Who Leaves and Who Stays? The Impact of the Teacher Wage Gap on Teacher Quality

Desislava Tartova*

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Abstract

This paper studies how uniform increases in teacher wages affect the average quality of the teaching workforce. I exploit a 2014 French reform that substantially increased wages in highly disadvantaged schools but only modestly in slightly less disadvantaged ones to identify the heterogeneous exit elasticities with respect to wages across teachers of different productivity levels. I show that high-productivity teachers—defined using plausibly unbiased measures of teacher value-added—are 2.5 times more responsive to wage increases in their exit decisions than low-productivity teachers. I develop a labor-supply framework showing that high-productivity teachers are more responsive to wages because their preferences for the outside option relative to teaching are less dispersed and, on average, closer to the threshold of indifference between leaving and staying in teaching. In policy counterfactuals that leverage the heterogeneous elasticities within the estimated framework, I show that targeting wage bonuses toward high-productivity teachers delivers the same gain in aggregate teacher quality as a uniform increase of comparable magnitude to that of the 2014 reform, at a fourth of the fiscal cost.

*Paris School of Economics (PSE). Email address: desislava.tartova@psemail.eu. I am very grateful to Luc Behaghel, Thomas Piketty and Jonah Rockoff for their guidance and support. I also thank Barbara Biasi, Gabrielle Fack, Manon Garrouste, Nina Guyon, Antoine Hubert de Fraisse, Sylvie Lambert, Karen Macours, David Margolis, Adrien Matray and Suanna Oh for their comments and suggestions. This work has been funded by a French government subsidy managed by the French National Research Agency (ANR) under the framework of the "Investments for the Future" program (reference ANR-17-EURE-001). The provision of MENJS-DEPP data was made possible by the Innovation, Data and Experimentation in Education (IDEE) project, funded by the ANR under the framework of the "Investments for the Future" program (reference ANR-21-ESRE-0034).

Teacher wages around the world fall short of those of similarly educated workers, with secondary-school teachers in the average OECD country earning 90 cents for every dollar their peers make and as low as 66 cents in the United States (OECD 2023). This gap has been steadily increasing over time—tripling since the late 1970s in the United States (Allegretto 2025). This stylized fact holds even if we examine the "feasible" wage gap—i.e. between teaching and occupations that teachers move to after leaving the profession—which has doubled over the last decade in France.[1] Such variation in relative wages is important, as it shapes both decisions of entry into and exit from the profession.

In the context of teachers' exit decisions, studies suggest that higher teacher wages—that is, a decrease in the wage gap—can improve retention in the teaching profession.[2] Yet, much less is known about whether it affects the retention of high- and low-productivity teachers differently. This raises the question: *can a uniform increase in teacher wages increase the quality of the teaching force by affecting its composition?* Understanding who stays and who leaves the profession in response to uniform wage changes is crucial, as teachers are an important driver of human capital accumulation and student long-term earnings (Chetty, Friedman, & Rockoff 2014a, 2014b), and targeting wage increases to productive teachers is politically challenging (Ballou 2001; Dee & Keys 2004).

The answer to this question is a priori not obvious. Higher wages retain some teachers in the profession who would otherwise exit—"marginal" teachers close to indifference between staying and leaving. The effect on aggregate quality depends on whether these teachers are relatively more or less productive. For a given productivity level, the share of marginal teachers can depend jointly on the average preference for outside options among teachers of that productivity level and the dispersion of preferences around that average. The literature does not provide clear guidance for either dimension. For instance, high-productivity teachers may have better outside wages (Chingos & West 2012), which may pull them toward leaving, but they may also have stronger pro-social motives, pulling them toward staying.[3]

Causal identification of whether uniform wage changes affect differently high- and low-productivity teachers' exit from the profession is empirically challenging. This is because identification requires wage variation that is exogenous to other determinants of teachers' heterogeneous labor supply decisions. In practice, teacher wages are often set by rigid, seniority-based pay scales, and while variation in outside wages is likely to covary with productivity, thus producing endogenous differences in incentives across teachers.

This paper provides the first causal evidence that a uniform increase in teacher wages retains high-productivity teachers in the profession disproportionately more than

[1] See Figure 1 for evidence.

[2] For studies linking higher teacher wages to retention, see the Related Literature section below.

[3] Using survey data to identify teachers' pro-sociality, Andersen, Heinesen, and Holm Pedersen (2014) shows higher pro-sociality is associated with higher student test scores.

low-productivity ones, rationalizes the results in a static discrete-choice framework of labor supply, and uses this framework to quantify the effect of counterfactual wage policies on teaching quality. Using a difference-in-differences approach, I exploit a 2014 French reform that raised wages significantly for teachers in highly disadvantaged middle schools nationwide (large uniform nominal bonus), while only marginally raising wages in slightly less disadvantaged middle schools (smaller uniform nominal bonus). I find that exit decisions of high-productivity teachers are 2.5 times as elastic to wages as those of low-productivity teachers, where I proxy teacher productivity with a novel, plausibly unbiased measure of teacher value-added (Tartova 2023), which does not rely on annual standardized testing that is unavailable in France. These results imply that uniformly increasing teacher wages raises aggregate teaching quality. I confirm this in a static discrete-choice framework where teachers choose between two sectors—teaching and an outside option—by comparing their indirect utility in each sector. The framework implies that the higher elasticity of high-productivity teachers reflects both a lower dispersion in their preferences for the outside option relative to teaching and an average preference closer to the indifference threshold. Estimating the framework using the heterogeneous elasticities, I show in policy counterfactuals that while a uniform increase in wages in the order of magnitude of that implied by the French reform does raise aggregate teacher quality, targeted wage increases towards high-productivity teachers can deliver the same gains in quality at a fourth of the cost.

To identify teachers' labor supply response to wage changes, I exploit the 2014 reform of the French national rigid pay scale and the institutional features of the French teacher labor market. The reform introduced sizable, uniform nominal bonuses for teachers in disadvantaged schools—selected based on pre-recorded student characteristics, creating a wage differential between highly disadvantaged ("REP+") schools and slightly less disadvantaged ("REP") schools. Annual wages for REP+ teachers rose, on average, by 7.1% relative to pre-reform wages over the first six years after the introduction of the bonus, compared to 2% for REP teachers. The treatment is also characterized by its large scope, with REP+ and REP teachers representing, respectively, 6.7% and 14.7% of all public middle school teachers. To isolate labor supply decisions, I focus on decisions to exit the teaching profession by tenured teachers, who in France cannot be dismissed for performance and represent more than 90% of all teachers.[4]

I estimate the causal effect of the wage increase induced by the reform on teacher exit using an event-study difference-in-differences design, comparing exit decisions of REP+

[4]By contrast, entry and internal mobility within the teaching sector are constrained by France's centralized teacher allocation system, so studying effective entry and mobility would conflate labor supply responses with labor demand and institutional constraints. Labor supply responses within the educational sector can be identified using data on ranked preferences (see, for instance, Bobba et al. (2021)). In subsequent co-authored work, Denker, Grenet, Silhol, Tartova, and Wilner (2025) study the implications of wage bonus policies for the educational gap, through the internal mobility margin, using such data.

teachers to those of REP teachers, based on panel data covering the universe of tenured teachers in these schools between 2009 and 2019.[5] My baseline empirical approach relies on instrumenting for actual exposure to the reform by exploiting the pre-reform allocation of teachers to schools, and thus estimating the Intention-to-Treat (ITT) effect. Focusing on teachers already assigned at REP+ and REP schools just before the announcement of the reform rules out biases that may arise from post-reform selection into treated schools (e.g. through mobility across schools) of individuals who are more elastic to wages. My preferred specification controls for teacher characteristics, school fixed effects, and commuting zone–by–year fixed effects.

I provide evidence consistent with the identification assumption that exit rates of teachers initially assigned to REP+ and REP would have evolved similarly after 2014, absent the reform. While pre-trends are unobservable in the baseline specification—since treatment is defined for teachers who had not exited by the reform year (i.e., conditional on survival to 2013–2014)—I assess the validity of the identifying assumption using placebo treatment groups based on earlier pre-reform teacher assignments. Across placebo treatment groups, the pre-period shows no differential pre-trend in exits for teachers initially assigned to REP+, relative to REP.

The reduced-form estimates indicate that, on average over the six years following the announcement, the wage bonus reduced the exit rate of teachers initially assigned to REP+ schools by 26.9 percent relative to the counterfactual exit rate predicted in the absence of the differential wage increase, compared to teachers initially assigned to REP schools. Instrumenting wage changes with the school-type-specific bonus implied by the reform in a Two-Stage Least Squares (2SLS) analysis yields an exit elasticity with respect to the wage of -5.5 for the average teacher assigned to a treated school prior to the reform.[6] In other words, a 1 percent increase in wages (approximately 344 euros per year, based on the 2013 average wage at REP+ in 2024 euros) reduces the probability of exit for the teaching profession by -5.5 percent. I also estimate the local average treatment effect (LATE) using the realized bonus of those assigned to a treated school—i.e. those who remained in treated schools post-reform. The implied LATE exit elasticity is -8.1.

To study the heterogeneous effect by teacher productivity, I use a novel measure of teacher value-added (TVA) developed in Tartova (2023). During the reform period, standardized tests in French middle schools were administered only at the end of 9th grade for Math and French, making it impossible to apply standard TVA estimators, which require lagged standardized test scores to control for student–teacher sorting

[5] I abstract from comparisons between teachers in disadvantaged and teachers in ordinary schools, as disadvantaged schools (REP+ and REP) were subject to additional common, concurrent measures related to improving student learning conditions and reinforce professional development, which could have impacted teachers' propensity to exit.

[6] To get to this elasticity, I use both the counterfactual exit rate and the counterfactual teacher wage of teachers initially assigned to REP+ schools predicted in the absence of a differential wage increase.

(Rockoff 2004; Kane & Staiger 2008; Chetty et al. 2014a). To address this issue, I estimate TVA using a method that relies on cross-sectional standardized test scores in Math and French and exploits "networks" of teachers who share students across subjects. Intuitively, if two Math teachers both teach classes with the same French teacher, the difference-in-difference between their respective students' Math and French scores isolates the relative contribution of the two Math teachers to their student grades by netting out both the ability of the average student in each class that is common across Math and French, and the common French teacher's effect. By transitivity, these pairwise comparisons can be linked together within schools, and—using teacher mobility—across schools. The unbiasedness of the TVA estimates relies on the assumption that there is no sorting on subject-specific ability—that is, sorting on differences in students' abilities in Math versus French—for which there is little to no evidence in middle schools based on tests on observables (Tartova 2023).[7]

The central result of this paper is that exit decisions of "high-TVA" teachers, i.e. teachers whose TVA is above median in their subject, are significantly more responsive to the reform-induced change in teacher wages, compared to those of "low-TVA" teachers. Specifically, I find that the estimated exit elasticity to the wage for high-TVA teachers is 2.5 times as large as for low-TVA teachers, using either the intended or realized bonus as an instrument for the wage change. Despite being estimated in the population of teachers at disadvantaged schools, I show that these heterogeneous exit elasticities are also representative of teachers in less disadvantaged schools, as they do not significantly vary with a REP+ school's level of disadvantagedness.[8]

Tests of the identification assumption using placebo treatment groups within the high- and low-TVA subsamples yield results consistent with a causal effect of the policy, rather than a continuation of pre-existing trends. I demonstrate that, if anything, there is a small positive differential linear trend for the subsample of high-TVA teachers that reverses sharply at the reform date.

The heterogeneous effect for high- and low-TVA teachers is robust to a range of alternative specifications, controls, and a rich set of fixed effects. It also persists after accounting for potential heterogeneous treatment effects across other observable dimensions, including experience, age, gender, and qualification level.

I explain this heterogeneity in teachers' responsiveness to wages and quantify the implications of changes in wages for aggregate teacher quality in a static discrete-choice model of labor-supply decisions with two sectors (teaching and outside option) and two

[7]The method provides a reliable proxy for the standard TVA estimator (Kane & Staiger 2008), as shown in both Monte Carlo simulations and observational data from New York City—where annual testing allows for the two methods to be compared (Tartova 2023).

[8]If this were not the case, the counterfactual analysis would remain internally valid and would still allow for estimating the effects of different policy counterfactuals on aggregate quality in disadvantaged schools.

teaching productivity types (high- and low-TVA). Utility from working in either sector depends linearly on three ingredients: sectoral wages, non-pecuniary preferences for each sector, and idiosyncratic sector-specific taste shocks. I assume that taste shocks follow Gumbel distributions that are specific to each TVA type, and that teaching wages do not vary by TVA, while outside wages are allowed to vary with TVA. I confirm that when more productive teachers have a higher exit semi-elasticity than low-productive ones, a uniform increase in teacher wages unambiguously increases aggregate teaching quality through the change in composition induced by this exit channel.

In the framework, high-TVA teachers are more responsive to wages because their relative preference for the outside option is drawn from a distribution that is both less dispersed and closer to the indifference threshold between staying and leaving (higher average preference for the outside option), such that a given wage increase pulls a larger share of them from being close to leaving to staying. The higher average preference for the outside option in the framework can be a result of either a higher wage gap between the outside option and teaching, or a lower relative preference for non-pecuniary factors, e.g. lower pro-social motives.

In order to study the effect of different counterfactual wage policies on aggregate quality, I estimate the parameters of the framework using five empirical moments: the two type-specific exit elasticities, the two type-specific equilibrium exit rates observed pre-reform, and the pre-reform average (across all teachers) wage gap between outside options and teaching. I construct the pre-reform average wage gap by combining nationally set seniority-based teacher wages with wages in feasible outside options. Specifically, I measure the latter with a proxy reflecting the average wage of non-teachers in the same commuting zone with the same observable characteristics, working in occupations that teachers are observed to transition into, based on exhaustive employer–employee panel data.[9] The parameters are perfectly identified under two additional assumptions. First, I assume that the observed average wage gap is the equilibrium-exit-rate-weighted average of the gaps for each type. Second, I assume that non-pecuniary preferences for teaching do not vary by TVA type. This implies that the variation in relative preferences for outside options of high- compared to low-TVA teachers are driven by variation in outside wages rather than variation in preferences for non-pecuniary factors.[10]

I propose, and provide empirical support for, one micro-foundation which is consistent

[9]To mitigate concerns that the wages of non-teachers in the same cell may not accurately reflect the outside wages available to teachers—particularly for late-career switchers who may initially earn below incumbents—I show that the wages of former teachers lie close to the 45-degree line when compared to wages of non-teachers in the same commuting zone–age–sex–year cell.

[10]This assumption is consistent with evidence that high-TVA teachers have either similar preferences for non-pecuniary factors (e.g., Johnston 2025) or stronger pro-social motives (e.g., Andersen et al. 2014). Importantly, relaxing this assumption—allowing high-TVA teachers to have stronger preferences for teaching—would imply an even higher implied outside wage to rationalize their observed behavior.

with the theoretical framework and the higher elasticity of high-TVA teachers. Specifically, teachers with better outside options—such as high-TVA teachers (Chingos & West 2012), and therefore higher relative preferences for the outside options, optimally allocate more attention to learning about outside opportunities, which leads to greater precision when making a decision, and hence a lower dispersion of their distribution of relative preferences. Consistent with this interpretation, I find that teachers with higher relative pre-reform outside option wages are more elastic to wages.

The estimated framework indicates that targeting wage increases toward high-productivity teachers are substantially more cost-effective than a uniform bonus. Specifically, for a counterfactual uniform bonus policy of comparable average size to the relative REP+ bonus (1800 euros annually), a targeted bonus towards high-TVA teachers achieves the same 0.015σ of TVA gain in aggregate quality over five years at only a fourth of the cost.

Finally, I show that an increase in outside wages can lead to disproportionately larger losses in aggregate teacher quality compared to the gains from an equally sized increase in teacher wages. For an increase in the outside wage 1800 euros annually, holding teacher wages constant, I show that aggregate quality decreases by 0.051σ over five years. The asymmetry between the effects of increasing teacher wages and increasing outside wages follows from low equilibrium exit rates combined with the Gumbel assumption, which implies that relative preferences follow a logistic distribution. Intuitively, when exit rates are low, most teachers lie well to the left of the indifference threshold, with only a small share of marginal teachers in the right tail of the preference distribution. Raising teacher wages shifts the distribution further left, reducing this already thin margin. By contrast, higher outside wages move the distribution toward the threshold, increasing the density of teachers near indifference and, consequently, sharply raising exit rates.

Overall, this paper makes three primary contributions. First, it provides the first causal estimates of teacher wage responsiveness by productivity. I show that high-TVA teachers are more responsive to a uniform wage increase than low-TVA teachers, which implies that raising wages uniformly unambiguously improves aggregate teacher quality through selective retention. Second, I quantify the relative cost-effectiveness of targeted versus uniform increases in wages for improving aggregate quality in policy counterfactuals that use the estimated heterogeneous elasticities. Third, I show that increases in outside wages lead to disproportionately larger losses in aggregate teacher quality compared to equivalently sized increases in teacher wages, under standard assumptions on the distribution of preferences and as long as teachers on average still prefer teaching to leaving (i.e., when exit probabilities are on average low).

Related Literature This paper relates to four main strands of the literature.

First, it relates to the literature studying the effects on teacher quality of changes in teachers' compensation that do not target specifically effective teachers. Growing quasi-experimental evidence on the intensive margin, studying whether teachers change the effort they exert, is mixed. Ree, Muralidharan, Pradhan, and Rogers (2017) and Bobba et al. (2021) show that uniform increases in teacher wages do not lead to a within-teacher increase in effort. Bates and Johnston (2025) finds complementary evidence, exploits an effective decrease in teachers' effective compensation through a reduction in pension accrual rates around the pension-eligibility threshold. Regarding the extensive margin, non-experimental studies suggest that larger wage gaps—arising either from higher outside wages or lower teacher wages—are associated with more exits and less entries of high-quality teachers, which suggests a lower aggregate teacher quality.[11] The most closely related paper to my quasi-experimental analysis is Bates and Johnston (2025), which also studies differential retention. The authors find that the effective decrease in teachers' effective compensation around the pension-eligibility threshold does not retain high-productivity teachers disproportionately less. The lack of heterogeneity of exit elasticities around retirement may not be representative for younger teachers, as the relevant late-career outside option is a non-productivity-specific pension, rather than a wage in another job, which may vary by teacher productivity.[12,13] By contrast, my setting allows focusing on teachers across the age distribution to address this gap. I provide the first causal evidence that high-productivity teachers exit the teaching sector disproportionately less following an increase in teacher wages, implying that increasing teacher wages uniformly increases aggregate teacher quality.[14]

Second, I contribute to the broader literature that analyzes and compares the effect of different teacher compensation structures on teaching quality. One stream of the literature analyzes the effects of targeted (as opposed to uniform) changes in teacher

[11]Such papers use geographical and/or time-series variation in teacher wages or outside wages (Bacolod 2007; Dolton & Marcenaro-Gutierrez 2011; Figlio 1997, 2002; Britton & Propper 2016; Hoxby & Leigh 2004), as well as variation in local labor market unemployment levels (Deneault 2023; Falch, Johansen, & Strøm 2009; Fraenkel 2022; Nagler, Piopiunik, & West 2020), proxying for teacher quality using either teachers' education, students' standardized test scores, teacher value-added, or school value-added.

[12]Relatedly, Biasi (2024) finds that teachers respond four times more to changes in wages than to changes in pension incentives in terms of extensive-margin labor supply responses.

[13]Related evidence on early-retirement incentives indicates that discouraging early retirement can increase test scores via within-teacher effort (Johnston, Rockoff, & Harrington 2025), whereas encouraging early retirement yields no or positive effects on test scores, suggesting low-productivity teachers may be more elastic to such policies (Fitzpatrick & Lovenheim 2014).

[14]This finding is consistent with experimental evidence outside of teaching that higher wages can attract more able applicants to public-sector positions in the context of marginalized Mexican municipalities (Bó, Finan, & Rossi 2013). Yet other experimental work shows that financial incentives do not uniformly improve the selection of more able candidates into public-sector jobs. In the context of the Zambian healthcare sector, Ashraf, Bandiera, Davenport, and Lee (2020) find that emphasizing career benefits attracts more skilled candidates, while in Uganda, Deserranno (2019) shows that higher advertised earnings increase application volumes but reduce the share of highly prosocial workers, who are the best performers in this role.

wages.[15] Broadly, targeted increases in wages for high-productivity teachers are shown to positively impact student test scores through increased effort and/or positive selection into treated schools. Another stream analyzes the relative cost-effectiveness of different compensation policies under different objectives. One approach is to use simulations. For instance, Rothstein (2015) simulates the impact of alternative teacher contracts on teacher quality in a model that incorporates dynamic self-selection and Bayesian learning. Another approach is to elicit the preferences for wage and non-wage amenities and simulate a structural model identified from the preference estimates (for instance, Johnston (2025) using a large discrete-choice experiment asking teachers to make hypothetical choices over teaching job offers at schools with different pay structures and non-wage amenities).[16] My counterfactual policy simulations reveal complementary evidence that targeted bonuses are substantially more cost-effective in improving aggregate teacher quality than uniform bonuses, through the exit channel. The advantage of my simulations is that they are disciplined by a sufficient statistic identified through real-world heterogeneous labor-supply responses, without the need to impose structure on teacher preferences.

