Salary Increases and Skills-Based Bonuses: Teacher Retention and Workforce Composition

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Abstract

This paper studies the interaction between unconditional salary increases and performance-based bonuses in determining teacher retention and workforce composition. I exploit Chile's *Carrera Docente*, a national reform that raised base salaries by approximately 20% for all public school teachers and introduced a career stage bonus linked to evaluation scores. Because the reform initially applied only to public schools, roughly half of the school system, I use a difference-in-differences design and find that exit rates fell by approximately 18%, but the effect is concentrated among low-value-added teachers. A regression discontinuity design at the career stage threshold shows that the performance-based bonus has no additional retention effect beyond the base salary increase. The retention gains of the reform are driven entirely by the base salary increase, with no additional contribution from the performance-based bonus.

Keywords: teacher retention, teacher value-added, career ladder, performance pay, salary structure

JEL codes: I21, J31, J45, M52

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1 Introduction

Teacher shortages are a growing concern across both developed and developing countries (UNESCO, 2023). In the United States, approximately 8% of public school teachers leave the profession each year (National Center for Education Statistics, 2023), with comparable or higher rates in England and other OECD countries (OECD, 2025). This attrition is costly: teacher turnover disrupts student learning, particularly in disadvantaged schools (Ronfeldt et al., 2013), and replacing departing teachers is expensive for school systems that must recruit, hire, and train new staff (Ingersoll, 2001). At the same time, a large body of evidence shows that teachers vary substantially in their effectiveness (Rivkin et al., 2005; Chetty et al., 2014; Jackson, 2018), making it important not only to retain teachers in general but to understand which teachers are retained and which are lost.

Raising teacher compensation is a natural policy response, but its effect on the composition of the workforce depends on how the raise is designed. If compensation increases uniformly, retention improves for whoever is closest to leaving, regardless of their effectiveness. If instead part of the raise is tied to evaluated performance, it creates a wage gradient that favors better teachers, provided that evaluations capture actual teaching effectiveness. Combining a uniform raise with a performance-based component is therefore appealing: the former ensures broad retention, the latter tilts the composition toward quality. The interaction between the two components, however, may work against this logic. If utility over income is concave, the performance gradient loses its bite precisely because it is layered on top of an already large base raise, placing recipients on a flatter portion of their utility function where additional income buys less retention. The retention response then concentrates among teachers who are near-indifferent between staying and leaving, and whether these marginal teachers are of high or low ability is an empirical question.

I study this question using a natural experiment provided by Chile's *Carrera Docente*, a career ladder enacted in 2016 that restructured the compensation of public school teachers around a five-tier system.[1] The reform introduced an unconditional baseline salary increase of approximately 20% for all public school teachers, alongside a career stage bonus that depends on scores in two mandatory evaluations (a teaching portfolio and a subject-knowledge exam) and on accumulated experience. The reform was initially restricted to public schools, while subsidized (voucher) schools remained outside the system during the analysis period, providing a natural comparison group. At the time of the reform, the Chilean school system was roughly evenly split between public and subsidized schools, and the first cohort of voucher schools entered the career system only in July 2019. I exploit

[1]The closest comparators to the Chilean reform are Colombia's merit-based teacher career system (Busso et al., 2024), Peru's *Carrera Pública Magisterial* (Tournier and Chimier, 2019), and Mexico's *Carrera Magisterial* (Santibáñez et al., 2007). In the United States, Tennessee operated a statewide career ladder from 1984 to 1997 (Dee and Keys, 2004), and Washington D.C.'s IMPACT program links teacher pay and career progression to evaluation scores (Dee and Wyckoff, 2015). Singapore operates a multi-track career ladder in which advancement is based on principal-led qualitative assessments rather than standardized instruments.

this differential exposure, together with the cutoff scores on the two mandatory evaluations that determine advancement across career stages, to separately identify the retention effects of the two components.

Using administrative records covering the universe of primary and secondary school teachers in Chile over the period 2012–2018 (approximately 170,000 unique teachers and 830,000 teacher-year observations), I estimate the overall effect of the reform on teacher exit in a difference-in-differences design that compares public school teachers to their counterparts in subsidized (voucher) schools, which remained outside the career system during the analysis period. I measure attrition using a five-year persistent definition: a teacher is classified as having exited only if she is absent from the data for five consecutive years, which avoids conflating permanent departures with temporary absences such as parental leave. The event study estimates show no differential trend before the reform and a growing reduction in exit rates afterward. By 2018, public school teachers are 0.7 percentage points less likely to exit relative to the pre-reform baseline, a decline of approximately 18%. Using imputed wages from the statutory salary schedule, this implies an elasticity of exit with respect to wages of approximately -0.9, similar to existing estimates (Falch, 2011; Hendricks, 2014).

To understand which teachers are more likely to be retained, I develop a simple model of teacher exit decisions. The model captures the two-component structure of the reform: a uniform base salary increase for all public teachers and a career stage bonus that depends on evaluation scores, which are a noisy measure of teaching ability. Teachers have concave utility over income and differ in their outside options and intrinsic motivation for teaching. The retention response concentrates among teachers closest to the exit margin, but who these marginal teachers are is ambiguous: if outside options increase sufficiently with ability, high-ability teachers are closer to exit and respond more; if instead more effective teachers derive greater intrinsic satisfaction from teaching, low-ability teachers are at the margin and benefit most. Moreover, because evaluations are positively correlated with ability, high-ability teachers are more likely to receive the career stage bonus, which should favor their retention. However, concavity works against this: the bonus is layered on top of the base raise, placing recipients on a flatter portion of their utility function where additional income buys less utility.

To measure ability empirically, I estimate teacher value-added (TVA) from standardized test scores in mathematics and reading, linking teachers to the universe of their students. The estimation follows a standard value-added model in which current test scores are a function of lagged scores, student and classroom characteristics (class size, gender composition, parental education and income). I use each teacher's most recent pre-reform TVA as a fixed measure of ability, which is available for approximately 15% of the pre-reform analysis sample, corresponding to about 19,000 teachers. Within this subsample, I estimate a negative effect on exit that is however concentrated among low-TVA teachers: their exit rate falls by 1.09 percentage points, while the effect for high-TVA teachers is indistinguishable from zero. To shed light on which channels drive this pattern, I test two predictions of the model. On the one hand, TVA is positively correlated with evaluation scores (a one standard deviation increase in Portfolio score is associated with a 0.03 standard deviation increase in TVA), confirming that high-ability teachers can expect larger career stage bonuses. On the other hand, low-ability teachers have higher pre-reform exit rates, placing them closer to the exit margin. The higher expected bonus therefore does not make high-ability teachers more responsive to the reform. A remaining question, important for the design of the career ladder, is whether this is despite the bonus having some retention effect, or whether the bonus has no effect at all.

I test this directly using a regression discontinuity design. Under the career system, teachers advance through stages based on whether their evaluation scores cross predetermined thresholds. Evaluations take place on a four-year cycle, so that roughly one quarter of the workforce is assessed each year; the rotation schedule is predetermined, so evaluated teachers do not self-select. I show that the evaluated sample is comparable to the overall population in observables. While in principle one could exploit any stage transition, not all thresholds offer a large compensation jump or a sufficient mass of teachers around the cutoff, since higher stages are difficult to achieve. I therefore focus on the transition from the second to the third stage, where the majority of teachers concentrate and where most will remain for the bulk of their career, and which carries an approximately 10% salary increase. Because career stage assignment depends on two evaluation scores (teaching portfolio and subject-knowledge exam), I use the teaching portfolio score as the running variable, recentered at the relevant threshold conditional on the other score. Most teachers concentrate around this cutoff, so the estimates capture the retention response of average performers. Using approximately 17,000 teachers evaluated in 2016 and 2017, I find a precisely estimated null effect of reaching a higher career stage on teacher exit. Taken together, these results suggest that the retention gains of the reform are driven by the uniform base salary increase, and that the skill-based component of the career ladder does not contribute to retention at the margin.

Related literature. This paper contributes to several strands of the literature. First, it speaks to the large body of work on teacher compensation and retention. Many papers find that higher salaries reduce teacher turnover, though the magnitude of the response varies across contexts, the source of the pay increase, and the identification strategy used. Elasticities are systematically higher when bonuses target teachers in hard-to-retain schools, in the range of -3 to -8 (Clotfelter et al., 2008; Springer et al., 2016; Tartova, 2025; Cowan and Goldhaber, 2018; Cabrera and Webbink, 2020; Camelo and Ponczek, 2021). When estimates exploit variation in wages or outside options, capturing a more representative group of teachers, the implied elasticities are lower, in the range of -1 to -1.5 (Falch, 2011; Hendricks, 2014), and closer to the estimate I obtain. I contribute to this literature by studying a national reform that restructured compensation for the entire public teaching workforce, providing a retention elasticity that is directly relevant for the design of large-scale salary policies.[2]

Second, this paper relates to the literature on incentive contracts for public sector workers, and teachers in particular (Finan et al., 2017). A growing body of work studies the design of teacher performance pay, distinguishing between output-based incentives tied to student test scores (Lavy, 2009; Duflo et al., 2012; Muralidharan and Sundararaman, 2011; Leaver et al., 2021) and input-based incentives that reward demonstrated teaching competence (Taylor and Tyler, 2012; Araya-Córdova et al., 2025). A key motivation for the latter is that tying pay to test scores may distort effort away from less measurable but equally valuable dimensions of teaching (Holmstrom and Milgrom, 1991). Career ladders, in which advancement depends on evaluated teaching quality, represent a prominent form of input-based incentive. I study such a system and show that the evaluations used for career advancement are positively correlated with teacher value-added, confirming that the career ladder does identify more effective teachers. However, the additional bonus associated with reaching a higher career stage has no detectable effect on retention, providing direct evidence that the salary-retention relationship is non-linear.[3][4] This suggests that while evaluation-based career systems may incentivize effort and professional development (Taylor and Tyler, 2012; Araya-Córdova et al., 2025), policymakers should not expect the performance-based component to deliver additional retention benefits when accompanied by a substantial unconditional raise.

Third, this paper contributes to the literature on the interaction between wage compensation and teaching quality. A large body of work shows that performance pay increases teacher effort (Lavy, 2009; Duflo et al., 2012; Muralidharan and Sundararaman, 2011; Taylor and Tyler, 2012; Araya-Córdova et al., 2025) and can attract and retain more effective teachers (Biasi, 2021; Dee and Wyckoff, 2015; Johnston, 2025). Since the reform combines a baseline salary increase with a performance-based bonus,[5] and I show that the bonus has no additional retention effect, the paper also relates more closely to the literature on the impact of unconditional wage increases on student outcomes (De Ree et al., 2018; Bobba et al.,

[2]A complementary literature studies how labor market conditions affect the quality and flow of teacher entrants rather than the retention of incumbents (Bacolod, 2007; Deneault, 2025; Fraenkel, 2022; Nagler et al., 2020).

[3]Indirect evidence consistent with non-linearity includes the declining turnover elasticity with experience documented by Hendricks (2014) and the similar retention effects of bonuses of very different sizes across contexts (Springer et al., 2016; Clotfelter et al., 2008).

[4]Berlinski and Ramos (2020) also use a regression discontinuity design to study the retention effect of a skill-based bonus in Chile (the AEP award), finding no effect. Their setting differs from mine in that the AEP targets top-performing teachers, a selected population for whom the additional wage premium may have limited marginal value.

[5]The evaluations used for career advancement are positively correlated with teacher value-added, connecting this paper to the literature on the determinants of teaching effectiveness and whether evaluation instruments capture it (Rockoff et al., 2011; Chetty et al., 2014; Bacher-Hicks, 2022).

2021), where the evidence suggests that raising pay uniformly does not by itself improve performance. I contribute to this literature by focusing on the retention margin: whether the composition of the teaching workforce in terms of quality changes when compensation increases.[6] I find that the reform disproportionately retains low-value-added teachers. This contrasts with the closest related paper, Tartova (2025), who studies location-specific bonuses for teachers in disadvantaged schools in France and finds that the retention effect is concentrated among high-value-added teachers. The key difference lies in the type of teacher studied: her compliers are teachers in hard-to-staff schools, while my estimates apply to the average public school teacher.[7]

Road map. The remainder of the paper is organized as follows. Section 2 describes the Chilean school system and the Carrera Docente reform. Section 3 develops a simple conceptual framework to guide the empirical analysis. Section 4 presents the data and the construction of the analysis samples. Section 5 estimates the overall effect of the reform on teacher retention and examines heterogeneity by teacher value-added. Section 6 tests whether the career stage bonus contributes to retention at the margin. Section 7 concludes.

2 The Teaching Career

In this section, I describe the main features of the teaching profession in Chile before the introduction of the new salary scheme, and how these features changed under the new system. I focus on aspects of the previous setup that help to understand how the scheme may affect teachers' decisions to stay in the profession.

Pay Scheme. Teachers used to earn substantially lower wages than individuals with comparable educational attainment employed in other sectors. Although this pattern is observed in the majority of OECD countries (OECD, 2023), it was particularly pronounced in the Chilean context. For example, Tincani (2021) argues that, conditional on education, earnings outside the teaching profession were approximately 60% higher.[8]

The mixed nature of Chile's school system, with coexisting public and private providers, introduces some variation in teacher compensation across school types. Schools in Chile can be broadly categorized into three types based on their funding and governance: public,

[6] An unconditional salary increase may also affect the quality of teacher entrants. See Dal Bó et al. (2013) and Nagler et al. (2020) for evidence on how compensation levels shape the quality of candidates entering public sector positions.

[7] Bates and Johnston (2025) also find no differential retention by quality, though their variation comes from pension changes, which primarily affect teachers near retirement age, while mine comes from salary changes that shift the incentive to stay for teachers across the entire career.

[8] Therefore, it is not surprising that the teaching career was relatively unattractive to university students. In fact, they tended to be negatively selected in terms of academic performance, as measured by university admission standardized test scores: students with a larger set of options regarding university programs were significantly less likely to choose a career in teaching (Gallegos et al., 2022; De Falco et al., 2024)

private subsidized (or voucher), and private institutions. In 2014, the teaching workforce was distributed approximately equally between public and subsidized schools (45% each), with the remaining 10% employed in private schools. Teachers working in public schools were paid according to a rigid pay scale established by the national Teacher Statute, which exclusively rewarded seniority. In contrast, private subsidized and private schools had greater discretion in setting salaries (Behrman et al., 2016). Despite this flexibility, average salaries across public and voucher schools were comparable, with the former exhibiting steeper earnings growth due to the legally mandated seniority-based increments.

Monitoring and bonuses. In the early 2000s, Chile implemented a national teacher evaluation system. A central component of this system was a mandatory portfolio assessment, introduced in 2003. The portfolio required teachers to submit a written unit plan for approximately eight lessons, an example of a student assessment, a video-recorded classroom lesson, and written reflections on their teaching.[9] In 2004, the government introduced the AVDI (*Asignación Variable por Desempeño Individual*), a voluntary assessment available only to teachers who performed sufficiently well in the portfolio.[10] The AVDI consisted of a standardized written test evaluating subject-matter expertise knowledge, using a combination of multiple-choice and short-answer formats tailored to the teacher's area of specialization.[11] It was considered generally not worth taking by a large share of teachers, given the monetary return associated. Both the portfolio evaluation and the AVDI were available only to teachers in public schools; teachers in voucher-funded or private schools were not subject to these evaluations.

