievement. I provide new evidence on this dimension by comparing achievement gains between the most disadvantaged and most advantaged schools under TIA. The results show that the gains were concentrated in schools with the highest levels of vulnerability.

The achievement gap shrank substantially. These heterogeneous improvements reduced educational disparities across schools with varying vulnerability levels. In adopting districts, the slope of the relationship between average test scores and the vulnerability index for the analyzed sample was -0.55 in math and -0.70 in reading, implying that more vulnerable schools perform markedly worse on standardized tests. This means that two schools differing by one point on the *vulnerability score*—roughly equivalent to a 25-percentile difference in vulnerability—were expected to differ by 0.55 standard deviations in average math test scores and 0.70 in reading. My estimates indicate that TIA attenuated this negative relationship by 0.049 and 0.041 points in math and reading, respectively. These results correspond to a 9% and 6% reduction in the achievement gap as measured by the correlation between test scores and school vulnerability. Importantly, I find limited evidence of systematic spillovers on student achievement among untreated schools, suggesting that the gains in treated schools are not accompanied by corresponding losses in nearby or otherwise

related schools.

The effects extend beyond average test scores. TIA reduced some disciplinary outcomes, particularly in more vulnerable schools. The program also changed the composition of the teacher workforce: TIA schools employed more experienced teachers, with larger increases in within-district experience at more vulnerable schools, consistent with improved teacher retention. Despite the larger allotments directed toward these schools, achievement gains increased faster than costs per student, making the program more cost-effective in more vulnerable schools.

These findings are consistent with the view that financial incentives can motivate teachers to remain in challenging environments and to focus more intensively on raising student achievement. In addition, these findings matter for policy: they suggest that well-designed teacher incentives can improve student achievement and reduce educational differences by motivating and retaining talented teachers, especially in the schools that have struggled the most.

II Related Work

These findings contribute to several areas of research. First, they add to the literature on performance-based incentives for teachers. Most evidence on teacher incentives comes from contexts where the main constraints on learning stem from limited resources and low baseline achievement—for instance, Glewwe, Ilias, and Kremer (2010) in Kenya, Muralidharan and Sundararaman (2011) in India, Mbiti et al. (2019) in Tanzania, Barrera-Osorio and Raju (2017) in Pakistan, and Leaver, Ozier, Serneels, and Zeitlin (2021) in Rwanda. In contrast, evidence from systems where the central challenges involve sustaining effort, accountability, and motivation within mature institutions is more limited and shows mixed results. Fryer (2013) studies an experiment in New York City and finds no average effect on achievement. Goodman and Turner (2013) show that the program led to small increases in achievement when teachers have incentives to free ride —a result often cited to underscore the importance of incentive design. Yet this program involved relatively modest bonuses (up to $3,000), and few studies have evaluated large-scale performance-pay reforms offering substantial awards like TIA's, which range from $3,000 to $32,000. Other U.S. studies, such as Springer et al. (2011)'s evaluation of Nashville's program, which rewarded between $5,000 and $15,000, also found null effects. Biasi (2021) report a state-level reform in Wisconsin in which school districts gained discretion over their compensation structures. Biasi (2021) show that the policy improved student achievement. Under TIA, districts can tailor elements of the performance-payment structure, but all modifications operate within a state-established performance-based pay framework that directs more resources to effective teachers in more vulnerable schools.

While an early evaluation of the Teacher Incentive Allotment (TIA) using synthetic control methods finds large achievement gains among the first 19 adopters (Kirksey, Lansford, Crevar, and Mansell 2024), this paper differs from that analysis in several ways. I exploit staggered adoption across nearly 440, allowing for a more representative assessment of the program; I find smaller but sustained achievement gains that build over one to two years; I incorporate potential spillover

effects; and provide relevant findings on the effect of the policy on educational inequality, which is a central component in the design of the policy.

I also contribute to the literature on reducing inequality in public service provision. For instance, it remains an open question whether performance-pay schemes affect educational equality (Biasi, Fu, and Stromme 2021). On one hand, teachers in more vulnerable communities may generate higher returns to effort because they start from a lower baseline and have stronger incentives to qualify for the bonus (Muralidharan and Sundararaman 2011). On the other hand, they often face binding constraints—such as limited resources, weaker management, or community instability—that may limit their ability to respond to incentives (Rasul and Rogger 2018). In this paper, I find that performance-based bonuses have significantly larger effects on student achievement in vulnerable schools. These schools receive larger financial incentives, and the bonuses are financed with state funds, suggesting that program design plays a crucial role in equality.

This work is also related to the literature on performance incentive design. Macartney, McMillan, and Petronijevic (2021) show that performance-based incentive design can shape both average student achievement and its distribution. In their framework, targeting incentives toward lower-performing students can reduce inequality without sacrificing average performance. TIA applies a related principle across schools by directing larger financial incentives toward teachers working in schools with more disadvantaged students. In addition, teachers' evaluations are not based on a fixed threshold, but on increases in their students' performance with respect to their previous results. I provide empirical evidence that such targeted performance incentives can increase average achievement while narrowing achievement gaps across schools.

Finally, my work relates to the literature examining the mechanisms through which performance-pay programs operate. A particularly relevant study is the two-tier experiment in Rwanda's primary schools by Leaver, Ozier, Serneels, and Zeitlin (2021), which separately identified the effects of incentives on selection - who enters and stays in the schools - and effort. By the second year, students taught under performance contracts scored about 0.2 standard deviations higher than those in the control group, with roughly one-fifth of the gain driven by improved teacher selection and the remainder by increased effort. In addition, given the difficulty of identifying effective teachers, policies that affect teacher retention and allocation may have important consequences for student achievement (Staiger and Rockoff 2010). I find that teachers' average within-district experience increases following TIA, particularly in higher-need schools, a pattern consistent with increased teacher retention in the schools toward which the program directs the largest financial incentives. In addition, my results show that TIA appears to have changed the composition of high-vulnerability schools in ways that are positively associated with achievement, but these changes are not sufficient to explain the overall increase in test scores, consistent with what Leaver, Ozier, Serneels, and Zeitlin (2021) reports.

The rest of the paper proceeds as follows. The following section describes the institutional background of TIA, outlining its implementation timeline, design, and the variation in incentive amounts across schools. The next section introduces the data and provides descriptive evidence

comparing adopting and non-adopting districts. The identification strategy focuses on the event study design and the variables created to build plausible spillovers. Then, I present the main results on student achievement and educational inequality, emphasizing heterogeneity by vulnerability level, which is closely linked to bonus size. Here I also study the effect of the program on additional students' outcomes-such as Advanced Placement programs at high schools and disciplinary outcomes-, and school-level changes in teacher characteristics. The next section studies the effect on student achievement for school districts that have implemented the program for four years or more, and contains cost-effectiveness analyses. The final sections present the paper's main conclusions.

III Institutional Background

The Texas Legislature established the Teacher Incentive Allotment (TIA) through House Bill 3 (HB 3) during the 86th Texas Legislative Session in 2019. This bill was passed and signed into law in June 2019. Republican leadership championed the bill, but it received broad bipartisan support because it emphasized increased funding for public education, teacher compensation, and student outcomes through the new Teacher Incentive Allotment program (TIA).

TIA is intended to attract and retain highly effective teachers, especially in rural and high-need schools. In 2019, the first cohort of school districts implemented the program, and the first *"designated teachers"* received their allotment in 2020. *"Designated teachers"* refers to teachers who, according to metrics established by the districts, are high-performing and deserve a bonus on top of the salary schedule defined by the school district.

High-performing teachers can fall into three designation categories based on their evaluation results: Recognized (the lowest designation level), Exemplary, or Master (the highest designation level). Teachers retain their designation for five school years. Designations may be revoked if the teacher is sanctioned in a way that suspends the teaching certificate or if the Texas Education Agency determines that the designation was improperly issued.[1] During the five-year period, the designation is portable across campuses and districts. A teacher may be re-submitted for a higher designation before the original five-year term expires; if the teacher is elevated (for example, from Recognized to Exemplary), the new designation replaces the previous one and restarts the five-year clock. After five years, the designation expires unless the district submits updated performance evidence that qualifies the teacher for renewal. Importantly, the designation does not automatically adjust downward if subsequent performance falls. This structure could, in principle, reduce effort after a teacher obtains a designation. At the same time, teachers with Recognized or Exemplary status face incentives to increase effort to move up to a higher-paying designation. The net effect on sustained effort is therefore ambiguous ex ante.

Adoption

1. For example, if the teacher is listed in the Do Not Hire registry, the issuing district's designation system is revoked, or an irregularity in the designation process is identified.

Participation in the Teacher Incentive Allotment program is voluntary for Texas school districts, and joining the program requires a multi-step process to ensure each district can rigorously identify high-performing teachers. First, districts must design their own *"local designation system"* adjusted to their needs. The system must combine teacher observation scores and measurements of student academic growth. Some districts also choose to incorporate other indicators, such as attendance. The local system can be tailored to the district's context, but it must meet standards pre-established by the state. Because adoption is voluntary, adopting districts may differ from non-adopters in ways that could bias the estimates. I address this concern with an event-study design, which requires that treated and untreated districts followed parallel trends in the absence of the program. I assess the plausibility of this assumption by examining pre-treatment dynamics and find no evidence of differential trends before implementation, supporting the assumption.

The entire approval process can take one to two years. Each year, applying districts prepare their systems and submit them for approval. After approval, the district can submit data on its teachers for consideration for TIA designation.

Teachers' eligibility and designation

Only teachers who are assigned to campuses that have been submitted and approved as part of the district's designation plan are eligible for consideration, and the Texas Education Agency must approve that list. Administrative data from the TIA website indicates that all districts participating in the program had included all schools. Eligible teachers must hold a valid Texas teaching certification, be employed full-time, and have an assignment that contributes to student credit under the state's PEIMS reporting system, which usually means core-subject teachers.