Third, a related literature analyzes how uniform bonuses in disadvantaged schools affect the educational gap through teacher quality. Studies provide indirect evidence for a positive selection into disadvantaged schools following the introduction of such bonuses, where teacher quality is proxied using certification or experience (Clotfelter, Glennie, Ladd, & Vigdor 2008; Silhol & Wilner 2022; Cabrera & Webbink 2020; Pugatch & Schroeder 2014, 2018). Using a measure of teacher value-added and data on applications for mobility within the teaching sector, Bobba et al. (2021) provides evidence that such policies indeed attract more productive teachers to disadvantaged (rural) schools. My paper contributes to this literature by showing that policies that increase wages at disadvantaged schools can reduce the educational gap with advantaged schools not only by reallocating teachers across schools, but also through disproportionately retaining more productive teachers in the teaching sector.

Fourth, the paper contributes to the broader literature on labor supply elasticities and imperfect competition in labor markets. A large body of work highlights monopsonistic

[15] For evidence on performance-pay or flexible wage schemes which favor high-productivity teachers, see for instance Atkinson et al. (2009); Biasi (2021); Burgess, Greaves, and Murphy (2022); Cowan and Goldhaber (2018); Dee and Keys (2004); Dee and Wyckoff (2015); Duflo, Hanna, and Ryan (2012); Figlio and Kenny (2007); Glewwe, Ilias, and Kremer (2010); Leaver, Ozier, Serneels, and Zeitlin (2021); Muralidharan and Sundararaman (2011); Biasi, Fu, and Stromme (2021); Brown and Andrabi (2025); Johnston (2025); Morgan, Nguyen, Hanushek, Ost, and Rivkin (2023); relatedly, see Barrera-Osorio and Raju (2017); Fryer (2013); Goodman and Turner (2013) for evidence on performance-pay based on school-wide performance which shows such bonuses are not effective at increasing test scores.

[16] Relatedly, Boyd, Lankford, Loeb, and Wyckoff (2013) estimate teacher preferences for school characteristics from equilibrium matches, Bobba et al. (2021) similarly infer preferences from observed sorting patterns using equilibrium matching. Fitzpatrick (2015) and Biasi (2024) both leverage quasi-experimental pension reforms to identify teachers' willingness to pay for retirement benefits.

features of labor markets, with related evidence in the public sector (e.g., Bó et al. 2013; Staiger & Rockoff 2010), where rigid pay scales and centralized allocation create limited mobility.[17] In teaching, evidence from quasi-experimental settings (often related to wage incentives at disadvantaged schools) find turnover elasticities to the teacher wage of between -3 to -6.[18] The size of these elasticities is increasing in the persistence of the wage variation and in the salience of the reform. As turnover encompasses both mobility to other schools and exit from the educational sector, these elasticities are not directly comparable to the ones in this paper. Specifically, mobility across schools is often rigid as positions are scarce, compared to the entirety of outside options. My quasi-experimental evidence from the French teacher labor market complements existing estimates by plausibly isolating an average exit elasticity to teacher wages of -8.1.[19]

The paper proceeds as follows. Section 1 presents the French institutional setting, and explains the 2014 reform, which introduced the wage bonus at disadvantaged schools. Section 2 introduces the data used in the study. Section 3 presents the network methodology for TVA estimation derived in Tartova (2023). Section 4 details the empirical strategy and results. Section 5 outlines the theoretical framework, provides evidence on a possible micro-foundation, and explores policy counterfactuals. Finally, Section 6 concludes.

1. Institutional setting

This section provides details on the French institutional setting and discusses in detail the 2014 reform that led to the differential increase in wages for REP+ and REP teachers.

1.1. The teaching profession in France

The teaching workforce in French public middle schools consists mainly of tenured teachers (more than 90% of the teacher workforce) and a smaller share of contractuals.

Becoming a tenured teacher in France Entry into a tenured position requires at least a Master's degree and passing the relevant national exam. There are two main national exams which provide the certification level needed to become a middle school teacher—*Capes*, which provides a basic certification level, and *Agrégation*, which provides an advanced certification level. The latter is much more difficult to obtain, as it is meant for individuals who target teaching at high schools or universities.[20] Newly recruited teachers serve a one-year probationary period with reduced teaching loads before receiving

[17] For broader evidence, see, for instance, Hirsch, Jahn, Manning, and Oberfichtner (2022); Manning (2003, 2011); Naidu and Carr (2022).

[18] For instance, Benhenda and Sims (2022); Clotfelter et al. (2008); Cowan and Goldhaber (2018); Falch (2011, 2017); Feng and Sass (2018); Hendricks (2014).

[19] While I cannot plausibly isolate labor supply through observed mobility, for comparability, I show that when taking into account both exits and transfers to other schools, I obtain a turnover elasticity to the teacher wage of -3.4.

[20] As a result, less than 6% of middle school teachers are Agrégation-certified.

full tenure. After tenure, a standard teaching load for a Capes-certified teacher is 18 hours per week, and for an Agrégation-certified teacher—15 hours per week.[21]

Teacher wages Teacher wages in France do not directly vary by teacher productivity. They are instead set on a national wage scale and depend primarily on the certification level (Agrégation-certified teachers are paid according to a higher pay scale) and seniority of the teacher.[22]

Identifying labor supply decisions For the purpose of identifying labor supply responses, I focus on the exit decisions of tenured teachers. Tenured teachers form the core of the teaching workforce and are civil servants in the State's public education service. Unlike contractual teachers, they are recruited via competitive national examinations, allocated to schools based on a centralized national system, hold permanent positions, progress automatically through a national pay scale based on seniority, and, importantly, cannot be dismissed for performance reasons, except in extreme cases involving disciplinary proceedings.[23,24]

By contrast, the decision to enter the teaching workforce at a particular school and reallocation across schools cannot be seen as pure labor supply decisions. Novice or tenured teachers are not directly recruited by schools, but are instead assigned to a school based on a centralized point-based system called *SIAM*. Points (and therefore priority) are given to teachers for their seniority, years of experience in their current school, the need for spousal reunification, and existing disability, as well as years of experience at disadvantaged schools. Consequently, observed mobility across schools or a teacher's initially allocated school may not be representative of their preferences.[25]

1.2. History of priority education in France and the 2014 reform

The French State established its first priority education system in 1982, with the goal of reducing the inequalities in academic success linked to social backgrounds. Originally,

[21]For comparison, the standard teaching load for a middle school teacher in the United States in 2013 is 27.25 hours per week (OECD 2015).

[22]In addition to the base wage, teachers may receive small bonuses due to overtime work, area of residence, number of children, and working at a disadvantaged school. In addition, having top scores on evaluations based on pedagogical classroom inspections and completion of administrative obligations helps reduce the number of years of experience needed to progress in seniority. Finally, teachers with at least 3 years of experience at disadvantaged schools also need fewer years of experience in order to progress on the seniority scale.

[23]While there are no official records of the precise number of teachers affected by such dismissal, this is less than 0.001%: official records point that for all civil servants in France in 2024 (roughly 5.8 million), only 59 individuals were dismissed for acts of sexual or gender-based violence (DGAFP 2025,INSEE 2025).

[24]I exclude contractual teachers from my analysis because their dismissal possibilities differ markedly from those of tenured staff. They are hired on fixed-term contract—primarily to cover teacher shortages or to replace absent civil servants, often for one school year or shorter. Their contract may not be renewed without justification.

[25]In co-authored work in progress, we use SIAM data of ranked preferences for allocation to uncover the labor supply responses within the educational sector to wage changes, in order to study the implications of wage bonuses at disadvantaged schools for the educational gap (Denker et al. 2025).

503 middle schools (roughly 10% of all public middle schools) were classified as belonging to a priority education zone.[26] By 1999, the number of middle schools had increased to 1,000—such that every 1 in 5 middle school students was part of the system. As part of this program, a yearly teacher bonus was first introduced in 1990, reaching 1,384 euros by 2006, in 2024 constant prices.

In 2006, priority schools were split in two tiers, based on the intensity of social and academic difficulties. The most disadvantaged middle schools (254) were given a RAR label whereas the rest of the disadvantaged middle schools (782) were relabelled as RRS. Importantly, while some additional resources were provided for RAR schools, such as the employment of additional teachers and educational assistants, the teacher bonus was held constant across tiers.

Timeline of the 2014 reform By 2013, multiple evaluations had concluded that the priority education policies to date had largely failed to achieve their objectives, and large socio-economic and academic achievement gaps persisted between students in priority and non-priority schools.[27] Moreover, disadvantaged schools continued to face chronic staffing difficulties, with higher teacher turnover, difficulty attracting experienced teachers, and over-representation of novice or contractual teachers. These findings prompted the government audit (*Modernisation de l'action publique*) published in July 2013 that recommended a complete redesign of the policy, outlining the introduction of two new tiers of priority education, REP (*Réseaux d'éducation prioritaire*) and REP+, to replace the RRS and RAR tiers, respectively.

In January 2014, the French Ministry of Education announced formally the overhaul of the priority education system, and outlining the detailed scope of the policy, importantly including the precise amounts of staff bonuses. Work began on redesigning the map of disadvantaged secondary schools. Self-selection of schools into the program was not possible and there was little scope for manipulation, as the precise criteria used by the government to classify schools were publicly unknown, and historic shares already reported to the Ministry were used. The new status classifications into REP and REP+ schools were based on an index created by the Ministry of Education which was based on four indicators: the historic shares of very disadvantaged students within the school, need-based scholarship holders, students who repeated a grade before entering middle school, and students living in or very close to a disadvantaged urban neighborhood.

During the 2014-2015 school year, priority was given to the 100 middle schools (and

[26]Primary schools were also part of the program, as well as few high schools. As my analysis focuses on middle schools, I focus on the evolution of the priority education system for middle schools.

[27]Previous research finds little to no effect of the policy on student outcomes (Bénabou, Kramarz, & Prost 2009), and in some cases negative effects—through increased disparities in teacher quality across schools (Beffy & Davezies 2013) and greater social segregation as advantaged students move to private schools (Davezies & Garrouste 2020).

their associated primary schools) in the highest difficulty, which already entered the new system and receive a set of student-related measures and the promise of the teacher bonus in 2015-2016. During the same school year, the rest of the list of schools which would become part of the system in the 2015-2016 school year was revealed, for a total of 365 REP+ and 732 REP middle schools.

During the 2015-2016 school year, all announced priority schools entered the program, and the announced measures, including the staff bonus, were put in place.

Common scope of the 2014 reform for REP+ and REP schools The 2014 reform of priority education introduced a common set of measures across REP and REP+ schools, many of which were directly targeted at improving student learning conditions. At the middle school level, the pedagogical strand of the reform emphasized differentiated instruction, supported by tools such as a digital platform (D'Col) which offered sixth-grade students targeted exercises in core subjects, and structured homework assistance programs for 2 hours per week. Schools also benefited from the deployment of additional non-teaching staff—including education assistants, social workers, and school nurses—intended to strengthen the support system around students in disadvantaged areas.

For teachers, the reform sought to reinforce professional development. Dedicated time of 1.5 hours per week was introduced for collective work with field experts. Novice teachers in REP and REP+ schools benefited from reinforced mentoring and closer supervision for 3 days per year (increased to 18 half-days in 2015).

Differential scope of the 2014 reform for REP+ schools Two additional measures were intended for REP+ middle school teachers, and not for REP teachers: a significantly higher wage bonus, which I exploit in my empirical strategy, and a small reallocation of teaching hours towards liaison with students and families, which I show led to an increase in working hours that may have, if anything, either negatively impacted exits, or acted as an additional wage bonus through overtime pay.

First, the main aspect of the differential reform was that REP+ teachers received a significantly higher financial bonus than REP teachers. Figure 2 presents the unveiling of the bonus introduced in REP and REP+ schools starting from the 2015-2016 school year (CPI-deflated to 2024 thousands of euros). As seen in Panel (a), in 2015-2016 the yearly bonus for teachers in REP+ schools doubled compared to the pre-reform amount (from 1,384 to 2,767 euros). For REP schools, the increase was smaller (to 2,075 euros). Post-2015, the bonus in REP schools was not increased further. For REP+ schools, the yearly bonus was further increased to 4,039 euros in 2018-2019 (announced in the summer of 2018 prior to the start of the school year), then to 5,334 euros in 2019-2020 (announced in the summer of 2019 prior to the start of the school year). In sum, on average, teachers at REP+ schools received an increase of 7.1% over the first six years, relative to the average wage of REP+ teachers in 2013 (Panel (b). For REP teachers, the increase for the average teacher was 2% percent, relative to the average wage of REP teachers in 2013.[28]

[Go to Figure 2]

Second, the reform mandated the reallocation of a small portion of teaching hours towards liaison with students and families for REP+ but not for REP teachers, leading to the reallocation of between 4 and 4.5% of the 35-hour working week.[29] This was done by re-weighting teaching service hours, such that each classroom hour started counting as 1.1 hours in the official calculation of teaching load.

In practice, official reports and union communications indicate that the reform rarely translated into the intended reduction in teaching hours, but instead led to an overall increase in working hours due to the additional time spent in liaison with families and students.[30] I find empirical evidence consistent with this in Section 4.7, indicating that the reform led to a 2.4% increase in weekly working hours.[31] On the one hand, if teachers responded negatively to longer working hours, this change may have increased their probability of exit, partially offsetting the effect of the bonus. On the other hand, as the additional hours were compensated as overtime pay—remunerated slightly better than before the reform, as I show in Appendix C.6—they may have acted as a small additional wage increase.[32] Section 4.7 shows that, under conservative assumptions, the latter does not significantly affect the baseline exit elasticity.

2. Data and measurement

Teacher-school panel data In order to identify the labor supply decisions of teachers, the main data I use in this paper are teacher panel data from the Statistical Office of the French Ministry of Education (DEPP).

I construct a national-level panel of all middle-school teachers in Metropolitan France

[28] Or still 7.1% and 2%, respectively at REP+ and REP, with respect to the average teacher wage in 2013.

[29] For a full-time Capes-certified (Agrégation-certified) teacher with a statutory teaching obligation of 18 (15) hours per week (out of a mandated 35-hour working week), this translated into an envisioned effective teaching load of approximately 16.4 (13.6) hours. The remaining 1.6 (1.4) hours per week were supposed to be reallocated to student follow-up and liaison with families.

[30] Independent public reports later confirmed that the reform was "often converted into annual overtime hours", undermining its goal (Cour des comptes 2018). Similarly, two major French teachers' unions, SGEN-CFDT and SNEP-FSU, noted that many REP+ teachers were still required to perform mandatory overtime, in addition to the introduced weekly hours for follow-ups with students and parents, contradicting the original purpose of the reform (SGEN-CFDT 2020, SNEP-FSU 2018).

[31] Given the ratio between 1) the difference between the mandated reallocated hours and the realized reallocated hours, and 2) the observed total weekly working hours of REP+ teachers in 2013, (1.6-0.76)/35.7.

[32] See Appendix C.6 for details on the calculation of overtime pay.

(i.e., excluding overseas territories) between the 2007-2008 and 2022-2023 school years, using the Bases Relais database.[33] The database is a collection of yearly snapshots of active teachers in the school registry of all public schools in France. It provides personal information on all teachers, such as their date of birth, gender, qualification, seniority, tenure, specialization, experience, as well as position in the wage scale. It also provides information on the schools, subjects, and classes taught by each teacher every year.

I restrict the sample of teachers to tenured teachers, in order to isolate pure labor supply decisions, given that the employment of contractual teachers is not immune to dismissals. Furthermore, I take the subsample of exits for individuals under 50 to avoid capturing exits due to (early) retirement, which would add noise to the estimation.

REP+ and REP school teachers represent, respectively, 6.7% and 14.7% of the sample of all teachers in 2013. REP+ and REP teachers share broadly similar characteristics, with REP+ teachers being somewhat younger, less experienced, and more often male. In my analysis, I therefore control for such characteristics to account for unconditional differences in exit rates. Both groups contrast with teachers in ordinary schools, who tend to be older, more experienced, and more qualified (see Table A.1 for descriptive statistics of teachers in 2013 by type of school). For this reason, I discuss the external validity of my results in Section 4.5.

The data allow me to identify the year, school, and commuting zone from which each teacher exits the profession. I define an *exit* in school year $t/t{+}1$ as 1 if a teacher is not observed in secondary school teaching during at least the following two school years, $t{+}1/t{+}2$ and $t{+}2/t{+}3$. The exit share in year $t/t{+}1$ is therefore the fraction of teachers last observed in that year. This definition abstracts from short-term absences (e.g., due to sick leave, maternity leave, or certification training) that might otherwise be misclassified as exits, and allows for re-entry from year $t{+}3/t{+}4$ onward. Because teachers can return within five years without losing civil-servant status, this approach captures both short- and long-term exits. The analysis is thus restricted to 2019–2020, the last year for which an exit can be observed with a potential return by 2022–2023.

Figure B.1 presents the aggregate exit rates by experience group for the sample of all tenured teachers. Exit rates have consistently been increasing across experience groups for the last decade, but most strikingly so for teachers at the beginning of their career, whose exit rates have nearly doubled since 2012. As of 2013, REP+ teachers do have higher average exit rates: 2.6%, compared to 2% at REP and 1.7% at ordinary (Table A.1).

Matched student-teacher data In order to estimate teacher value-added (my measure of teacher productivity) and labor supply elasticities by TVA level, I match the teacher panel data to cross-sectional data on student test scores and personal

[33] I focus on Metropolitan France only and exclude overseas territories as the latter have local labor markets that function very differently.

information, provided by the databases Scolarité, Sysca and DNB, using classroom identifiers.[34]

To estimate TVA, I focus on the universe of Math and French teachers who have taught 9th-grade students at least once between 2009 and 2020, as middle-school students at the time only sat standardized exams in Math and French at the end of the 9th grade (called the DNB). Table A.2 shows that the average Math and French teachers in 2013-2014 are very similar to the average teacher (across all subjects), with two exceptions. First, Math (French) teachers are much less (more) likely to be female. Second, the average 2013 exit rate for both Math and French teachers is higher than that of the average teacher. At REP+ schools, Math and French teachers exit at a rate of 3.3% and 3.8%, respectively, while their counterparts at REP schools exit at a rate of 2.5% and 2.7%, respectively.

Overall, the panel of middle school tenured teachers below 50 with a TVA measure comprises 67.9% of Math teachers and 68.8% of French teachers (Table A.2). This is because 1) only 79% of all Math and French teachers teach the 9th grade, and 2) the merge between student and teacher data leads to the loss of roughly 10 percentage points of 9th-grade Math and French teachers, due to missing or wrongly encoded classroom identifiers on either the teacher- or the student-side of the data. These issues are more pronounced at REP and REP+ schools, leading to lower numbers of identified teachers with TVA at REP+ (57.4% of Math teachers and 58.3% of French teachers) and REP (62.7% and 62.6% of Math and French teachers, respectively).[35]

Teacher salary data To estimate the exit elasticities to the teacher wage and the labor supply framework, I obtain publicly available data on time-varying teacher wages by teachers' position on the wage scale (echelon) for teachers with standard qualification (Capes, more than 90% of teachers in my sample).[36]

Matched employer-employee data To estimate the outside-option wage, required to estimate the labor supply framework, I obtain exhaustive French employee–employer matched data from tax returns (BTS-Postes, previously called DADS Postes), provided by the National Institute of Statistics and Economic Studies (INSEE), covering the period 2009–2021. To extend the BTS-Postes cross-sections into a longitudinal dataset covering the full 2009–2021 period, I apply the methodology proposed by Godechot, Palladino, and Babet (2023). The matched employer-employee panel allows me to observe the full employment trajectory over the period 2009-2021 for all workers.

To define the set of outside option occupations to which middle school teachers

[34]Table A.1 presents variation across types of schools in average students' characteristics for the year 2013-2014, which is consistent with the criteria used to define disadvantaged school labels.

[35]Specifically, 14 and 17 percentage points of 9th-grade Math teachers at REP and REP+ schools, respectively, and 14.4 and 16 percentage points of 9th-grade French teachers are lost in the fuzzy matching algorithm. For more details on the merge of the data, see Appendix C.1.

[36]The data is made available by Lucas Chancel on his website.

transition, I focus specifically on middle-school teachers below 50 years old who are observed to transition to another occupation out of teaching at some point over this period. Figure B.2 reports the top 15 broad occupations in which exiting teachers are employed $k = \{1, 3, 5\}$ years after leaving teaching, based on national exit shares by occupation.[37,38] Appendix C.2 provides more details on the methodology used for creating an exhaustive panel, the sample restrictions and measurement choices, and a discussion on the set of outside options for teachers.

Using the set of all outside-option occupations, $\mathcal{O}$, I define the outside-option wage of a teacher of a given sex g, age group a, who teaches in a given commuting zone z in year t, as:

$$OutsideOptionWage_{gazt} = \frac{\overline{S_o} \cdot D_{ogazt}}{\sum_o \overline{S_o} \cdot D_{ogazt}} \cdot OutsideOptionWage_{ogazt} \tag{1}$$

where occupation $o \in \mathcal{O}$, $\overline{S_o}$ is the average share of teachers exiting to o over the period 2009-2021, and D_{ogazt} is the share of workers at o in a given cell $gazt$.

A concern for the estimation of the counterfactual outside-option wage for teachers might be that the wages actually attained by teachers transitioning to a given occupation may be lower than the those of incumbents. Figure B.6 provides evidence of a strong and close to 1-to-1 correlation between (i) the mean wage in a given occupation o for individuals of a given sex g, age group a, commuting zone z and year t, and (ii) the mean wage that teachers exiting to an occupation o obtain 3 years after exit, within the same sex g, age group a, commuting zone z and year t. This suggest that the average outside-option wage, $OutsideOptionWage_{gazt}$, provides a good estimate of the true average local outside-option wage for teachers of a given sex g and age group a.[39]

3. Teacher value-added estimation

To study heterogeneous elasticities by teacher productivity, I proxy productivity with teacher value-added (TVA), estimated using the network method proposed in Tartova

[37] The largest shares move into administrative jobs, either education-related (≈20% by $t+3$ post-exit) or not (≈20%). Within administration, they take either public-sector positions (executive or management roles in government, financial services staff, or technical work for the State) or private-sector roles (general or sales administration, litigation, or personnel management).