2.1 Introduction of the *Carrera Docente*

In 2016, Chile enacted Law 20.903, creating the *Carrera Docente*, or *Teacher Professional Development System* (TPDS), a nationwide reform to structure public and voucher school teachers' careers. The system comprised a five-tier career ladder (Initial, Early, Advanced, Expert I, Expert II), preceded by a provisional entry stage (Access).

Moreover, the reform introduced an unconditional baseline salary increase of approximately 30% upon system entry.[12] It also linked salary increases to career progression, where

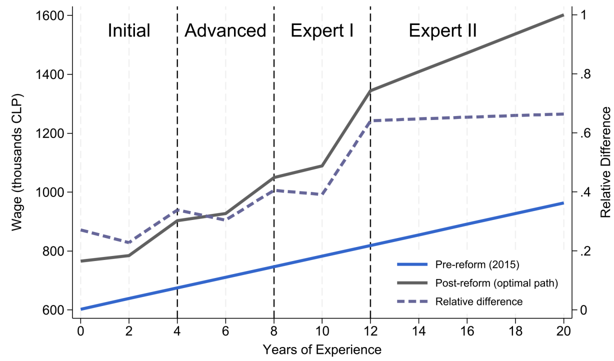
[9] Teachers were placed into one of four performance categories: *Unsatisfactory*, *Basic*, *Competent* or *Outstanding*. The portfolio was evaluated along with a peer interview, a report from the school principal or supervisor, and a self-evaluation, with the results synthesized into a final rating. However, the largest weight (60%) to define the performance was given to the portfolio. While the evaluation framework formally included the possibility of sanctions—such as dismissal following two consecutive "Unsatisfactory" ratings—evidence suggests these were rarely applied, and the vast majority of teachers received ratings of "Competent" or higher. As such, the system was often perceived more as a monitoring mechanism than as a tool for high-stakes accountability.

[10] They would have to be rated "Competent" or "Outstanding."

[11] Successful candidates received a bonus equivalent to 15% to 25% of their base salary, paid quarterly over two to four years depending on their score. The salary bonus would therefore correspond to a monthly increase of roughly 5–7%. The take-up of the AVDI was around 50%.

[12] The reform substantially increased the *Bonificación de Reconocimiento Profesional* (BRP), a bonus granted to

Figure 1: Salary as a function of experience, before and after the reform



Note: Wages before and after the reform as a function of experience, assuming the teacher advances to the next career stage as soon as eligible. The dashed line shows the relative wage difference between the two schemes.

each stage corresponds to a specific set of bonuses composed of a fixed component and an experience-based supplement. In contrast to the previous system, salary increases are no longer determined solely by years of experience. Instead, they are contingent upon both teaching experience and performance in two evaluation instruments: the Portafolio and the ECEP (*Evaluación de Conocimientos Específicos y Pedagógicos*). The Portafolio remains structurally similar to its predecessor in the pre-2016 evaluation system and captures pedagogical practice. The ECEP, a standardized test assessing subject-matter knowledge, replaces the earlier AVDI. Unlike its predecessor, the ECEP is mandatory.

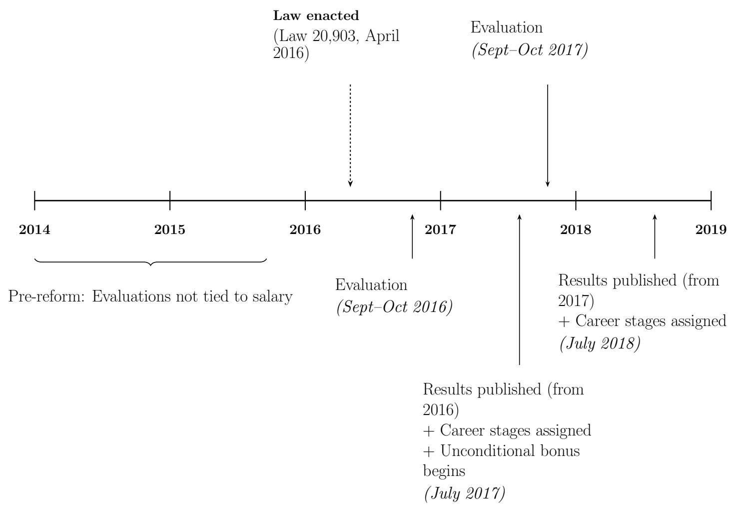
Advancement to a higher stage requires meeting both a minimum number of years of teaching and minimum threshold scores on the Portafolio and ECEP. Teachers with sufficient experience could, in principle, access the uppermost stage (Expert II), whereas early-career teachers, even those with high evaluations, may be restricted to intermediate stages such as Advanced. A minimum of four years of experience is required to access the Early and Advanced stages, eight years for Expert I, and twelve years for Expert II.[13] Evaluations and advancement take place on a four-year cycle, so that only a fraction of the workforce is assessed in any given year. A teacher who fails to meet the thresholds for the next stage must remain at her current level until the following evaluation round. While promotion up to Advanced is mandatory, further progression beyond this point is voluntary.

Figure 1 plots the old and new salary schemes as a function of years of experience, assuming the teacher advances to each subsequent stage at the earliest opportunity. A teacher who does so could eventually earn a salary nearly 70% higher than under the

all teachers holding a *título profesional* from an accredited higher education institution. In our analysis sample, 96.8% of teachers graduated from a university, instituto profesional, or escuela normal, and are therefore eligible for this bonus.

[13]Table B6 illustrates how different combinations of scores and experience determine access to specific career stages.

Figure 2: Timeline of the reform



previous system.[14]

Phased-in. The initial implementation of the reform was restricted to public schools. Voucher schools were incorporated in a staggered way: the first cohort (765 subsidized schools, or 13% of all subsidized schools) was selected in 2017 and entered the system in July 2019.[15] A second cohort of 786 schools was selected in 2018 and entered in July 2020. Evaluations followed a four-year rotation cycle, and this was maintained after the reform. The first cohort to be assessed under the new system in 2016 therefore consisted of teachers who had last been evaluated in 2012.

Figure 2 summarizes the timing of the reform. The first cohort took the Portafolio and ECEP in September–October 2016. Results were published and career stages assigned in July 2017, at which point salary increases and stage-specific bonuses began.[16] A second cohort was evaluated in September–October 2017, with results published and stages assigned in July 2018.

[14]See Table B1 for a more detailed overview of the bonuses.

[15]The incorporation of voucher schools proceeded in two stages. During the first, voluntary stage (2017–2025), school operators could apply to enter the system, subject to an annual quota set by the Ministry of Education. When applications exceeded available slots, priority was given to schools serving a higher share of disadvantaged students. All remaining voucher schools must enter in a mandatory stage beginning in 2026.

[16]Because not all teachers could be tested immediately, the reform incorporated a transitional mechanism (*asimilación de tramo*) based on pre-existing evaluation results. Teachers' most recent Portfolio scores were used when available, and the AVDI was allowed to substitute for the ECEP. Approximately 45% of teachers who used this mechanism were assigned a career stage in 2017, distributed as 16% Early, 52% Advanced, 23% Expert I, and 8% Expert II.

3 Conceptual Framework

This section develops a simple model of teacher exit decisions. The purpose of the model is to guide the empirical framework, aiming to describe the main interaction behind the exit decision and the structure of the new career. Moreover, it helps understand how the reform may differentially affect retention across the ability distribution.

Consider a population of public school teachers indexed by teaching effectiveness, or ability, $a \in \mathbb{R}$. Each teacher makes a binary decision: *stay* in public teaching or *exit* to an outside option. Under the pre-reform regime, the teaching wage is $w^{pre} = w_0$, independent of ability or performance.[17] The reform introduces two components:

$$w^{post}(s) = w_0 + \underbrace{\Delta}_{\text{base salary increase}} + \underbrace{b(s)}_{\text{career stage bonus}},$$

where $\Delta > 0$ is a uniform salary increase for all public teachers, s is an evaluation score, and $b(s)$ is a bonus that depends on career stage placement. Career stages are determined by a threshold rule: teachers with $s \geq \tau$ are assigned to a higher stage and receive a bonus $B > 0$. The evaluation score is a noisy measure of teaching effectiveness, $s = a + \nu$, where ν is measurement error independent of a. A teacher with ability a qualifies for the bonus with probability $1 - F_\nu(\tau - a)$, which is increasing in a: more effective teachers are more likely to reach a higher career stage, but the mapping is imperfect.

Each teacher has an outside option with value $\omega(a)$, with evidence suggesting that more able teachers face better outside options (Bacolod, 2007; Nagler et al., 2020; Chingos and West, 2012). Teachers have strictly concave utility over income, $u(\cdot)$ with $u' > 0$ and $u'' < 0$, and teaching provides a non-pecuniary benefit $\mu = \bar{\mu}(a) + \varepsilon$, where $\bar{\mu}(a)$ captures systematic differences in intrinsic motivation across the ability distribution, for which there is also some evidence (Boyd et al., 2008; Goldhaber et al., 2011), and ε is an idiosyncratic preference shock with logistic distribution $\Lambda(\cdot/\sigma)$. A teacher stays if the total value of teaching exceeds the outside option, $u(w) + \bar{\mu}(a) + \varepsilon \geq u(\omega(a))$, so the exit probability is:

$$h(a, w) = \Lambda\left(\frac{u(\omega(a)) - u(w) - \bar{\mu}(a)}{\sigma}\right). \tag{1}$$

Define the *net outside value* $z(a, w) \equiv \left[u(\omega(a)) - u(w) - \bar{\mu}(a)\right]/\sigma$, so that $h = \Lambda(z)$. Teachers with higher z are more likely to exit. Since the reform raises wages by at least $\Delta > 0$ for all public teachers and utility is strictly increasing, it reduces z and therefore exit. Whether the reform retains high- or low-ability teachers more, however, is ambiguous. The model identifies two main channels that pull in opposite directions.

[17] In practice, pre-reform wages depend on seniority. I abstract from this for clarity; what matters is that pre-reform wages do not depend on evaluated performance.

Baseline exit rate. The framework predicts that the same wage increase produces a larger reduction in exit probability for teachers who are closer to the exit margin. The treatment effect is therefore concentrated among teachers with higher baseline exit rates. Which group this is depends on the balance between outside options $\omega(a)$ and non-pecuniary benefits $\bar{\mu}(a)$. If outside options improve sufficiently with ability ($u'(\omega(a))\omega'(a)$ large), high-ability teachers have higher baseline exit rates and respond more to the salary increase, consistent with the standard Roy model prediction (Lazear, 2000; Brown and Andrabi, 2023). If instead more effective teachers derive greater intrinsic satisfaction from teaching ($\bar{\mu}'(a) > 0$ large enough), low-ability teachers are closer to the exit margin and the reform disproportionately retains them.

Differential exposure to the career stage bonus. Because evaluation scores are positively correlated with ability, high-ability teachers are more likely to receive the career stage bonus, and their average post-reform wage is higher. All else equal, this channel would favor the retention of high-ability teachers. However, by concavity, the utility value of a given salary increase is lower for teachers who already earn more. Since high-ability teachers receive larger bonuses, they are on a flatter portion of the utility function, and the additional income buys them less utility. This attenuates any preferential retention of high-ability teachers through the career stage bonus (see Appendix C for formal derivation).

The sign of the ability gradient is therefore an empirical question, governed by the relative strength of these channels.

4 Data and Samples

4.1 Data

I use administrative data from multiple sources linked at the individual level.

Teachers. I rely on administrative records covering the universe of teachers working in primary and secondary education from 2004 onwards, provided by the Ministry of Education (MINEDUC). These data contain detailed information on teachers' employment status, such as the type of contract and total hours worked. I also observe information on the school where teachers are employed, including whether it is public, subsidized, or private. From 2017 onward, the data additionally reports the teacher's career stage within the TPDS. Moreover, I observe teacher evaluation outcomes: the AVDI and the Portfolio assessments are available from 2002 onward, while results from the ECEP are available from 2016. The data do not contain information on salaries. However, because public school teachers are paid according to a statutory salary schedule, I can impute wages from the observed variables in the data (experience, contracted hours, teaching level, career stage, and degree type). Details are provided in Appendix D.

Students. I match teachers to the universe of their students and observe student performance on standardized tests. These data come from SIMCE (*Sistema de Medición de la Calidad de la Educación*), a national assessment administered by the Agencia de Calidad de la Educación. SIMCE tests are administered annually to students in selected grades, and cover mathematics and reading in every year. For each student, I also observe a rich set of sociodemographic characteristics collected through a survey administered together with the test, including parental education and household income.

The analysis sample imposes two restrictions. First, I exclude private school teachers and subsidized school teachers employed in schools that entered the Carrera Docente before 2020, to avoid capturing behavioral responses of teachers in voucher schools that already anticipate the contract. Second, I restrict the sample to teachers under the age of 50 to avoid confounding exit from the profession with retirement-driven exit.[18]

4.2 Measurement

Measuring exit rates. A central challenge in studying teacher attrition is defining when a teacher has genuinely left the profession. In administrative data, the simplest measure is a one-year exit: the teacher is not observed in the data in year $t+1$. However, this definition conflates permanent departures with temporary absences such as parental leave, sabbaticals, or administrative gaps. In the sample, 7.5% of teachers under 50 are not observed in the following year.[19] Yet among these one-year absentees, over half (50.3%) reappear in the data within five years. This suggests that the one-year measure substantially overstates genuine attrition. Throughout the paper, I therefore use a five-year exit measure: a teacher is classified as having exited at time t if she is not observed in any of years $t+1$ through $t+5$. This more conservative definition captures persistent departures while allowing for temporary interruptions, and reduces the average exit rate to 3.7%.

Figure A2 plots exit rates by years of experience for public and subsidized school teachers. Exit rates in subsidized schools are generally higher than in public schools across the experience distribution.[20] Among teachers with fewer than five years of experience, the five-year exit rate averages approximately 6%, compared to 2–3% for those with 5 to

[18]The first restriction removes approximately 40,000 private school teachers (13% of the workforce) and approximately 56,000 subsidized school teachers in 1,509 schools that were incorporated into the career system in July 2019 or July 2020 (18% of the workforce). The age restriction removes an additional 86,000 teachers (27% of the workforce).

[19]This figure is comparable to the annual attrition rates reported for other countries: approximately 8% of public school teachers in the United States leave the profession each year (National Center for Education Statistics, 2023), 9–10% in England, and 6.5% on average across OECD countries (OECD, 2025).

[20]Teachers in public and subsidized schools are subject to different labor legislation. Public school teachers are governed by the *Estatuto Docente* (Teacher Statute), which provides strong employment protections, including restrictions on dismissal. Teachers in subsidized schools, by contrast, are employed under the *Código del Trabajo* (Labor Code), the standard private-sector labor regulation, which affords employers greater flexibility in hiring and termination decisions.

15 years. Comparing panels (a) and (b) reveals that the one-year exit measure exhibits a similar shape but at higher levels throughout.

Measuring teacher ability: value-added in test scores. To measure teaching effectiveness, I estimate teacher value-added (TVA) from student performance on the SIMCE standardized tests in mathematics and reading. SIMCE is administered annually, but coverage varies across grades and years.[21] I restrict the estimation sample to teachers observed in grades 6, 8, and 10, where I can link each teacher to the students in her classroom and observe both current and lagged test scores.