Teachers' allotment

The state finances the allotment assigned to designated teachers, but the funds generated by each designation are not automatically transferred to that teacher. Instead, for each designated teacher, the TIA allocates funds to the teacher's district. Districts are required to spend at least 90% of this allotment on teacher compensation. However, they have discretion over how to distribute the allotment, for example, whether to direct the entire amount to the designated teacher or to share a portion with other staff. In practice, most districts allocate roughly 90% of the funds associated with a given designation to the designated teacher.

The amount generated by each designated teacher depends on two key factors: a *"school-level score"* and the designation level of the teacher. The *"school-level score"* combines the school's socioeconomic tier—based on the share of economically disadvantaged students—with the school's rural status. I refer to this school-level combination of socioeconomic tier and rural status as a *"vulnerability score"*, since it reflects the degree of contextual disadvantage due to economic disadvantage and geographic isolation. The second component is the teacher's designation level, which depends on the teacher's performance.

When a designated teacher moves to a different district, the designation remains valid, and the

teacher continues to generate TIA funding. However, the amount associated with that designation may change, because TIA allotments are a function of the *"vulnerability score"* of the school. As a result, the same teacher can generate different funding amounts in different placements. Importantly, the designation continues to generate funding even if the teacher moves to a district that has not adopted TIA.

The allotment formula for each designation level is straightforward: it increases linearly with the school's *vulnerability score*. This score is a continuous index ranging from 0 (least vulnerable) to 4 (most vulnerable), constructed by the Texas Education Agency. Table 1 presents the allotment formulas for each designation level. Both the base payment and the multiplier grow with the designation rank—being highest for *Master* teachers, followed by *Exemplary*, and then *Recognized* teachers.

The magnitude of these differences in financial incentives is substantial. For instance, a teacher with a *Master* designation in the least vulnerable school receives an allotment of $12,000, while the same designation in the most vulnerable school yields $32,000. The steep gradient in allotment by school vulnerability implies that designated teachers face substantial incentives to transfer to higher-need schools, where their designation yields greater financial rewards. Across designation levels, the difference between a *Recognized* and an *Exemplary* teacher ranges from roughly $3,000 in low-vulnerability schools to $9,000 in high-vulnerability ones, and the gap between an *Exemplary* and a *Master* teacher increases from about $6,000 to $14,000. These amounts are sizable relative to the average base pay of teachers in the sample, which was slightly above $55,000 in 2018, highlighting the strong financial incentives embedded in the program's design.

Table 1: Allotment Formula for Designated Teachers, by Designation Level

Designation	Annual Allotment Formula
Recognized	$3,000 + $1,500 × vulnerability score
Exemplary	$6,000 + $3,000 × vulnerability score
Master	$12,000 + $5,000 × vulnerability score

Notes: The vulnerability score measures the socioeconomic disadvantage of the school and ranges from [0 to 4].*Source:* Texas Teacher Incentive Allotment, https://tiatexas.org/.

IV Data

A Data

This study leverages administrative data from the Texas Education Agency (TEA). The datasets cover the universe of Texas public schools and school districts from the 2010–2011 through 2023–2024 school years.

I use student performance data from the State of Texas Assessments of Academic Readiness

(STAAR) test at the school-by-grade-by-subject level. I also collect school-level data on disciplinary outcomes, student enrollment, and other relevant variables. In addition, I construct school-level data on teacher cross-school mobility using teacher-by-year personnel records from Texas's PEIMS database, obtained through a Freedom of Information Act request. This data includes teachers' identifiers, schools and school districts, demographics, qualifications, experience, and yearly school assignments. These records are linked to data indicating which districts adopted the program, which teachers received TIA designations each year, their designation level, and a school-level vulnerability index used to define the bonus schedule for designated teachers.

Finally, I incorporate contextual variables. The primary contextual variables relate to measures of the impact of COVID-19 over the years 2020 and 2021. To control for differential trends caused by COVID, I need to control for variables that are closely associated with the implications of COVID for education. This is the reason why I use the number of weeks spent in hybrid and remote teaching modes during 2020 at the district level. Some big districts had performance pay systems in years before TIA; those school districts were removed from this analysis.[2]

B Adoption

Tables 2 and 3 describe differences between schools in districts that eventually adopted TIA and those in districts that never adopted the program, as well as differences across adoption cohorts for years prior to TIA (2013 to 2018). Table 2 shows that adopters -who are the treated group- differed substantially from non-adopters -the control group- before implementation. Students in adopting districts had lower baseline achievement, with average mathematics and reading scores 0.29 and 0.40 standard deviations below those in control districts, respectively. Adopting districts also served a more disadvantaged and more heavily Hispanic student population, and enrollment was also larger. In contrast, the district-level measures of rural status and pandemic instructional modes are relatively similar across the two groups. The differences in Table 2 highlight the importance of accounting for pre-existing differences between adopting and non-adopting districts. The empirical strategy therefore relies on changes in outcomes associated with the staggered introduction of TIA rather than cross-sectional differences in levels. The large number of schools and districts in both groups also provides a broad comparison sample: eventual adopters comprise 439 districts and 3,019 schools, compared with 690 districts and 2,554 schools among never-adopters.

Table 3 further shows that observed differences between schools are related to the timing of adoption. Earlier adopters tended to have substantially lower pre-treatment achievement than districts adopting later. For example, districts adopting in 2019 had average mathematics and reading scores of -0.096 and -0.228 standard deviations, respectively. By contrast, average achievement rises across later cohorts: districts adopting in 2024 had pre-treatment scores of 0.089 standard deviations in mathematics and 0.137 in reading. Never-adopting districts had still higher averages,

2. The school districts with prior performance pay programs are: Spring ISD, San Antonio ISD, Dallas ISD, Houston ISD, and Austin ISD. All these districts had their own performance pay program for at least one year between 2005 and 2019. Some of them included a few schools from the district.

at 0.180 and 0.217 standard deviations, respectively. Thus, adoption generally occurred earlier among lower-performing districts. These patterns are consistent with TIA initially expanding among districts serving students with greater educational needs, followed by adoption among progressively higher-performing districts. They also have implications for interpreting the empirical results. The staggered-adoption estimates must account for systematic differences across adoption cohorts, while estimates based on districts with several years of TIA exposure disproportionately characterize early adopters. Consequently, the estimates presented below should be interpreted as effects for these earlier-adopting districts (Average Treatment Effect on the Treated) rather than as necessarily representative of the effects that later adopters or never-adopting districts would experience after the same number of years of implementation.

Table 2: Pre-Treatment Characteristics by TIA Adoption

	Control	Treated	Difference	$-value$
Panel A. Student characteristics				
Math test score	0.200	-0.094	-0.294	0.0000
Reading test score	0.227	-0.173	-0.399	0.0000
White (%)	38.585	24.511	-14.073	0.0000
Asian (%)	4.870	3.669	-1.201	0.0000
Black (%)	8.764	11.876	3.112	0.0000
Hispanic (%)	41.935	55.483	13.547	0.0000
Other race/ethnicity (%)	5.846	4.461	-1.385	0.0000
Income-to-poverty ratio	0.387	-0.072	-0.460	0.5411
Panel B. School characteristics				
TIA point value	1.407	1.688	0.281	0.0000
Magnet school	0.002	0.024	0.022	0.0000
Enrollment	574.8	672.5	97.7	0.0000
Panel C. District characteristics				
Weeks hybrid instruction	3.846	0.000	-3.846	0.0900
Weeks remote instruction	1.385	-0.000	-1.385	0.3326
Rural district	0.385	0.500	0.115	0.7765
Panel D				
Nr. Enrolled Students (2018)	1,468,029	2,030,378		
Nr. Schools	2,554	3,019		
Nr. Districts	690	439		

Notes: Control denotes schools in districts that never adopt the Teacher Incentive Allotment (TIA), while Treated denotes schools in districts that eventually adopt TIA. Student characteristics are measured over the 2013–2018 pretreatment period. School- and district-level characteristics are measured in 2018. Differences are calculated as Treated minus Control. Student-level characteristics are weighted by enrollment or the number of students taking the corresponding test. The tested differences include robust standard errors."

Table 3: Pre-Treatment Characteristics by TIA Adoption Cohort

TIA adoption year	Number of district	Enrollment	Math	Reading
2019	27	238,339	-0.096	-0.228
2020	5	17,024	-0.666	-0.714
2021	40	247,293	-0.491	-0.449
2022	151	639,405	-0.201	-0.314
2023	135	543,639	0.013	-0.010
2024	81	344,678	0.089	0.137
Control	693	1,468,029	0.180	0.217

Notes: The table reports characteristics of schools by the year in which their district first adopted the Teacher Incentive Allotment (TIA). All characteristics are measured in 2018, prior to the introduction of TIA. Enrollment is the average enrollment across schools. Math and reading test scores are standardized and weighted by the number of test takers.

V Identification Strategy

A Theoretical Framework

The main mechanism through which TIA intends to increase student performance is by changing teachers' incentives through the possibility of receiving a bonus. This changes the returns to increasing performance for teachers at schools under TIA, but it also changes the expected returns to increasing performance at other schools, potentially generating spillovers or competition effects. More precisely, consider teachers at school j, and let school j related school be another school k. Let's keep the concept of *related school* rather abstract for now, but you can think about schools that are close by, schools where teachers at j tend to move, or any other aspect that might connect j and k. The implementation of the policy at the district where school j is can affect school outcomes through two channels. First, adoption increases the value of improving performance at a treated school. This is the *direct effect* of the program at school j. Second, adoption by other schools, k, changes the attractiveness of school j relative to school k. This potentially creates *spillovers*: a school's outcomes may respond not only to its own participation in TIA, but also to TIA participation and generosity at other related schools.