[38] One caveat of the database is that it does not allow to distinguish between exiting teachers' taught subject. However, as the majority of occupations are general and can be performed by both Math and French teachers, this is less concerning.

[39] As exiting teachers may also move to a different labor market (and indeed, roughly 40% do), I also estimate a version of the outside-option wage which takes into account national, rather than local, weights of labor demand and wages. This alternative measure of the outside-option is highly correlated with the local one, but predicts a higher estimate of the teacher wage gap than that predicted from the local outside-option wage. As a result, I take the the local estimate of the outside wage, $OutsideOptionWage_{gazt}$, as my main, more conservative, estimate. For moving teachers, $OutsideOptionWage_{gazt}$ may be lower than their true counterfactual outside-option wage.

(2023). This approach is necessary because in France standardized testing occurs only at the end of 9th grade, making it impossible to use lagged student scores to control for unobserved ability—a requirement for unbiased TVA estimation in standard approaches (see, e.g., Rockoff 2004; Kane & Staiger 2008; Chetty et al. 2014a). Importantly, the method has been shown to produce estimates and standard deviation of TVA that are highly correlated with those produced by the standard Kane and Staiger (2008) method, using matched teacher-student data from New York City, where students are assessed with standardized exams annually (Tartova 2023).

In what follows, I summarize the method.

Intuition behind the method The method produces plausibly unbiased TVA estimates by exploiting cross-sectional variation in student test scores across subjects—applicable in my context, where students take standardized Math and French tests at the end of 9th grade—and by leveraging "networks" of teachers. Intuitively, if a student's score is the sum of student ability, a teacher effect, and idiosyncratic noise, then one can measure the relative value-added of two Math teachers by taking the difference-in-differences of their students' Math and French scores. This nets out the difference in the average student ability (that is common across Math and French) between the two Math teachers. However, differencing French scores introduces one more source of variation: the contribution of the French teachers of these students to their French scores. To net out the latter, the paper applies this general difference-in-differences method by only comparing Math teachers who have taught groups of students (i.e., "classroom") that have been taught by the same French teacher (i.e., networks of teachers). By transitivity, pairwise differences between Math teachers can be linked within schools and—using teacher mobility—across schools.

Formally, a network is defined as:

DEFINITION (Teacher networks): *For a subset of classes $\mathcal{C}^n \subseteq \mathcal{C}$, a network of teachers is a subset of teachers $\mathcal{J}^n \subseteq \mathcal{J}$, for which for each class $c \in \mathcal{C}^n$, there exists at least one class $c' \in \mathcal{C}^n \setminus \{c\}$ such that the teacher for subject z in class c also teaches in class c':*

$$\exists c' \in \mathcal{C} \setminus \{c\},\ j(c,z) = j(c',z).$$

To illustrate the intuition behind the definition of teacher networks, I borrow Figure 3 from Tartova (2023). Blue nodes represent Math teachers and yellow nodes French teachers. An edge between a Math and a French teacher indicates that they have taught the same classroom at least once. Although M_2 and M_6 never share the same French teacher, they are connected through M_1 via L_1 and L_4, illustrating connectivity by transitivity. The number of classroom observations equals the number of teachers, allowing all teacher effects to be identified.

[Go to Figure 3]

Identification assumption The unbiasedness of the TVA estimates relies on the assumption that Math teachers are not sorted to classrooms based on the relative ability between Math and French in these classrooms. This is because French test scores control for student ability in Math to the extent that Math and French student ability is common, but students may be better in Math than in French.

While the assumption cannot be tested for unobservable student characteristics such as ability, Tartova (2023) performs tests of the assumption based on observable characteristics and shows evidence that, while there seems to be sorting between teachers and students on common ability, there is little evidence of sorting on subject-specific ability at the middle school level.

The paper further shows in Monte Carlo simulations that as long as student-teacher sorting is not based on subject-specific ability, the method's estimate are highly correlated with the true teacher effects with minimal mean squared error (consistent with the fact that teachers are observed in few classrooms—a common feature of TVA estimations).

Estimation To estimate the TVA of Math or French teachers, I restrict the sample of teachers to the subset of teachers $\mathcal{J}^n$ that represents the largest connected network. As teachers move across schools, the network of teachers $\mathcal{J}^n \subseteq \mathcal{J}$ spans across schools, connecting within-school networks with at least one mobile teacher. In the context of France where teachers often move across schools, more than 95% of teachers are connected in the same network and can therefore be compared. This leads me to a sample of roughly 16,600 Math and 21,200 French teachers under the age of 50 between 2009-2010 to 2021-2022.

To estimate the teacher value-added of teachers in a network, I use the following OLS specification:

$$\Delta A_i^{*MF} = \alpha + \mathbf{X}_i\beta + \sum_{m\in\mathcal{J}_M^n} \mu_m \mathbb{1}(j(M) = m) + \sum_{l\in\mathcal{J}_F^n} \mu_f \mathbb{1}(j(F) = f) + \varepsilon_i \qquad (2)$$

In this equation, ΔA_i^{*MF} is the difference between the Math and French scores for student i. Importantly, μ_m are Math teacher fixed effects for teacher m, μ_f are French teacher fixed effects for teacher f.

I control for observable student, classroom and school characteristics $\mathbf{X}_i$ which may affect test scores in Math and French differently. Specifically, I control for the socio-economic status (SES) of student i, their age, gender, and needs-based grant, advanced classes taken, and exam repetitions. Classroom-level controls include all student-level controls averaged at the classroom-level, e.g. number of peers, average SES, average age, and percentage of female students. I include the same controls aggregated at the school level, and further add school-level lagged average test scores in Math and French, to control for the relative quality of a school in Math, compared to French. Finally, ε_i is the idiosyncratic error term. To reduce the noise in the estimates, I restrict the sample of classrooms to those with at least 5 students.

As teachers, and especially less experienced ones, often teach few classrooms, which may lead to noise in their TVA estimates, I shrink TVA estimates using the Empirical Bayes shrinkage method outlined in Tartova (2023). The shrinkage method is outlined in Appendix C.3.

Standard deviation of TVA The estimated standard deviation (SD) of TVA in France for the subsample of teachers below the age of 50, using the network method and exploiting the mobility of teachers across schools, closely resembles estimates found in the U.S. literature. In Math, holding all else equal, a 1 SD better Math (French) teacher leads to 0.13 (0.11) SD higher test scores.[40] This means that moving a student from a teacher who is at the 5th percentile of the teacher value-added distribution to someone who is at the 95th percentile would lead to a 0.44 (0.36) SD increase in student exam scores.[41]

Observable characteristics associated with the TVA estimates I show that these TVA estimates are correlated with certain observable characteristics (Figure B.3). Specifically, Math TVA is positively correlated with experience, having a high qualification (Agrégation), being female, and having a higher pedagogical score during classroom observations by external examinators. For French, having more experience is not significantly correlated with TVA. Having a high qualification is positively associated with having a high TVA for French, by an order of magnitude similar to that for Math teachers. Being female and having a higher pedagogical score are also positively associated with French TVA.[42,43]

4. Effects of wages on teacher exit

In this section, I present the empirical strategy used to identify the causal effect of teacher wages on exit from the profession and show both that the average teacher is highly responsive to changes in the wage, and that high-productivity teachers are 2.5

[40]The SD is computed as the square root of σ^2_μ, where $\sigma^2_\mu = Cov(\widehat{A}_{mt}, \widehat{A}_{mt'}) - \sigma^2_l$, such that $\sigma^2_l = Cov(\widehat{A}_{lt}, \widehat{A}_{lt'})$.

[41]By comparison, Chetty et al. (2014a) finds teacher effects in lower-secondary education in New York City in the magnitude of 0.13 SD in Math and 0.12 SD in Reading.

[42]The level of correlation between TVA and the observables is different across Math and French. Particularly, Math TVA is more strongly correlated with every observable characteristic. This means that it is harder to pinpoint what makes a productive French teacher based on observable characteristics.

[43]These results are broadly in line with existing literature, which generally finds either small or no correlation between TVA and having a higher qualification, and a positive correlation with experience at the beginning of one's career (see, for instance, Rockoff 2004; Rivkin, Hanushek, & Kain 2005; Kane, Rockoff, & Staiger 2008; Wiswall 2013; Jackson 2014; Bold et al. 2016; Bietenbeck, Piopiunik, & Wiederhold 2018; Bau & Das 2020).

times more elastic to wages than low-productivity teachers.

4.1. Empirical strategy

To identify the causal effect of the change in the wage, I compare the exit rate of tenured teachers at REP+ to the exit rate of tenured teachers at REP schools in a difference-in-differences framework. My main empirical strategy estimates an Intention-to-Treat (ITT) effect, instrumenting for actual exposure to the reform by exploiting the pre-reform (2013) allocation of teachers to schools. To account for the fact that teachers may move out of their pre-reform assigned school after the reform is implemented, I also identify the local average treatment effect (LATE) which represents the instrumented responsiveness of pre-assigned teachers who actually received the treatment. I restrict the analysis to school years up to and including 2019–2020, which is the last year for which my definition of exit can be applied.

A consequence of the ITT estimation, where treatment is defined prior to the reform, is that the identified effect obtained from the difference-in-differences framework is free of possible endogenous relative selection into treatment after the reform. Specifically, teachers whose labor supply is more elastic to wages would be more likely to select into treated schools because of the reform, for instance by moving from other type of schools. If the relative composition in the treated schools shifts towards more elastic teachers, allowing treatment to depend dynamically on the post-period school assignment may be biased upwards.

Formally, for the subsample of teachers j who are teaching at school s in commuting zone z in year in 2013-2014, I estimate the following event-study difference-in-differences specification for each year t:

$$exit_{jst} = \sum_{2013}^{2019} \eta_k \mathbb{1}(t = k) \times \mathbb{1}(type_j = REP+) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst} \tag{3}$$

The outcome variable $exit_{jst}$ is a dummy equal to 1 in the last year before j exits (to reflect exit probability), and 0 otherwise, such that a teacher is dropped out of the panel in the year they exit the teaching sector. The treatment dummy $\mathbb{1}(type_j = \text{REP+})$ is equal to 1 if teacher j was employed in 2013 at a school s that would later be classified by the reform as REP+ after 2014, and zero if teacher j was employed in 2013 at a school that would later be classified as REP.[44] The indicator $\mathbb{1}(t = k)$ equals 1 if the observation is in the academic year $k/k+1$. The base year is 2013-2014, as the year during which the reform (and wage increase) was announced. To identify the average difference-in-differences effect, I also substitute the year dummies with a dummy, $Post_t$, equal to 1 in all years between 2014 and 2019, and 0 in 2013. The coefficients of interest, η_k, capture

[44] A school is considered as REP+ or REP, if it entered the REP+ or REP program in 2014-2015 or any subsequent academic year.

the causal effect of having been pre-assigned to a school that later became REP+, and therefore received the disproportionate increase in wages, on a teacher's probability of exiting the profession in year k, relative to their probability of exit in 2013-2014, as compared to teachers assigned to schools that later became REP.

To account for unconditional differences in exit rates across teacher characteristics, I include a rich set of 2013 teacher controls $\mathbf{X}_j$. Specifically, I control for being a female teacher, having a high qualification ("Agrégation"), experience tercile, age tercile, and subject taught. I account for time-invariant differences in exit probabilities across schools by including 2013-school fixed effects, θ_s, based on teachers' school assignments in 2013. Finally, I include commuting zone-by-year fixed effects λ_{zt}, where the commuting zone is defined based on the teacher's location in 2013, which control for shocks to local labor markets (such as changes in outside opportunities or demographics) that could influence teachers' exit decisions. This implies that my specification is comparing teachers in REP+ and REP schools that are in the same commuting zone in 2013.[45]

Standard errors are two-way clustered by teacher and by school-year to account for potential correlation in unobserved shocks—both serial correlation in teacher-specific factors and cross-sectional dependence among teachers in the same (2013-assigned) school and year, for example due to contemporaneous changes in the REP+ bonus, communication about the reform, or changes in local labor market conditions.[46]

From reduced-form estimates to exit elasticities with respect to the wage I translate the reduced-form coefficients to exit elasticities with respect to the wage, by exploiting the intended school-specific wage bonus amount generated by the reform as exogenous variation in the teaching wage in a Two-Stage Least Squares approach (2SLS).

To align the timing of exit decisions with wage expectations, I code the intended bonus treatment in year t using the bonus schedule of year $t+1$ whenever the full scope and scale of that schedule (i.e. the list of targeted schools and the corresponding amounts) were known before the interim period between t and $t+1$.[47] This is because, as explained above, I define an exit in school year t (e.g. 2014–2015) as equal to 1 if a teacher is observed for the last time in t and thus leaves the system between school years t and $t+1$. Because an effective exit between t and $t+1$ (recorded at t) is likely to be based on information revealed by or during the interim period between t and $t+1$ (i.e. the summer break) about wages in $t+1$, it is important to align the timing of wage information with

[45] My results are robust to using an alternative for local labor markets, specifically, *académie*-by-year fixed effects (i.e. regional-educational-authority-by-year) λ_{rt}.

[46] The results are robust to alternative choices of clustering, specifically teacher, 2013-assigned school, and teacher and 2013-assigned school (see Table A.3).

[47] For example, the first differential bonus increase for REP+ teachers was implemented in 2015–2016. While the size of the wage increase was already known in 2013–2014, the complete list of treated schools only became available during 2014–2015. This implies that the full treatment was known to teachers in 2014–2015, so that the 2015–2016 bonus would affect exit decisions made at the end of the 2014–2015 school year.

this decision. Because each increase effective in $t+1$ was known to teachers by the interim period between t and $t+1$, this coding choice is equivalent to shifting the policy schedule one year earlier in the analysis. The resulting instrument is plotted in Figure B.4.

In the first stage of the 2SLS approach, I regress the initial-school-level intended bonus $Bonus^I_{jst}$ for teacher j at school s in year t on the interaction of the post-reform indicator with the REP+ treatment, controlling for the same observable teacher characteristics as in the baseline reduced-form specification:

$$Bonus^I_{jt} = \beta Post_t \times \mathbb{1}(type_j = REP+) + \gamma \mathbf{X}_{jt} + \theta_s + \lambda_{zt} + \varepsilon_{jst} \tag{4}$$

In the second stage of the 2SLS approach, I regress the teacher exit dummy on the fitted values of the bonus from the first stage, with the same set of controls:

$$exit_{jst} = \eta^I \widehat{Bonus^I_{jt}} + \gamma \mathbf{X}_{jt} + \theta_s + \lambda_{zt} + \varepsilon_{jst} \tag{5}$$

The coefficient η^I in regression 5 represents the average impact of the intended bonus on the probability of teacher j to exit. Because the interaction term in the first stage is based on an indicator for the post-reform period, my two-stage approach does not mechanically condition exits in year t on the realized bonus in year t, but instead conditions exits in the post-period to the average intended bonus in the post-period. This is done in order to reduce noise in the estimates and to avoid assuming a narrow dependence of exit on the contemporaneous intended bonus.

I compute the elasticity of exit with respect to the wage as:

$$\varepsilon^{ITT}_{exit} = \eta^I \cdot \frac{\overline{Wage}^I_{REP+,CF}}{P(exit)_{REP+,CF}} \tag{6}$$

where $\overline{Wage}^I_{REP+,CF}$ is the estimated average intended counterfactual wage for treated teachers in the post-reform period, had it not been for the differential reform, and $P(exit)_{REP+,CF}$ is the average counterfactual exit rate of treated teachers.[48]

[48]The counterfactual exit rate (or, respectively, wage) is defined as the average exit rate (wage) that initially assigned REP+ teachers would have experienced in the post-period, had they not experienced a differential increase in wages compared to initially assigned REP teachers, under the assumption of no differential trend in exit rates (wage) between the two groups after 2013. Specifically, the counterfactual exit rate (hereafter $\overline{exit}_{REP+}$) is constructed from the estimates of $exit_{jst} = \alpha + \beta_1 Post_t \times REP+_j + \beta_2 Post_t + \beta_3 REP+_j + \varepsilon_{jst}$, such that $\overline{exit}_{REP+} \equiv \alpha + \beta_2 + \beta_3$, where α represents the pre-reform exit rate for initially assigned REP teachers, β_2 the change in the exit rate for initially assigned REP teachers post-reform, and β_3 the pre-reform difference in exit rates between initially assigned REP+ and REP teachers. Similarly, $\overline{Wage}_{REP+,CF}$) is constructed from the estimates of $Wage_{jst} = \alpha' + \beta'_1 Post_t \times REP+_j + \beta'_2 Post_t + \beta'_3 REP+_j + \varepsilon_{jst}$, such that $\overline{Wage}_{REP+,CF} \equiv \alpha' + \beta'_2 + \beta'_3$, where α' represents the pre-reform average wage for initially assigned REP teachers, β'_2 the change in the wage for initially assigned REP teachers post-reform, and β'_3 the pre-reform difference in wages between initially assigned REP+ and REP teachers.

Accounting for imperfect compliance As some teachers that are treated based on their 2013-2014 assignment may move out of treated schools after the reform is implemented, I also estimate the LATE for pre-reform REP+-assigned teachers who stayed in these schools after the reform was implemented. Specifically, the 2SLS now instruments the realized bonus received by j, $Bonus^R_{jst}$, rather than the intended bonus with the ITT variable $Bonus^R_{jst}$ in Equations 4 and 5. The η^R coefficient from the corresponding regression 5 now represents the average impact from receiving the bonus on the probability of teacher j to exit after the reform.

The corresponding LATE elasticity of exit with respect to the wage is:

$$\varepsilon^{LATE}_{exit} = \eta^R \cdot \frac{\overline{Wage}^R_{REP+,CF}}{P(exit)_{REP+,CF}} \tag{7}$$

where $\overline{Wage}^R_{REP+,CF}$ and $P(exit)_{REP+,CF}$ denote the LATE analogues of the corresponding ITT counterfactual wage and exit rate for treated teachers in the post-reform period.

Heterogeneity by TVA I study the heterogeneous exit responsiveness by a teacher's level of productivity in subsample analysis, whereby I estimate the corresponding ITT and LATE elasticities for the subsamples of high- and low-productivity teachers. Productivity is proxied by the TVA measures constructed following the method of Tartova (2023), as described in Section 3.

Specifically, I classify teacher j as a "high-TVA" teacher in subject m (Math or French) if their TVA exceeds the subject-by-commuting-zone-specific median of the TVA distribution among all teachers with estimated TVA observed in the pre-period. The indicator is defined within commuting zones to reflect the fact that the distribution of teacher quality may vary across local labor markets. I also demonstrate the robustness of the results to alternative ways of splitting the TVA distribution.[49]

To confirm the stability of the differential effect of the wage increase on high-TVA REP+ teachers' probability to exit, I further use triple difference-in-differences specifications to control for an extensive set of fixed effects, and for potential differential treatment effects by observable teacher characteristics that are correlated with TVA.[50]

4.2. Identification assumption

The validity of the difference-in-differences design relies on the parallel trends assumption, which requires that, absent the reform, exit rates for REP+ and REP teachers would have

[49] As in the analysis for the average teacher, standard errors are two-way clustered by teacher and by school-year. Results are robust to different clustering (see Table A.4).

[50] Standard errors in the triple difference-in-differences analysis are two-way clustered by teacher and by school-by-year-by-TVA-type, to account for the cross-sectional dependence among teachers of a given TVA type in the same (2013-assigned) school and year.

followed similar trajectories after 2013-2014. The parallel trends assumption cannot be tested in the pre-period within the ITT sample because, by construction, any teacher observed in a treated or control school in 2013-2014 cannot have exited in previous years.

To assess the plausibility of the identifying assumption, I redefine the ITT on a placebo sample based on an earlier pre-reform assignments: notably, in 2009-2010 and 2011–2012. This allows to test whether the exit rates of teachers assigned at future REP+ n-years before the reform ($n \in \{2, 4\}$) were on a differential trend during the $n - 1$ years before the reform, compared to those of teachers assigned at REP n-years before the reform.[51]

For each subsample—all teachers, high-TVA teachers, and low-TVA teachers—I find a structural break in the relative probability of exit at the time of the reform, consistent with a causal impact of the policy on the average teacher and on the subsamples of high- and low-TVA teachers, rather than a continuation of pre-existing trends (Figure 4). Specifically, there is no evidence of differential pre-trends for the sample of all teachers and the subsample of low-TVA teachers (Panel (a) and Panel (c), respectively). For high-TVA teachers, I find, if anything, a small linearly increasing differential pre-trend, which would indicate that teachers assigned at REP+ schools prior to the reform were exiting disproportionately more and more compared to teachers at REP schools. Under the assumption that the relative trend in exits would have continued had it not been for the reform, the identified effect for high-TVA teachers can be considered a lower bound in absolute terms of the true exit elasticity to the wage.