The TVA estimation follows a standard value-added model. For each subject $j \in \{\text{math}, \text{reading}\}$, I estimate:

$$Y^j_{icst} = \beta Y^j_{ics,t-1} + X'_{icst}\gamma + C'_{cst}\delta + \alpha^j_{it} + \epsilon^j_{icst}, \tag{2}$$

where Y^j_{icst} is the standardized SIMCE score of student i in classroom c, school s, at time t; $Y^j_{ics,t-1}$ is the student's lagged score in the same subject; X_{icst} includes student gender; C_{cst} captures classroom composition (class size, share female, lagged class average score, and parental education and income quintiles); and the model includes school type and grade-by-year fixed effects. The teacher-by-year fixed effects α^j_{it} are the TVA estimates: they capture each teacher's contribution to student learning in year t, net of student and classroom characteristics.

4.3 Three analysis samples

The empirical analysis proceeds in three steps, each relying on a different sample. First, I evaluate the overall effect of the career system on teacher retention using the full sample of public and subsidized school teachers described above, exploiting the fact that the reform was initially restricted to the public sector. Table B3 presents descriptive statistics for this sample, split by school type and reform period. Women account for roughly three quarters of the teaching workforce, and the average teacher is about 35 years old with 8 years of experience. Public school teachers are more likely to work in rural areas (21% vs. 4%) and serve a more disadvantaged student body: SIMCE math and reading scores are about 30 points lower, and the share of mothers with a college degree is 4% in public schools compared to 15% in subsidized schools.

Second, to assess whether the reform differentially retains high- or low-ability teachers, I restrict attention to the subset of teachers for whom I can estimate pre-reform value-added. Because TVA requires linking teachers to SIMCE-tested classrooms, this measure is available only for a fraction of the workforce. More precisely, I restrict to teachers whose value-added

[21] Table B2 reports the full SIMCE testing schedule by grade and year. I estimate TVA using the universe of teachers linked to SIMCE-tested classrooms, without imposing the age or sector restrictions of the DiD sample.

is estimated in 2013 or 2014.[22] This yields a subsample of approximately 19,000 teachers, or about 15 percent of the full analysis sample. Table B4 in the appendix compares the two groups: they are broadly similar in demographic and school characteristics, though teachers with TVA exhibit a somewhat lower five-year exit rate (1.8 versus 3.9 percent).

Third, to examine whether the additional career stage bonus contributes to retention at the margin, I focus on the subset of public school teachers who were evaluated under the new system in 2016 or 2017. As described in Section 2, teachers are called for evaluation on a regular four-year cycle, so the evaluated population in any given year is determined by the rotation schedule rather than by self-selection. I describe this sample in detail and compare it to the full population of public school teachers in Section 6.

5 The Career System and Teacher Retention

In this section, I study the overall effect of the introduction of the new career system on teacher retention. Because the reform was initially applied only to public schools, while subsidized schools remained outside the system during the analysis period, I exploit this differential exposure in a difference-in-differences framework.

5.1 Empirical Strategy

I estimate the following event study specification:

$$\text{Exit}_{ist} = \alpha_s + \sum_{t \neq 2015} \beta_t \left(\text{Public}_i \times \mathbf{1}[\text{Year} = t]\right) + \gamma_t + \delta(\text{Exp}_{it}) + \varepsilon_{ist} \tag{3}$$

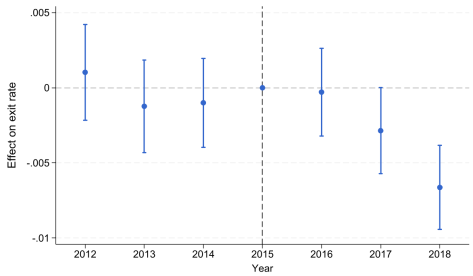
where Exit_{ist} is an indicator for whether teacher i in school s exits the profession at time t, defined as not being observed in any of years $t+1$ through $t+5$. Public_i is an indicator for teachers employed in public schools, α_s are school fixed effects, γ_t are year fixed effects, and $\delta(\text{Exp}_{it})$ are experience fixed effects. The coefficients β_t trace out the differential change in exit rates for public school teachers relative to subsidized school teachers, normalized to zero in 2015, the last pre-reform year. Standard errors are clustered at the school $\times$ year level. Causal interpretation of β_t requires that, absent the reform, exit rates in public and subsidized schools would have followed parallel trends, the standard assumption in this setting.

5.2 Average effect on exit

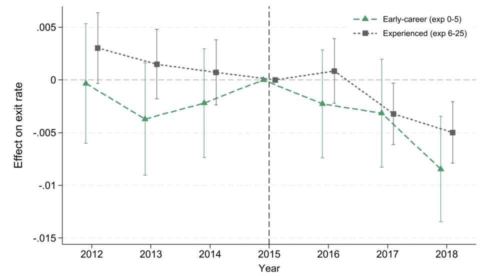
Figure 3 presents the main results. Panel (a) reports the event study coefficients from equation (3) for the full sample. The effect is absent in 2016, the year in which the first

[22] Araya-Córdova et al. (2025) show that the reform changed teaching practices and effort, which would compromise the policy-invariance of post-reform TVA measures.

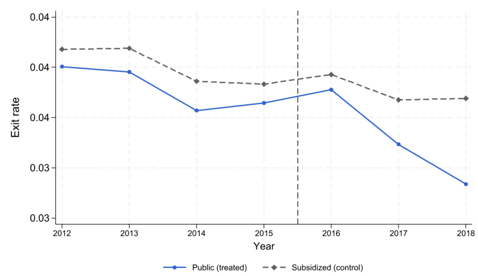
Figure 3: Effect of the career system on teacher exit



(a) All teachers



(b) By experience



(c) Raw exit rates

Note: Event study coefficients β_t from equation (3) with 95% confidence intervals. Baseline year: 2015. Panel (a) shows the full sample. Panel (b) splits by early-career (0–5 years of experience) and experienced (6+ years) teachers. Panel (c) shows raw mean exit rates by school type. Teachers under 50 in public and subsidized schools, 2012–2018. Standard errors clustered at the school × year level.

cohort of teachers was evaluated under the new system but had not yet received any salary adjustment. It becomes detectable in 2017, when career stages were assigned and the associated salary increases began, and continues to grow in 2018. By 2018, public school teachers are 0.7 percentage points less likely to exit than subsidized school teachers relative to the pre-reform period. It is worth noticing that, after the 2016 evaluation, teachers already knew that salary increases were forthcoming, yet the retention response does not appear until the money is actually delivered, consistent with some recent evidence that teachers are more responsive to changes in current compensation than to equally valued future benefits (Biasi, 2019).[23]

Given the large baseline differences in exit rates across the experience distribution, I estimate the event study separately for early-career (0–5 years of experience) and experienced (6+ years) teachers, reported in panel (b). Early-career teachers exit at a rate of

[23] Several mechanisms may explain why receipt matters more than announcement. Teachers may face liquidity constraints that prevent them from borrowing against future salary increases, so that only actual receipt relaxes their budget constraint (Kaplan and Violante, 2014; Zeldes, 1989). Alternatively, present-biased preferences may lead teachers to heavily discount a raise arriving in several months relative to one appearing in this month's paycheck (Laibson, 1997; Kaur et al., 2015).

approximately 6% before the reform, while the rate among experienced teachers is 2–3%. Despite these very different starting points, we can detect a significant decrease in exit rates for both groups, amounting to roughly 15% relative to the baseline for early-career teachers and 20% for experienced teachers. The reform therefore appears to have been effective at increasing retention broadly, including among experienced teachers whose baseline propensity to leave was already low.

Using imputed wages from the statutory salary schedule (Appendix D), I can quantify the implied elasticity. The average imputed wage for public teachers increased by approximately 22 percent between 2015 and 2017 (Figure A3). Combined with a 19 percent reduction in exit (0.7 percentage points on a 3.7 percent baseline), this implies an elasticity of exit with respect to wages of approximately -0.9. Because the reform restructured compensation for the entire public teaching workforce, this elasticity is directly informative for the design of large-scale salary policies. Existing estimates based on broad wage variation are scarce and come from indirect sources, such as cross-municipal differences in Norway (-1.3, Falch 2011) or seniority-driven pay growth in the United States (-1.4, Hendricks 2014). The much larger elasticities of -3 to -8 found for targeted retention bonuses (Clotfelter et al., 2008; Springer et al., 2016; Tartova, 2025; Cowan and Goldhaber, 2018; Cabrera and Webbink, 2020) are less relevant for predicting the effect of an across-the-board raise, since compliers in those settings are in hard-to-staff schools.

Robustness. The pre-reform coefficients in both panels are small and statistically indistinguishable from zero, pointing in the direction that exit rates in public and subsidized schools were evolving similarly before the reform. This is further confirmed by the raw exit rates in panel (c): the exit rate among subsidized school teachers remains roughly constant throughout the sample period, while it drops for public school teachers after 2016.

I test the robustness of these results in several ways. First, Figure A4a shows that the event study pattern is virtually identical when using one- through four-year exit measures instead of the five-year measure used in the main specification. Second, Figure A4b shows that the estimates are stable when progressively adding the teacher and school characteristics described in Table B3.

Other reforms. During the same period, Chile implemented some reforms in the education system and one may argue that these could confound the effect on teacher retention.[24] At the school level, the most significant concurrent policy change was the Ley de Inclusión (Law 20,845), which replaced school-level admissions in subsidized schools with a centralized lottery system (the Sistema de Admisión Escolar, SAE). The stated goal of the SAE was to reduce socioeconomic segregation in the subsidized sector by eliminating selective admissions.

[24] See, for example, De Falco and Reichlin (2025) and Bucarey (2018) on the expansion of financial aid, and De Falco (2025) and Gallegos et al. (2022) on targeted scholarships for prospective teachers.

To the extent that the reform changed the composition of students in subsidized schools, it could in principle have affected teacher retention, since student composition is a well-documented determinant of teacher turnover (Hanushek et al., 2004; Ronfeldt et al., 2013). However, the available evidence indicates that the SAE had limited effects on student sorting (Kutscher et al., 2023; Elacqua et al., 2025). Moreover, in Appendix E I directly test whether the SAE rollout affected teacher exit rates by exploiting its staggered regional implementation in a difference-in-differences design, and I find no evidence that it did.

Wage spillovers. Voucher schools may have responded to the reform by raising their own wages to remain competitive, which would attenuate the estimated effect (Tincani, 2021). The wage increase was large and difficult to match for schools that did not receive the state transfers funding the reform. The flat trend in exit rates among subsidized school teachers visible in panel (c) of Figure 3 indirectly suggests no such response. In Appendix F, I provide a more direct test using household survey data (CASEN) and find that composition-adjusted real wages for private sector teachers were essentially unchanged between 2013 and 2017, while those for public teachers increased by approximately 18%, consistent with the increase estimated using imputed wages.

5.3 Who Gets Retained?

Before turning to the heterogeneity analysis, I validate two assumptions of the conceptual framework using the TVA estimates from Section 4.2.

TVA and the evaluations. A central assumption of the conceptual framework is that evaluation scores are positively correlated with teaching effectiveness: $s = a + \nu$. This relationship is what generates differential exposure to the career stage bonus, since higher-ability teachers are more likely to surpass the evaluation thresholds and reach higher career stages, raising their expected post-reform wage. Whether this assumption holds empirically is therefore important for interpreting the heterogeneity analysis that follows. Table B5 reports OLS coefficients from regressions of TVA on standardized evaluation scores, matching each evaluation observation to the closest TVA measured within three years (see Figure A5 for a non-parametric representation). A one standard deviation increase in the Portfolio score is associated with a 0.03 standard deviation increase in TVA, and the relationship is stable across the pre- and post-reform periods. The AVDI and ECEP show similar coefficients.

To put these magnitudes in perspective, the standard deviation of teacher value-added is typically estimated at 0.10 to 0.15 for math (Bacher-Hicks, 2022). A coefficient of 0.03 therefore represents roughly one-quarter of the cross-teacher dispersion in effectiveness, a magnitude comparable to those found in studies that directly correlate classroom observation protocols or teacher knowledge assessments with value-added in the United States

(Rockoff et al., 2011; Bacher-Hicks et al., 2019).

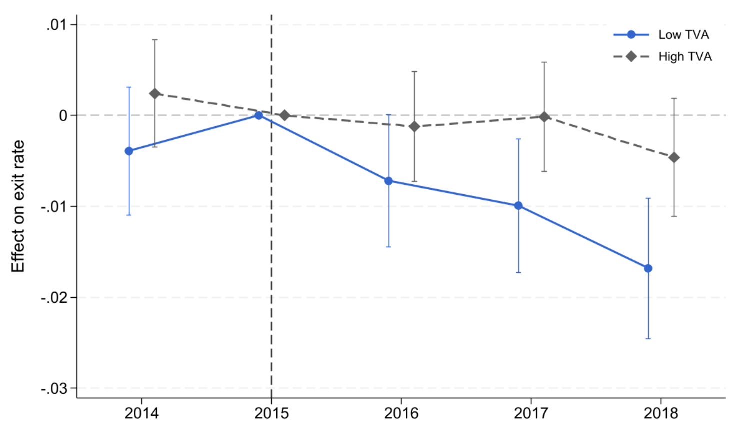
Ability and exit rates. The conceptual framework highlights that the sign of the treatment effect gradient also depends on which teachers are closer to the exit margin. To assess this empirically, I examine how pre-reform exit rates vary with ability. Figure A6 reports, for both Portfolio scores and TVA, the coefficient on a high-ability indicator from regressions of exit rates within two-year experience bins, under three specifications: no controls, school characteristics with year fixed effects, and school fixed effects with year fixed effects. Across all specifications, the coefficient is negative at mid-experience levels, indicating that teachers with higher evaluation scores or higher TVA are less likely to exit.

Together, these results indicate that both measures of ability are negatively associated with baseline exit, consistent with the conceptual framework's scenario where the non-pecuniary channel dominates the outside option channel. This is consistent with evidence from the United States, where less effective teachers are generally found to be more likely to leave (Hanushek et al., 2004; Boyd et al., 2008; Goldhaber et al., 2011).

5.4 Effect of the Reform by Teacher Ability

As mentioned before, this analysis focuses on the subset of teachers for whom I can estimate pre-reform teacher value-added (TVA). Each teacher is assigned their most recent pre-treatment TVA and is tracked forward from that measurement year. I classify teachers as high or low TVA based on the within-sector and within-subject median in the pre-reform period.

Figure 4: Effect of the reform on exit by pre-reform teacher value-added



Note: Event study coefficients β_t from equation (3) estimated separately for teachers above (high TVA) and below (low TVA) the within-sector median of pre-reform TVA. Baseline year: 2015. Standard errors clustered at the school $\times$ year level. 95% confidence intervals shown.

Table B4 in the appendix compares the TVA subsample to the full DiD sample. The two groups are broadly similar in demographic and school characteristics, though teachers with TVA exhibit a lower five-year exit rate (1.8 versus 3.9 percent). TVA can be measured in 2013 or 2014, but 72 percent of the sample has its most recent TVA in 2014. To avoid an unbalanced first year in which only the smaller 2013 cohort contributes, I start the panel in 2014: teachers whose TVA is measured in 2013 are tracked from 2014 onward, alongside those measured in 2014. As a robustness check, I also restrict to the 2014 cohort only.