The direct effect of TIA is expected to vary with the school's vulnerability score, V_j, for several reasons. The most evident reason is that bonuses are larger at more vulnerable schools, leading to different treatment intensities across schools; however, there might be other reasons behind having heterogeneous effects by V_j, for example, schools with more disadvantaged students might start from a lower baseline, having more room for improvements in test scores or, on the contrary, it might be harder to improve teaching practices due to monetary constraints. I therefore allow the direct effect of TIA to depend on V_j. This captures both an average effect of adopting TIA and differential effects associated with schools' vulnerability levels, such as the policy's vulnerability-

based compensation schedule.

Perhaps the most interesting group of relevant schools that induce spillover effects are schools that represent outside options for their teachers. Suppose that the flow of teachers between school j and school k is high, and school k is under a TIA regime. School k would become more attractive to teachers at school j because they could obtain an additional performance-contingent bonus, potentially generating adverse effects from teacher outflows or demotivation at school j. This effect is going to be more pronounced if V_k is high – and hence its performance-based bonus.

The spillovers from k to school j depend on j's treatment status. For an untreated school, introducing TIA at school k changes compensation opportunities outside j, and the strength of this incentive depends on the size of the bonus available at the destination school, which varies with V_k. When school j is also treated, the same potential spillovers may arise, but their magnitude depends not only on the bonus offered at school k, but on the difference in bonus generosity between the two schools, which varies with $V_k - V_j$.

TIA implementation at k could also affect school j through other mechanisms that are not necessarily associated with the size of the bonus, such as the adoption of educational practices used at school k, or discouragement if TIA generates an improvement in the quality of school k relative to school j, etc.

These mechanisms imply that TIA can affect performance differences across schools even when the school does not implement the program, threatening the SUTVA condition required in conventional event study designs. Naturally, all these potential spillovers might well be negligible, making an event study correctly specified. The next section explains in detail the empirical strategy used to identify the program's treatment effect on the treated, its consequences for educational inequality, and to test its potential spillovers.

B Empirical Strategy

The empirical analysis distinguishes the direct effect of TIA from spillovers generated by the adoption of the program at other schools. The main specification is as follow:

$$\begin{aligned} Y_{jgt} = {} & \alpha_{jg} + \lambda_{gt} + X'_{jt}\gamma + \beta_0 \text{TIA}_{jt} + \beta_1 \text{TIA}_{jt} \left(V_j - \bar{V}\right) \\ & + \beta_2 \text{TIA}_{jt} S^T_{jt} + \beta_3 \left(1 - \text{TIA}_{jt}\right) S^U_{jt} + \beta_4 \left(1 - \text{TIA}_{jt}\right) S^P_{jt} + \varepsilon_{jgt}, \end{aligned} \tag{1}$$

where Y_{jgt} denotes the standardized test score in school j, grade g, and year t. The term α_{jg} denotes school-by-grade fixed effects and λ_{gt} denotes grade-by-year fixed effects. The vector X_{jt} contains additional controls described below. Standard errors are clustered at the school-district level. TIA_{jt} is an indicator equal to one when the district containing school j operates TIA in year t, and $(V_j - \bar{V})$ is the difference between school j vulnerability score and the average vulnerability score $\bar{V}$.[3] The associated coefficients β_0 and β_1 capture different dimensions of the *direct effect* of

3. Notice that V_j does not vary over time in this specification. I use the vulnerability score from 2019, the first

TIA. S_{jt}^{T}, S_{jt}^{U} and S_{jt}^{P} are measures that capture the exposure of school j to others school under TIA incentives. These variables and their associated coefficients (β_2, β_3 and β_4) capture the *spillover effects* discussed in the previous subsection

The estimated direct effect of TIA for a school with vulnerability V_j is therefore

$$\tau(V_j) = \beta_0 + \beta_1(V_j - \bar{V}). \tag{2}$$

The coefficient β_1 captures how the effect of TIA changes with the intensity of the program's financial incentive β_1, for example, implies that the effect of TIA on achievement is larger in more vulnerable schools, where TIA provides larger financial incentives. Therefore, β_1 tests whether the inter-school educational gap associated with schools' vulnerability scores is reduced.

To measure potential spillovers, I define the set of schools relevant to school j using teacher mobility before the introduction of TIA, specifically between 2013 and 2018, to rule out concerns that these weights are affected by the program. In particular, for each pair of schools j and k, I construct the weight

$$\omega_{jk} = \frac{\#\text{ teachers moving from } j \text{ to } k \text{ during 2013–18}}{\#\text{ teachers at school } j \text{ during 2013–18}}, \tag{3}$$

so that ω_{jk} measures the share of teachers that during years 2013 to 2018 moved from school j who to school k. Teacher outflows are not the only way to measure connections across schools. However, observed teacher outflows provide a natural measure of exposure to other schools because the main spillover mechanisms posited here operate through teacher incentives at schools that represent relevant outside options.

As discussed in the theoretical framework, the relevance of bonus size at alternative schools depends on whether school j is itself treated. For a school already exposed to TIA, the relevant incentive depends on the difference in TIA generosity between the origin and potential destination. Considering this, I define

$$S_{jt}^{T} = \text{TIA}_{jt} \sum_{k \neq j} \omega_{jk} \text{TIA}_{kt} \left(V_k - V_j\right), \tag{4}$$

a measure that is positive when the TIA schools to which teachers from j historically tend to move are, on average, more vulnerable than j, and therefore offer relatively higher bonuses, and negative otherwise. Thus, β_2 captures how outcomes at treated schools vary with the relative TIA generosity of the outside options faced by their teachers.

year in which it was calculated. Although the Texas Education Agency updates this measure annually, I fix V_j at its 2019 value because subsequent values may themselves be affected by TIA. In the analysis sample, the average vulnerability score is $\bar{V} \approx 1.5$.

For an untreated school, the relevant comparison is different because teachers at j do not have access to TIA incentives. Therefore, for untreated schools, I define

$$S_{jt}^{U} = (1 - \text{TIA}_{jt}) \sum_{k \neq j} \omega_{jk} \text{TIA}_{kt} V_k. \tag{5}$$

This measure captures the vulnerability-weighted exposure of an untreated school to TIA at historically relevant destinations.

Additionally, schools outside TIA districts may still be affected by exposure to schools participating in TIA, independently of the treatment intensity induced by the vulnerability of related schools. For instance, consider two otherwise identical schools whose related schools have the same vulnerability scores, but where one is more exposed to schools participating in TIA. We would expect the latter to be more affected by its own lack of participation in the program. Hence, I create the following variable;

$$S_{jt}^{P} = \sum_{k \neq j} \omega_{jk} \text{TIA}_{kt}. \tag{6}$$

S_{jt}^{P} is the share of school j's historical teacher outflows directed toward schools that are exposed to TIA in year t. Thus, a value of $S_{jt}^{P} = s^p$ indicates that schools accounting for s^p share of the pre-TIA outflows of teachers from j are under TIA in year t, capturing exposure to the presence of TIA in other schools, independently of differences in bonus size across those destinations.

Accordingly, β_2 and β_3 capture the component of exposure to TIA destinations that varies with the vulnerability of potential destinations, while β_4 captures how the spillover changes regardless of the vulnerability score of the destination schools for untreated schools.

We identify the direct and spillover effects using different sources of variation. The direct-effect coefficients, β_0 and β_1, exploit the staggered introduction of TIA across districts and differences in school vulnerability. The spillover coefficients instead exploit changes in TIA adoption among schools connected to j through the predetermined teacher mobility network. Two schools with the same treatment status and vulnerability score can therefore face different exposure to the policy because teachers historically leaving those schools move to potential destinations with different vulnerability scores, or to potential destinations that implement TIA at different times.

The interpretation of these coefficients as causal effects requires that, conditional on the fixed effects and controls, changes in TIA exposure through the pre-policy mobility network are not correlated with other shocks to achievement at school j.

A particular concern is that TIA adoption may be correlated across economically or geographically related districts. I assess this assumption using dynamic versions of Equation (1), which allow the direct effects to vary with event time. This specification allows me to (i) characterize the evolution of the effects following TIA adoption and (ii) allow me to examine whether differential

changes in achievement precede treatment by testing pre-treatment parallel trends.

Still, we should not interpret $\tau(V_j)$ as the causal effect of increasing the size of the bonus because V_j is not random. In fact, schools with different values of V_j might have different treatment effects because schools with different socioeconomic characteristics might respond differently to the same bonus amount. These heterogeneous effects by vulnerability may stem from different mechanisms; for example, more vulnerable schools might start from a lower baseline, have more room for improvement, and therefore report a larger effect. In later sections, I explore the bonus effect by exploiting differences in V_j that are locally as good as random.

VI Results

A Effect on Test Scores

Table 4 reports the estimated effects of the program on standardized mathematics and reading test scores, as well as on the share of students in each performance category. For both subjects, standardized test scores (Column 1) increase following the implementation of TIA for a school with the average vulnerability score. For mathematics, the estimated effect at the average level of vulnerability is 0.065 standard deviations (s.d.).

The interaction between TIA implementation and the vulnerability score is 0.049 standard deviations in math and 0.041 standard deviations in reading. Given the range of the vulnerability score, these estimates imply that the effect of TIA is approximately 0.20 standard deviations larger in the most vulnerable schools than in the least vulnerable schools. The pre-TIA achievement gap associated with school vulnerability—measured by the coefficient from a regression of test scores on the school vulnerability score using years prior to 2019—is -0.55 standard deviations in math and -0.70 standard deviations in reading. Thus, the estimated effects imply that TIA reduces the achievement gap across schools with different vulnerability levels by about 9% in math and 6% in reading. This reduction in inequality, however, is not necessarily driven by the larger bonuses offered in schools with higher vulnerability scores. The effects of TIA may instead be larger in more vulnerable schools because of other factors correlated with the vulnerability score that make the policy more effective in these settings. For example, these schools may have greater scope for improvement because they start from lower achievement levels.