[Go to Figure 4]

4.3. Average effect of wages on teacher exit

I begin by estimating Equation 3 for the sample of all incumbent tenured teachers at treated and control schools in the year prior to the reform without controls and fixed effects, before progressively introducing controls and fixed effects to demonstrate that the results are robust to different specifications. As shown in Figure 5, moving progressively from specifications with (i) treatment period dummy and treatment status dummy but no controls and no fixed effects, (ii) including school and year fixed effects, (iii) further adding teacher controls, (iv) adding regional-educational-authority-by-year fixed effects, or as an alternative to those, (v) adding commuting-zone-by-year fixed effects, does not significantly impact the coefficients η_k.

[Go to Figure 5]

[51] I do not use these alternative treatment definitions as my baseline specifications, since issues of compliance would be even more pronounced if treatment were defined two or four years before the reform.

Overall, the estimates across all specifications point to a persistent and sizable effect of the relative wage increase for REP+ teachers. The reform led to an average reduction in exits from the teaching profession in the post-period, compared to the last pre-reform year, of about 0.59 percentage points for REP+ teachers relative to their REP counterparts (see Table 1). This effect is substantial, corresponding to roughly a 26.9 percent decline relative to the counterfactual exit rate these teachers would have experienced in the absence of the reform.The corresponding 2SLS ITT coefficient points to an estimate of the exit elasticity to the wage a teacher is offered ε_{exit}^{ITT} of -5.5.[52] Accounting for imperfect compliance with treatment, the 2SLS LATE estimate of the exit elasticity to the wage a teacher receives is even larger, with the $\varepsilon_{exit}^{LATE}$ of -8.1 implying that a 1% increase in wages reduces the exit probability of teachers who actually received the treatment by 8.1%.

[**Go to Table 1**]

In Section 4.6, I show that these estimates are robust to different definitions of the treatment schools, a variety of sample restrictions, and alternative treatment definition which compares the exit rates of teachers effectively at REP+ to those effectively at REP.

My estimates are not perfectly comparable to the turnover elasticities typically reported in the literature—usually ranging between -3 and -6 (e.g., Benhenda & Sims 2022; Clotfelter et al. 2008; Cowan & Goldhaber 2018; Falch 2011, 2017; Feng & Sass 2018; Hendricks 2014)—because turnover combines both exits from the profession and school-to-school mobility, the latter being often constrained by limited vacancies (i.e., limited labor demand). As a result, turnover elasticities are expected to be smaller in magnitude than exit elasticities.[53]

4.4. Heterogeneous effects of wages on teacher exit by TVA type

This subsection focuses on the central empirical result of this paper, showing that high-TVA teachers are 2.5 times more responsive to wages in their decision to exit the teaching profession. The estimated heterogeneous LATE semi-elasticities provide the key parameters for the theoretical framework used to study the relative cost-effectiveness of counterfactual bonus policies (see Section 5).

I begin by estimating Equation 3 separately for high- and low-TVA incumbent tenured teachers assigned to treatment and control schools in the year prior to the reform, without controls and fixed effects, before progressively introducing controls and fixed effects to

[52]Using Equation 6 and the observed $P(exit)_{REP+,CF}$ and $\overline{Wage}_{REP+,CF}$, which are reported at the bottom of regression table 1.

[53]Even though mobility decisions do not represent pure labor supply decisions in my framework, I aggregate exits and transfers out of treated schools to measure turnover for comparability to the literature. I obtain an ITT turnover elasticity of -2.3, which translates to a LATE elasticity of -3.4 (see Table A.5).

demonstrate that the results are robust to different specifications.

As shown in Figure 6, moving progressively from specifications with (i) treatment period dummy and treatment status dummy but no controls and no fixed effects, (ii) including school and year fixed effects, (iii) further adding teacher controls, (iv) adding regional-educational-authority-by-year fixed effects, or as an alternative to those, (v) adding commuting-zone-by-year fixed effects, does not significantly impact the coefficients η_k for the subsamples of high- (panel (a)) and low-TVA teachers (panel (b)).

[Go to Figure 6]

The reduced-form difference-in-differences estimates show a strong and significant decrease in the exit probability of high-TVA teachers initially assigned to REP+ schools, compared to high-TVA teachers initially assigned to REP schools, after the introduction of the reform. Specifically, over the first six years following the introduction of the reform, the average exit rate for high-TVA teachers at REP+ schools experiences a 1.4pp drop relative to the average exit rate for high-TVA teachers at REP schools—a reduction of 64.6% compared to the counterfactual exit level for this subgroup (see Table 2).

[Go to Table 2]

By contrast, low-TVA teachers initially assigned to REP+ schools experience a significantly smaller decrease in exit probability: with the exception of a jump in 2017-2018, the probability of exit of low-TVA teachers is practically unchanged compared to pre-reform levels, relative to those initially assigned at REP schools. The reduced-form average effect is small in magnitude and insignificant: the relative decrease in exit rates for low-TVA teachers is at a magnitude of 0.5pp—a reduction of 26.5% compared to the counterfactual exit level for this subgroup (see Table 2).

The results indicate that high-TVA teachers are 2.5 times more responsive to wages than their low-TVA counterparts. The corresponding ITT exit elasticities to the wage for the samples of high- and low-TVA teachers who were assigned to REP+ schools prior to the reform are, respectively, -13.3 and -5.5 (columns 2 and 5 in Table 2). The latter is not significantly different from zero (columns 2 and 5 in Table 2). Given that compliance is imperfect and teachers may move across schools for idiosyncratic reasons, the 2SLS LATE estimates of the two elasticities grow to, respectively, -19.8 (statistically significant) and -8 (statistically insignificant).[54] This result indicates a very strong elasticity to the wage for high-TVA teachers who actually remained at REP+ schools: a 1% increase in teacher

[54]For reference, Table A.6 shows that the LATE exit elasticity for the average Math and French teachers is -14.8.

wages reduced their exit probability by as much as -19.8%.

4.5. External validity

While the counterfactual analysis would remain internally valid even if exit elasticities differed across school types—and would still allow estimation of the effects of different policy counterfactuals on aggregate quality in disadvantaged schools—I find evidence that the heterogeneous exit elasticities of high- and low-TVA teachers do not significantly vary for treated schools with different degree of disadvantagedness. This suggests broader generalizability of the identified heterogeneous elasticities.

Specifically, neither high- nor low-TVA teachers pre-assigned to REP+ schools in the top tercile of "disadvantagedness" respond more to the wage bonus than those in the bottom tercile, where "disadvantagedness" is measured by the 2013 share of students classified as "very disadvantaged" based on their parents' occupation (Table A.7).[55]

4.6. Robustness checks

In this section, I show that both the heterogeneous exit responsiveness of high-TVA teachers, relative to low-TVA, and the exit responsiveness of the average teacher are robust to a series of tests.

Overall, the stable differentially higher elasticity for high-TVA teachers (in absolute terms) implies that a uniform wage increase is particularly effective at retaining more productive teachers and thus at increasing the aggregate quality of the teaching force through the exit channel. I confirm this in my theoretical framework in Section 5.

Robustness of the differential responsiveness of high-TVA to additional fixed effects The differential results for high- relative to low-TVA teachers remain robust after controlling for an extensive set of fixed effects in a triple-difference specification (Table A.9 and Figure B.10). Specifically, I sequentially add subject-by-high-TVA fixed effects, treatment-by-year fixed effects, and school-by-year fixed effects. Including school-by-year fixed effects controls for time-varying shocks to exit rates at the school level, ensuring that identification comes from comparisons between teachers within the same school and year. The estimates of differential responsiveness of high-TVA teachers remain economically large and stable, even though the average ITT coefficient only significant at 10% due to the temporary dip in low-TVA exits in 2017–2018.

Robustness of the differential responsiveness of high-TVA to other differential treatment effects To assess whether the heterogeneous treatment effects by teacher productivity are driven by specific observable characteristics that are correlated with TVA, I also estimate an extended version of the ITT specification allowing for differential treatment effects across these dimensions. This exercise tests

[55] I define disadvantagedness based on the share of very disadvantaged students, as it is the single best predictor of the definition of the treatment variable in 2013, as compared to the share of need-based grant recepients and the share of primary school repeaters (see Table A.8).

whether the observed heterogeneity by TVA reflects a broader productivity gradient rather than being driven by one particular characteristic, such as teacher experience or age, thereby supporting the generalizability of the results to contexts where the determinants of productivity may differ. Table A.10 depicts the coefficients from these alternative specifications. The estimated heterogeneous ITT treatment effects by TVA remain stable across specifications, indicating that the stronger responsiveness of high-TVA teachers is not driven by correlated characteristics such as experience, age, certification, or gender.

Robustness to different definitions of high TVA Finally, I also show that the results are robust to alternative definitions of the $HighTVA_{jmz}$ dummy. Specifically, I redefine the $HighTVA_{jmz}$ around the median within *académie* z, or alternatively around the median within school, in the pre-2013 period (see Table A.11 Panel (a)). I also redefine $HighTVA_{jmz}$ as the pre-2013 top tercile, or top quartile of the TVA distribution in subject m within commuting zone z (see Table A.11 Panel (b)). The results for both the high- and low-TVA groups are qualitatively and quantitatively unchanged.

I also split the triple difference-in-differences regressions by TVA in Math and in French (see Table A.12). The difference between high- and low-TVA responsiveness is larger for Math than it is for French, consistent with the idea Math teachers may have better outside options, both both results depict the same general conclusion: high-TVA teachers are more responsive than low-TVA teachers.

Robustness to definitions of treatment schools I show that the baseline ITT results for the sample of all teachers and the subsamples of high- and low-TVA teachers are robust to different definitions of the treatment schools (see Tables A.13, A.14). More precisely, the results are robust to defining as treated teachers only the sample of teachers who were at a school in 2013 that became a REP+ school in 2014 (i.e. the first 87 REP+ schools in Metropolitan France), or only in 2014 or 2015. In addition, they are robust to excluding from treatment the teachers in schools that became REP+ in 2014 specifically (the first announced treated group).

Robustness to more controls I also show that the baseline ITT results for the three subsamples of interest are robust to the addition of additional controls. Specifically, they are robust to interacting the 2013-level teacher controls added to the baseline regression with either a dummy $Post_t$, or with yearly dummies (see Tables A.15, A.16).

Robustness to sample restrictions I show the baseline ITT results for the three subsamples of interest are robust to different sample restrictions. Specifically, the results are robust to alternative definitions of the exit variable—restricting to no re-entry for at least three, rather than two, years, excluding schools which opened after 2013-2014 or shut down prior to 2019-2020, excluding teachers who are novice in 2013-2014, and extending the sample of teachers to a less conservative retirement age cut—55 or 58 years old, instead of 50 years old (see Tables A.17, A.18). I also show that the results are robust to taking different subsamples of teachers—Capes-certified only, or Agregation-certified only, though the latter result is economically larger but statistically insignificant due to small sample size (Table A.19).

Robustness to alternative treatment definition: ATT approach The ITT approach has two limitations which I address in in this robustness check for the three subsamples of interest. First, restricting the analysis to the sample of teachers already in the teaching sector in 2013 reduces statistical power. Second, as this sample ignores novice entrants in the post-reform period, which leads to older teachers than those actually present in REP+ or REP schools (see Figure B.5), the estimated coefficients may not reflect the responsiveness of the average teacher population at REP+ if responses to wage changes vary with age.

As a robustness check, I estimate the Average Treatment Effect on the Treated (ATT), comparing the exit rates of teachers effectively at REP+ to those of teachers effectively at REP schools in year t, and show that the ATT estimates are in line with the ITT estimates. Formally, the treatment time-invariant dummy variable $\mathbb{1}(type_s = \text{REP+})$ is a time-invariant dummy equal to 1 for a school s that was eventually classified as REP+ after 2013-2014, and 0 if a school was classified as REP. A school is considered as REP+ or REP, if it entered the REP+ or REP program in 2014-2015 or any subsequent academic year. Treated teachers are therefore teachers who are in school for which $\mathbb{1}(type_s = \text{REP+}) = 1$ at a given year t. Because the composition of teachers changes after 2018 (specifically based on relative experience at REP+ (Figure B.11), I restrict the analysis until either 2015 ("short-run"), or 2017 ("medium-run") to quantitatively limit concerns of endogenous selection into treatment. Event-study estimates are shown in Figure B.12.

The ATT 2SLS estimates imply short- and medium-run elasticities to the wage of, respectively, -10.2 and -8.5 for the average teacher (see Table A.20). Overall, these estimates are of comparable magnitude with the LATE estimates over the same time period (columns (3) and (7) of Tables A.20, A.21, respectively, for the subsamples of all, high- and low-TVA teachers).

4.7. Ruling out the role of the differential reallocation of teaching hours

Since the organizational and pedagogical aspects of the reforms were largely identical across REP and REP+ schools, the difference in the wage increase treatment after 2015 remains the key treatment between the treated and control groups.

Consistent with anecdotal evidence discussed in Section 1, I show that the additional treatment for REP+ teachers—the intended small reallocation of the working hours from teaching to liaison with students and families (4-4.5%), was even smaller than announced. The number of hours that Capes teachers spent teaching decreased by 0.8 instead of 1.6, which implies that this leg of the reform actually increased overall working hours by 0.8

hours on average, rather than simply reallocating teaching hours to other activities, as teachers were still obliged to take on the extra commitment of liaison with students and families (see Table A.22).[56]

If incentives to work overtime remained unchanged at the time of the reform, these additional hours can be regarded as involuntary, since teachers who preferred to work longer hours could already do so before the reform by voluntarily taking on overtime. I show that there was a small increase in monetary incentives for the average Capes teacher for the extra hours worked, of approximately 107.1 euros annually, in 2024 euros (see Appendix C.6 for a detailed computation of the overtime pay rule points). Assuming that teachers voluntarily decided to work more to obtain these 107.1 euros, this amount can be considered an additional effective bonus for REP+ teachers. Assuming that teachers voluntarily decided to work more to obtain these 107.1 euros, this amount can be considered an additional effective bonus for REP+ teachers. Under this assumption, the baseline LATE exit elasticity to the wage of -8.1 computed using the effective bonus excluding this extra incentive would instead be -7.4.[57]

Lastly, I rule out the importance of the residual concerns that the reform-led small reallocation of teaching hours towards other forms of school service may have impacted teacher exit decisions, for example by attracting better students and therefore improving the school environment. I show that adding school-by-year or one- or two-year lagged school-by-year controls for student composition to the baseline regression does not quantitatively change the baseline reduced-form ITT estimates (see Tables A.23, A.24). While these controls may be considered "bad controls" as their variation in the post-reform period is endogenous to the reform, the stability of the estimates across these alternative specifications provides evidence that my results are not driven by observable changes in the time-varying student composition.

5. A Discrete-choice framework for teacher labor supply

In this section, I rationalize the empirical finding that high-productivity teachers are more responsive to wages in their decision to leave the profession using a simple discrete-choice framework of labor supply. I then use the framework to quantify how counterfactual policies that affect the wage gap—the difference between teachers' outside-option wages and their teaching wage—impact the aggregate quality of the teaching workforce.

In counterfactual analysis, I show two important results. First, I show that the gain in aggregate quality achieved by a uniform bonus in the order of magnitude of the relative

[56]The same conclusions follow for the average REP+ high-qualification (Agrégation) teacher (who has mandated 15 hours prior to the reform and a supposed decrease in teaching hours of 1.4 after the reform, and the subsamples of high- and low-TVA teachers (Table A.22).

[57]To get to this back-of-the-envelope computation, note that the first stage LATE coefficient in Table 1 would change to 1.185+0.1071 = 1.2921, leading to a second-stage η coefficient of -0.00592/1.2921 = -0.00458, which is then substituted in Equation 7.

REP+ bonus (1800 euros) can be achieved by a bonus targeted toward high-TVA teachers for a fourth of the fiscal cost. Second, I demonstrate that under standard distributional assumptions (thin-tailed, unimodal distributions of relative preferences for the outside option), increases in outside wages lead to disproportionately larger losses in aggregate quality.

5.1. Setup

Discrete choice over two options I consider a simple static discrete choice model in which individuals choose between two options—teaching ($d = 0$) or an outside option ($d = 1$).

Heterogeneity by productivity Each individual is characterized by their teaching productivity c (which could be proxied by TVA for individuals who are already in teaching). For simplicity, I assume that productivity is discrete, such that there are only two types of productivity—high and low, $c \in \{H, L\}$. Each individual observes their own idiosyncratic preferences and makes a choice that maximizes their utility.

Indirect utility The indirect utility associated with each option $d \in \{0, 1\}$ is given by:

$$U_d^c = V_d^c + e_d^c,$$

where V_d^c is the deterministic part of utility, and e_d^c is an idiosyncratic taste shock.

Distributional asssumption for e_d^c The idiosyncratic taste shocks e_d^c are i.i.d. and follow a Gumbel distribution:

$$e_d^c \sim \text{Gumbel}(\mu^c, \sigma^c), \quad c \in \{H, L\},\ d \in \{0, 1\}.$$

Without a loss of generality, I assume $\mu^c = 0$.

Linear utility in wages and amenities I assume utility from teaching ($d = 0$) is a linear function of the teaching wage w_0 and preferences for non-wage amenities q_0^c:

$$V_0^c = w_0 + q_0^c,$$

and utility from the outside option ($d = 1$) as:

$$V_1^c = w_1^c + q_1^c,$$

where w_1^c is the wage in sector $d = 1$, and q_1^c is the non-wage preference for the outside option $d = 1$. Importantly, w_0 is not a function of productivity—teachers are not paid according to their productivity. The non-wage preference for teaching q_0^c can be thought of as pro-sociality—a teachers' preferences for the non-wage amenities associated with teaching, and q_1^c can be thought of as one's motivation for being in outside option $d = 1$.

Without a loss of generality, I assume $q_1^c = 0$.

5.2. Predictions

Equilibrium exit probabilities Define the wage gap between an individual's outside option and teaching as:

$$\Delta w^c = w_1^c - w_0,$$

An individual chooses the outside option ($d = 1$) if their relative preference for the outside option passes the indifference threshold between staying in and leaving teaching:

$$U_1^c > U_0^c \quad \Leftrightarrow \quad \Delta w^c - q_0^c > e_0^c - e_1^c.$$

An example of a distribution of relative preferences for the outside option (versus teaching) is illustrated in Figure 7. For the i.i.d. Gumbel-distributed e_0^c and e_1^c with common scale σ_e^c, the difference $\varepsilon^c \equiv e_1^c - e_0^c$ follows a logistic distribution with mean 0 and scale σ_e^c. Hence, the probability of exit for each type c is given by:

$$P_1^c = \frac{1}{1 + \exp\left(\frac{q_0^c - \Delta w^c}{\sigma_e^c}\right)} \tag{8}$$

The exit probability P_1^c reflects the share of individuals whose relative preference for the outside option, $\Delta w^c - q_0^c + \varepsilon^c$, exceeds zero (the shaded mass of teachers in Figure 7). Because the logistic distribution of ε^c is symmetric and most dense around zero, the largest mass of individuals lies near the indifference threshold when P_1^c is at 0.5, that is when $\Delta w^c = q_0^c$. When P_1^c is below 0.5—that is, when the expected relative preference for the outside option is negative ($\Delta w^c < q_0^c$)—most teachers prefer to stay, and only those with large positive idiosyncratic shocks exit.

For each type, the exit probability P_1^c increases with the wage gap Δw^c, meaning that the higher outside wages are relative to teaching wages, the higher the probability of exiting teaching is. Conversely, the exit probability is lower the higher non-wage amenities associated with teaching q_0^c are: individuals with greater pro-social motives are less likely to exit unconditionally.

[Go to Figure 7]

The level of the exit probability P_1^c also depends on the scale parameter σ_e^c, which captures how heterogeneous teachers are in their relative preferences of the outside option. When σ_e^c is small, teachers of a given type value their outside options relatively similarly, so choices are driven mostly by systematic differences in wages and amenities: individuals with slightly better outside options almost surely leave, and those with slightly worse ones almost surely stay. By contrast, when σ_e^c is large, relative preferences are more dispersed, and idiosyncratic taste shocks dominate utility differences, making choices appear more random and pushing P_1^c toward 0.5 regardless of wages and amenities.

Exit semi-elasticities with respect to the wage gap For a change in Δw^c, the baseline exit probability for type c changes by:

$$\frac{\partial P_1^c}{\partial \Delta w^c} = \frac{1}{\sigma_e^c} \cdot P_1^c \cdot (1 - P_1^c) \tag{9}$$

An increase in Δw^c shifts the mean of the distribution of relative preferences $\Delta w^c - q_0^c + \varepsilon^c$ toward the indifference threshold, bringing a growing share of individuals close to the indifference threshold. The density of "marginal" teachers therefore increases, and the responsiveness of exit probabilities to wage changes becomes larger as P_1^c approaches 0.5, holding the dispersion of idiosyncratic taste shocks constant.[58]

A larger dispersion of idiosyncratic taste shocks (the scale parameter σ_e^c) decreases the responsiveness of teachers of type c to a change in Δw^c, holding the equilibrium exit probability constant. For two groups of teachers with the same equilibrium exit probability (i.e. with the same mass of teachers past the indifference threshold prior to a change in the wage gap), a smaller σ_e^c implies that the mean relative preference at baseline must lie closer to the indifference threshold. The closer a distribution of relative preferences is to the threshold, the more teachers are marginal. Thus, a smaller the change in the wage gap is needed to move a mass of teachers across the indifference threshold.

The relative position of the distributions of preferences of high- and low-TVA types, determined by the respective mean preference and dispersion of taste shocks, therefore determines which type c responds more to a change in the wage gap. I show that uniformly raising teacher pay unambiguously increases average teacher quality if high-type teachers are more responsive to wages than low-type teachers (see Appendix C.5 for proof). Estimating the exogenous parameters from this framework allows to quantifying the relative cost-effectiveness of a uniform pay increase to a targeted pay increase.