I estimate equation (3) separately for teachers above and below the within-sector TVA median. The retention effect of the reform appears concentrated among low-TVA teachers. Figure 4 presents the event study estimates separately for the two groups, showing that the post-reform decline in exit rates is visibly present for teachers below the median. Table 1 summarizes this pattern by reporting the pooled post-reform DiD coefficient for each group. Across all specifications, the point estimate for low-TVA teachers is substantially larger than for high-TVA teachers: when pooling both cohorts and defining post as 2016–2018 (column 1), the effect is -1.09 percentage points ($p < 0.01$) for low-TVA teachers versus -0.19 percentage points for high-TVA teachers, not distinguishable from zero. The gap is larger when restricting the post period to 2017–2018 (columns 3 and 4).

Table 1: Reform Effect on Teacher Exit by Ability Level

	Post = 2016–2018		Post = 2017–2018	
	Both cohorts (1)	2014 only (2)	Both cohorts (3)	2014 only (4)
Low TVA	-0.0109***	-0.0109***	-0.0132***	-0.0124***
	(0.0031)	(0.0035)	(0.0033)	(0.0038)
High TVA	-0.0019	-0.0009	-0.0023	-0.0010
	(0.0025)	(0.0028)	(0.0027)	(0.0031)
Difference (High − Low)	0.0091**	0.0100**	0.0109**	0.0114**
	(0.0040)	(0.0045)	(0.0043)	(0.0049)
N (Low TVA)	37,102	28,039	37,102	28,039
N (High TVA)	37,673	28,612	37,673	28,612

Note: Each cell reports the coefficient on Post × Public from a difference-in-differences regression of the 5-year exit rate on year fixed effects, a public school indicator, their interaction, and experience controls, with school fixed effects and standard errors clustered at the school × year level. The baseline year is 2015. Teachers are classified as high or low TVA using subject-specific within-sector medians: a teacher with both math and reading TVA is classified high (low) if above (below) the median on both subjects; teachers with only one subject use that subject's classification. *Both cohorts* pools teachers with TVA measured in 2013 and 2014; *2014 only* restricts to the 2014 cohort. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robustness. Threats to the DiD design, such as concurrent reforms and wage spillovers, were addressed in Section 5 and apply to this subsample as well. The pre-reform coefficient in Figure 4 is close to zero for both groups, indicating that exit rates in public and subsidized schools were evolving similarly not only on average but also within each TVA group. Moreover, the results are robust to restricting to the 2014 TVA cohort only (Table 1, columns 2 and 4) and to adding the teacher and school characteristics from Table B3 (Figure A8). A concern specific to this analysis is that measurement error in TVA could affect the classification of teachers into high and low ability, so that the differential effect depends on the particular value-added specification used. Figure A7 shows this is not the case: re-estimating TVA with different sets of controls, always keeping the lagged test score, produces stable coefficients and a stable gap between groups.

As documented above, low-TVA teachers have higher baseline exit rates, placing them closer to the exit margin. The salary increase therefore produces a larger retention response among them, precisely because they are the ones closer to leaving. This finding contrasts with Tartova (2025), who studies wage bonuses targeted at teachers in disadvantaged schools in France and finds that high-value-added teachers are approximately 2.5 times more responsive. The two populations are different: her compliers are teachers in hard-to-staff schools, where the mechanisms discussed in the conceptual framework may bite differently. My results speak to a large-scale reform affecting all public school teachers, among whom less effective teachers are closer to leaving. This distinction matters for policy: both the overall retention elasticity (which is considerably larger in hard-to-retain settings) and the heterogeneity by ability estimated in such subgroups should be generalized with caution to reforms that raise compensation across the board.

Yet this pattern may appear puzzling in light of the reform's design: because TVA and evaluation scores are positively correlated (Table B5), high-ability teachers can expect larger career stage bonuses, which should, if anything, strengthen their incentive to stay. That the retention effect nonetheless concentrates among low-ability teachers raises the question of whether the career stage bonus contributes to retention at all, or whether the effect operates entirely through the base salary increase. The next section aims to shed light on this question directly.

6 Non-Linearity of the Salary Increase

The previous two sections established that the reform reduces exit and that low-ability teachers benefit more. I now examine whether the career stage bonus, the skill-based component of the reform, contributes to retention at the margin. Among teachers who have already received the baseline salary increase, I estimate the marginal impact of an additional bonus using a regression discontinuity design that exploits the cutoff rules governing career stage assignment.

6.1 Regression Discontinuity Design

As described in Section 2, the assignment of career stages depends on two evaluation scores, Portfolio and ECEP, conditional on experience (Table B6). This creates a two-dimensional threshold structure with multiple margins at which a teacher can advance to a higher stage (e.g., Early to Advanced, Advanced to Expert I, and so on). To make the analysis as transparent as possible, I proceed in two steps.

First, I focus on a single margin: the transition from Early to Advanced. I therefore restrict the sample to teachers with at least four years of experience, the minimum required for eligibility to the Advanced stage. This choice is motivated by two considerations. On the one hand, the Early-to-Advanced boundary concentrates the largest mass of evaluated teachers, providing greater statistical power to detect an effect (see Figure A9 for the joint distribution of scores). On the other hand, it allows me to study the broadest population of teachers, since higher transitions require stricter minimum experience thresholds, while still capturing a non-negligible jump in compensation (Table B1). Second, I reduce the two-dimensional assignment problem to a single running variable.[25] I construct a single running variable by recentering each teacher's Portfolio score at the relevant Early-to-Advanced threshold conditional on their ECEP category.[26] Teachers with a positive recentered score fall above the cutoff and are assigned to the Advanced stage; those with a negative value remain in the Early stage.

I estimate the following regression discontinuity model:

$$Exit_{i,t+1} = \beta_0 + \beta_1 \cdot \mathbb{1}[\widetilde{P}_{it} \geq 0] + f(\widetilde{P}_{it}) + X_i'\gamma + u_i, \tag{4}$$

where $Exit_{i,t+1}$ is the exit outcome for teacher i in year $t+1$. Teachers are evaluated in year t but learn their career stage assignment and begin receiving the associated salary adjustment only in year $t+1$ (Figure 2), so this is the first year in which the exit decision can be affected. A teacher is classified as having exited if she is not observed in any of years $t+2$ through $t+k+1$, where k is the exit horizon. $\widetilde{P}_{it}$ is the recentered Portfolio score from the year-t evaluation, $f(\cdot)$ is a local polynomial estimated separately on each side of the cutoff, and X_i is a vector of pre-determined covariates. Bandwidths are chosen optimally following Calonico et al. (2019), and inference is based on bias-corrected confidence intervals with robust standard errors. The parameter of interest is β_1, which captures the causal effect of crossing the Early-to-Advanced threshold on teacher exit at the cutoff.

[25] For a discussion of multi-score regression discontinuity designs, see Papay et al. (2011) and Reardon and Robinson (2012).

[26] The Portfolio cutoff for the Early-to-Advanced transition is 2.50 for teachers with ECEP in $[1.88, 2.75)$, 2.25 for ECEP in $[2.75, 3.38)$, and 2.00 for ECEP ≥ 3.38. I use the Portfolio rather than the ECEP as the running variable because the ECEP score distribution exhibits bunching at certain scale values, including at career stage thresholds (Figure A1, panel b). The Portfolio, by contrast, is scored by trained evaluators on multiple rubric indicators and the final score is a weighted average, yielding a much smoother distribution. The bunching in the ECEP violates the smoothness assumption required for using it as a running variable, but does not affect the RDD along the Portfolio dimension, as confirmed by the density continuity test presented below.

RD sample. The RD sample consists of 17,440 public school teachers evaluated in 2016 or 2017 who have at least four years of experience and for whom both Portfolio and ECEP scores are observed. Table B7 compares this sample to the full population of public school teachers. The RD sample is somewhat older (mean age 37.5 vs. 35.0) and more experienced (10.5 vs. 7.5 years), as expected given the minimum experience requirement. Once conditioning on at least four years of experience, however, the two populations are very similar across all observable characteristics, consistent with the four-year rotation schedule determining who is evaluated rather than self-selection.

Identification. The identifying assumption underlying the RDD is that potential outcomes vary smoothly through the cutoff, so that any discontinuity in the outcome can be attributed to the change in career stage assignment. I assess this assumption through the standard set of validity checks. First, I test for manipulation of the running variable using the density continuity test of McCrary (2008). Figure A10 shows no evidence of a discontinuity in the density of the recentered Portfolio score at the cutoff, suggesting that teachers are unable to precisely manipulate their score to cross the threshold. Second, I verify that pre-determined covariates are balanced around the cutoff: none of the RD estimates for teacher or school characteristics is statistically different from zero at conventional levels (Table B8).

Figure A11 displays the first stage: the probability of reaching the Advanced stage or higher increases by approximately 85 percentage points at the cutoff of the recentered Portfolio score, while the probability of remaining in the Early stage drops symmetrically.[27] Figure A12 confirms that this translates into a jump in imputed wages of approximately 68,000 CLP per month (about 10 percent) at the cutoff.[28]

6.2 The effect of receiving an additional bonus

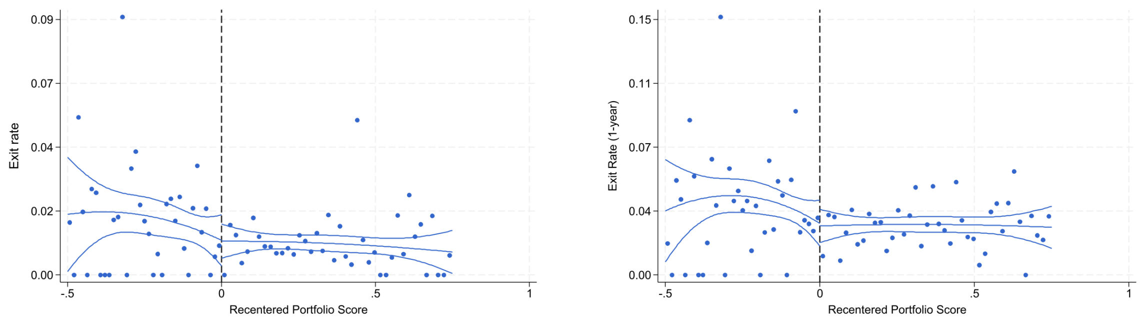
Figure 5 plots exit rates as a function of the recentered Portfolio score around the Early-to-Advanced threshold, using both the five-year and one-year exit measures. There is no visible discontinuity at the cutoff for either outcome: exit rates are flat through the threshold, with the quadratic fits and confidence intervals overlapping on both sides. Table 2 confirms this: the effect of reaching the Advanced stage on exit is a precise zero, both economically and statistically. Adding covariates to increase precision does not change the conclusion (Panel B): the point estimates remain close to zero and the standard errors are comparable. The result is also robust to the choice of bandwidth (Figure A14).

As with any regression discontinuity design, this estimate is local to teachers near the Early-to-Advanced threshold. Several features of the setting, however, suggest that the complier population is not unusually selected. The relevant Portfolio cutoffs fall squarely

[27]The jump is not 100% because of the transitional mechanism (*asimilación de tramo*): some teachers below the cutoff were provisionally assigned to a higher stage based on pre-existing evaluation results.

[28]Figure A13 shows the probability of assignment to each career stage as a function of both Portfolio and ECEP scores, confirming that the two-dimensional threshold structure operates as expected.

Figure 5: Exit rates around the Early-to-Advanced threshold



(a) Five-year exit

(b) One-year exit

Note: Binned means of exit rates as a function of the recentered Portfolio score at the Early-to-Advanced threshold. Panel (a) uses five-year exit; panel (b) uses one-year exit. Quadratic fits with 95% confidence intervals estimated separately on each side of the cutoff.

in the middle of the score distribution (Figure A1, panel a), so the compliers are not drawn from the tails. Moreover, because evaluations take place on a four-year rotation, the RD sample captures a broad cross-section of the teaching workforce rather than a self-selected group. Consistent with this, the observable characteristics of the RD sample are similar to those of the overall population of public school teachers (Table B7). While one should be cautious in extrapolating a local estimate, these features suggest that the null effect is unlikely to be driven by the particular composition of teachers at the cutoff.

Table 2: Effect of reaching the Advanced stage on teacher exit

	No controls		With controls	
	Five-year exit	One-year exit	Five-year exit	One-year exit
Conventional	-0.0019	-0.0028	0.0017	-0.0013
	(0.0060)	(0.0097)	(0.0061)	(0.0099)
Robust	-0.0017	-0.0021	0.0024	-0.0006
	(0.0072)	(0.0118)	(0.0074)	(0.0120)
Mean of dep. var.	0.0134	0.0326	0.0134	0.0326
Bandwidth	0.226	0.232	0.213	0.230
Eff. N	6,333	6,541	5,936	6,290

Note: Local polynomial RD estimates following Calonico et al. (2019). The running variable is the recentered Portfolio score at the Early-to-Advanced threshold. reports the standard local polynomial estimate; reports the bias-corrected estimate with robust standard errors. Bandwidth selected by MSE-optimal procedure. Triangular kernel. Standard errors in parentheses, adjusted for mass points in the running variable. Controls include gender, age, experience, contracted hours, tenure at school, rural, region, SIMCE scores, and share of mothers with college education.

Taken together, the difference-in-differences and regression discontinuity results provide direct evidence that the salary-retention relationship is non-linear. The reform as a whole reduced exit substantially, but the additional career stage bonus, layered on top of the base salary increase, produces no further effect. Because the *Carrera Docente* bundles both components, this setting offers a unique opportunity to isolate the marginal contribution of a performance-based bonus among teachers who have already received a large unconditional raise. Previous studies could only infer diminishing returns indirectly, for example from the fact that turnover elasticities decline with experience as seniority raises wages (Hendricks, 2014), or from the observation that retention bonuses of very different sizes produce similar effects across contexts (Springer et al., 2016; Clotfelter et al., 2008).

7 Conclusion

This paper studies whether a career ladder that ties teacher pay to evaluated performance can reduce attrition and improve the composition of the teaching workforce. Chile's *Carrera Docente* combined a uniform baseline salary increase for all public school teachers with a career stage bonus linked to evaluated performance, providing a setting in which the two components can be studied separately.

The reform as a whole reduced teacher exit by approximately 18%, with an implied elasticity of -0.9. This retention effect, however, is concentrated among low-value-added teachers: their exit rate falls significantly, while the effect for high-value-added teachers is indistinguishable from zero. The pattern is consistent with less effective teachers being closer to the exit margin, and therefore more responsive to the salary increase, because more effective teachers derive greater non-pecuniary satisfaction from the profession. A regression discontinuity design that isolates the career stage bonus confirms that the skill-based component plays no role: teachers just above and just below the advancement threshold exit at the same rate.

These findings have direct implications for the design of teacher compensation policy. If the goal is to reduce attrition, a uniform salary increase appears sufficient, and the evaluation infrastructure required to assign teachers to career stages does not deliver additional retention benefits. The career ladder may serve other purposes, such as incentivizing effort or professional development (Araya-Córdova et al., 2025), but its contribution to retention is negligible. Moreover, because the reform disproportionately retains less effective teachers, it does not improve the average quality of the teaching workforce.