The estimated spillover effects are relatively small and statistically insignificant. For treated schools, the coefficients on spillover exposure are -0.425 for mathematics and -0.337 for reading, with the latter statistically insignificant at the conventional levels. These coefficients correspond to the effect of an extreme exposure in which the historical teacher-flow weight toward related treated schools is one and those schools have a vulnerability score one point higher than the origin school. A more plausible exposure, in which the historical teacher-flow weight is 0.01 and destination schools have vulnerability scores one point higher, implies effects of approximately -0.004 s.d. for mathematics (-0.425×0.01) and -0.003 s.d. for reading (-0.337×0.01). The estimated spillover effects for untreated schools are statistically insignificant in both subjects. Importantly, this result

provides some reassurance regarding potential contamination of the comparison group.

Columns 2 through 5 of Table 4 report effects on the share of students in each of the four performance categories, ordered from the lowest to the highest level of achievement. These categories indicate the extent to which students demonstrate the knowledge and skills expected at their grade level. In mathematics, TIA increases the share of students in the highest category (Masters Grade Level) by approximately 0.5 percentage points and reduces the share in one of the lower categories (Approaches Grade Level). For reading, the changes are concentrated toward the bottom of the achievement distribution. There is a substantial and statistically significant decline in the share of students who do not meet the expected grade-level standard, accompanied by increases in the next two performance categories. The estimated effect on the share of students at the Masters Grade Level is slightly negative but statistically insignificant. Across these outcomes, the estimated spillover effects are generally small and statistically insignificant.

Figure 2 presents the dynamic effects on standardized test scores using the Callaway and Sant'Anna estimator. Reassuringly, the pre-treatment pseudo-ATTs are generally close to zero, indicating that, prior to TIA adoption, changes in mathematics and reading achievement among future TIA schools were not systematically different from the corresponding changes among comparison schools. These patterns are consistent with the parallel-trends assumption. Following TIA adoption, test scores gradually increase. The estimated effects rise to approximately 0.05 s.d. during the first three years of implementation and reach levels slightly above 0.1 s.d. in subsequent years. One interpretation is that improvements in student achievement take time to materialize, with the effects of TIA accumulating as schools and teachers adjust to the program. An alternative explanation is changes in the composition of cohorts identifying the effects at different event times. Estimates for the first years after adoption average effects across both early and later adopters, whereas estimates at longer horizons are identified only by earlier-adopting cohorts. If early adopters differ in ways that generate larger treatment effects—for example, if they began with lower baseline achievement—this composition could partly account for the larger estimates at longer horizons. In a later section, I examine the dynamics over the first four years of implementation while restricting the sample to districts that adopted TIA during the program's early years. The estimates exhibit a similar increasing pattern.

Table 4: Effects of TIA on Student Achievement

Panel A. Mathematics	Score	Not met	Approaches	Meets	Masters
	(1)	(2)	(3)	(4)	(5)
TIA	0.065	0.022	-0.719	-0.285	0.901
	[0.008]	[0.957]	[0.000]	[0.174]	[0.000]
TIA × Vulnerability Score(V_j)	0.049	-0.107	-0.477	-0.190	0.750
	[0.010]	[0.752]	[0.000]	[0.296]	[0.000]
TIA × S^T	-0.425	7.635	0.486	-3.865	-3.969
	[0.048]	[0.068]	[0.764]	[0.104]	[0.090]
(1-TIA) × S^U	0.467	15.738	-14.217	-5.596	6.469
	[0.373]	[0.078]	[0.001]	[0.259]	[0.250]
(1-TIA) × S^P	0.509	-23.853	10.397	6.046	3.172
	[0.616]	[0.198]	[0.203]	[0.556]	[0.770]
Pre-TIA mean, treated	-0.068	26.520	30.815	22.737	19.721
Pre-TIA mean, control	0.216	22.207	29.720	24.547	23.163
Observations	164,796	123,597	123,597	123,597	123,597
Treated schools	2,570	2,570	2,570	2,570	2,570
Control schools	2,274	2,274	2,274	2,274	2,274
Years	2013–25	2016–25	2016–25	2016–25	2016–25
School × grade FE	Yes	Yes	Yes	Yes	Yes
Grade × year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Panel B. Reading	Score	Not met	Approaches	Meets	Masters
TIA	0.057	-1.580	1.144	0.768	-0.412
	[0.010]	[0.000]	[0.000]	[0.000]	[0.148]
TIA × Vulnerability Score(V_j)	0.041	-1.050	0.832	0.758	-0.558
	[0.067]	[0.000]	[0.000]	[0.000]	[0.042]
TIA × S^T	-0.337	6.842	-2.744	-0.171	-3.643
	[0.181]	[0.055]	[0.099]	[0.928]	[0.218]
(1-TIA) × S^U	0.747	-15.970	14.284	12.720	-8.876
	[0.141]	[0.018]	[0.001]	[0.003]	[0.150]
(1-TIA) × S^P	-0.202	-2.512	-1.738	-4.930	5.160
	[0.829]	[0.846]	[0.830]	[0.547]	[0.664]
Pre-TIA mean, treated	-0.161	31.947	28.730	18.147	21.045
Pre-TIA mean, control	0.237	26.541	27.927	19.887	25.362
Observations	166,560	124,920	124,920	124,920	124,920
Treated schools	2,569	2,569	2,569	2,569	2,569
Control schools	2,272	2,272	2,272	2,272	2,272
Years	2013–25	2016–25	2016–25	2016–25	2016–25
School × grade FE	Yes	Yes	Yes	Yes	Yes
Grade × year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Notes: P-values reported in brackets. Each column reports a separate regression for different outcomes. In columns labeled "Score", the dependent variable is the school-by-grade-level score on STAAR tests for grades 3 to 8, standardized within year and grade level; all other outcomes are measured in percentage points. Regressions and pre-TIA means are weighted by the number of students taking the corresponding STAAR assessment. All specifications include school-by-grade and grade-by-year fixed effects, as well as year interactions with rural status, 2020 enrollment, independent-school status, magnet status, and COVID-19 learning-mode measures. Standard errors are clustered at the district level.

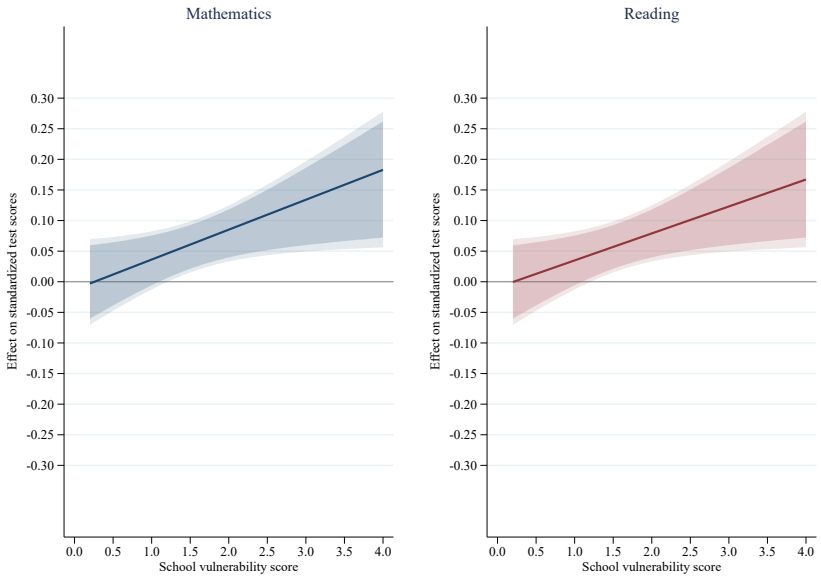


Figure 1: Estimated Linear Heterogeneous Effects of TIA on Student Achievement

Notes: The figure reports estimates of the effect of TIA on standardized test scores. Points represent estimated coefficients calculated as $\tau(v) = \beta_0 + \beta_1 \times TIA \times (v - \bar{V})$, where v takes values between 0 and 4. Shaded areas report 90 and 95 % confidence intervals where I test whether $\tau(v) = 0$ for each value of v.

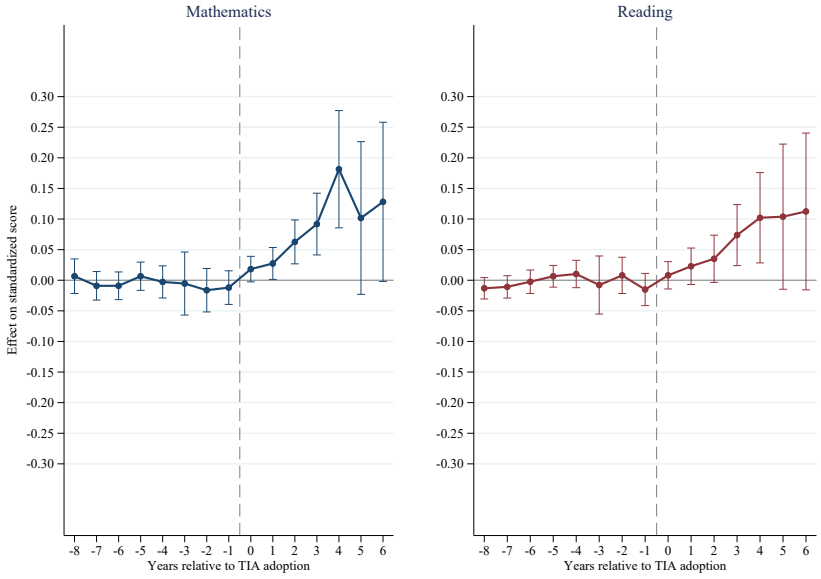


Figure 2: Callaway & Sant'Anna Effects of TIA on Student Achievement

Notes: The figure reports estimates of the effect of TIA on standardized test scores using the Callaway and Sant'Anna specification. Points represent estimated coefficients and vertical lines report 95 percent confidence intervals. Standard errors are clustered at the district level.