5.3. Estimation of the framework

Estimating the exogenous parameters from this discrete choice framework (Δw^H, Δw^L, q_0^H, q_0^L, σ_e^H, σ_e^L) allows me to run policy counterfactuals on changes in the wage gap and to simulate their effect on aggregate quality.

[58] Once P_1^c exceeds 0.5—when the expected relative preference for the outside option becomes positive ($\Delta w^c > q_0^c$)—most teachers already prefer the outside option, and the mass of individuals near indifference begins to decline. Further increases in the wage gap then move fewer teachers across the threshold, as there are fewer and fewer "marginal" teachers, and responsiveness to additional increases in the wage gap falls. Hence, responsiveness is increasing in P_1^c for $P_1^c < 0.5$ and decreasing for $P_1^c > 0.5$, reaching its maximum when the largest share of teachers are "marginal".

I show that these exogenous parameters can be estimated using moments from the data and the identified semi-elasticities with respect to wages obtained from the 2SLS LATE approach. Specifically, I use five empirical moments and one identifying restriction to recover the six exogenous parameters, as detailed below.

Ex-ante exit probabilities The first and second empirical moments correspond to the baseline probabilities of exiting the teaching sector for each teacher type, $c \in \{H, L\}$. I equate the equilibrium exit probability equation for each type to the ex-ante exit probabilities observed for that type in REP+ schools in 2013 (see P^H_{exit} and P^L_{exit} in Table 3).

LATE semi-elasticities The third and fourth empirical moments correspond to the responsiveness of the exit probability to wages for each teacher type, $c \in \{H, L\}$. I equate $\frac{\partial P^c_{\text{exit}}}{\partial \Delta w^c}$ to (minus) the estimated η coefficients of responsiveness to teacher wages for high- and low-TVA teachers obtained from regression 5 (see Table 2).

Empirical estimate of the average wage gap The fifth empirical moment corresponds to the type-specific wage gaps. To obtain these, I estimate the average wage gap directly from the data by combining information on teacher wages with the weighted average wages in the outside occupations into which teachers are observed to transition (for details on data and measurement, see Section 2 and Appendix C.7). While I cannot match these data to my main teacher panel and therefore cannot identify the outside-option wage separately for high- and low-TVA teachers, I can identify the average outside-option wage and thus the average wage gap for the representative teacher. The fifth moment equates this average wage gap, estimated for the average REP+ teacher in 2013, to the weighted linear combination of the exogenous type-specific wage gaps, where the weights are given by the relative exit probabilities of each teacher type. Formally,

$$\Delta \overline{w} \;=\; \omega^H \Delta w^H \;+\; \omega^L \Delta w^L \text{ where } \omega^c \;=\; \frac{P^c_{\text{exit}}}{P^H_{\text{exit}} + P^L_{\text{exit}}}, \quad c \in \{H, L\}.$$

Non-pecuniary preferences To exactly identify the six parameters in the framework, I assume that the non-pecuniary preferences for teaching are identical across types. More formally,

$$q^H_0 \;=\; q^L_0 \;=\; q_0.$$

This assumption is in line with recent literature on teacher preferences (see Johnston 2025), which underlines that high- and low-TVA teachers have similar preferences for non-wage amenities. Conversely, Andersen et al. (2014) finds that students taught by teachers with higher pro-social motives have higher test scores. Importantly, relaxing my assumption—allowing high-TVA teachers to have stronger preferences for teaching—would not change either the means or the dispersions of the distributions of preferences, it would just imply an even higher outside wage for high-TVA and a lower outside wage for low-TVA to rationalize their observed behavior.[59]

Under this assumption, the estimated η^H and η^L, together with the estimate of the average wage gap $\Delta\overline{w}$ and the ex-ante exit probabilities P^H_{exit} and P^L_{exit}, constitute sufficient statistics for recovering the impact of a change in the wage gap on aggregate teacher productivity induced by exit responses. The resulting parameter estimates from the framework are reported in Table 3.

[Go to Table 3]

Discussion of the identified parameters Table 3 reveals that, in the context of the model, the empirical patterns—the higher responsiveness of high-TVA teachers at REP+ schools to changes in the wage gap and the similar baseline exit probabilities for high- and low-TVA teachers—can only arise from a specific joint configuration of the underlying parameters. Because both groups exhibit similar exit probabilities, the pairs $(\Delta w^c - q^c_0, \sigma^c_e)$ must jointly generate the same position on the logistic curve.

This means that if one group (high-TVA teachers) has a smaller dispersion of idiosyncratic shocks—i.e., a smaller σ^c_e—its mean relative preference for the outside option must lie closer to the indifference threshold to reproduce the same observed exit rate.

High-TVA teachers indeed exhibit a much smaller dispersion of preferences (σ^H_e = 1.9 versus σ^L_e = 5.1, implying a steeper logistic slope because in the data they are more responsive at the same probability of exit. To maintain this same exit probability while having a smaller dispersion of preferences, their mean relative preference for the outside option ($\Delta w^H - q^H_0$ = -7) at baseline must therefore lie closer to the indifference threshold than that of low-TVA teachers (-19.7).

This configuration is illustrated in Figure 8. The distribution of relative preferences for high-TVA teachers (blue curve) is narrower and centered closer to the indifference threshold, implying a dense mass of "marginal" individuals who are most sensitive to wage changes. By contrast, the distribution for low-TVA teachers (red curve) is flatter and shifted further away, meaning fewer teachers lie near indifference and exit responses to wage changes are correspondingly weaker.

[Go to Figure 8]

[59] To see this, let $q_0 \neq q_0^{c,true}$, such that $q_0^{L,true} < q_0 < q_0^{H,true}$. Then, a H-type chooses to leave teaching if $\Delta w^{H,true} > q_0 + (q_0^{H,true} - q_0) + e^H_0 - e^H_1$, or $\Delta w^{H,true} - (q_0^{H,true} - q_0) > q_0 + e^H_0 - e^H_1$. Under the assumption of equal non-wage preferences, $\Delta w^H = \Delta w^{H,true} - (q_0^{H,true} - q_0)$. It follows that $\Delta w^{H,true} > \Delta w^H$.

5.4. Possible micro-foundation for the dispersion in taste shocks

The dispersion in taste shocks σ_e^c can be micro-founded within a model of rational inattention (see e.g., Matějka & McKay 2015; Sims 1998, 2003). I propose that teachers face uncertainty about their outside-option wage w_1^c and choose how much "attention" θ^c to allocate to learning about it. Acquiring more precise information is costly, but it reduces the noise in decision-making when comparing teaching with outside options. Because the value of information rises with the stakes of the decision, teachers with a larger expected outside-option wage—that is, with higher wage gap Δw^c—optimally allocate more attention to learning about outside opportunities, which leads to greater precision when making a decision. This implies that the scale parameter σ_e^c is inversely related to attention, θ^c, which itself increases with the wage gap Δw^c. As a result, teachers with on average higher expected wage gaps optimally acquire more information about outside options, which leads to a lower σ_e^c for this group of teachers—they are able to take the decision to leave with more precision.

Within the framework, the higher responsiveness of high-TVA teachers could thus be rationalized by them facing a higher wage gap. Because attention (and thus responsiveness) is increasing in the outside-option wage, teachers with larger ex-ante wage gaps—those whose outside options pay more relative to teaching—should exhibit stronger behavioral responses to a policy that narrows that gap. This would be consistent with the finding in the literature that they obtain better wages at exit, compared to low-TVA teachers (Chingos & West 2012).

While I cannot credibly isolate that this mechanism is causing the higher responsiveness of high-TVA teachers as I do not observe the wage gap per TVA type, I can test empirically whether, following the introduction of the bonus at REP+, exit probabilities fall more sharply among teachers observed to have higher baseline wage gaps. [60]

The average REP+ teacher with a higher baseline wage gap is significantly more responsive to the increase in teacher wages than counterparts with a lower baseline wage gap, both economically and statistically (Table 4). Decomposing the baseline wage gap into its two components—the outside-option wage and the teacher wage, I show that the higher responsiveness stems from teachers with higher outside option wages, rather than those with lower teacher wages (who, therefore, received a larger wage increase in percentage terms).[61] This distinction is in line with the proposed micro-foundation.

[Go to Table 4]

[60] The estimated wage gap is increasing in age, decreasing in the teacher's position on the wage scale (and relatedly, experience), and lower for female teachers (see Table A.25).

[61] This pattern is also consistent in complementary evidence which shows that teachers with characteristics positively associated with a larger wage gap respond more to the reform (see Table A.26).

5.5. Policy counterfactuals

Simulated effects of changes in the wage gap on average teaching quality Having estimated the parameters of the model, I can now use them to simulate policy counterfactuals and assess how changes in the wage gap affect the aggregate quality of the teaching workforce.

To evaluate the implications of teacher labor supply choices for average teaching quality, I define average quality at time t as the weighted average of type-specific productivity:

$$Q_t = s_{H,t}\,\nu_H + s_{L,t}\,\nu_L,$$

where $s_{c,t}$ is the share of type-c teachers ($c \in \{H, L\}$) among those employed at time t, and ν_c denotes the average productivity (TVA) of type c.

Assuming no entry or replacement between periods, the share of each type c in period $t+1$ equals its initial share multiplied by the probability of remaining in teaching, normalized by the total probability of remaining across types:

$$s_{c,t+1} = \frac{s_{c,t} P_0^c}{\sum_f s_{f,t} P_0^f}.$$

where $P_0^c = 1 - \widehat{P^c_{\text{exit}}}$ is the predicted probability that a type-c teacher remains in teaching for a given wage gap Δw^c.

Average quality at $t+1$ under a given policy scenario, p (e.g. status quo or a policy which changes the wage gap), is then:

$$Q^{\text{p}}_{t+1} = s^{\text{p}}_{H,t+1}\,\nu_H + s^{\text{p}}_{L,t+1}\,\nu_L,$$

The effect of the policy on aggregate teacher quality is therefore measured as the difference in the change in average quality across a scenario of a change in policy and the scenario of the status quo:

$$\Delta Q_{t+1} \equiv (Q^{\text{policy}}_{t+1} - Q_t) - (Q^{\text{status quo}}_{t+1} - Q_t) \;=\; Q^{\text{policy}}_{t+1} - Q^{\text{status quo}}_{t+1}$$

Because $\nu_H > \nu_L$, this policy effect depends on how the relative retention of high- and low-type teachers responds to the change in the wage gap, that is, on the difference in their exit semi-elasticities $(\eta_H - \eta_L)$.

To simulate the dynamics of quality over multiple periods, I repeat this updating process iteratively. For each horizon $k > 1$, the new type shares are obtained by applying the same retention probabilities P_0^c from the corresponding scenario to the shares from the previous period. This recursion is repeated for both the policy and status quo scenarios, using the same set of estimated retention probabilities for each scenario. Aggregate quality at horizon $t+k$ is then computed under each scenario and the cumulative policy effect is given by the difference between Q_{t+k}^{policy} and $Q_{t+k}^{\text{status quo}}$.

Table 5 presents a series of counterfactual policies and reports the corresponding change in the probability of exit and change in aggregate teacher quality.

[**Go to Table 5**]

Uniform vs. targeted increase in teacher wage I first consider a policy counterfactual in which teacher wages increase uniformly by the same magnitude as the reform, holding the outside-option wage constant: REP+ teachers got on average 1800 euros more annually as part of the policy, relative to REP teachers, and this increase was permanent. Under this treatment the exit probability of high-TVA teachers falls by -61.5%, while that of low-TVA teachers falls by -29.1%. Assuming no replacement, this translates into an increase in average teaching quality of 1.5% of a SD in TVA (relative to 2013 levels) over 5 years, relative to a counterfactual with no change in the wage gap.

To achieve the same gains in aggregate TVA, the government can target a bonus to high-TVA teachers only which would cost only 800 euros per teacher annually—4.5 times lower cost compared to the total fiscal cost of the uniform bonus (3600 euros distributed across low- and high-TVA). If, instead, the bonus amount of 3600 euros was fully directed towards high-TVA teachers, the gains in TVA are 2.4 times higher compared to those of the uniform bonus.

This is because targeted bonuses directly raise the retention incentives of the most productive teachers, who are also more responsive to wage changes. By contrast, a uniform wage increase raises pay for all teachers, including those with lower productivity and weaker responsiveness, thereby diluting its impact on the composition of the teaching workforce.

Uniform increase in teacher wage vs. uniform increase in outside-option wage As a further counterfactual, I simulate a uniform increase in the outside-option wage by the same amount of 1800 euros. Under this scenario, the exit probability of high-TVA teachers increases by 154%, and the probability of low-TVA teachers—by 40.7%. Assuming no replacement, this translates into a decrease in average teaching quality of 5.1% of a SD over five years, relative to a counterfactual with no wage gap change.

It follows that the effect of an increase in the wage gap is asymmetric to the effect of a decrease in the wage gap, under logistic distributions of relative preferences. This follows directly from the low baseline probabilities of exit for both types (below 0.5). When the baseline exit probability P_1^c is low, most teachers already prefer to remain in teaching, and only a small share are close to indifference. A further decrease in the wage gap (i.e. a wage increase for teachers) therefore affects few individuals, as most teachers are already on the staying side of the distribution (see Figure 9 Panel (a)). By contrast, an increase in the wage gap (i.e. a rise in outside-option wages) shifts the distribution toward the region with a higher density of individuals near the indifference threshold, where small changes in relative preferences induce many teachers to switch from staying to leaving (Panel (b)).

[**Go to Figure 9**]

Discussion of counterfactual results Taken at face value, these simulations yield two main insights about how wage policy shapes the composition and quality of the teaching workforce through the exit channel.

First, a targeted bonuses are significantly more cost-effective in improving aggregate teacher quality through the exit channel: for a uniform bonus of 1800 euros, a targeted bonus of 1800 euros achieves the same gains.

Second, in an environment where outside-option wages are increasing relative to teacher wages, failing to adjust teacher wages accordingly can erode teaching quality over time. When labor market conditions improve outside of teaching, the opportunity cost of remaining in teaching increases disproportionately for high-TVA teachers. Without compensating adjustments in teacher wages, retention among the most productive teachers can fall substantially, leading to a gradual and potentially large decline in the average TVA of the teaching workforce. This insight helps rationalize why teacher quality may stagnate or decline in periods of rising economy-wide wages: even small and gradual increases in outside opportunities can shift a non-negligible mass of marginal high-TVA teachers out of teaching.

6. Conclusion

This paper provides the first causal evidence on whether the wage elasticity of teachers' exit decisions differs by teacher productivity. I then quantify the implications of changes in the teacher wage gap for the aggregate quality of the teaching workforce implied by the exit channel.

Exploiting a quasi-experimental wage increase introduced by the 2014 French REP+ reform, I find that teacher exit decisions are highly elastic to wages, and that high-productivity teachers—those with above-median value-added—are about three times as responsive as their lower-productivity counterparts.

Using a simple static model of labor supply with two productivity types, I show that a bonus targeted to high-TVA teachers can achieve the same gains in aggregate teacher quality as those achieved by a uniform bonus of the magnitude of 1800 for a fourth fourth of the fiscal cost. Furthermore, I show that under standard distributional assumptions, an equivalent increase in outside option wages leads to disproportionally larger negative impact on aggregate quality. It follows that if increases in outside option wages are not matched by an increase in teacher quality, they can substantially erode teacher quality.

Taken together, these results highlight that teacher wage policies have important compositional effects that go beyond retention rates.

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Tables and Figures

Figure 1. The wage gap between outside-option and teaching wages is widening in France
Note: The figure presents the widening of the average annual wage gap (in thousands of euros). $OutsideOptionWage_{gazt}$ is the weighted-average wage of non-teachers in occupations identified as outside options for teachers, using teacher exits. It is defined as

$$OutsideOptionWage_{gazt} = \frac{\overline{S_o} \cdot D_{ogazt}}{\sum_o \overline{S_o} \cdot D_{ogazt}} \cdot OutsideOptionWage_{ogazt}$$

where occupation $o \in \mathcal{O}$, $\overline{S_o}$ is the average share of teachers exiting to o over the period 2009-2021, and D_{ogazt} is the share of workers at o in a given cell $gazt$. $TeachingWage_{est}$ varies per position on the pay scale (echelon) e and year t. School year $t/t+1$ is denoted as t. School year $t/t+1$ is denoted as t. Both teaching wages and outside option wages are CPI-deflated to 2024 thousands of euros. Source: Bases Relais, BTS-Postes, Teacher wage data (Chancel), 2009/2010-2020/2021.

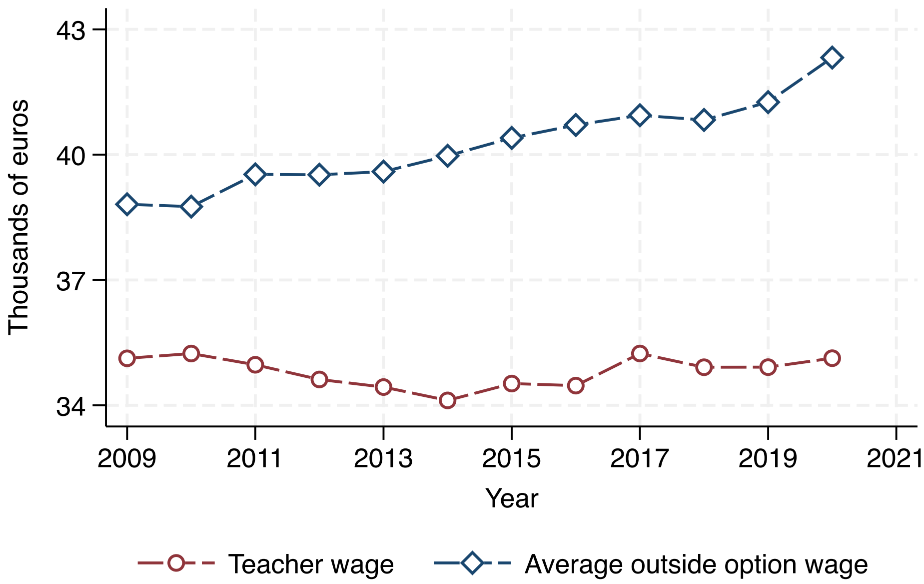

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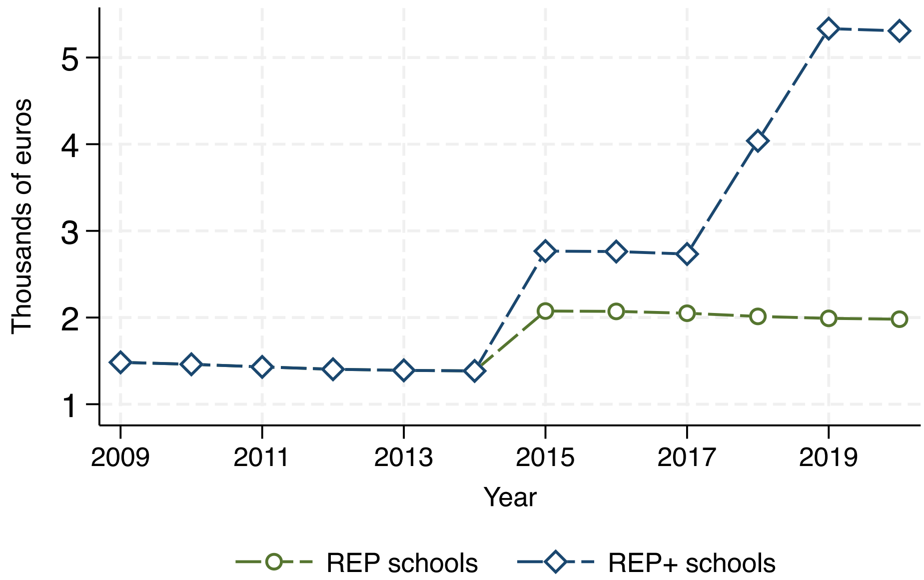
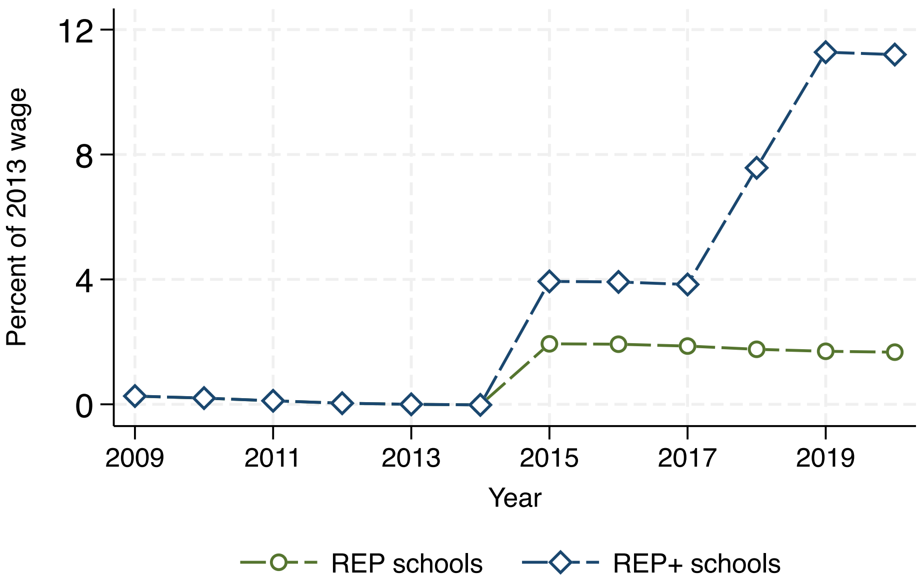
Figure 2. Reform-induced larger wage bonus for teachers at REP+ schools (vs. REP)

Note: Figures show the evolution of the annual wage bonus for teachers in REP and REP+ schools relative to teachers in ordinary schools, expressed in 2024 CPI-deflated thousand euros. Panel (a) reports the gross yearly bonus. Panel (b) shows the bonus as a share of the average 2013 wage among teachers in my sample for each school type. Underlying data: Bases Relais, Teacher wage data (Chancel), 2009–2010 to 2020–2021.