This paper focuses on the retention margin and does not speak to how the reform affects the entry decisions of prospective teachers. If the career ladder attracts higher-ability candidates into the profession, the compositional effects documented here could be offset at the entry margin. Studying this channel, as well as the long-run effects of the reform as more cohorts of teachers are evaluated and reassigned, is an important direction for future work.

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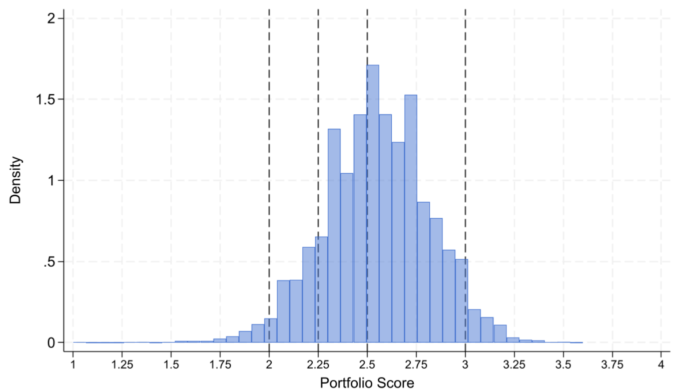
Appendix - Salary Increases and Skills-Based Bonuses: Teacher Retention and Workforce Composition

Appendices

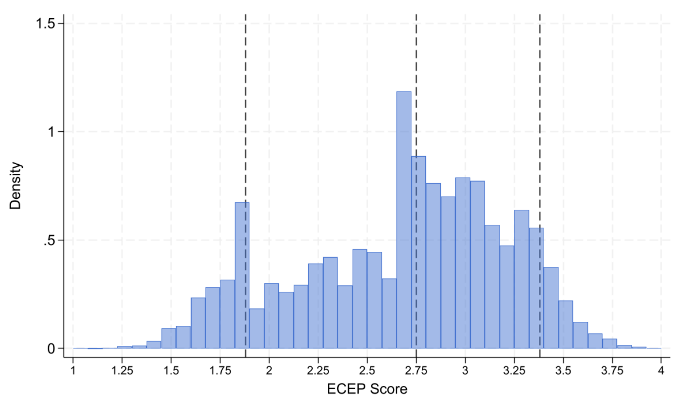
A Additional Figures

Figure A1: Distribution of evaluation scores

(a) Portfolio scores

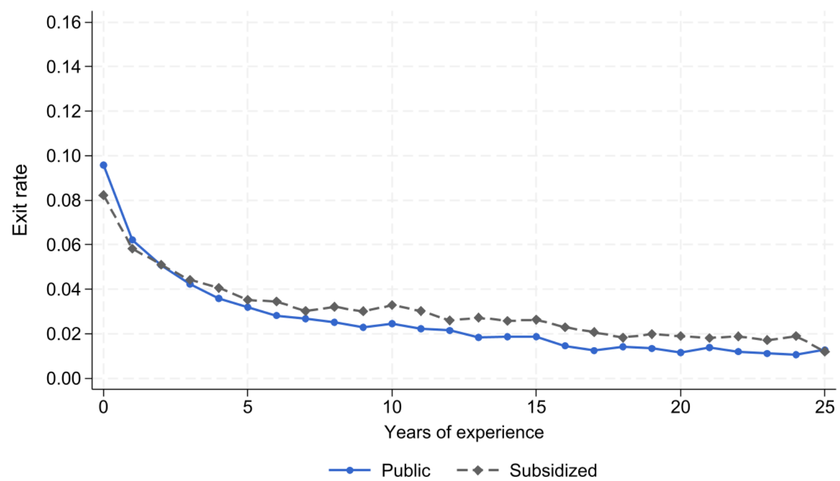


(b) ECEP scores

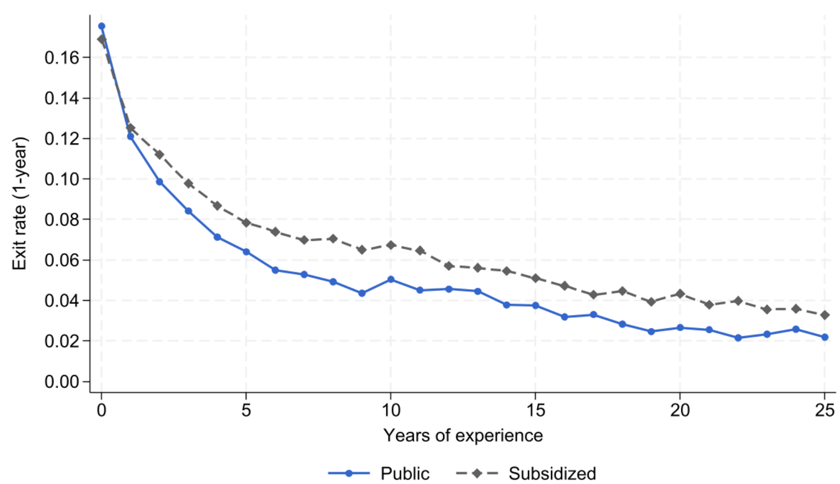


Note: Distribution of Portfolio (top) and ECEP (bottom) scores for public school teachers evaluated in 2016 or 2017 with at least four years of experience. Dashed vertical lines indicate the thresholds determining career stage assignment (Table B6).

Figure A2: Exit rates by experience and school type



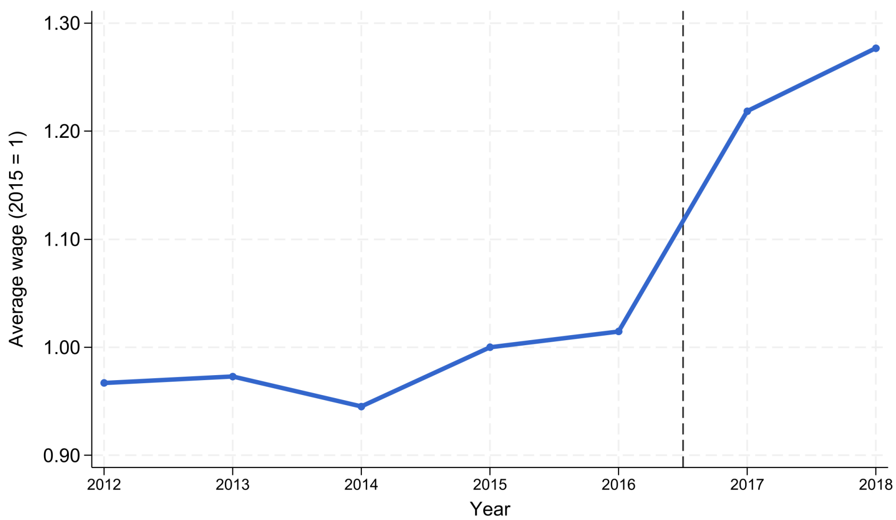
(a) Five-year exit



(b) One-year exit

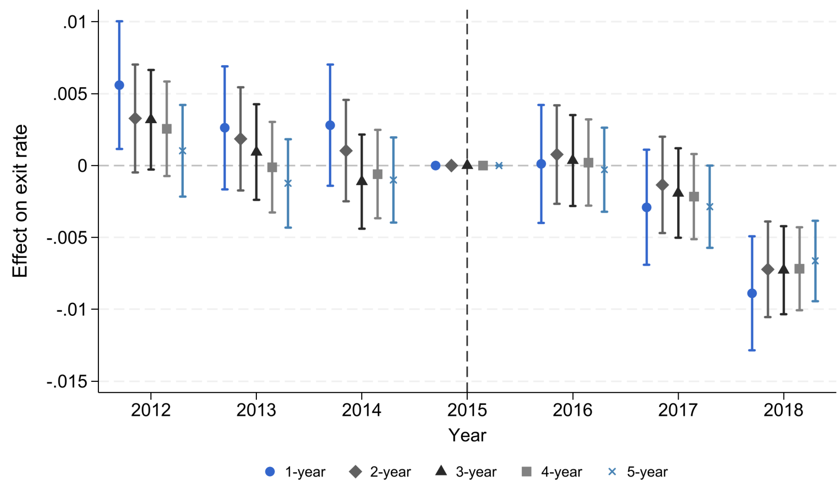
Note: Exit rates by years of experience, separately for public and subsidized school teachers. Panel (a) uses five-year exit; panel (b) uses one-year exit. Sample restricted to teachers under 50, pre-reform period (2012–2015).

Figure A3: Average theoretical wage over time

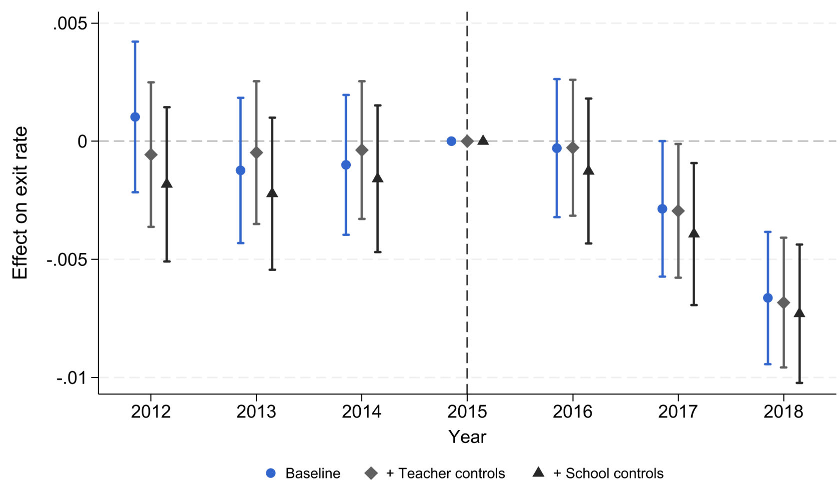


Note: Average theoretical wage across all teachers in the analysis sample, normalized to 2015. Computed from the statutory salary schedule using each teacher's observed hours, experience, teaching level, career stage, and BRP eligibility.

Figure A4: Robustness of the main event study



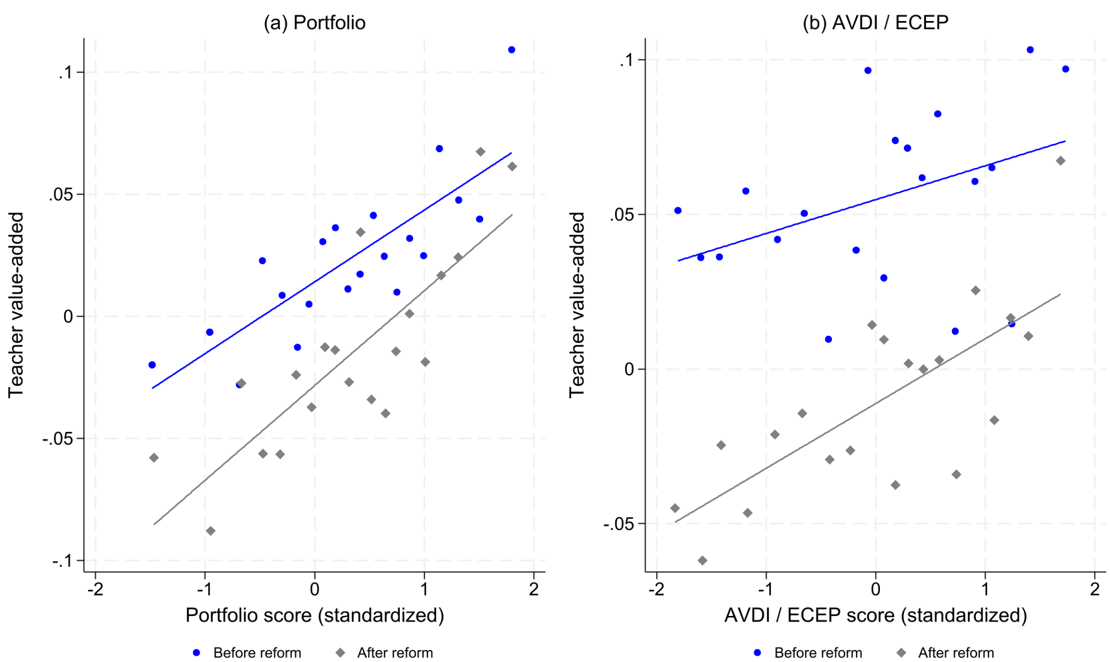
(a) Alternative exit horizons



(b) Progressive controls

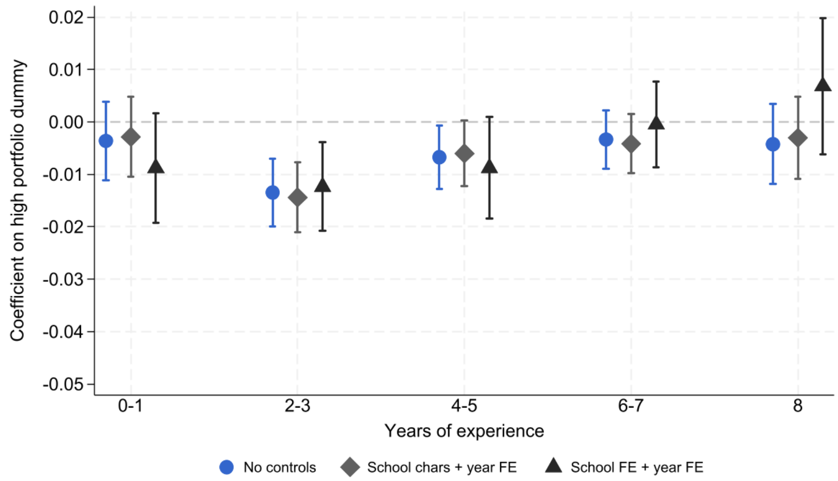
Note: Panel (a) shows event study coefficients β_t from equation (3) using exit measures defined as not being observed in any of years $t+1$ through $t+k$, for $k = 1, \ldots, 5$. The main specification uses $k = 5$. Panel (b) shows estimates with progressively richer controls. "Baseline" includes school fixed effects and experience fixed effects. "+ Teacher controls" adds gender, age, age squared, tenure at current school, university graduation, degree duration, and teaching level indicators (preschool, primary, secondary). "+ School controls" further adds school-level SIMCE scores and the share of mothers with college education. Standard errors clustered at the school $\times$ year level. 95% confidence intervals shown.

Figure A5: Relationship between teacher value-added and evaluation scores

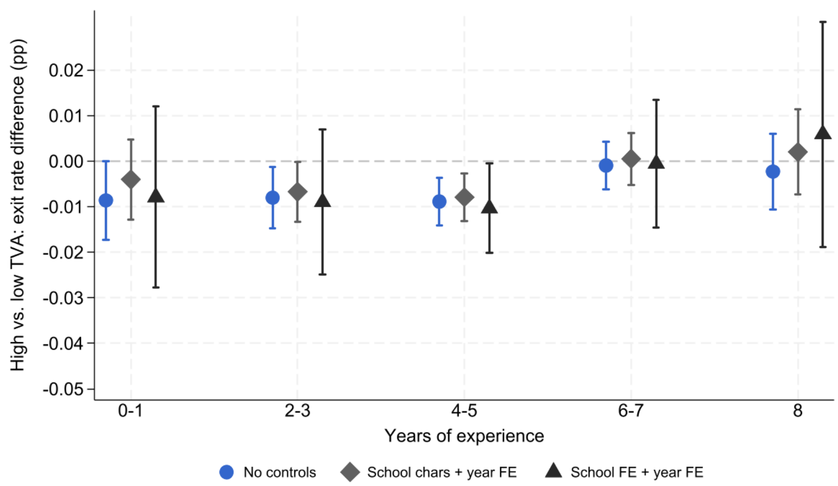


Note: Binned scatter plots of teacher value-added (TVA) against standardized evaluation scores, estimated separately before the reform (2012–2015) and after the reform (2016–2018). Each evaluation observation is matched to the closest TVA measured within three years. The left panel uses Portfolio scores; the right panel uses AVDI scores before the reform and ECEP scores after. Evaluation scores trimmed to $[-2, 2]$ standard deviations.