B Effect on Additional Outcomes

TIA may affect dimensions of school performance beyond standardized test scores. I therefore examine disciplinary outcomes and Advanced Placement (AP) participation and performance, which provide complementary measures of student behavior and access to advanced coursework.

Table 5 considers outcomes related to Advanced Placement (AP) and student discipline. Advanced Placement courses offer high school students college-level coursework and the opportunity to earn college credit. Columns (1)–(3) examine AP outcomes. The outcome in column (1) is an indicator for whether a school has any students participating in AP, column (2) measures the percentage of students participating in AP, and column (3) measures the percentage of AP participants performing above the critical threshold. Across these outcomes, I find little evidence that TIA produces systematic changes in either AP participation. Neither the direct TIA coefficient nor its interaction with vulnerability is statistically significant for any of the AP outcomes.

For disciplinary outcomes, columns (4)–(7) report the percentage of enrolled students involved in each type of disciplinary action. Column (4) measures the percentage of students receiving any disciplinary action, while columns (5)–(7) report the percentages assigned to a Disciplinary Alternative Education Program (DAEP), receiving an in-school suspension, and receiving an out-of-school suspension, respectively. I find that TIA implementation is associated with a small overall reduction in disciplinary actions, with larger reductions in more vulnerable schools. DAEP placements show a different pattern: TIA schools appear to increase the share of students assigned to DAEP programs. Several mechanisms could account for this result. On the one hand, TIA schools may respond more strongly to behavioral problems among more vulnerable students; on the other hand, the increase could reflect stricter enforcement of school codes of conduct. I therefore do not interpret this estimate as conclusive evidence of a broad effect on disciplinary practices, although it could be worth further examination. For in-school and out-of-school suspensions, the estimated effects point in the same direction and are similar in magnitude. At schools with average vulnerability, TIA reduces the shares of students receiving in-school and out-of-school suspensions by 1.3 and 0.7 percentage points, respectively. In both cases, these estimates correspond to reductions of approximately 10% relative to their pre-TIA means.

The remaining coefficients examine whether TIA generates spillovers across schools. For the AP outcomes, none of these spillover estimates is statistically significant. The corresponding estimates for in-school and out-of-school suspensions are statistically insignificant. Exposure among TIA schools, $TIA \times S^T$, is not statistically significant across the disciplinary outcomes as well. The disciplinary outcomes provide some evidence of spillovers among untreated schools. In particular, $(1-TIA)\times S^U$ is negatively associated with the incidence of any disciplinary action and with DAEP placement, while $(1-TIA)\times S^P$ is positively associated with both outcomes. Taken together, these estimates do not indicate a broad or uniform pattern of spillovers across the disciplinary measures, although the significant coefficients for any disciplinary action and DAEP suggest that some forms of exposure among untreated schools may be worth further examination.

Table 5: Effects of TIA on Advanced Placement and Disciplinary Outcomes

	Advanced Placement (AP)			Disciplinary Actions (DA)			
	(1) Has AP	(2) Parti-cipation	(3) Above critical	(4) Any DA	(5) DAEP	(6) In-school suspension	(7) Out-of-school suspension
TIA	0.006	0.116	-5.511	-0.002	0.002	-0.013	-0.007
	[0.074]	[0.958]	[0.154]	[0.240]	[0.000]	[0.024]	[0.026]
TIA × Vulnerability Score(V_j)	-0.001	-2.912	-1.439	-0.007	0.000	-0.013	-0.012
	[0.716]	[0.172]	[0.685]	[0.000]	[0.843]	[0.010]	[0.000]
TIA × S^T	0.040	-10.373	25.409	-0.034	-0.010	0.024	-0.086
	[0.254]	[0.582]	[0.493]	[0.036]	[0.070]	[0.672]	[0.017]
(1-TIA) × S^U	0.027	-54.594	-51.509	-0.152	-0.048	-0.225	-0.118
	[0.840]	[0.388]	[0.487]	[0.014]	[0.000]	[0.109]	[0.139]
(1-TIA) × S^P	-0.038	43.411	120.146	0.243	0.109	0.058	0.084
	[0.871]	[0.721]	[0.503]	[0.029]	[0.000]	[0.839]	[0.547]
Pre-TIA mean, treated	0.014	70.777	26.925	0.078	0.005	0.151	0.066
Pre-TIA mean, control	0.003	23.447	41.150	0.064	0.006	0.121	0.035
Observations	63,383	1,601	1,277	63,383	63,383	63,383	63,383
Treated schools	34,065	1,000	880	34,065	34,065	34,065	34,065
Control schools	29,318	601	397	29,318	29,318	29,318	29,318
Years	2013–24	2013–24	2013–24	2013–24	2013–24	2013–24	2013–24
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: P-values are reported in brackets. Each column reports a separate regression for school-level outcomes on Advanced Placement and participation in TIA related variables based on the main specification. The outcome in column (1) is an indicator equal to one if the school has students participating in Advanced Placement (AP). Columns (2) and (3) use school-year observations where participation is positive. The outcomes in columns (2)–(7) are measured as percentages of students. Regressions and pre-TIA means are weighted by school enrollment in 2020. All specifications include school and year fixed effects, as well as year interactions with rural status, 2020 enrollment, independent-school status, magnet status, and COVID-19 learning-mode measures. Standard errors are clustered at the district level.

C Effect on Teachers Composition

TIA incentives are directed at teachers. It is therefore important to examine whether schools attract or retain different types of teachers in response to the program, as changes in teacher composition could account for part of the observed increase in test scores. In this section, I examine whether and how TIA affects teacher composition at the school level.

Table 6 examines changes in the racial composition of teachers in TIA schools (columns 1–4), as well as whether TIA schools employ more experienced teachers. The results show that TIA increases the share of Black teachers by approximately 1 percentage point while reducing the share of White teachers. The interactions with school vulnerability provide little evidence that these effects differ systematically across schools with different vulnerability scores. I do, however, find some evidence of spillovers in the share of White teachers. TIA schools exposed to related schools with higher vulnerability scores experience an increase in the share of White teachers, whereas untreated schools exposed to these schools experience a decrease.

For the second set of results in Table 6, I find no evidence that TIA changes student-to-teacher ratios, which could affect the amount of instructional attention students receive, or base salaries, which exclude TIA bonuses. I do, however, find an increase in the average experience of teachers in TIA schools. This increase is primarily driven by greater within-district experience and is larger in schools with higher vulnerability scores. This pattern is consistent with the increase in experience operating primarily through greater teacher retention within the district. Reassuringly, I find no evidence of spillover effects on teacher experience, which might otherwise indicate that the observed changes are driven by transfers of experienced teachers between schools with different treatment status.

A natural follow-up question is whether the increase in teacher experience can account for the improvement in student achievement. The magnitude of the change in experience suggests that this mechanism alone is unlikely to explain much of the estimated achievement effect. TIA increases average teacher experience by approximately 0.14 years. Even using relatively large estimates of the returns to teacher experience from the existing literature, the implied effect on student achievement would be small relative to the approximately 0.04 standard deviation increase in test scores documented above. Thus, while the shift toward a more experienced teaching workforce may contribute to the achievement effects of TIA, it is unlikely to account for most of them.

Table 6: Effects of TIA on Teachers Composition

	Share of Teachers by Race				School Level Average Teachers Outcomes			
	(1) Black	(2) Hisp.	(3) White	(4) Other	(5) Students to Teachers Ratio	(6) Salary	(7) In District Experience	(8) Overall Experience
TIA	1.081	0.326	-1.405	-0.001	-0.102	303.969	0.141	0.177
	[0.004]	[0.187]	[0.001]	[0.995]	[0.225]	[0.193]	[0.083]	[0.091]
TIA × Vulnerability Score(V_j)	-0.313	-0.144	0.466	-0.011	0.000	128.178	0.135	0.103
	[0.264]	[0.499]	[0.148]	[0.952]	[0.994]	[0.528]	[0.049]	[0.256]
TIA × S^T	-3.817	-2.793	7.674	-1.036	-0.039	1494.156	0.498	-0.162
	[0.239]	[0.365]	[0.060]	[0.598]	[0.966]	[0.506]	[0.539]	[0.883]
(1-TIA) × S^U	-7.349	-10.737	18.555	-0.454	-1.418	-7137.691	1.372	-1.592
	[0.422]	[0.129]	[0.111]	[0.882]	[0.522]	[0.120]	[0.444]	[0.526]
(1-TIA) × S^P	30.402	34.630	-66.521	1.450	1.130	22846.694	2.062	8.968
	[0.123]	[0.014]	[0.008]	[0.811]	[0.781]	[0.014]	[0.573]	[0.073]
Pre-TIA mean, treated	10.208	31.670	55.089	3.034	15.769	53908.125	7.824	10.709
Pre-TIA mean, control	7.045	29.393	60.035	3.527	15.168	54570.774	8.243	11.407
Observations	59,194	59,194	59,194	59,194	59,194	59,194	59,194	59,194
Treated schools	31,174	31,174	31,174	31,174	31,174	31,174	31,174	31,174
Control schools	28,020	28,020	28,020	28,020	28,020	28,020	28,020	28,020
Years	2013–24	2013–24	2013–24	2013–24	2013–24	2013–24	2013–24	2013–24
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: P-values are reported in brackets. Each column reports a separate regression for school-by-year observations reflecting teacher-related characteristics. Outcomes in columns (1) to (4) are the percentage of teachers reported to be of a certain race/ethnicity; results for columns (5) to (8) are average teacher characteristics typically associated with student performance: the number of students per teacher (student-to-teacher ratio), base salary, and experience in the district and overall experience.