(a) Bonus amount



(b) As a share of average wage

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Figure 3. Example of a teacher network required to estimate TVA

Note: The figure illustrates an example of a teacher network constructed for a group of Math and Literature teachers. Each node represents a teacher—either in Math (blue) or Literature (yellow)—and each edge indicates that the two teachers share at least one classroom. Source: Tartova (2023).

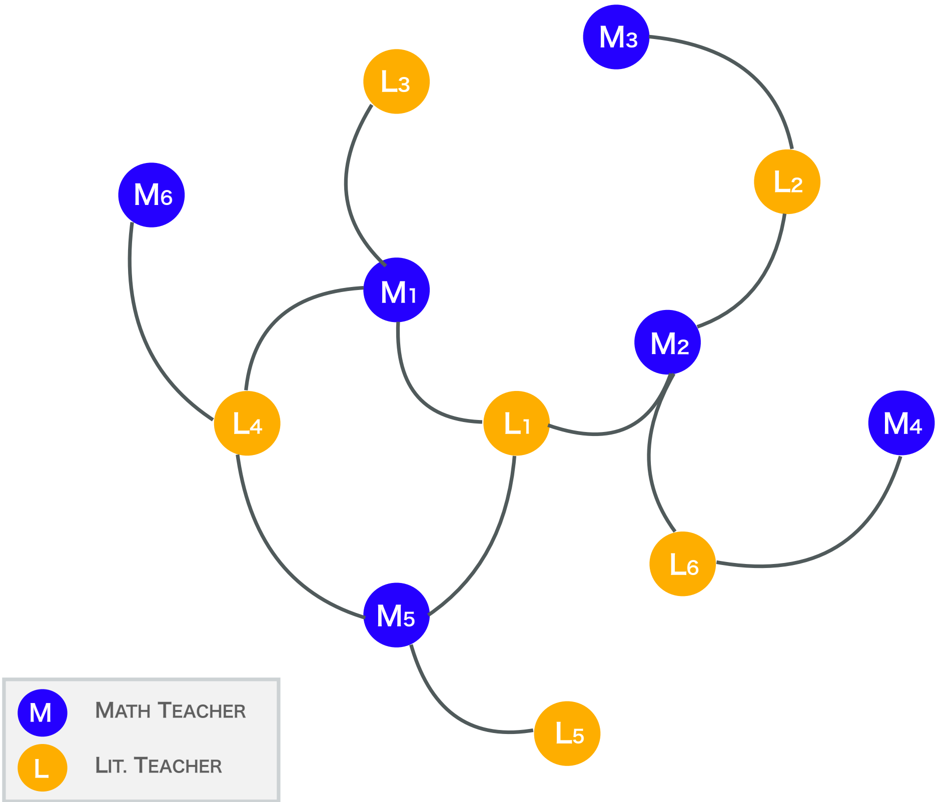

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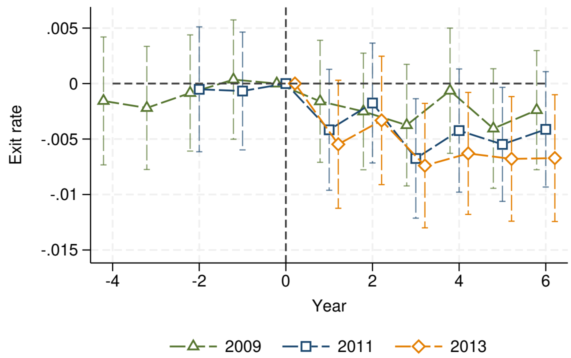
Figure 4. No differential pre-trend for REP+ teachers under earlier ITT assignment

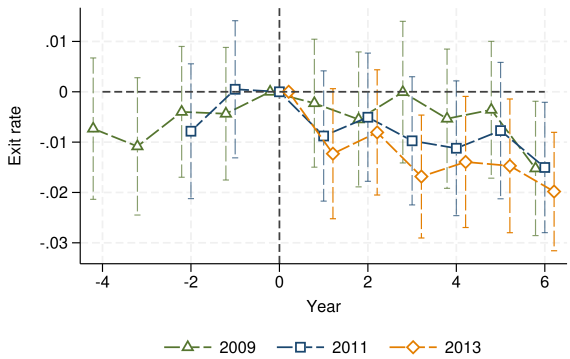
Note: Figures show the estimated coefficients η_k from the event-study ITT difference-in-differences regression (3), under alternative treatment definitions. The sample includes all teachers employed at REP or REP+ schools in the treatment assignment year. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \sum_{k=2009}^{2019} \eta_k \, \mathbb{1}(t = k) \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}$$

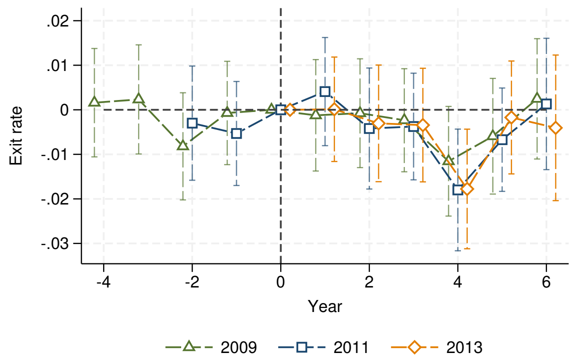
$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $\mathbb{1}(t = k)$ indicates school year $k/k{+}1$. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. The "2013" specification uses 2013/2014 assignments; "2011" and "2009" use 2011/2012 and 2009/2010. Year 0 is 2013/2014. 95% confidence intervals shown. Standard errors clustered by teacher and school-year, where school is measured in the treatment assignment year. $\mathbf{X}_j$ includes teacher controls measured in the treatment assignment year: gender, qualification, experience, age, and subject taught. θ_s and λ_{zt} are school and commuting-zone-year fixed effects, measured in the assignment year. Estimates are relative to 2013/2014. Panel (a) includes all teachers; panels (b) and (c) restrict to high- and low-TVA teachers, defined around the median TVA within the commuting zone in the treatment assignment year. See Section 4 for details. Underlying data: Bases Relais, Scolarité, Sysca, DNB, 2009/2010-2019/2020.



(a) All teachers



(b) High TVA



(c) Low TVA

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Figure 5. Event-study DiD (ITT): Larger drop in REP+ teachers' exit rates (vs. REP)

Note: Figure shows the estimated coefficients η_k from the event-study ITT difference-in-differences regression (3), using the 2013/2014 treatment assignment. The sample includes all teachers employed at REP or REP+ schools in 2013/2014. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \sum_{k=2013}^{2019} \eta_k \mathbb{1}(t = k) \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $\mathbb{1}(t = k)$ indicates school year $k/k+1$. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. The figure reports five specifications: (1) no controls; (2) adding school and year fixed effects; (3) adding teacher controls (gender, qualification, experience, age, subject taught); (4) replacing year effects with académie-by-year fixed effects; and (5) replacing the latter with commuting-zone-by-year fixed effects. Pre-trends are normalized to zero, as the ITT sample includes teachers present in 2013/2014. 95% confidence intervals are shown. Standard errors are clustered by teacher and school-year. Controls, fixed-effects, and clusters are measured in 2013/2014. Estimates are relative to 2013/2014. See Section 4 for details. Underlying data: Bases Relais, 2013/2014–2019/2020.

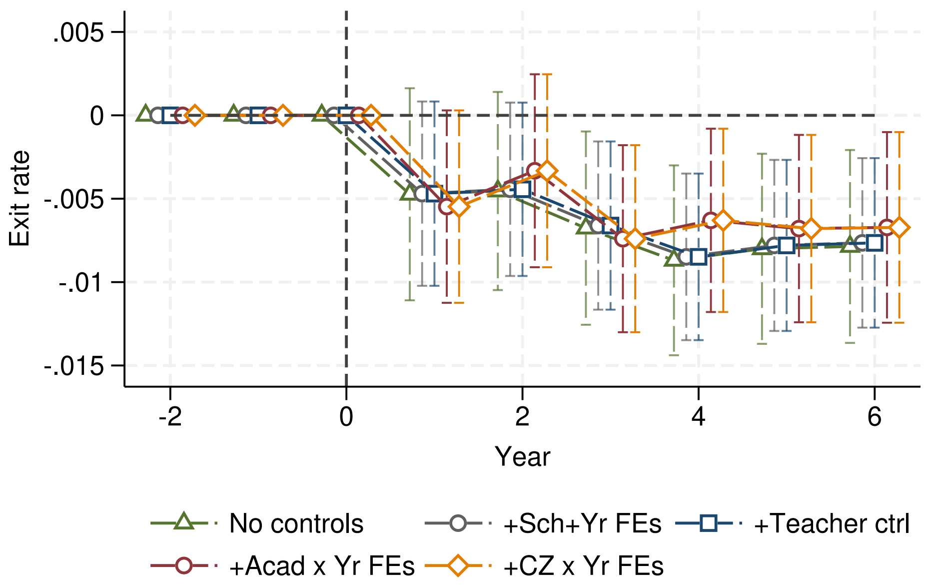

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Table 1. DiD estimates (ITT): Larger drop of exit rates for REP+ teachers (vs. REP)

Note: Table reports estimates from the ITT difference-in-differences regression (3) and (5), using the 2013/2014 treatment assignment. The sample includes all teachers employed at REP or REP+ schools in 2013/2014. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. The specification includes teacher-level controls (gender, qualification, experience, age, and subject taught), as well as school and commuting-zone-by-year fixed effects. Standard errors (in parentheses) clustered by teacher and school-year. Controls, fixed-effects, and clusters are measured in 2013/2014. Column (1) reports reduced-form estimates of the effect of the REP+ treatment on exits. Columns (2)–(5) present 2SLS estimates instrumenting for the intended or realized wage bonus. The first stage relates the post-reform REP+ status to the intended or realized wage bonus, and the second stage estimates its effect on exit probability. All estimates are relative to 2013/2014. The mean counterfactual exit rate and counterfactual wage for REP+ teachers are reported for each column. The counterfactual exit rate (wage) is defined as the average exit rate (wage) that initially assigned REP+ teachers would have experienced in the post-period, had they not experienced a differential increase in wages compared to initially assigned REP teachers, under the assumption of no differential trend in exit rates between the two groups after 2013. The corresponding relative percentage decrease in exit rates and the elasticity with respect to the wage (computed using equation (6)) are also reported. The bonus and counterfactual wage are CPI-deflated to 2024 thousands of euros. See Section 4 for details. Underlying data: Bases Relais, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Reduced-form	2SLS			
		Second stage (ITT)	First stage (ITT)	Second stage (LATE)	First stage (LATE)
	(1)	(2)	(3)	(4)	(5)
Post x REP+	-0.00592**		1.682***		1.185***
	(0.00232)		(0.00248)		(0.0300)
Bonus (intended)		-0.00352**			
		(0.00138)			
Bonus (realized)				-0.00499**	
				(0.00198)	
Teacher controls	✓	✓	✓	✓	✓
School FEs	✓	✓	✓	✓	✓
CZ × Year FEs	✓	✓	✓	✓	✓
Observations	148857	148857	148857	148857	148857
R-squared	0.018	0.001	0.833	0.001	0.300
First-stage F stat		4892		1564	
Counterfactual exit	0.022	0.022		0.022	
As perc. of exit	-0.269				
Counterfactual wage		34.744		35.615	
Elasticity to wage		-5.545		-8.071	

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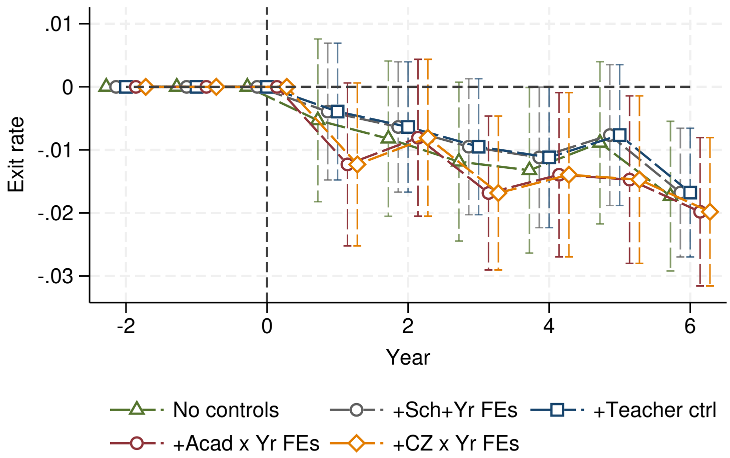
Figure 6. Event-study DiD by TVA (ITT): High-TVA teachers respond more

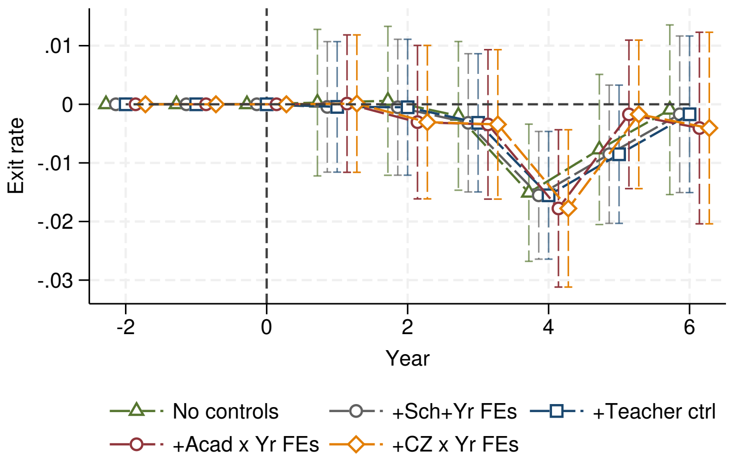
Note: Figures show the estimated coefficients η_k from the event-study ITT difference-in-differences regression (3), using the 2013/2014 treatment assignment. The sample includes all Math and French teachers employed at REP or REP+ schools in 2013/2014 with measured TVA. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \sum_{k=2013}^{2019} \eta_k \, \mathbb{1}(t = k) \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $\mathbb{1}(t = k)$ indicates school year $k/k{+}1$. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. The figures report five specifications: (1) no controls; (2) adding school and year fixed effects; (3) adding teacher controls (gender, qualification, experience, age, subject taught); (4) replacing year effects with académie-by-year fixed effects; and (5) replacing the latter with commuting-zone-by-year fixed effects. Pre-trends are normalized to zero, as the ITT sample includes teachers present in 2013/2014. 95% confidence intervals are shown. Standard errors are clustered by teacher and school-year. Controls, fixed-effects, and clusters are measured in 2013/2014. Estimates are relative to 2013/2014. Panel (a) reports results for teachers with high TVA, and Panel (b) for those with low TVA, defined relative to the median TVA within the commuting zone in 2013. See Section 4 for details. Underlying data: Bases Relais, Scolarité, Sysca, DNB, 2013/2014–2019/2020.



(a) High TVA



(b) Low TVA

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Table 2. DiD estimates by TVA (ITT): High-TVA teachers respond more

Note: Table reports estimates from the ITT difference-in-differences regression (3) and (5), using the 2013/2014 treatment assignment. The sample includes all Math and French teachers employed at REP or REP+ schools in 2013/2014 with measured TVA. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. The specification includes teacher-level controls (gender, qualification, experience, age, and subject taught), as well as school and commuting-zone-by-year fixed effects. Standard errors (in parentheses) clustered by teacher and school-year. Controls, fixed-effects, and clusters are measured in 2013/2014. Columns (1) and (4) report reduced-form estimates of the effect of the REP+ treatment on exits. Columns (2)–(3) and (5)–(6) present 2SLS estimates instrumenting for the intended or realized wage bonus. The second stage estimates the effect of the bonus on exit probability. All estimates are relative to 2013/2014. The mean counterfactual exit rate and counterfactual wage for REP+ teachers are reported for each column. The counterfactual exit rate (wage) is defined as the average exit rate (wage) that initially assigned REP+ teachers would have experienced in the post-period, had they not experienced a differential increase in wages compared to initially assigned REP teachers, under the assumption of no differential trend in exit rates between the two groups after 2013. The corresponding relative percentage decrease in exit rates and the elasticity with respect to the wage (computed using equation (6)) are also reported. The bonus and counterfactual wage are CPI-deflated to 2024 thousands of euros. Columns (1)-(3) report results for teachers with high TVA, and columns (4)-(6) for those with low TVA, defined relative to the median TVA within the commuting zone in 2013. See Section 4 for details. Underlying data: Bases Relais, Scolarité, Sysca, DNB, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	High TVA			Low TVA		
	Reduced-form (1)	2SLS (ITT) (2)	2SLS (LATE) (3)	Reduced-form (4)	2SLS (ITT) (5)	2SLS (LATE) (6)
Post x REP+	-0.0140***			-0.00470		
	(0.00518)			(0.00514)		
Bonus (intended)		-0.00830***			-0.00282	
		(0.00306)			(0.00308)	
Bonus (realized)			-0.0121***			-0.00400
			(0.00458)			(0.00439)
Teacher controls	✓	✓	✓	✓	✓	✓
School FEs	✓	✓	✓	✓	✓	✓
CZ × Year FEs	✓	✓	✓	✓	✓	✓
Observations	24054	24054	24054	22322	22322	22322
R-squared	0.091	0.002	-0.006	0.090	0.002	0.002
First-stage F stat		4061	332		4020	380
Counterfactual exit	0.022	0.022	0.022	0.018	0.018	0.018
As perc. of exit	-0.646			-0.265		
Counterfactual wage		34.680	35.568		34.580	35.427
Elasticity to wage		-13.251	-19.799		-5.508	-8.015

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Figure 7. Example distribution of indirect utility from outside option (vs. teaching)

Note: The figure illustrates an example distribution of the relative preference for the outside option among teachers of type c, $\Delta w^c - q_0^c + \varepsilon^c$. Δw^c captures the wage gap between the outside option and teaching, q_0^c the non-pecuniary utility of teaching, and ε^c an idiosyncratic preference shock following a logistic distribution with location 0 and scale σ_ε. The vertical dotted line at zero represents the indifference threshold between staying in teaching and exiting. The shaded area corresponds to the mass of teachers for whom $\Delta w^c - q_0^c + \varepsilon^c > 0$, i.e., those whose total relative preference favors leaving the teaching sector. See Section 5 for details.

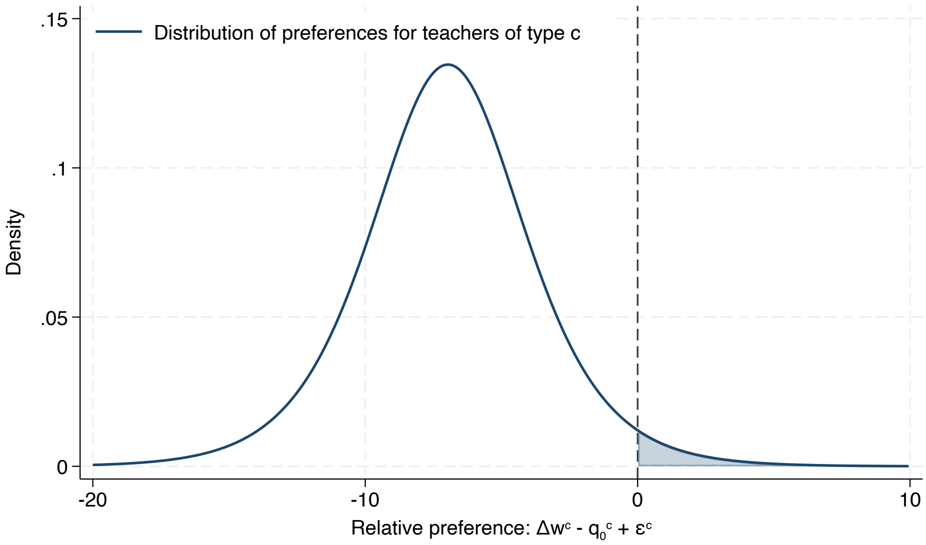

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Table 3. Estimated Parameters of the Discrete Choice Framework

Note: The table reports the estimated parameters of the discrete choice framework described in Section 5. The parameters $(\Delta w^H, \Delta w^L, q_0, \sigma_e^H, \sigma_e^L)$ are identified using five empirical moments and one restriction equating the non-pecuniary utility of teaching across types ($q_0^H = q_0^L = q_0$). Empirical moments include: (i) baseline exit probabilities for high- and low-TVA teachers (P^H_{exit}, P^L_{exit}); (ii) semi-elasticities of exit with respect to wages (η^H, η^L) estimated from the 2SLS regressions in Table 2; and (iii) the average wage gap between outside and teaching wages ($\Delta\overline{w}$). The assumption of equal non-wage preferences allows exact identification of the six parameters. Estimated parameters are used to simulate counterfactual changes in the wage gap and their effect on aggregate teacher quality. See Section 5 for details.