Figure A6: Effect of high ability on exit rate by experience bin



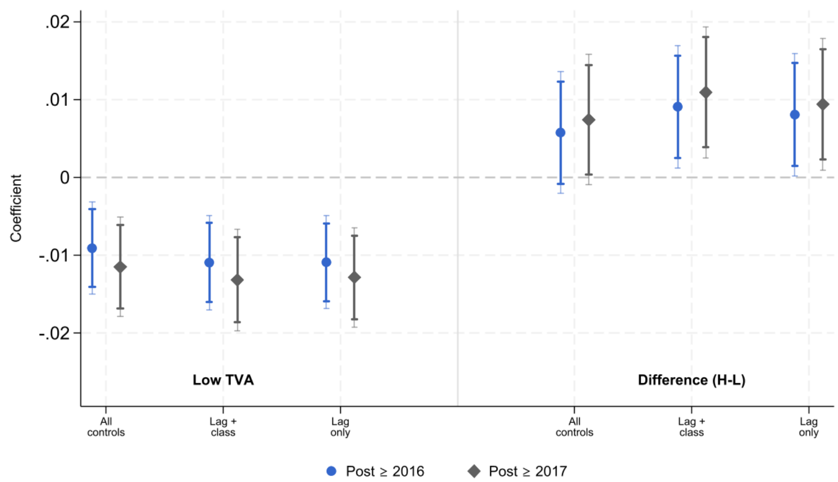
(a) Portfolio



(b) Teacher value-added

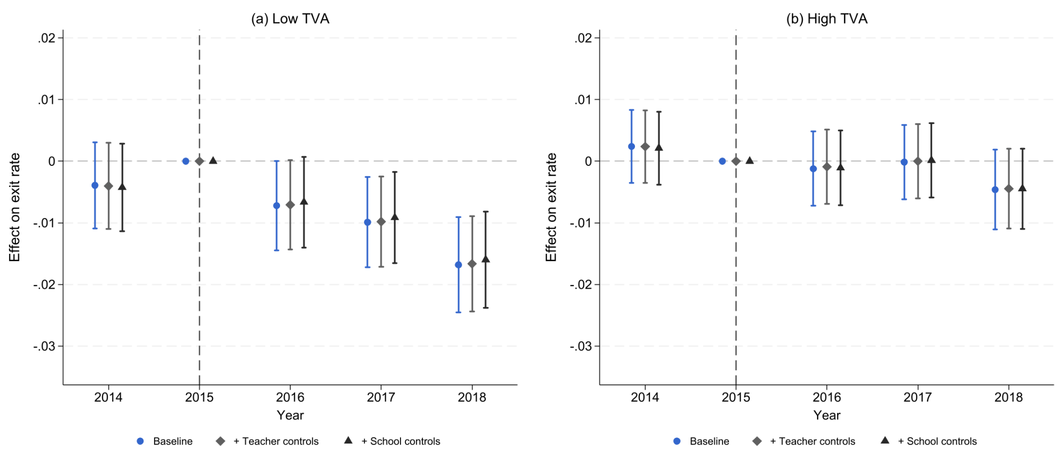
Note: Coefficient on a high-ability indicator (above within-experience-bin median) from regressions of five-year exit within two-year experience bins. Panel (a) uses Portfolio scores; panel (b) uses combined TVA. Three specifications: no controls (circles), school characteristics with year fixed effects (diamonds), and school fixed effects with year fixed effects (triangles). Pre-reform period (2012–2015), public school teachers under 50. 95% confidence intervals based on robust standard errors.

Figure A7: Robustness of TVA heterogeneity to alternative estimation strategies



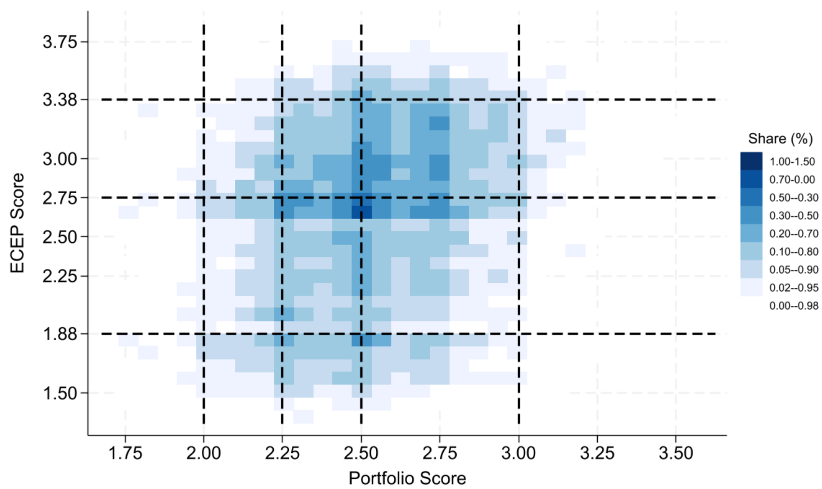
Note: Left panel shows the pooled post-reform DiD coefficient for low-TVA teachers; right panel shows the difference between high- and low-TVA coefficients. Each group of bars corresponds to a different TVA estimation strategy: all controls (lagged score, classroom, and school composition), lagged score and classroom controls (baseline specification), and lagged score only. Teachers are classified using subject-specific within-sector medians. Thick bars show 90% confidence intervals; thin bars show 95% confidence intervals. Post $\geq$ 2016, both cohorts.

Figure A8: Robustness of TVA heterogeneity to additional controls



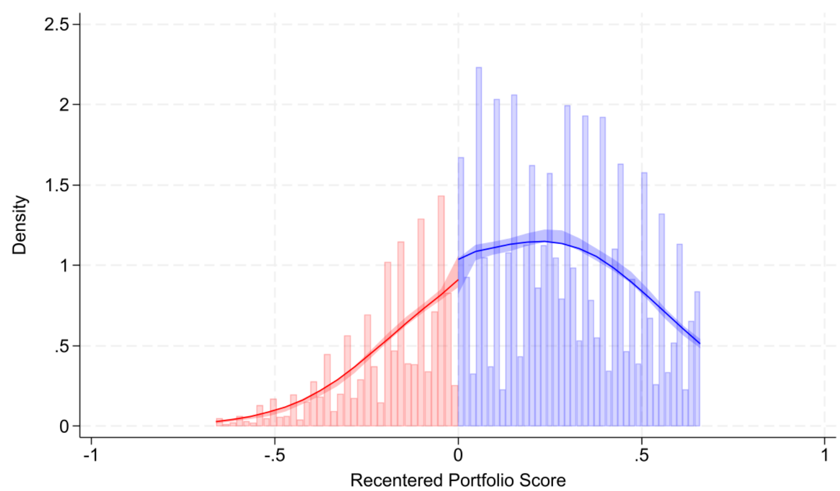
Note: Event study coefficients from equation (3) estimated separately for low-TVA (panel a) and high-TVA (panel b) teachers under three specifications: baseline (school and experience fixed effects), adding teacher controls (gender, age, age squared), and further adding school controls (SIMCE math and reading scores, share of mothers with college education). Teachers are classified using subject-specific within-sector medians. 95% confidence intervals shown. Standard errors clustered at the school $\times$ year level.

Figure A9: Joint distribution of Portfolio and ECEP scores



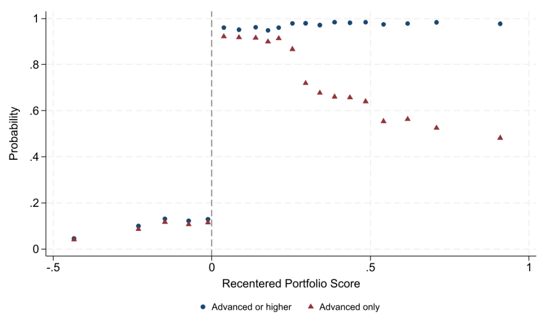
Note: Joint distribution of Portfolio and ECEP scores for public school teachers evaluated in 2016 or 2017 with at least four years of experience. Each cell shows the share of teachers in a 0.05 × 0.05 score bin. Dashed lines indicate career stage thresholds (Table B6).

Figure A10: Density continuity test

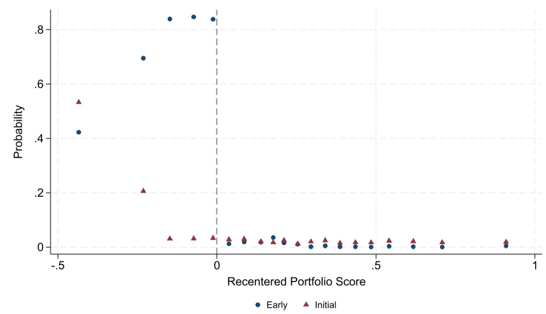


Note: Density continuity test following Cattaneo et al. (2020) for the recentered Portfolio score at the Early-to-Advanced threshold. Histograms show observed frequencies; curves show local polynomial density estimates with pointwise confidence intervals on each side of the cutoff.

Figure A11: First stage: probability of career stage assignment



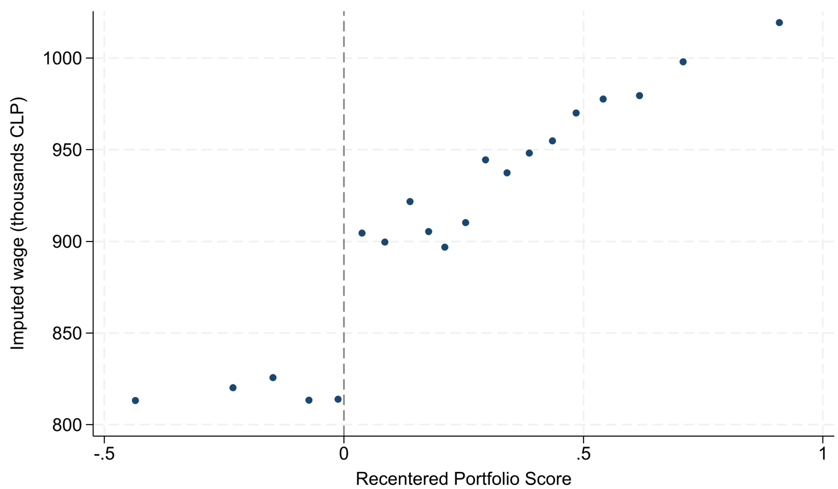
(a) Advanced or higher / Advanced only



(b) Early / Initial

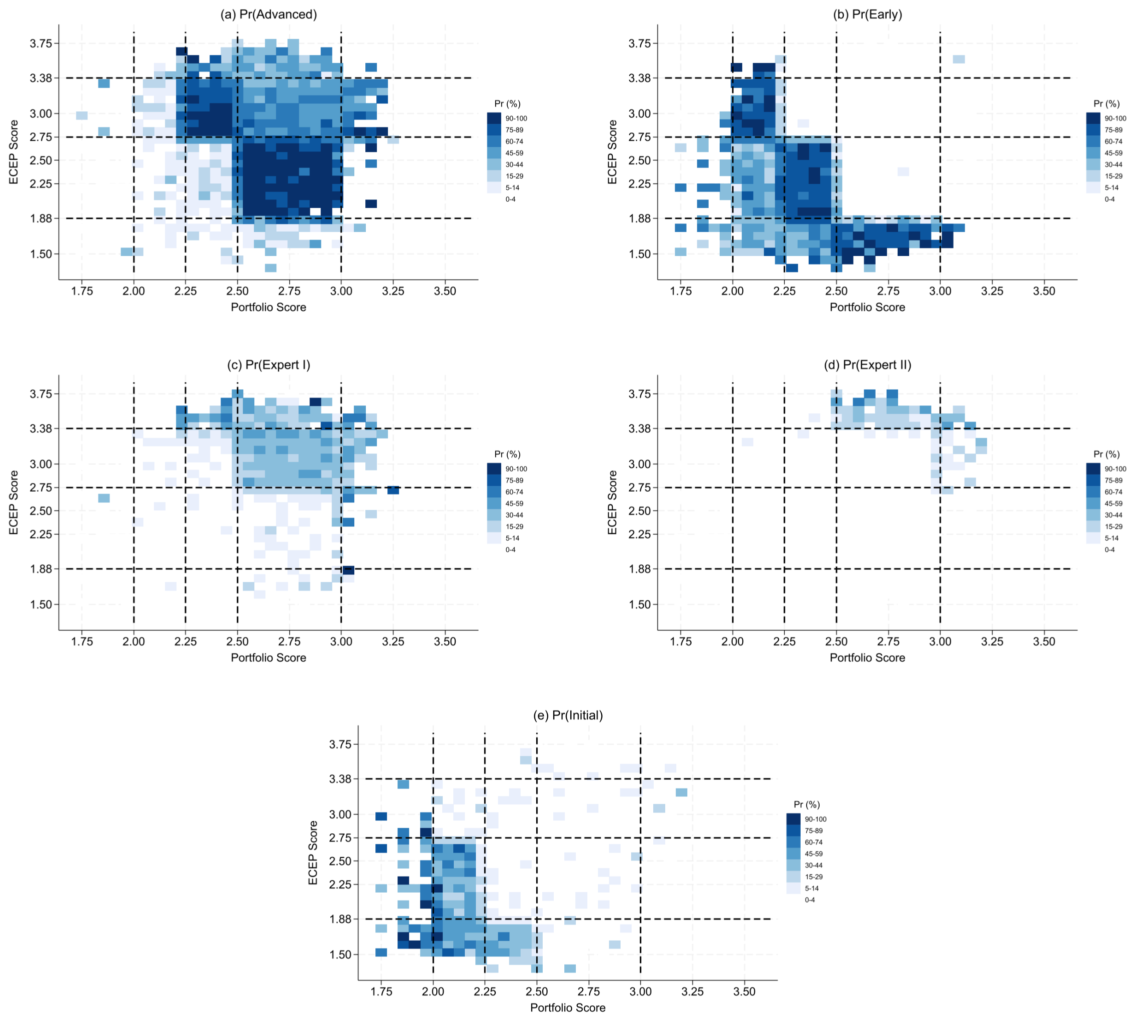
Note: Binscatter of the probability of being assigned to each career stage against the recentered Portfolio score. The running variable is the teacher's Portfolio score minus the ECEP-specific threshold for reaching Advanced. Panel (a) shows the probability of reaching Advanced or higher (circles) and Advanced only (triangles). Panel (b) shows the probability of being assigned to Early (circles) and Initial (triangles). 20 equal-sized bins on each side.