One potential explanation for the increase in student achievement under TIA is that the program changes the composition of students or teachers in treated schools. In particular, the larger effects observed in more vulnerable schools could partly reflect changes in observable characteristics that are themselves associated with achievement. To assess the importance of this channel, Table 7 compares the estimated effects of TIA on actual test scores with its effects on predicted test scores. Predicted scores capture the component of achievement associated with observable student and teacher characteristics. The model is estimated using only pre-treatment observations, and I predict results for pre- and post- treatment years based on those results using Lasso. Restricting estimation to pre-treatment years limits concerns that treatment-induced changes in these characteristics affect the estimated prediction model, while Lasso regularization reduces overfitting. Thus, an increase in predicted achievement after TIA would indicate that the observable composition of treated schools changed in a direction that, based on pre-TIA relationships, would be expected to increase achievement.

The results provide little evidence that changes in observable composition account for an important part of the average achievement effects of TIA. The main specification implies average effects of 0.065 standard deviations in mathematics and 0.057 standard deviations in reading for schools at the average level of vulnerability scores. In contrast, the corresponding effects on predicted achievement are only 0.002 standard deviations in both subjects and are statistically indistinguishable from zero. The comparison is somewhat different when considering heterogeneity by school vulnerability. The estimated interaction between TIA and the vulnerability score is 0.049 for actual mathematics achievement, compared with 0.021 for predicted mathematics achievement. For reading, the corresponding coefficients are 0.041 and 0.026. Figure 3 illustrates the implication of these estimates. The black lines show the predicted achievement associated with observable student and teacher characteristics, while the colored lines show actual achievement effects. Both tend to increase with school vulnerability, indicating that some of the stronger effects of TIA in high-vulnerability schools coincide with changes in observable composition that would predict higher achievement. However, the slope for actual achievement is considerably steeper, particularly in mathematics. Observable compositional changes therefore appear to account for only part of the relationship between vulnerability and the effects of TIA; for places with vulnerability scores where predicted scores are positively affected by TIA, the effect explained by the predicted score is not higher than 30% of the total effect on test scores in math and 40 % in reading. The spillover effects on predicted scores in untreated schools are similar in magnitude to those in the main results. Some of these effects are statistically significant. However, at this stage, I treat predicted scores as observed rather than estimated outcomes, and the standard errors therefore do not account for uncertainty from the prediction stage. I consequently interpret their statistical significance with caution.

These results are useful for interpreting the heterogeneous effects documented above. TIA appears to have changed the composition of high-vulnerability schools in ways that are positively associated with achievement—for example, through changes in teacher or student characteristics—but

these changes are not sufficient to explain the overall pattern. The gap between actual and predicted achievement becomes larger as vulnerability increases, suggesting that a substantial portion of the differential achievement gains operates through channels not captured by these observable characteristics. These may include changes in teacher effort or productivity, improvements in retention or matching that are not fully summarized by the included characteristics, or other responses to the incentive structure. Because predicted achievement is an estimated outcome and the exercise is descriptive rather than a formal decomposition, these results should be interpreted as evidence on the plausibility of observable compositional change as a mechanism rather than as a precise estimate of its contribution.

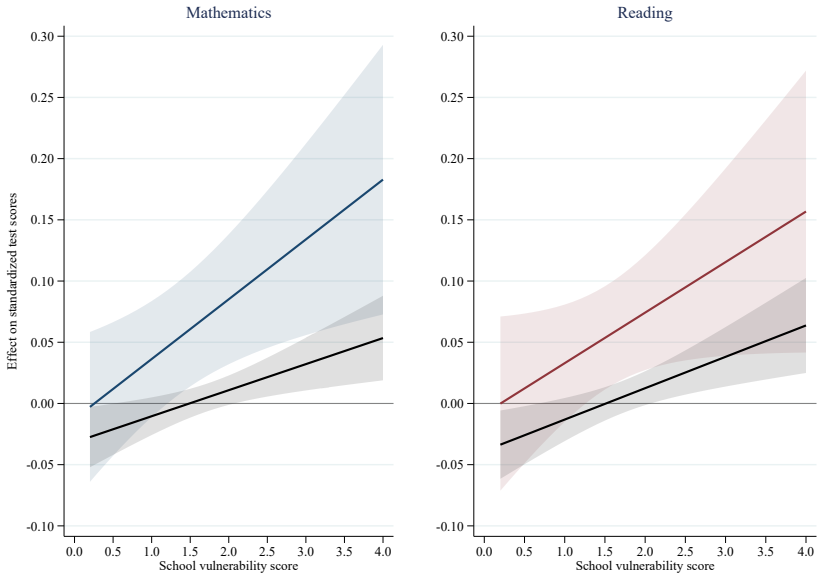


Figure 3: Estimated Linear Heterogeneous Effects of TIA on Student Achievement

Notes: The figure reports estimates of the effect of TIA on standardized test scores and predicted test scores based on observable student and teacher characteristics. Points represent estimated coefficients calculated as $\tau(v) = \beta_0 + \beta_1 \times TIA \times (v - \bar{V})$, where v takes values between 0 and 4. Shaded areas report 90 and 95 % confidence intervals where I test whether $\tau(v) = 0$ for each value of v. Because predicted outcomes are treated as observed rather than estimated, they do not account for uncertainty from the prediction stage and therefore likely understate statistical uncertainty.

VII Effect on Test Scores after 4 years and Cost-Effectiveness

While the preceding analysis uses all available treatment cohorts to estimate the average treatment effect on the treated (ATET) for schools that have implemented the program so far, the resulting estimate averages across school districts that have been exposed to the program for different lengths of time. This makes it difficult to interpret the estimate as the effect of TIA after a specific number of years of implementation. To support this interpretation, I restrict the sample to never-treated schools and schools in districts that have implemented TIA for at least four years. I then estimate

Table 7: Effects of TIA on Student Achievement and Predicted Achievement Based on Teachers and Students Characteristics

	Math	Predicted Math	Reading	Predicted Reading
	(1)	(2)	(3)	(4)
(5)	(6)			
TIA	0.065	0.055	0.002	0.051
0.057	0.002			
	[0.008]	[0.017]	[0.750]	[0.025]
[0.008]	[0.801]			
TIA × Vulnerability Score(V_j)	0.049	0.032	0.021	0.023
0.041	0.026			
	[0.009]	[0.068]	[0.004]	[0.310]
[0.062]	[0.002]			
TIA × S^T	-0.425	-0.242	0.030	-0.108
-0.328	0.055			
	[0.047]	[0.337]	[0.607]	[0.717]
[0.196]	[0.420]			
(1-TIA) × S^U	0.467	-0.410	0.437	0.026
0.748	0.475			
	[0.372]	[0.430]	[0.000]	[0.958]
[0.140]	[0.001]			
(1-TIA) × S^P	0.509	2.088	-0.555	1.031
-0.176	-0.590			
	[0.616]	[0.042]	[0.028]	[0.276]
[0.851]	[0.053]			
$PredictedMathScore$		1.003		
		[0.000]		
$PredictedReadingScore$				1.010
				[0.000]
Pre-TIA mean, treated	-0.005	-0.005	-0.001	-0.006
-0.006	-0.001			
Pre-TIA mean, control	0.007	0.006	-0.001	0.007
0.007	-0.002			
Observations	164,796	144,614	144,614	144,605
164,654	144,605			
Treated schools	2,570	2,518	2,518	2,518
2,570	2,518			
Control schools	2,274	2,246	2,246	2,246
2,274	2,246			
Years	2013–25	2013–24	2013–24	2013–24
2013–25	2013–24			
School × grade FE	Yes	Yes	Yes	Yes
Yes	Yes			
Grade × year FE	Yes	Yes	Yes	Yes
Yes	Yes			
Controls	Yes	Yes	Yes	Yes
Yes	Yes			

Notes: P-values are reported in brackets. Each column reports a separate regression for the indicated outcome. The sample includes school-by-grade-by-year observations from schools that have operated under TIA for more than four years and from schools in non-adopting districts. Columns "Math" and "Reading" report estimates from the main specification. Columns "Predicted Math" and "Predicted Reading" use predicted test scores based on observable

the ATET for this restricted sample, providing an estimate of the effect of TIA after four years of implementation among early-adopting districts.

This estimate should be interpreted with caution. In particular, it should not be interpreted as the effect that TIA would have had after four years in districts that adopted later or never adopted the program. As shown in the descriptive analysis, early-adopting districts differ from later adopters and non-adopters along several observable dimensions; they may also differ on unobserved dimensions that could motivate these schools to implement the program first because they expected higher benefits. The exercise therefore identifies the four-year effect for the group of early adopters rather than an effect that can necessarily be generalized to all Texas school districts. Despite this limitation, focusing on a common duration of exposure provides two useful benchmarks. First, it allows me to assess the cost-effectiveness of TIA after four years by comparing the estimated achievement gains with cumulative program expenditures for these schools over the same period. Second, it enables comparisons with other programs and related policies by measuring effects after a clearly defined implementation period. Expressing both impacts and costs over a common four-year horizon makes it possible to compare TIA's cost-effectiveness with that of other educational interventions.