	Symbol	**Value**
Inputs		
High TVA exit probability	P^H_{exit}	0.023
Low TVA exit probability	P^L_{exit}	0.021
Semi-elasticity (High TVA)	$\eta^H = \partial P^H_{\text{exit}} / \partial \Delta w^H$	0.012
Semi-elasticity (Low TVA)	$\eta^L = \partial P^L_{\text{exit}} / \partial \Delta w^L$	0.004
Average wage gap	$\Delta\overline{w}$	4.8
Estimated Parameters		
Non-pecuniary utility of teaching	$q_0^H = q_0^L = q_0$	17.9
Scale parameter (High TVA)	σ_e^H	1.9
Scale parameter (Low TVA)	σ_e^L	5.1
Wage gap (High TVA)	Δw^H	10.9
Wage gap (Low TVA)	Δw^L	-1.9

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Figure 8. TVA-specific distributions of indirect utility from outside option (vs. teaching)

Note: The figure plots the estimated distributions of the relative preference for the outside option among teachers of type c (High- and Low-TVA), $\Delta w^c - q_0^c + \varepsilon^c$. Δw^c captures the wage gap between the outside option and teaching, q_0^c the non-pecuniary utility derived from teaching, and ε^c an idiosyncratic preference shock following a logistic distribution with location 0 and scale σ_ε. The vertical dotted line at zero represents the indifference threshold between staying in teaching and exiting. The shaded area corresponds to the mass of teachers of type c for whom $\Delta w^c - q_0^c + \varepsilon^c > 0$, i.e., those whose total relative preference favors leaving the teaching sector. The distributions are estimated using the parameters reported in Table 3. See Section 5 for details on the estimation and interpretation of these parameters.

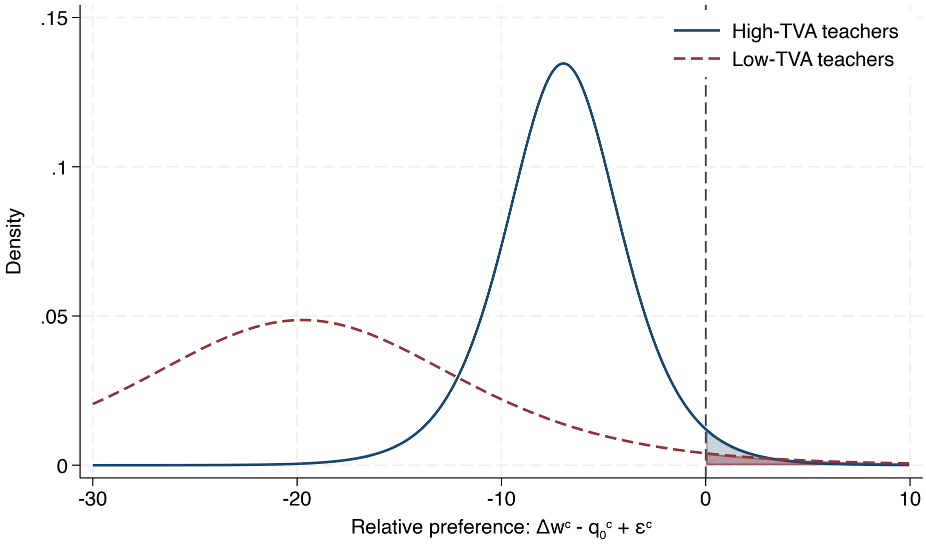

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Table 4. Micro-foundation for the dispersion in taste shocks: Teachers with larger wage gaps respond more

Note: Table reports estimates from the ITT difference-in-differences regression (3), using the 2013/2014 treatment assignment, and augmented with triple interactions. Sample includes teachers employed at REP or REP+ schools in 2013/2014. Formally, for teacher j at school s and wage-gap quantile group q:

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) \times HighWageGap_j + \cdots + \gamma \mathbf{X}_j + \theta_{sq} + \theta_{st} + \varepsilon_{jst}.$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. Columns (1) and (3) report results using a high-wage-gap indicator defined, respectively, above the median or in the top tercile of the 2013 wage gap distribution. Columns (2) and (4) replace the total wage gap with its two components: the outside wage and the teaching wage. The specification includes teacher-level controls (gender, qualification, experience tercile, age tercile, and subject taught), interacted with the high wage gap indicator (columns (1) and (3)) or with high outside wage and high wage indicators (columns (2) and (4)), as well as School × Wage Gap Quantile, School × Outside Wage Quantile, School × Wage Quantile, and School × Year fixed effects as indicated in the table. Intermediate tercile observations are excluded from the sample. Lower-level interaction terms are not reported for exposition. Standard errors (in parentheses) are clustered by teacher and school-year. All controls, fixed effects, and clusters are measured in 2013/2014. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Underlying data: Bases Relais, BTS-Postes, Teacher wage data (Chancel), 2013/2014–2019/2020.

	Around Median		Top/Bottom Tercile	
	(1)	(2)	(3)	(4)
	Exit	Exit	Exit	Exit
Post × REP+ × High Wage Gap	-0.0119**		-0.00529*	
	(0.00495)		(0.00318)	
Post × REP+ × High Outside Wage		-0.0124***		-0.00848**
		(0.00459)		(0.00410)
Post × REP+ × High Wage		-0.00186		0.00246
		(0.00584)		(0.00490)
Teacher controls	✓	✓	✓	✓
School × Wage Gap Quantile FE	✓	–	✓	–
School × Outside Wage Quantile FE	–	✓	–	✓
School × Wage Quantile FE	–	✓	–	✓
School × Year FE	✓	✓	✓	✓
Observations	129,490	129,487	87,207	87,175
R-squared	0.060	0.060	0.086	0.098

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Table 5. Counterfactual outcomes under uniform and targeted wage changes

Note: Table reports simulated counterfactual outcomes from the discrete choice framework described in Section 5. Each row corresponds to a permanent change in either teacher wages or outside-option wages, with amounts expressed in thousands of euros (k euros). $\%\Delta P_1^H$ and $\%\Delta P_1^L$ denote the percentage change in the yearly exit probability for high- and low-TVA teachers, respectively, relative to the baseline. ΔQ_{t+5} and ΔQ_{t+10} report the implied change in aggregate teaching quality after 5 and 10 years, measured in standard deviations of TVA, relative to the baseline scenario with no policy change. Average quality Q_t is defined as the share-weighted average productivity of high- and low-TVA teachers. Uniform policies apply the same change to all teachers (or all outside options), while Targeted policies apply changes only to high-TVA teachers. All simulations assume no entry or replacement of exiting teachers and use the estimated parameters in Table 3.

Policy Counterfactual	% ΔP_1^H	% ΔP_1^L	ΔQ_{t+5}	ΔQ_{t+10}
Teacher Wage Increases				
Uniform increase (1.8k euros)	-61.5%	-29.1%	+1.5% SD	+3% SD
Targeted increase (equivalent, 0.8k euros)	-34.5%	0%	+1.5% SD	+3% SD
Targeted increase (3.6k euros)	-85.3%	0%	+3.6% SD	+7.3% SD
Outside Wage Increases				
Uniform increase (1.8k euros)	+154%	+40.7%	-5.1% SD	-10.2% SD
Targeted increase (1.8k euros)	+154%	0%	-6.7% SD	-13.3% SD

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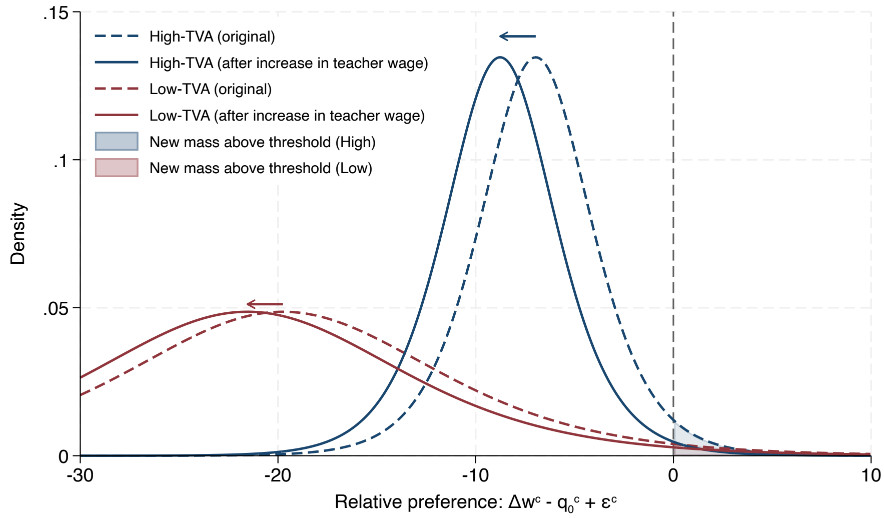
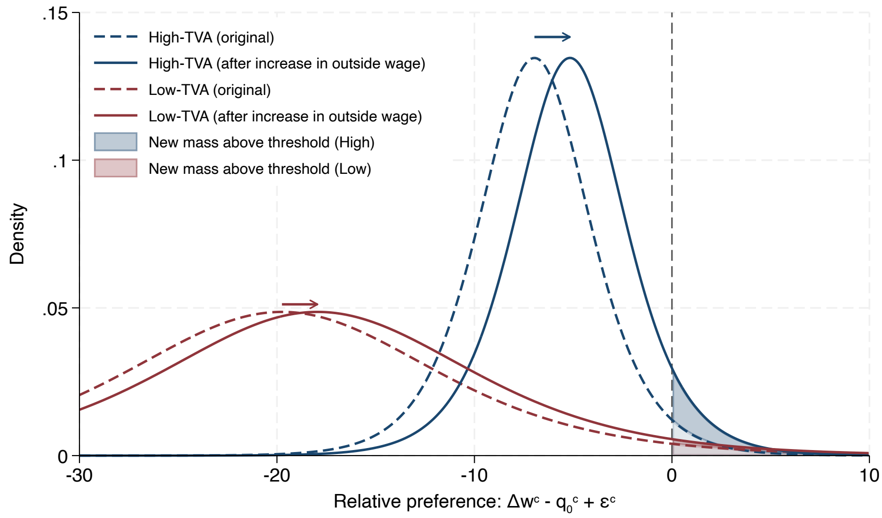
Figure 9. Asymmetric effects of teacher versus outside wage increases on aggregate teaching quality

Note: The figure illustrates the estimated distributions of teachers' relative preference for the outside option, $\Delta w^c - q_0^c + \varepsilon^c$, for high- and low-TVA types ($c \in H, L$), together with the counterfactual distributions implied by shifts in the wage gap. Here, Δw^c denotes the wage gap between the outside option and teaching, q_0^c the non-pecuniary utility from teaching, and ε^c an idiosyncratic preference shock distributed logistically with mean 0 and scale σ_ε. The vertical dotted line at zero marks the indifference threshold between staying in teaching and exiting. The shaded area represents the mass of teachers for whom $\Delta w^c - q_0^c + \varepsilon^c > 0$, i.e., those whose relative preference favors leaving the profession. Baseline distributions are estimated using the parameters in Table 3. Panel (a) shows the small shift in aggregate quality following a teacher wage increase (holding outside wages constant), while panel (b) illustrates the disproportionately larger decline in quality resulting from an equivalent increase in outside wages. See Section 5 for estimation details and interpretation.



(a) Increase in teacher wage



(b) Increase in outside wage

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Appendix

A. Tables

A.1. Descriptive statistics

Table A.1. Teacher and student characteristics by school type, 2013–2014

Note: The table reports mean values of each variable, with standard deviations in parentheses. For the first row, the numbers represent the total number of teachers, with percentages of the overall sample in parentheses. "High qualification" refers to teachers holding the *agrégation*. The sample includes tenured teachers under the age of 50, employed in public middle schools in 2013–2014. "Experience" measures total years of teaching, while "Experience in school" refers to the number of years spent in the current school. "Exit rate" equals 1 for teachers who leave the public school system after 2013–2014. Student composition variables ("Share very disadvantaged students," "Share foreign students," "Share scholarship students," and "Share very advantaged students") are computed from student-level data aggregated at the school level. Source: DEPP, Bases Relais and Scolarité, 2013–2014.

	Category of school			
	REP	REP+	Ordinary	All schools
N	18,069 (14.7%)	8,211 (6.7%)	96,626 (78.6%)	122,906 (100.0%)
Female	0.620 (0.485)	0.598 (0.490)	0.665 (0.472)	0.654 (0.476)
Experience	8.961 (5.828)	8.468 (5.513)	11.335 (6.201)	10.795 (6.192)
High qualification	0.053 (0.224)	0.048 (0.214)	0.056 (0.230)	0.055 (0.228)
Experience in school	3.616 (2.397)	3.513 (2.377)	3.875 (2.375)	3.813 (2.382)
Age	36.304 (6.376)	35.924 (6.144)	38.730 (6.145)	38.186 (6.267)
Exit rate	0.020 (0.141)	0.026 (0.160)	0.017 (0.129)	0.018 (0.133)
Re-entry after exit	0.005 (0.071)	0.005 (0.074)	0.004 (0.066)	0.005 (0.067)
Average wage	34.769 (3.192)	34.440 (2.918)	34.375 (3.652)	34.437 (3.548)
Average outside option wage	39.627 (7.200)	39.259 (6.666)	39.614 (6.774)	39.593 (6.831)
Share very disadvantaged students	0.572 (0.096)	0.731 (0.096)	0.366 (0.132)	0.420 (0.167)
Share foreign students	0.073 (0.065)	0.123 (0.088)	0.025 (0.039)	0.039 (0.056)
Share scholarship students	0.263 (0.163)	0.374 (0.271)	0.143 (0.109)	0.176 (0.151)
Share very advantaged students	0.085 (0.041)	0.034 (0.024)	0.215 (0.128)	0.184 (0.130)

Table A.2. Math and French teacher characteristics by school type, 2013–2014

Note: The table reports mean values of each variable, with standard deviations in parentheses. For the first row, the numbers represent the total number of teachers, with percentages of the overall sample in parentheses. "High qualification" refers to teachers holding the *agrégation*. The sample includes tenured Math and French teachers under the age of 50, employed in public middle schools in 2013–2014. "Experience" measures total years of teaching, while "Experience in school" refers to the number of years spent in the current school. "Exit rate" equals 1 for teachers who leave the public school system after 2013–2014. "Math/French specialization" indicates whether the teacher holds a specialization in the corresponding subject. "Has TVA" equals 1 for teachers whose teaching value-added (TVA) measure is available. Source: DEPP, Bases Relais, Scolarité, Sysca, and DNB, 2013–2014.

	Category of school			
	REP	REP+	Ordinary	Total
Math teachers				
N	3,687 (15.2%)	1,813 (7.5%)	18,729 (77.3%)	24,229 (100.0%)
Female	0.522 (0.500)	0.496 (0.500)	0.567 (0.495)	0.555 (0.497)
Experience	8.900 (5.796)	8.655 (5.513)	10.965 (6.144)	10.478 (6.113)
High qualification	0.040 (0.196)	0.045 (0.207)	0.047 (0.212)	0.046 (0.209)
Experience in school	3.568 (2.390)	3.486 (2.372)	3.738 (2.381)	3.693 (2.383)
Age	36.368 (6.497)	36.278 (6.214)	38.371 (6.055)	37.910 (6.195)
Math specialization	0.621 (0.485)	0.557 (0.497)	0.702 (0.458)	0.678 (0.467)
Exit rate	0.025 (0.156)	0.033 (0.179)	0.020 (0.139)	0.021 (0.145)
Re-entry after exit	0.005 (0.072)	0.004 (0.062)	0.003 (0.053)	0.003 (0.057)
Teaches 9th grade Math	0.773 (0.419)	0.751 (0.433)	0.798 (0.401)	0.791 (0.407)
Teaches 9th grade Math (merged to student data)	0.635 (0.481)	0.582 (0.493)	0.707 (0.455)	0.687 (0.464)
Has Math TVA	0.627 (0.484)	0.574 (0.495)	0.700 (0.458)	0.679 (0.467)
French teachers				
N	5,088 (16.2%)	2,435 (7.8%)	23,822 (76.0%)	31,345 (100.0%)
Female	0.754 (0.431)	0.741 (0.438)	0.820 (0.384)	0.803 (0.398)
Experience	9.269 (5.680)	8.687 (5.444)	11.299 (6.144)	10.766 (6.095)
High qualification	0.044 (0.206)	0.037 (0.190)	0.053 (0.224)	0.050 (0.219)
Experience in school	3.540 (2.379)	3.473 (2.361)	3.736 (2.387)	3.684 (2.386)
Age	36.791 (6.174)	36.334 (6.064)	38.582 (6.127)	38.117 (6.186)
French specialization	0.642 (0.479)	0.577 (0.494)	0.734 (0.442)	0.707 (0.455)
Exit rate	0.027 (0.161)	0.038 (0.192)	0.021 (0.143)	0.023 (0.151)
Re-entry after exit	0.006 (0.074)	0.005 (0.070)	0.005 (0.071)	0.005 (0.071)
Teaches 9th grade French	0.771 (0.420)	0.744 (0.437)	0.799 (0.401)	0.790 (0.407)
Teaches 9th grade French (merged to student data)	0.627 (0.484)	0.583 (0.493)	0.712 (0.453)	0.688 (0.463)
Has French TVA	0.626 (0.484)	0.577 (0.494)	0.709 (0.454)	0.685 (0.464)

A.2. Regression tables

Table A.3. DiD estimates (ITT) for the average teacher robust to different clustering

Note: Table reports estimates from the ITT difference-in-differences regression (3), using the 2013/2014 treatment assignment. The sample includes all teachers employed at REP or REP+ schools in 2013/2014. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. The specification includes teacher-level controls (gender, qualification, experience, age, and subject taught), as well as school and commuting-zone-by-year fixed effects. The table reports reduced-form estimates of the effect of the REP+ treatment on exits with different choices for clustering of standard errors. Standard errors (in parentheses) are clustered by teacher and school-year (column (1)), teacher (column (2)), school-year (column (3)), or school (column (4)). Controls, fixed-effects, and clusters are measured in 2013/2014. See Section 4 for details. Underlying data: Bases Relais, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) Baseline	(2) Teacher	(3) School-year	(4) School
Post × REP+	-0.00592**	-0.00592**	-0.00592**	-0.00592**
	(0.00232)	(0.00264)	(0.00233)	(0.00267)
Teacher controls	✓	✓	✓	✓
School FEs	✓	✓	✓	✓
CZ × Year FEs	✓	✓	✓	✓
Observations	148,857	148,857	148,857	148,857
R-squared	0.018	0.018	0.018	0.018

Table A.4. DiD estimates (ITT) by TVA robust to different clustering

Note: Table reports estimates from the ITT difference-in-differences regression (3), using the 2013/2014 treatment assignment. The sample includes all teachers employed at REP or REP+ schools in 2013/2014. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. The specification includes teacher-level controls (gender, qualification, experience, age, and subject taught), as well as school and commuting-zone-by-year fixed effects. The table reports reduced-form estimates of the effect of the REP+ treatment on exits with different choices for clustering of standard errors. Panel (a) shows results for teachers with above-median value-added (High TVA), and Panel (b) for those below the median (Low TVA). Standard errors (in parentheses) are clustered by teacher and school-year (column (1)), teacher (column (2)), school-year (column (3)), or school (column (4)). Controls, fixed-effects, and clusters are measured in 2013/2014. See Section 4 for details. Underlying data: Bases Relais, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(a) High-TVA teachers

	(1) Baseline	(2) Teacher	(3) School-year	(4) School
Post × REP+	-0.0140***	-0.0140**	-0.0140***	-0.0140**
	(0.00518)	(0.00590)	(0.00516)	(0.00601)
Teacher controls	✓	✓	✓	✓
School FEs	✓	✓	✓	✓
CZ × Year FEs	✓	✓	✓	✓
Observations	24,054	24,054	24,054	24,054
R-squared	0.091	0.091	0.091	0.091

(b) Low-TVA teachers

	(1) Baseline	(2) Teacher	(3) School-year	(4) School
Post × REP+	-0.00470	-0.00470	-0.00470	-0.00470
	(0.00514)	(0.00607)	(0.00507)	(0.00566)
Teacher controls	✓	✓	✓	✓
School FEs	✓	✓	✓	✓
CZ × Year FEs	✓	✓	✓	✓
Observations	22,322	22,322	22,322	22,322
R-squared	0.090	0.090	0.090	0.090

Table A.5. DiD estimates (ITT): Teacher turnover declines with the wage increase

Note: Table reports estimates from the 2SLS regressions of the type of (5), using the 2013/2014 treatment assignment. The sample includes all teachers employed at REP or REP+ schools in 2013/2014. Formally, for teachers j at school s and commuting zone z:

$$turnover_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}.$$

$turnover_{jst}$ equals 1 in the last year a teacher is observed in a school (exit or mobility). $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. The specification includes teacher-level controls (gender, qualification, experience, age, and subject taught), as well as school and commuting-zone-by-year fixed effects. Standard errors (in parentheses) are clustered by teacher and school-year. Controls, fixed effects, and clusters are measured in 2013/2014. Column (1) reports the ITT coefficient, while column (2) reports the corresponding LATE coefficient. The mean counterfactual turnover rate and counterfactual wage for REP+ teachers are reported for each column. The relative percentage decrease in turnover and the elasticity with respect to the wage (computed using equation (6)) are also reported. All wages and bonuses are CPI-deflated to 2024 thousands of euros. Source: Bases Relais, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) 2SLS (ITT)	(2) 2SLS (LATE)
Bonus (realized)	-0.00528*	
	(0.00300)	
Bonus (realized)		-0.00750*
		(0.00431)
Teacher controls	✓	✓
School FEs	✓	✓
CZ x Year FEs	✓	✓
Observations	148857	148857
R-squared	0.007	0.007
First-stage F stat	4892	1564
Counterfactual outflow	0.080	0.080
Counterfactual wage	34.744	35.615
Outflow elasticity to wage	-2.306	-3.356