Figure A12: First stage: imputed wage at the Early-to-Advanced threshold



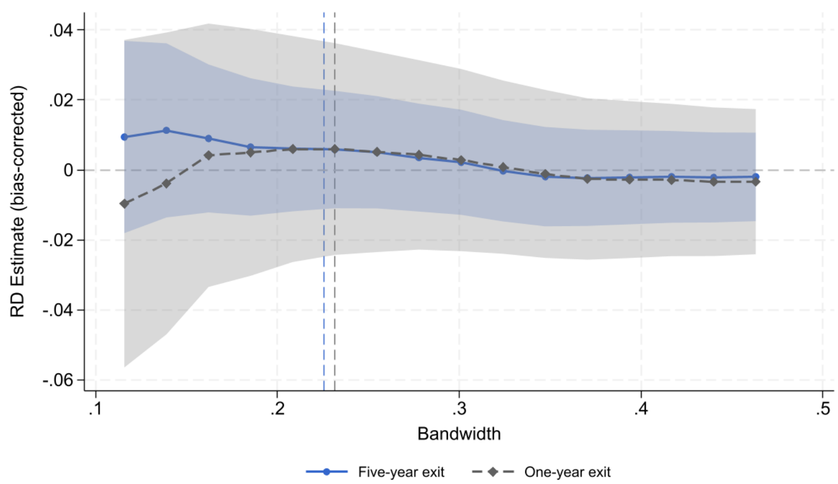
Note: Binscatter of imputed monthly wages (thousands of CLP) against the recentered Portfolio score. Imputed wages are computed from the statutory salary schedule (Appendix D). 20 equal-sized bins on each side.

Figure A13: First stage: career stage assignment by Portfolio and ECEP scores



Note: Probability of being assigned to each career stage as a function of Portfolio and ECEP scores. Each cell shows the mean probability in a 0.05×0.05 score bin (bins with fewer than 3 observations excluded). Dashed lines indicate career stage thresholds (Table B6). RD sample: public school teachers evaluated in 2016 or 2017 with at least four years of experience.

Figure A14: Bandwidth sensitivity



Note: Bias-corrected RD estimates of the effect of reaching Advanced on teacher exit, estimated over a grid of bandwidths from 50% to 200% of the MSE-optimal bandwidth. Shaded areas show 95% confidence intervals based on robust standard errors. Vertical dashed lines indicate the MSE-optimal bandwidth for each outcome.

B Additional Tables

Table B1: Career stage bonuses: fixed and variable salary components

Category	**Fixed Bonus (CLP)**	**Variable Bonus (CLP)**
Initial		$13,076 \times \frac{Hrs}{44} \times \frac{Exp}{30}$
Early		$43,084 \times \frac{Hrs}{44} \times \frac{Exp}{30}$
Advanced	90,000	$86,714 \times \frac{Hrs}{44} \times \frac{Exp}{30}$
Expert I	125,000	$325,084 \times \frac{Hrs}{44} \times \frac{Exp}{30}$
Expert II	190,000	$699,593 \times \frac{Hrs}{44} \times \frac{Exp}{30}$

Note: Fixed and experience-based bonuses associated with each career stage, expressed in Chilean pesos (CLP). *Hrs* refers to the number of contractual working hours, and *Exp* to years of experience.

Table B2: Cohorts Taking the SIMCE by Year

	Cohort												
Year	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012
2006			4										
2007				4									
2008					4								
2009						4							
2010	10						4						
2011				8				4					
2012		10							4		2*		
2013			10			8		6		4		2*	
2014				10			8		6		4		2*
2015					10			8		6		4	
2016						10					6		4
2017							10			8			
2018								10					6
2019												8	

Note: Cohorts are identified by the year in which students start 1st grade of elementary school. The table shows in which grades different cohorts took the SIMCE. Grades 4, 6, and 8 correspond to elementary school grades, and 10th grade corresponds to high school (2do medio). Second graders are only tested on their Spanish knowledge. I use grades 6, 8, and 10 for the TVA estimation, since the value-added model requires a lagged test score. Fourth grade is excluded because no prior SIMCE score is available.

Table B3: Summary Statistics

	Public				Subsidized			
	Pre-reform		Post-reform		Pre-reform		Post-reform	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Panel A: Teacher characteristics								
Female	0.74	0.44	0.74	0.44	0.77	0.42	0.77	0.42
Age	35.5	7.5	35.1	6.9	34.6	7.1	34.6	6.7
Experience (years)	7.8	6.9	7.6	6.3	8.0	7.1	8.1	7.1
Tenure at current school	4.5	5.2	4.2	4.7	4.8	5.5	4.9	5.5
University graduate	0.85	0.36	0.88	0.33	0.87	0.33	0.90	0.29
Degree duration (semesters)	8.6	1.5	8.7	1.4	8.9	1.4	9.0	1.3
Contracted hours (weekly)	34.7	9.8	37.0	9.3	34.9	9.5	35.8	9.2
Primary education	0.61	0.49	0.60	0.49	0.47	0.50	0.47	0.50
Panel B: School characteristics								
Far North	0.06	0.24	0.06	0.24	0.06	0.25	0.06	0.24
North	0.08	0.27	0.08	0.27	0.07	0.26	0.07	0.26
Center	0.50	0.50	0.50	0.50	0.63	0.48	0.62	0.49
South	0.26	0.44	0.27	0.44	0.18	0.38	0.19	0.39
Far South	0.10	0.29	0.10	0.29	0.06	0.24	0.06	0.24
Rural	0.21	0.41	0.21	0.41	0.04	0.20	0.04	0.20
SIMCE math score	239.1	25.4	237.6	25.2	267.6	27.2	266.7	26.9
SIMCE reading score	239.3	20.8	239.3	21.2	259.4	20.5	258.4	19.8
Share mothers with college	0.04	0.05	0.05	0.07	0.15	0.14	0.17	0.14
Teacher-year observations	218,734		204,934		224,522		178,397	
Unique teachers	83,454		87,635		85,530		79,560	
Schools	6,671		6,129		4,808		4,455	

Note: The table reports means and standard deviations for the DiD analysis sample. The sample includes teachers under the age of 50 employed in public and subsidized schools between 2012 and 2018. Pre-reform refers to 2012–2015, post-reform to 2016–2018. SIMCE scores and parental background variables are available for approximately 88% of teacher-year observations (93% in public and 83% in subsidized schools).

Table B4: Summary Statistics: TVA Subsample

	All (2013–2014)		TVA sample	
	Mean	SD	Mean	SD
Teacher characteristics				
Female	0.76	0.43	0.73	0.44
Age	35.0	7.3	35.2	7.5
Experience (years)	7.8	7.1	7.8	6.9
Tenure at current school	4.6	5.4	4.7	5.3
University graduate	0.89	0.31	0.95	0.22
Degree duration (semesters)	8.8	1.4	8.9	1.3
Contracted hours (weekly)	34.7	9.7	36.6	7.5
Primary school teacher	0.54	0.50	0.68	0.47
Exit rate (5-year)	0.039	0.193	0.018	0.134
Share in 2013	0.49	0.50	0.28	0.45
Share in 2014	0.51	0.50	0.72	0.45
Rural	0.13	0.33	0.16	0.37
Public school	0.50	0.50	0.50	0.50
School characteristics				
Far North	0.06	0.24	0.06	0.23
North	0.07	0.26	0.07	0.25
Center	0.57	0.50	0.57	0.49
South	0.22	0.41	0.22	0.42
Far South	0.08	0.27	0.08	0.28
SIMCE math score	251.1	29.1	250.7	29.6
SIMCE reading score	248.0	22.7	247.1	22.7
Share mothers with college	0.09	0.12	0.09	0.11
TVA coverage				
Share with math TVA			0.45	
Share with reading TVA			0.55	
Observations	223,426		19,149	
Teachers	129,552		19,149	

Note: The table compares the full DiD sample to the subsample of teachers with pre-treatment teacher value-added (TVA) estimates. The column includes all public and subsidized school teachers under 50 observed in 2013–2014. The column shows teachers with at least one TVA measure (math or reading) in 2013–2014, measured at their most recent TVA year. TVA is estimated from a value-added model using student SIMCE test scores, controlling for lagged scores, classroom and school characteristics, with teacher, school type, and grade-by-year fixed effects.

Table B5: Relationship between Teacher Value-Added and Evaluation Scores

	Before reform			After reform		
	Coeff.	SE	N	Coeff.	SE	N
Portfolio	0.030***	(0.004)	10,162	0.036***	(0.004)	9,553
AVDI / ECEP	0.023***	(0.005)	5,258	0.022***	(0.003)	9,210

Note: Each cell reports the coefficient from a bivariate regression of teacher value-added (TVA) on the standardized evaluation score. TVA is the average of math and reading value-added estimates. Each evaluation observation is matched to the closest TVA measured within three years. The second evaluation instrument is AVDI before the reform (2012–2015) and ECEP after the reform (2016–2018). All evaluation scores are standardized within year. Robust standard errors in parentheses.

Table B6: Career stage assignment based on performance in evaluation tests

Portfolio	**ECEP**			
	< 1.88	$[1.88, 2.75)$	$[2.75, 3.38)$	≥ 3.38
≥ 3.00	Advanced	Expert I	Expert II	Expert II
$[2.50, 3.00)$	Advanced	Advanced	Expert I	Expert II
$[2.25, 2.50)$	Early	Early	Advanced	Expert I
$[2.00, 2.25)$	Early	Early	Early	Advanced
< 2.00	Initial	Initial	Initial	Initial

Note: Career stage assignment as a function of Portfolio and ECEP score ranges. Eligibility for each stage also depends on a minimum number of years of experience: 4 years for Early and Advanced, 8 years for Expert I, and 12 years for Expert II.

Table B7: Summary Statistics: RD Sample vs. All Public School Teachers

	All public		Exp ≥ 4		RD sample	
	Mean	SD	Mean	SD	Mean	SD
2016						
Teacher characteristics						
Female	0.74	0.44	0.75	0.43	0.77	0.42
Age	35.0	7.1	37.5	6.4	37.7	6.4
Experience (years)	7.5	6.4	10.6	5.7	10.7	5.7
Contracted hours (weekly)	36.3	9.4	37.4	8.6	37.1	8.4
Exit rate (5-year)	0.038	0.191	0.022	0.148	0.017	0.128
School characteristics						
Rural	0.21	0.41	0.21	0.41	0.23	0.42
SIMCE math score	238.1	26.0	238.5	25.8	239.1	25.9
SIMCE reading score	240.5	21.5	241.0	21.5	242.0	21.3
Share mothers with college	0.05	0.06	0.05	0.06	0.05	0.06
Evaluation scores						
Portfolio score					2.53	0.27
ECEP score					2.65	0.51
Observations	64,705		42,455		9,124	
2017						
Teacher characteristics						
Female	0.73	0.44	0.75	0.43	0.75	0.43
Age	35.1	6.9	37.5	6.2	37.4	6.1
Experience (years)	7.6	6.3	10.4	5.6	10.4	5.4
Contracted hours (weekly)	37.3	9.2	38.4	8.3	38.7	7.6
Exit rate (5-year)	0.032	0.177	0.017	0.129	0.016	0.126
School characteristics						
Rural	0.21	0.41	0.21	0.41	0.21	0.41
SIMCE math score	240.5	24.0	241.1	24.0	239.5	24.0
SIMCE reading score	241.4	20.9	241.9	20.8	240.8	21.0
Share mothers with college	0.05	0.06	0.05	0.07	0.05	0.06
Evaluation scores						
Portfolio score					2.55	0.29
ECEP score					2.70	0.55
Observations	68,552		46,625		10,915	

Note: Each panel shows statistics for a given evaluation year. "All public" includes all public school teacher-year observations. "Exp $\geq$ 4" restricts to teachers with at least four years of experience. "RD sample" further restricts to teachers evaluated in that year. Exit rate (5-year) in the first two columns equals one if the teacher is not observed in years $+1 through + 5$. In the RD sample, exit equals one if this condition holds at the test year *or* the year after (stage assignment year). Evaluation scores are reported only for teachers with non-missing scores.

Table B8: Covariate Balance at the Early-to-Advanced Threshold

	Mean	RD Estimate	Bandwidth	Eff. N
Teacher characteristics				
Female	0.756	0.0165	0.257	7,220
		(0.0227)		
Age	37.4	-0.2690	0.260	7,360
		(0.3368)		
Experience (years)	10.6	0.2376	0.236	6,541
		(0.3005)		
Contracted hours	38.2	0.1384	0.311	8,581
		(0.3824)		
Tenure at school	6.1	-0.2518	0.252	7,220
		(0.2662)		
University graduate	0.890	0.0056	0.217	6,129
		(0.0193)		
Degree duration (sem.)	8.7	0.0226	0.212	6,124
		(0.0839)		
Primary education	0.626	-0.0040	0.204	6,010
		(0.0280)		
School characteristics				
Rural	0.215	0.0255	0.227	6,333
		(0.0241)		
Center	0.485	-0.0120	0.311	8,581
		(0.0242)		
South	0.275	-0.0134	0.203	6,010
		(0.0266)		
SIMCE math	239.7	0.6814	0.288	7,498
		(1.1501)		
SIMCE reading	241.5	0.1977	0.270	7,290
		(1.0517)		
Share mothers college	0.051	0.0008	0.242	6,431
		(0.0031)		

Note: Each row reports local polynomial RD estimates of the discontinuity in a pre-determined covariate at the Early-to-Advanced Portfolio threshold, following Calonico et al. (2019). The running variable is the recentered Portfolio score. Bandwidth selected by MSE-optimal procedure for each covariate. Triangular kernel. Standard errors in parentheses. * <0.10, $**$ $<$ 0.05, *** <0.01.

C Conceptual Framework: Derivations

This appendix provides formal derivations for the results in Section 3.

C.1 Setup and Notation

Teachers have ability a, concave utility $u(\cdot)$ over income ($u' > 0$, $u'' < 0$), and face an outside option $\omega(a)$. The pre-reform wage is w_0. The post-reform wage is $w^{post}(a) = w_0 + \Delta + \bar{b}(a)$, where $\Delta > 0$ is the base salary increase and $\bar{b}(a) = B \cdot [1 - F_v(\tau - a)]$ is the expected career stage bonus. Teaching provides non-pecuniary benefit $\bar{\mu}(a) + \varepsilon$, with ε logistic with scale σ.

The exit probability is:

$$h(a, w) = \Lambda\left(\frac{u(\omega(a)) - u(w) - \bar{\mu}(a)}{\sigma}\right) \equiv \Lambda(z(a, w)),$$

where $z(a, w) = [u(\omega(a)) - u(w) - \bar{\mu}(a)]/\sigma$ is the net outside value.

C.2 The Reform Reduces Exit

The treatment effect for a teacher with ability a is:

$$TE(a) = h\big(a,\, w_0 + \Delta + \bar{b}(a)\big) - h(a,\, w_0).$$

Since $\Delta > 0$ and $\bar{b}(a) \geq 0$, we have $w^{post}(a) > w_0$. By strict monotonicity of u:

$$u(w^{post}(a)) > u(w_0),$$

which implies $z(a, w^{post}(a)) < z(a, w_0)$. Since Λ is strictly increasing:

$$TE(a) = \Lambda(z(a, w^{post}(a))) - \Lambda(z(a, w_0)) < 0. \qquad \square$$

C.3 Concavity Attenuates the Career Stage Bonus

The RDD estimand compares teachers just above and below the threshold τ:

$$RDD = h(a,\, w_0 + \Delta) - h(a,\, w_0 + \Delta + B).$$

This depends on the utility gain $u(w_0 + \Delta + B) - u(w_0 + \Delta)$.