Results are reported in Table 8. As expected, these schools exhibit larger positive effects in both mathematics and reading, as well as stronger heterogeneous effects by school vulnerability. For mathematics, the average treatment effect on the treated at the mean vulnerability score among early adopters is 0.099 standard deviations. The results also indicate a substantial reduction in the achievement gap across schools with different levels of vulnerability: the effect of TIA increases by 0.094 standard deviations for each one-point increase in the vulnerability score. Reading exhibits a similar pattern, with an estimated effect of 0.075 standard deviations at the mean vulnerability score and an additional 0.089 standard deviations for each one-point increase in vulnerability. Columns (2)–(4) show patterns similar to those in the previous analysis. In mathematics, TIA reduces the share of students who only approach the required performance level and increases the share who master the content. In reading, most of the effect is concentrated at the bottom of the achievement distribution, with a reduction in the share of students failing to meet the required performance level.

Most coefficients associated with the spillover measures are statistically insignificant. One exception is the effect on mathematics achievement among untreated schools exposed to treated schools. The estimate implies an increase of approximately 0.027 standard deviations for a one-percentage-point increase in the share of teachers moving to related schools participating in TIA (2.729×0.01). This result suggests that non-adopting schools may benefit from greater exposure to related schools participating in TIA. However, because most estimated spillover effects are statistically insignificant, I interpret this result cautiously rather than as evidence of a systematic spillover effect.

A Cost Effectiveness

To assess the cost-effectiveness of TIA, it is important to account for heterogeneity in both program effects and costs. Schools with higher vulnerability scores may experience larger effects, but they may also face higher program costs because TIA provides larger allotments for designated teachers in more vulnerable schools. Moreover, larger achievement effects could coincide with a greater number of teachers receiving designations and, consequently, higher total expenditures.

Table 9 reports program costs, student enrollment, and estimated treatment effects for four bins of the vulnerability score. For districts that implemented TIA for at least four years, program costs are calculated over the first four years of implementation using the total allotments provided to schools based on teacher designation status. The table also reports cumulative student enrollment over these four years and the resulting program cost per student per year. These expenditures provide a lower bound on the total cost of implementing TIA and may therefore lead the analysis to overstate the program's cost-effectiveness. In particular, school districts may incur additional administrative and implementation costs that are not captured by teacher allotments.

The results show that treatment effects increase substantially with school vulnerability, while costs per student increase at a considerably slower rate. Consequently, the estimated cost-effectiveness of TIA is greater among schools with higher vulnerability scores. For example, considering mathematics achievement, schools with vulnerability scores between 1 and 2 spend approximately $721 per student per year for an effect size of one standard deviation. The corresponding costs are approximately $260 and $336 for schools with vulnerability scores between 2 and 3 and between 3 and 4, respectively. Thus, despite the larger allotments provided to more vulnerable schools, the larger estimated achievement effects more than compensate for their higher costs. These estimates also suggest that TIA compares favorably with the cost-effectiveness of related educational interventions documented in the literature. Such comparisons should be interpreted cautiously, however, because studies may differ in their populations, implementation horizons, definitions of costs, outcome measures, and empirical designs. Moreover, because the cost calculations for TIA exclude administrative and other implementation expenses, comparisons based solely on teacher allotments are likely to present TIA somewhat more favorably than comparisons based on comprehensive program costs.

VIII Conclusion

This paper studies the effects of the Texas Teacher Incentive Allotment (TIA), a large-scale performance-pay program that combines incentives for teacher performance with substantially larger payments in disadvantaged and rural schools. Exploiting the staggered adoption of TIA across Texas school districts, I examine how the program affects student achievement and whether its effects differ across schools with different levels of vulnerability. The design of TIA provides a particularly useful setting for studying whether performance incentives can improve public-service delivery while directing greater resources toward the places facing the greatest challenges.

Table 8: Effects of TIA on Student Achievement After 4 Years of Implementation

Panel A. Mathematics

	(1) Score	(2) Not met	(3) Approaches	(4) Meets	(5) Masters
	(1)	(2)	(3)	(4)	(5)
TIA	0.099	-0.493	-0.942	-0.364	1.664
	[0.018]	[0.384]	[0.001]	[0.225]	[0.000]
TIA × Vulnerability Score(V_j)	0.094	-0.532	-0.623	0.191	0.894
	[0.010]	[0.355]	[0.006]	[0.533]	[0.015]
TIA × S^T	-0.106	2.824	-1.191	0.716	-2.172
	[0.830]	[0.766]	[0.734]	[0.901]	[0.556]
(1-TIA) × S^U	-0.343	30.413	-10.882	-12.050	-3.693
	[0.601]	[0.004]	[0.026]	[0.057]	[0.619]
(1-TIA) × S^P	2.729	-52.363	-2.621	15.236	32.796
	[0.042]	[0.022]	[0.789]	[0.263]	[0.031]
Pre-TIA mean, treated	-0.272	29.626	31.353	21.474	17.316
Pre-TIA mean, control	0.216	22.207	29.720	24.547	23.163
Observations	91,243	65,962	65,962	65,962	65,962
Treated schools	606	606	606	606	606
Control schools	2,274	2,274	2,274	2,274	2,274
Years	2013–24	2016–24	2016–24	2016–24	2016–24
School × grade FE	Yes	Yes	Yes	Yes	Yes
Grade × year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Panel B. Reading

	(1) Score	(2) Not met	(3) Approaches	(4) Meets	(5) Masters
	(1)	(2)	(3)	(4)	(5)
TIA	0.075	-2.036	1.240	0.953	-0.293
	[0.013]	[0.000]	[0.000]	[0.000]	[0.487]
TIA × Vulnerability Score(V_j)	0.089	-1.855	1.356	0.828	-0.388
	[0.054]	[0.002]	[0.000]	[0.006]	[0.351]
TIA × S^T	0.124	-0.136	-3.784	4.421	-0.298
	[0.827]	[0.987]	[0.245]	[0.179]	[0.954]
(1-TIA) × S^U	0.069	5.219	13.863	-2.980	-12.850
	[0.912]	[0.505]	[0.031]	[0.564]	[0.141]
(1-TIA) × S^P	1.422	-58.606	13.701	34.881	3.729
	[0.244]	[0.000]	[0.274]	[0.001]	[0.833]
Pre-TIA mean, treated	-0.396	35.294	28.802	17.138	18.618
Pre-TIA mean, control	0.237	26.541	27.927	19.887	25.362
Observations	92,351	66,758	66,758	66,758	66,758
Treated schools	606	606	606	606	606
Control schools	2,272	2,272	2,272	2,272	2,272
Years	2013–24	2016–24	2016–24	2016–24	2016–24
School × grade FE	Yes	Yes	Yes	Yes	Yes
Grade × year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Notes: P-values reported in brackets. Each column reports a separate regression for different outcomes. This sample contains only school-by-grade-by-year observations for schools with more than 4 years under the TIA regime, or non-adopters. For the adopters included in the regression, I drop observations more than four years into TIA. In columns labeled "Score", the dependent variable is the school-by-grade-level score on STAAR tests for grades 3 to 8, standardized within year and grade level; all other outcomes are measured in percentage points. Regressions and pre-TIA means are weighted by the number of students taking the corresponding STAAR assessment. All specifications include school-by-grade and grade-by-year fixed effects, as well as year interactions with rural status, 2020 enrollment, independent-school status, magnet status, and COVID-19 learning-mode measures. Standard errors are clustered at the district level.

Table 9: Costs and Effects of TIA by School Vulnerability

	Cost	Enrolledment	Cost per Student per Year	Effect Math	Effect Reading
[0-1]	17,217,144	474,735	36.27	0.0203	-0.0293
(1-2]	29,835,998	626,157	47.65	0.0663	0.0884
(2-3]	36,722,680	639,916	57.39	0.2215	0.1587
(3-4]	17,521,506	223,505	78.39	0.2333	0.2225

Notes: This table reports program costs and estimated effects on student achievement by school vulnerability score. The sample is restricted to schools in districts that implemented TIA for at least four years. Vulnerability-score intervals are reported in the first column. Cost is the total amount allocated to schools through TIA teacher designations during the first four years of implementation. Enrollment is cumulative student enrollment over the same four-year period. Cost per student per year is calculated as total TIA expenditures divided by cumulative enrollment. Effects in mathematics and reading report the estimated average treatment effects on the treated over the four-year period, in standard deviations. Program costs exclude districts' own administrative and other implementation expenses and therefore provide a lower bound on the total cost of TIA.

I find that TIA improves student achievement in both mathematics and reading. At the average school vulnerability score, the program increases standardized test scores by 0.065 standard deviations in mathematics and 0.057 standard deviations in reading. The effects become larger as schools become more vulnerable: a one-point increase in the vulnerability score is associated with an additional treatment effect of 0.049 standard deviations in mathematics and 0.041 standard deviations in reading. Relative to pre-program differences in achievement across schools, these estimates imply meaningful reductions in the achievement gap associated with school vulnerability. The distributional effects within the achievement distribution also differ somewhat across subjects. In mathematics, the program increases the share of students reaching the highest performance category, while in reading the improvements are concentrated more heavily among students at the bottom of the achievement distribution. The event-study estimates provide little evidence of differential pre-trends and indicate that achievement effects increase during the years following implementation.

The effects of TIA extend beyond standardized test scores. I find little evidence of changes in Advanced Placement participation or performance, but the results indicate reductions in several disciplinary outcomes, particularly in more vulnerable schools. The program is associated with lower rates of in-school and out-of-school suspension, although the results for assignment to Disciplinary Alternative Education Programs differ from those for other disciplinary measures. These findings suggest that the effects observed in test scores are accompanied by changes in some other dimensions of student outcomes, although the evidence is not uniform across measures.