By strict concavity of u, for any w, $B > 0$, and $\Delta > 0$:

$$u(w + \Delta + B) - u(w + \Delta) < u(w + B) - u(w).$$

This follows because u' is strictly decreasing: $u'(w + \Delta) < u'(w)$.

By the mean value theorem:

$$u(w_0 + \Delta + B) - u(w_0 + \Delta) = u'(\tilde{w}) \cdot B$$

for some $\tilde{w} \in (w_0 + \Delta,\, w_0 + \Delta + B)$, where $u'(\tilde{w}) < u'(w_0)$ by concavity. The larger is Δ, the smaller is $u'(\tilde{w})$, and the smaller is the utility gain from the bonus B. When Δ is large relative to B, the RDD effect is therefore attenuated. $\square$

C.4 Derivation of the Ability Gradient

Since the career stage bonus has negligible retention effects, we approximate the treatment effect using the base salary increase alone:

$$TE(a) \approx \Lambda\big(z(a) - D\big) - \Lambda\big(z(a)\big),$$

where $z(a) \equiv z(a, w_0) = [u(\omega(a)) - u(w_0) - \bar{\mu}(a)]/\sigma$ is the pre-reform net outside value and $D \equiv [u(w_0 + \Delta) - u(w_0)]/\sigma > 0$ is the normalized utility gain from the base salary increase (constant across teachers).

Differentiating with respect to a:

$$\frac{\partial\, TE}{\partial a} = \Big[\lambda\big(z(a) - D\big) - \lambda\big(z(a)\big)\Big] \cdot z'(a), \tag{5}$$

where $\lambda(\cdot)$ is the logistic density.

Sign of the density difference. Since $D > 0$, we have $z(a) - D < z(a)$. The logistic density $\lambda(z) = \Lambda(z)(1 - \Lambda(z))$ is symmetric and single-peaked at $z = 0$:

- If $z(a) > D/2 > 0$ (teacher closer to exiting than to staying), then $z(a) - D$ is closer to zero than $z(a)$, so $\lambda(z(a) - D) > \lambda(z(a))$: the bracket is positive.
- If $z(a) < 0$ (teacher strongly prefers staying), then both $z(a)$ and $z(a) - D$ are negative and $z(a) - D$ is farther from zero, so $\lambda(z(a) - D) < \lambda(z(a))$: the bracket is negative.

For teachers near the exit margin, where the treatment effect is largest, the bracket $[\lambda(z - D) - \lambda(z)]$ is positive.

Sign of $z'(a)$. Differentiating $z(a)$:

$$z'(a) = \frac{u'(\omega(a))\,\omega'(a) - \bar{\mu}'(a)}{\sigma}.$$

Two forces operate:

- **Outside option channel**: $u'(\omega(a))\,\omega'(a)$. If outside options improve with ability ($\omega'(a) > 0$), this pushes z up (toward exit).

- **Non-pecuniary channel**: $\bar{\mu}'(a)$. If effective teachers derive more satisfaction from teaching ($\bar{\mu}'(a) > 0$), this pushes z down (toward staying).

Case (a): Outside option dominates. If $u'(\omega)\,\omega' > \bar{\mu}'$, then $z'(a) > 0$: high-ability teachers have higher baseline exit rates. For teachers near the exit margin, the bracket is positive, so:

$$\frac{\partial\, TE}{\partial a} > 0,$$

meaning the treatment effect is more negative for high-ability teachers: the reform retains them more (positive sorting).

Case (b): Non-pecuniary channel dominates. If $\bar{\mu}' > u'(\omega)\,\omega'$, then $z'(a) < 0$: low-ability teachers have higher baseline exit rates. For these marginal teachers:

$$\frac{\partial\, TE}{\partial a} < 0,$$

meaning the treatment effect is more negative for low-ability teachers: the reform retains them more (adverse sorting). $\square$

Role of concavity in ability heterogeneity. Even under Case (a), concavity of u moderates the advantage of high-ability teachers. Since high-ability teachers are more likely to receive the career stage bonus $\bar{b}(a)$, their post-reform wage $w_0 + \Delta + \bar{b}(a)$ is higher. By concavity, the marginal utility of this income is lower. Thus, even when high-ability teachers receive more money from the reform, the utility value of this additional money is diminished, further attenuating any preferential retention of high-ability teachers.

Role of imperfect measurement. The correlation between ability a and the evaluation score $s = a + \nu$ determines how effectively the career stage bonus targets high-ability teachers. When measurement error ν has large variance, the expected bonus $\bar{b}(a) = B \cdot [1 - F_\nu(\tau - a)]$ varies little across ability levels: the career ladder fails to differentiate between high and low ability. In the limit of pure noise ($\mathrm{Var}(\nu) \to \infty$), $\bar{b}(a) \to B/2$ for all a, and the bonus becomes a de facto flat payment. This further reduces any ability-based sorting induced by the skill-based component of the reform.

D Imputed Wages

Administrative records from MINEDUC do not include teacher salaries directly. I therefore construct imputed wages for each teacher-year observation by applying the statutory formulas from the *Estatuto Docente* (DFL 1, 1996, as modified by Law 20,903) to observable teacher characteristics. The imputation relies on four dimensions: teaching level, contracted hours, years of experience, and career stage.

Base wage. The base component is the *Remuneración Básica Mínima Nacional* (RBMN), set by the *Escala Única de Remuneraciones*. The RBMN varies by teaching level. For 2015, the hourly rates for a 44-hour contract were: \$12,293 for *educación básica*, \$12,935 for *educación media* (approximately 5% higher), and \$17,845 for *educación parvularia*. I assign each teacher to a level based on the type of education offered at the teacher's school. In 2015, 65.6% of teachers in the sample are classified as *básica*, 26.7% as *media*, and 7.7% as *parvularia*. The base wage for teacher i in year t is:

$$W_{it}^{base} = r_{\ell(i)} \times h_{it},$$

where $r_{\ell(i)}$ is the hourly RBMN for the teacher's level ℓ and h_{it} denotes contracted weekly hours.

Experience component. The experience supplement is a percentage of the base wage that increases with biennia of service (two-year blocks), capped at 15 biennia (30 years). Under the pre-reform schedule (Article 48 of the *Estatuto Docente*), the rates were 6.76% for the first biennium and 6.66% for each additional biennium, yielding a maximum supplement of 100% of the base wage. The reform halved these rates to 3.38% and 3.33%, respectively, with a new cap of 50%. Define $b_{it} = \min\big(\lfloor \text{experience}_{it}/2 \rfloor, 15\big)$. The experience component is:

$$W_{it}^{exp} = \begin{cases} \big(0.0676 + 0.0666 \times \max(b_{it} - 1, 0)\big) \times W_{it}^{base} & \text{if } t \leq 2016, \\ \big(0.0338 + 0.0333 \times \max(b_{it} - 1, 0)\big) \times W_{it}^{base} & \text{if } t \geq 2017, \end{cases}$$

with $W_{it}^{exp} = 0$ when $b_{it} = 0$.

Career stage bonus. From 2017 onward, teachers assigned to a career stage receive an additional bonus (*Asignación por Tramo de Desarrollo Profesional*, Article 49). This bonus has two parts. The progression component (Article 49b) depends on the teacher's stage and scales in proportion to both contracted hours (relative to a 44-hour contract) and biennia of experience (relative to the 15-biennia maximum). Its maximum values are reported in Table B1. The fixed component (Article 49c) is paid only to teachers in the Advanced stage or above and scales with contracted hours but not experience. The total career stage bonus for teacher i is therefore:

$$W_{it}^{tramo} = P_s \times \frac{h_{it}}{44} \times \frac{b_{it}}{15} + F_s \times \frac{h_{it}}{44},$$

where P_s and F_s denote the progression and fixed components for stage s, respectively.

Total imputed wage. The imputed monthly wage is:

$$W_{it} = \begin{cases} W_{it}^{base} + W_{it}^{exp} & \text{if } t \leq 2016, \\ W_{it}^{base} + W_{it}^{exp} + W_{it}^{tramo} & \text{if } t \geq 2017. \end{cases}$$

One component of statutory pay is excluded from the imputation: the *Bonificación de Reconocimiento Profesional* (BRP), an unconditional bonus for teachers holding a professional degree. Because virtually all teachers in the sample are eligible (96.8%), the BRP adds a constant to wages and does not contribute to cross-sectional variation.

E Placebo Test: Centralized Admissions (SAE)

Chile's Ley de Inclusión (Law 20,845) replaced school-level admissions in subsidized schools with the Sistema de Admisión Escolar (SAE), a centralized lottery. The SAE was rolled out regionally over several years, as detailed in Table E9. I exploit this staggered rollout to test whether the SAE itself affected teacher exit rates.

Table E9: SAE regional rollout

SAE year	Regions	Share of schools (%)
2017	Tarapacá, Coquimbo, O'Higgins, Los Lagos	23.2
2018	Arica y Parinacota, Antofagasta, Atacama, Valparaíso, Maule, Biobío, Araucanía, Los Ríos, Aysén	49.7
2019	Metropolitana	27.2

Note: Share of schools computed from the analysis sample in 2015. Magallanes is excluded (2016 pilot with 63 schools). Ñuble is excluded (created in 2018, no pre-period).

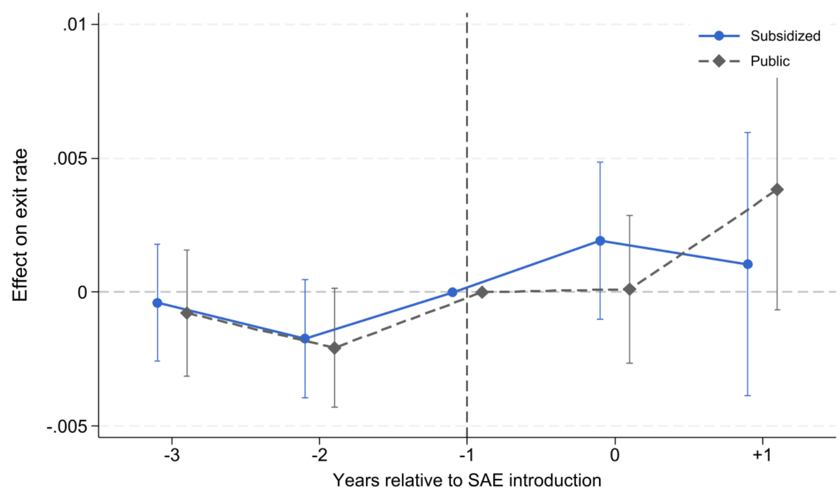
The specification is a staggered difference-in-differences event study. Let $r(i)$ denote the region of school i, and let g_r denote the SAE adoption year for region r. I define relative time as $k_{it} = t - g_{r(i)}$ and estimate:

$$\text{exit}_{it} = \sum_{k \neq -1} \beta_k \cdot \mathbf{1}[k_{it} = k] + \gamma \cdot \text{experience}_{it} + \alpha_i + \delta_t + \varepsilon_{it}, \tag{6}$$

where α_i are school fixed effects, δ_t are year fixed effects, and the sum runs over $k \in \{-3, -2, 0, +1\}$, with $k = -1$ as the omitted baseline. Standard errors are clustered at the school level. Metropolitana, whose SAE adoption in 2019 falls outside the estimation window (2012–2018), enters the sample as a never-treated group. I estimate the model separately for subsidized and public school teachers.

Results are shown in Figure E15.

Figure E15: SAE rollout and teacher exit: staggered event study



Note: The figure reports event study coefficients β_k from equation (6), estimated separately for subsidized and public school teachers. The baseline is $k = -1$. 95% confidence intervals shown.

F Wage Spillovers

Data. I use data from the Chilean national socioeconomic household survey (CASEN) for 2013 and 2017. The CASEN is a repeated cross-section conducted every two years by the Ministry of Social Development, with sample sizes of approximately 200,000 to 270,000 individuals per wave. The wage measure is the corrected total labor income constructed by the CASEN team, which aggregates salary, overtime, bonuses, and secondary job income with imputation for non-response. This variable is consistently defined across both waves. I deflate wages to 2017 pesos using the annual average consumer price index.

Population. I identify teachers using four-digit occupation codes from the CIUO-88 classification (secondary teachers, primary and preschool teachers at higher and mid-level qualifications) combined with four-digit economic activity codes from the CIIU Rev. 3 classification (primary education, general secondary education, and technical secondary education). I classify teachers as public or private based on the employment category variable, which distinguishes public sector employees (central government or municipal) from private sector employees. This variable does not separate subsidized from fully private schools; however, since fully private schools employ a small share of teachers, the private category is predominantly composed of subsidized school teachers.

Summary statistics. Table F10 reports descriptive statistics for the teacher sample by sector and year. The two groups are broadly comparable in age, gender composition, and years of schooling, and these characteristics are stable across waves. Public teachers are slightly older on average and report somewhat fewer weekly hours. In 2013, public teachers earned substantially less than their private sector counterparts (534,000 vs. 608,000 CLP).

Table F10: CASEN teacher sample: summary statistics

	2013		2017	
	Public	Private	Public	Private
Age	40.8	38.4	40.8	39.2
Female (%)	77.6	79.0	77.0	75.4
Years of schooling	16.4	16.4	16.3	16.5
Weekly hours	41.2	45.3	40.5	39.9
Married (%)	47.9	42.9	44.3	44.3
Monthly wage (CLP)	534,416	607,725	689,757	680,363
Observations	1,252	1,097	1,317	1,295

Note: Weighted means using CASEN survey weights. Monthly wage is the corrected total labor income in nominal CLP. Teachers are identified by CIUO-88 occupation codes and CIIU Rev. 3 activity codes for the education sector. Public/private classification is based on the self-reported employment category.

Comparing wages over time. To account for compositional differences across survey waves, I residualize wages on a rich set of observable characteristics: age and its square, gender, years of schooling, weekly hours worked, region fixed effects, an urban indicator, four-digit occupation code fixed effects (capturing teaching level), and marital status. I then add back the grand mean for interpretability. Table F11 reports the results. Before the reform, the ratio of public to private composition-adjusted wages was 0.87. By 2017, public wages had grown by 18.1% in real terms, closing the gap entirely. Private teacher wages, by contrast, were essentially unchanged (−0.2%), consistent with the absence of a competitive wage response from subsidized schools.

Table F11: Composition-adjusted real teacher wages by sector (2017 CLP)

	2013	2017	Change
Mean residualized real wage			
Public	591,344	698,612	+18.1%
Private	678,286	676,999	−0.2%
Ratio (Public/Private)	0.87	1.03	
Observations			
Public	1,248	1,311	
Private	1,093	1,291	

Note: Wages are corrected total labor income from the CASEN, deflated to 2017 pesos using the annual average CPI. Residualized wages are obtained by regressing real wages on age, age squared, gender, years of schooling, weekly hours worked, region fixed effects, urban/rural, occupation code fixed effects, and marital status, using survey weights, and adding back the grand mean. Teachers are identified by CIUO-88 occupation codes and CIIU Rev. 3 activity codes for the education sector. Public/private classification is based on the self-reported employment category.