The results also point to changes in the composition of the teacher workforce. TIA increases average teacher experience, with much of this increase coming from greater within-district experience and with larger changes in more vulnerable schools. This pattern is consistent with improved teacher retention contributing to the effects of the program. At the same time, the magnitude of the increase in experience is too small to account for most of the estimated improvement in student achievement using conventional estimates of the returns to teacher experience. Changes in workforce composition therefore appear to be one mechanism through which TIA operates, but

they are unlikely to provide a complete explanation for the achievement gains.

An important feature of the analysis is that TIA can potentially affect schools that do not themselves participate in the program. Because designated teachers retain their status when moving across schools and the value associated with a designation varies with school vulnerability, TIA changes the relative attractiveness of schools within the teacher labor market. I explicitly allow for such spillovers using pre-policy teacher mobility patterns to identify schools that represent relevant outside options. Overall, the results provide limited evidence of systematic spillovers on achievement, particularly among untreated schools. This finding provides some reassurance that indirect exposure to TIA does not substantially contaminate the comparison group used to identify the program's direct effects.

Finally, the results for districts observed for at least four years indicate that the effects of TIA remain substantial as the program matures and are particularly large in high-vulnerability schools. Combining these estimates with program expenditures shows that, although TIA spends more per student in more vulnerable schools, achievement effects increase faster than costs. The program is therefore more cost-effective in the schools toward which its funding formula directs the largest incentives. These comparisons should be interpreted cautiously because the expenditure measure does not capture all administrative and implementation costs and because comparisons with other educational interventions depend on differences in populations, outcomes, and program design.

Taken together, the findings highlight an important feature of the design of performance-pay policies. Performance incentives need not generate a trade-off between improving average outcomes and reducing disparities across locations. TIA combines rewards for individual teacher performance with a compensation schedule that directs larger financial incentives toward disadvantaged schools. In this setting, achievement increases on average while the largest gains occur in schools serving more vulnerable students. The results therefore suggest that the distributional consequences of performance pay depend importantly on how incentives are designed and targeted. More broadly, they show how compensation systems in the public sector can be structured not only to reward performance, but also to direct incentives toward locations where improvements in public-service delivery are particularly valuable.

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A Designation details

The Teacher Incentive Allotment (TIA) is a Texas initiative that awards high-performing teachers one of three designation levels – Recognized, Exemplary, or Master – which correspond to significant annual stipend allotments for their district (ranging roughly from $3,000 up to $32,000 depending on designation and campus factors). Each TIA district must design a local designation system that evaluates teacher effectiveness based on two components: classroom teacher observation and student growth. (Districts may include additional optional measures like student surveys, teacher leadership roles, etc.) Each participating district's system must be approved by the Texas Education Agency (TEA), and designations are valid for five years. Teachers with National Board Certification automatically earn a Recognized designation.

Components

Teacher Observation: Most districts use the Texas Teacher Evaluation and Support System (T-TESS) rubric for the observation component. T-TESS includes four domains with 16 dimensions, scored 1–5 from Improvement Needed (1) to Distinguished (5). A teacher observation score is typically the average of these eight dimensions' ratings. For example, Clyde CISD notes that the summative teacher observation score is the average across the eight T-TESS dimensions in Domains 2 & 3 (each dimension is worth up to 5 points). To be eligible for any designation, TEA requires a minimum rating of "Proficient" (level 3) on all 8 of these dimensions, this meas that no area can be below proficient.

Student Growth: Districts have flexibility in choosing valid student growth measures for each teacher group. Common growth measures include value-added models (VAM) using state STAAR exams, Student Learning Objectives (SLOs), pre- and post-tests, or portfolios. The student growth component for a teacher is usually expressed as the *"percentage of that teacher students who met or exceeded an expected growth target''* on the chosen assessment. For example, TEA's statewide growth standard defines this as the percent of students who achieved or surpassed their predicted score (in a STAAR-based growth model). Districts ensure their growth measures are reliable and aligned to standards, and they train staff to administer and score them consistently.

Weighting of Observation vs. Growth in District Plans

Each TEA-approved local TIA plan must specify how the teacher observation score and student growth results are **weighted** to produce an overall TIA composite score or designation score. The state allows districts to determine the weighting, provided both components are significant. In practice, districts use weightings ranging from about 20% up to 50% for the observation, with the remainder (50%–80%) for student growth. Here are some examples:

- Clyde CISD: Teacher Observation 20% / Student Growth 80%.
- Galena Park ISD: Teacher Observation 30% / Student Growth 70%.
- Lampasas ISD: Teacher Observation 30% / Student Growth 70%.
- Ingram ISD: Teacher Observation 40% / Student Growth 60%.

- Seguin ISD: Teacher Observation 50% / Student Growth 50%.

Despite varying weights, all plans must demonstrate a strong correlation between high observation ratings and high student growth – a validity check so that, for example, a teacher with stellar observations but low student growth (or vice versa) wouldn't be inappropriately designated. Some districts also incorporate a small percentage for "additional factors." For instance, Rice ISD's plan adds a fixed 10-point credit for high teacher attendance.

Eligible assignments

Each district can decide which assignments (subjects and grade levels) can be evaluated by the TIA system. Typically, districts choose core subjects for all grade levels. Here is an example of the teaching assignments and tools used for evaluation in Rice ISD:

Figure 4: Teaching Assignments and instruments, Rice ISD

Grade Level/Teaching Assignment	Teacher Observation	Who sets Expected Growth Target? How?	Student Growth (Pre-/Post-Tests)
Phase 1			
Pre-K	T-TESS	District (Graduated Percent Increase)	Circle/CLI
4th-8th Math and Reading, Algebra I, English I and English II (including SPED)	T-TESS	STAAR Transition Tables	STAAR
3rd Reading, 3rd Math, 5th Science, 8th Science, 8th SS, US History, Biology (including SPED)	T-TESS	District (Graduated Percent Increase)	BOY Released STAAR/EOY STAAR
K-2 Reading/Math (including SPED)	T-TESS	mCLASS	mCLASS
Intro to Culinary, Principles of Ag/Food/Natural Resources, Principles of Health Sci, Principles of Arts/Audio/Visual Tech	T-TESS	District (Graduated Percent Increase)	YouScience/ Precision Exams
SPED: Life Skills (Grades 4-11)	T-TESS	STAAR Transition Tables	STAAR-ALT
*All RCISD teachers eligible for designation through the National Board Certification			
Phase 2			
BIM	T-TESS	District (Graduated Percent Increase)	YouScience/ Precision Exams
Cosmetology, Physics, Spanish I, Chemistry, Algebra II, and Geometry	T-TESS	District (Graduated Percent Increase)	District-created

Source: Teacher Incentive Allotment (TIA), Teacher Overview 2025-26, Rice Independent School District.

Scoring Rubrics and Cut Score Thresholds for Designations

After combining the observation and growth components according to the chosen weights, teachers receive a composite TIA score (often on a 5-point scale or converted point system). Each district sets cut score thresholds to determine which designation (if any) a teacher earns. TEA provides statewide performance standards as a reference, derived from statewide data, to guide these cut points. The statewide minimum standards for designation are:

- Recognized: ∼3.7 average on T-TESS (Domains 2&3) and 55% of students meeting growth targets.
- Exemplary: ∼3.9 average on T-TESS and 60% of students meeting growth.
- Master: ∼4.5 average on T-TESS and 70% of students meeting growth.

These figures correspond to roughly the top 33% (Recognized), 20% (Exemplary), and 5% (Master) of teachers statewide on each metric. Districts are not forced to use these exact numbers, but teachers must meet at least the minimum Proficient on all observation dimensions (as noted) and generally meet or exceed these growth percentages to be designated. In practice, many districts do adopt or stay close to the state's benchmarks. That said, some districts choose more stringent local criteria. For instance, Galena Park ISD fixed much higher student growth cut-offs: 70% of students growing for Recognized, 80% for Exemplary, and 90% for Master– far above the state minimums. This reflects a local decision to limit designations to truly exceptional growth outcomes in their context. Each district analyzes its own data distribution to finalize cut scores, and TEA allows that the percentage of teachers designated can vary by district. Score Calculation Examples: In TIA systems, a teacher final score is calculated by applying the weights to their observation and growth results, then comparing to the cut scores. A simple example from Lampasas ISD: a teacher with a 4.48 T-TESS average and a 4.25 growth score (on a 5-point scale) would have a composite of $4.48 \times 30\% + 4.25 \times 70\% = 4.319$. This falls above the Exemplary cut (3.9) but below Master (4.5), resulting in an "Exemplary" designation recommendation.

Visualizing Performance Scoring and Designation Distribution

District TIA plans often include tables or scorecards to help teachers understand how the two components translate into a designation. These scorecards lay out the points or ranges needed. For instance, Rice CISD published a composite score chart: a Recognized teacher needed a total score ∼67–71, Exemplary ∼71–81, and Master∼ 81+ (out of 100). Those composite scores were the sum of Teacher Observation points (T-TESS average × 8), Student Growth points (% of students meeting growth × 50), plus any attendance points. In that model a teacher meeting the minimum Recognized criteria (3.7 avg and 55% growth) would score around 67 points total, just at the Recognized cut. These kinds of tables help visualize how each component contributes to the overall designation. Such breakdowns make it clear that a balance of strong teaching practices and strong student outcomes is required. If a teacher excels in observations but only a minority of students meet growth, they likely won't reach the designation cut. Conversely, high growth alone isn't sufficient without at least proficient-level instruction in the classroom (per the observation ratings requirement). By design, only teachers who demonstrate consistent excellence in both teaching practice and student progress earn TIA designations. Each approved district plan includes detailed rubrics and data validation steps to ensure the scoring is fair and credible.