Table A.6. DiD estimates (ITT): TVA-estimation subsample

Note: Table reports estimates from the ITT difference-in-differences regression (3) and (5), using the 2013/2014 treatment assignment. The sample includes Math and French teachers employed at REP or REP+ schools in 2013/2014, as well as the subsample of those with a TVA measure. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}.$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. The specification includes teacher-level controls (gender, qualification, experience, age, and subject taught), as well as school and commuting-zone-by-year fixed effects. Standard errors (in parentheses) are clustered by teacher and school-year. Controls, fixed effects, and clusters are measured in 2013/2014. Columns (1) and (4) report reduced-form estimates of the effect of the REP+ treatment on exits. Columns (2) and (5) present 2SLS ITT estimates instrumenting for the intended wage bonus, and columns (3) and (6) present 2SLS LATE estimates instrumenting for the realized wage bonus. The first stage relates post-reform REP+ status to the intended or realized wage bonus, and the second stage estimates its effect on exit probability. All estimates are relative to 2013/2014. The mean counterfactual exit rate and counterfactual wage for REP+ teachers are reported at the bottom for each subsample. The corresponding relative percentage decrease in exit rates and the elasticity with respect to the wage (computed using equation (6)) are also reported. All bonuses and wages are CPI-deflated to 2024 thousands of euros. Source: Bases Relais, Scolarité, Sysca, DNB, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Math & French			Math & French with TVA		
	(1)	(2)	(3)	(4)	(5)	(6)
	Reduced-form	2SLS (ITT)	2SLS (LATE)	Reduced-form	2SLS (ITT)	2SLS (LATE)
Post x REP+	-0.0127***			-0.0101***		
	(0.00368)			(0.00362)		
Bonus (intended)		-0.00758***			-0.00595***	
		(0.00219)			(0.00213)	
Bonus (realized)			-0.0104***			-0.00854***
			(0.00307)			(0.00310)
Teacher controls	✓	✓	✓	✓	✓	✓
School FEs	✓	✓	✓	✓	✓	✓
CZ x Year FEs	✓	✓	✓	✓	✓	✓
Observations	63602	63602	63602	46535	46535	46535
R-squared	0.040	0.003	0.001	0.047	0.002	-0.001
First-stage F stat		4822	935		4952	653
Counterfactual exit	0.028	0.028	0.028	0.021	0.021	0.021
As perc. of exit	-0.448			-0.491		
Counterfactual wage		34.677	35.563		34.636	35.504
Elasticity to wage		-9.234	-13.049		-10.055	-14.797

Table A.7. Relative responsiveness to the wage increase for REP+ schools by level of disadvantagedness

Note: Table reports estimates from the ITT difference-in-differences regression (3), extended to allow for heterogeneity in treatment effects across REP+ schools by their share of disadvantaged students in 2013/2014. The sample includes all teachers employed at REP or REP+ schools in 2013/2014. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \beta_1\, Post_t \times \mathbb{1}(type_j = \text{REP+},\, tercile_{s,dis} = 1) + \beta_2\, Post_t \times \mathbb{1}(type_j = \text{REP+},\, tercile_{s,dis} = 3) + \cdots + \varepsilon_{jst}.$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+},\, tercile_{s,dis} = k)$ equals 1 for teachers initially assigned to REP+ schools belonging to tercile k of the 2013 distribution of the share of very disadvantaged students, and 0 otherwise; REP teachers form the baseline group, and the middle tercile of REP+ schools is excluded from the regression. Columns (1)–(4) report reduced-form ITT estimates separately for all teachers, Math and French teachers, and the subsamples with high and low teacher value-added (TVA), with the latter two based on the median within commuting-zone–subject cells in 2013/2014. The specification includes teacher-level controls (gender, qualification, experience, age, and subject taught), as well as school and commuting-zone-by-year fixed effects. Standard errors (in parentheses) are clustered by teacher and school-year. Controls, fixed effects, and clusters are measured in 2013/2014. All estimates are relative to 2013/2014. Counterfactual exit rates for low- and high-disadvantaged REP+ schools, as well as the corresponding relative percentage changes, are reported at the bottom. The table also reports the p-value from a t-test of the null hypothesis that the two triple-interaction coefficients are equal. Source: Bases Relais, Scolarité, Sysca, DNB, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) All	(2) Math & French	(3) High TVA	(4) Low TVA
Post=1 × REP+ (tercile disadv.)=1	-0.00292	-0.0106*	-0.0149*	0.000335
	(0.00361)	(0.00559)	(0.00803)	(0.00836)
Post=1 × REP+ (tercile disadv.)=3	-0.00675*	-0.00856	-0.0130*	-0.00158
	(0.00374)	(0.00524)	(0.00755)	(0.00703)
Teacher controls	✓	✓	✓	✓
School FEs	✓	✓	✓	✓
CZ x Year FEs	✓	✓	✓	✓
Observations	133362	57069	21922	19843
R-squared	0.018	0.041	0.093	0.094
Counterfactual exit (low)	0.018	0.023	0.017	0.020
As perc. of exit (low)	-0.165	-0.453	-0.889	0.017
Counterfactual exit (high)	0.023	0.025	0.026	0.008
As perc. of exit (high)	-0.290	-0.340	-0.490	-0.190
T-test p-value	0.436	0.775	0.851	0.842

Table A.8. Explaining the REP+ status

Note: Table reports estimates from cross-sectional regressions where the dependent variable equals 1 if a teacher's school was classified as REP+ (and 0 otherwise) for the sample of teachers at REP+ and REP schools for the year 2013/2014. Each column includes a single measure of school-level disadvantageness as the main explanatory variable: the share of very disadvantaged students (column 1), the share of repeaters in primary school (column 2), and the share of students receiving need-based grants (column 3). All variables are measured in 2013/2014. Source: Bases Relais, 2013/2014. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) REP+	(2) REP+	(3) REP+
Share of very disadv. stud.	2.280***		
	(22.96)		
Share of repeaters in primary		2.282***	
		(13.21)	
Share of need-based grant recepients			0.540***
			(7.75)
Constant	-1.111***	-0.213***	0.143***
	(-17.69)	(-5.13)	(5.61)
Observations	939	939	939
R-squared	0.360	0.157	0.060

Table A.9. Triple DiD (ITT) estimates by TVA

Note: Table reports estimates from the ITT triple-difference regression, extending the baseline specification (3) to include heterogeneity by teacher value-added (TVA), using the 2013/2014 treatment assignment. The sample includes Math and French teachers employed at REP or REP+ schools in 2013/2014 for whom a TVA measure is available. Formally, for teachers j at school s, in commuting zone z, and subject m:

$$exit_{jstm} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) \times HighTVA_{jmz} + \cdots + \delta \mathbf{X}_j + \theta_{s \times TVA} + \lambda_{zt \times TVA} + \varepsilon_{jstm}.$$

$exit_{jstm}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. $HighTVA_{jmz}$ equals 1 for teachers with TVA above the median within commuting-zone–subject cells in 2013/2014, and 0 otherwise. The specification progressively adds teacher-level controls (gender, qualification, experience, age, and subject teaught) interacted with high-TVA dummies, as well as high-TVA–specific fixed effects at the school, subject, and commuting-zone-by-year levels, as well as school-type-by-year, or school-by-year fixed-effects, as indicated by the rows. Lower-level interaction terms are not reported for exposition. Standard errors (in parentheses) are clustered by teacher and school-by-year-by-TVA type. Controls, fixed effects, and clusters are measured in 2013/2014. All estimates are relative to 2013/2014. Source: Bases Relais, Scolarité, Sysca, DNB, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Exit (1)	Exit (2)	Exit (3)	Exit (4)	Exit (5)
Post × REP+ × High TVA	-0.00938	-0.00946	-0.00948	-0.00942	-0.0113*
	(0.00736)	(0.00735)	(0.00735)	(0.00735)	(0.00669)
Post × REP+	-0.00472	-0.00470	-0.00469		
	(0.00513)	(0.00513)	(0.00513)		
Teacher controls	–	✓	✓	✓	✓
School x High TVA FEs	✓	✓	✓	✓	✓
CZ x Year x High TVA FEs	✓	✓	✓	✓	✓
Subject x High TVA FEs	–	–	✓	✓	✓
REP+ x Year FEs	–	–	–	✓	–
School x Year FEs	–	–	–	–	✓
Observations	46324	46324	46324	46324	46279
R-squared	0.089	0.091	0.091	0.091	0.184

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.10. Triple DiD estimates by TVA (ITT): Robustness to other sources of heterogeneity

Note: Table reports estimates from ITT triple-difference regressions, extending the baseline specification (3) to include heterogeneity by teacher value-added (TVA) and its interaction with other observable teacher characteristics, using the 2013/2014 treatment assignment. The sample includes Math and French teachers employed at REP or REP+ schools in 2013/2014 for whom a TVA measure is available. Formally, for teachers j at school s, in commuting zone z, and subject m:

$$exit_{jstm} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) \times HighTVA_{jmz} + \cdots + \varepsilon_{jstm}.$$

Column (1) presents the baseline triple-difference specification. Columns (2)–(7) sequentially add triple interaction terms between $Post_t \times \mathbb{1}(type_j = \text{REP+})$ and observable teacher characteristics—experience terciles, age terciles, gender, and qualification—to test robustness to other sources of heterogeneity. All teacher controls are interacted with the high-TVA dummy. The specification includes teacher-level controls (gender, qualification, experience, age, and subject specialization) and high-TVA–specific fixed effects at the school and commuting-zone-by-year levels, as well as school-by-year fixed-effects. Lower-level interaction terms are not reported for exposition. Standard errors (in parentheses) are clustered by teacher and school-by-year-by-TVA type. Controls, fixed effects, and clusters are measured in 2013/2014. All estimates are relative to 2013/2014. Source: Bases Relais, Scolarité, Sysca, DNB, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) Exit	(2) Exit	(3) Exit	(4) Exit	(5) Exit	(6) Exit	(7) Exit
Post × REP+ × High TVA	-0.0113*	-0.0113*	-0.0114*	-0.0110	-0.0111	-0.0115*	-0.0110
	(0.00669)	(0.00669)	(0.00671)	(0.00679)	(0.00674)	(0.00670)	(0.00683)
Post × REP+ × Experience terc.=2			0.00664				-0.000000982
			(0.0105)				(0.0139)
Post × REP+ × Experience terc.=3			-0.00172				-0.0174
			(0.0125)				(0.0182)
Post × REP+ × Age terc.=2				0.00514			0.00846
				(0.0106)			(0.0136)
Post × REP+ × Age terc.=3				0.00971			0.0217
				(0.0113)			(0.0171)
Post × REP+ × Female					-0.00305		-0.00183
					(0.00946)		(0.00950)
Post × REP+ × High qualif.						-0.00301	-0.00373
						(0.0282)	(0.0279)
Teacher controls	✓	✓	✓	✓	✓	✓	✓
School x High TVA FEs	✓	✓	✓	✓	✓	✓	✓
CZ x Year x High TVA FEs	✓	✓	✓	✓	✓	✓	✓
Subject x High TVA FEs	✓	✓	✓	✓	✓	✓	✓
School x Year FEs	✓	✓	✓	✓	✓	✓	✓
Observations	46279	46279	46279	46279	46279	46279	46279
R-squared	0.184	0.184	0.184	0.184	0.184	0.184	0.184

Table A.11. Triple DiD estimates by TVA (ITT): Robustness to TVA-type measurement

Note: Table reports ITT estimates from the difference-in-differences regression extending the baseline specification (3), using alternative definitions of the TVA groups. The sample includes Math and French teachers employed at REP or REP+ schools in 2013/2014 for whom a TVA measure is available. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}.$$

The dependent variable $exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. The specification includes teacher-level controls (gender, qualification, experience, age, and subject), as well as school and commuting-zone-by-year fixed effects. Standard errors (in parentheses) are clustered by teacher and school-year. Controls, fixed effects, and clusters are measured in 2013/2014. Panel (a) defines high- and low-TVA as, respectively, above- or below-median value-added (TVA) within different regional levels: in columns (1) and (4), TVA is split within commuting zone (baseline), in columns (2) and (5), within *académie*, in columns (3) and (6), within school. Panel (b) defines high- and low-TVA within commuting zone as, respectively, above and below median (baseline in columns (1) and (4)), top and bottom tercile (columns (2) and (5)), and top and bottom quartile (columns (3) and (6)). All estimates are relative to 2013/2014. Source: Bases Relais, Scolarité, Sysca, DNB, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(a) Above/below median within different regional levels

	High TVA			Low TVA		
	(1) Baseline	(2) TVA w/ acad.	(3) TVA w/ school	(4) Baseline	(5) TVA w/ acad.	(6) TVA w/ school
Post x REP+	-0.0140***	-0.0121**	-0.0157***	-0.00470	-0.00568	-0.00231
	(0.00518)	(0.00508)	(0.00535)	(0.00514)	(0.00515)	(0.00478)
Teacher controls	✓	✓	✓	✓	✓	✓
School FEs	✓	✓	✓	✓	✓	✓
CZ x Year FEs	✓	✓	✓	✓	✓	✓
Observations	24054	23834	25658	22322	22569	20796
R-squared	0.091	0.089	0.079	0.090	0.095	0.099

(b) Different cutoffs within commuting zone

	High TVA			Low TVA		
	(1) Baseline	(2) Top tercile	(3) Top quartile	(4) Baseline	(5) Bottom tercile	(6) Bottom quartile
Post × REP+	-0.0140***	-0.0126**	-0.00950	-0.00470	-0.00491	-0.00214
	(0.00518)	(0.00575)	(0.00665)	(0.00514)	(0.00625)	(0.00700)
Teacher controls	✓	✓	✓	✓	✓	✓
School FEs	✓	✓	✓	✓	✓	✓
CZ × Year FEs	✓	✓	✓	✓	✓	✓
Observations	24054	13747	10022	22322	13107	9652
R-squared	0.091	0.140	0.173	0.090	0.141	0.161

Table A.12. Triple DiD estimates by TVA (ITT) by subject taught

Note: Table reports estimates from the ITT triple-difference regression, extending the baseline specification (3) to include heterogeneity by teacher value-added (TVA) by subject taught, using the 2013/2014 treatment assignment: Math or French (baseline, column (1)), Math (column (2)), and French (column (3)). The sample includes Math and French teachers employed at REP or REP+ schools in 2013/2014 for whom a TVA measure is available. Formally, for teachers j at school s, in commuting zone z, and subject m:

$$exit_{jstm} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) \times HighTVA_{jmz} + \cdots + \delta \mathbf{X}_j + \theta_{s\times TVA} + \lambda_{zt\times TVA} + \varepsilon_{jstm}.$$

$exit_{jstm}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. $HighTVA_{jmz}$ equals 1 for teachers with TVA above the median within commuting-zone–subject cells in 2013/2014, and 0 otherwise. Teacher-level controls (gender, qualification, experience, age, and subject taught) are interacted with high-TVA dummies. Fixed effects are indicated below each column. Lower-level interaction terms are not reported for exposition. Standard errors (in parentheses) are clustered by teacher and school-by-year-by-TVA type. Controls, fixed effects, and clusters are measured in 2013/2014. All estimates are relative to 2013/2014. Source: Bases Relais, Scolarité, Sysca, DNB, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Baseline (Math & French)	Math	French
	(1)	(2)	(3)
	Exit	Exit	Exit
Post × REP+ × High TVA	-0.0113*	-0.0140	-0.00947
	(0.00669)	(0.0120)	(0.0120)
Teacher controls	✓	✓	✓
School × High TVA FEs	✓	✓	✓
CZ × Year × High TVA FEs	✓	✓	✓
Subject × High TVA FEs	✓	✓	✓
School × Year FEs	✓	✓	✓
Sample	[2013,2019]	[2013,2019]	[2013,2019]
Observations	46279	18432	26299
R-squared	0.184	0.355	0.283

Table A.13. DiD estimates (ITT): robustness to school-level exposure measures

Note: Table reports ITT estimates from the difference-in-differences regression extending the baseline specification (3), using alternative definitions of school-level exposure to the REP+ reform. The baseline sample includes all teachers employed at REP or REP+ schools in 2013/2014. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}.$$

The dependent variable $exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. The specification includes teacher-level controls (gender, qualification, experience, age, and subject), as well as school and commuting-zone-by-year fixed effects. Standard errors (in parentheses) are clustered by teacher and school-year. Controls, fixed effects, and clusters are measured in 2013/2014. Column (1) reports the baseline ITT estimate. Column (2) excludes teachers assigned in 2013 to the first 87 schools announced as REP+ in metropolitan France before the 2014–2015 academic year. Column (3) restricts the sample to teachers assigned in 2013 to those first 87 treated schools. Column (4) focuses on teachers assigned in 2013 to REP+ or REP schools included in the first two treatment waves (announced before the 2015–2016 academic year). All estimates are relative to 2013/2014. Counterfactual exit rates for REP+ teachers and associated percentage changes are reported at the bottom. Source: Bases Relais, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) Baseline	(2) Excluding treated in 2014	(3) Treated in 2014	(4) Treated in 2014/2015
Post × REP+	-0.00592**	-0.00503**	-0.00849**	-0.00533**
	(0.00232)	(0.00255)	(0.00386)	(0.00235)
Constant	0.0219***	0.0220***	0.0212***	0.0220***
	(0.00108)	(0.00109)	(0.00107)	(0.00110)
Teacher controls	✓	✓	✓	✓
School FEs	✓	✓	✓	✓
CZ x Year FEs	✓	✓	✓	✓
Observations	148857	135992	115095	143619
R-squared	0.018	0.018	0.019	0.018
Counterfactual exit	0.022	0.021	0.025	0.022
As perc. of exit	-0.269	-0.240	-0.343	-0.242

Table A.14. DiD estimates (ITT): robustness to school-level exposure measures by TVA

Note: Table reports ITT estimates from the difference-in-differences regression extending the baseline specification (3), using alternative definitions of school-level exposure to the REP+ reform. The sample includes Math and French teachers employed at REP or REP+ schools in 2013/2014 for whom a TVA measure is available. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}.$$

The dependent variable $exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. The specification includes teacher-level controls (gender, qualification, experience, age, and subject), as well as school and commuting-zone-by-year fixed effects. Standard errors (in parentheses) are clustered by teacher and school-year. Controls, fixed effects, and clusters are measured in 2013/2014. Columns (1)–(4) report ITT estimates using, respectively, the baseline specification, a sample excluding the first 87 REP+ schools announced before 2014–2015, only those first treated schools, and the first two treatment waves (2014–2015). Panel (a) shows results for teachers with above-median value-added (High TVA), and Panel (b) for those below the median (Low TVA). All estimates are relative to 2013/2014. Counterfactual exit rates for REP+ teachers and associated percentage changes are reported at the bottom. Source: Bases Relais, Scolarité, Sysca, DNB, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(a) High-TVA teachers

	(1) Baseline	(2) Excluding treated in 2014	(3) Treated in 2014	(4) Treated in 2014/2015
Post × REP+	-0.0140***	-0.0117**	-0.0200**	-0.0129**
	(0.00518)	(0.00578)	(0.00904)	(0.00536)
Constant	0.0228***	0.0266***	0.0217***	0.0224***
	(0.00509)	(0.00522)	(0.00565)	(0.00517)
Teacher controls	✓	✓	✓	✓
School FEs	✓	✓	✓	✓
CZ x Year FEs	✓	✓	✓	✓
Sample	[2013,2019]	[2013,2019]	[2013,2019]	[2013,2019]
Observations	24054	22175	18909	23213
R-squared	0.091	0.092	0.102	0.092
Counterfactual exit	0.023	0.020	0.030	0.023
As perc. of exit	-0.622	-0.599	-0.659	-0.572

(b) Low-TVA teachers

	(1) Baseline	(2) Excluding treated in 2014	(3) Treated in 2014	(4) Treated in 2014/2015
Post × REP+	-0.00470	-0.00711	0.00226	-0.00390
	(0.00514)	(0.00631)	(0.00600)	(0.00518)
Constant	0.0343***	0.0369***	0.0351***	0.0350***
	(0.00597)	(0.00642)	(0.00720)	(0.00623)
Teacher controls	✓	✓	✓	✓
School FEs	✓	✓	✓	✓
CZ x Year FEs	✓	✓	✓	✓
Sample	[2013,2019]	[2013,2019]	[2013,2019]	[2013,2019]
Observations	22322	20219	17177	21544
R-squared	0.090	0.094	0.104	0.089
Counterfactual exit	0.017	0.023	0.004	0.018
As perc. of exit	-0.270	-0.309	0.561	-0.218

Table A.15. DiD estimates (ITT) for the average teacher robust to controlling for teacher-characteristics-by-time

Note: Table reports estimates from the ITT difference-in-differences regression (3), using the 2013/2014 treatment assignment. The sample includes all teachers employed at REP or REP+ schools in 2013/2014. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) + \gamma_k \mathbf{X}_j \times \mathbb{1}(t = k) + \theta_s + \lambda_{zt} + \varepsilon_{jst}$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. The specification includes teacher-level controls (gender, qualification, experience, age, and subject taught), as well as school and commuting-zone-by-year fixed effects. Teacher controls are included in levels (column (1), the baseline specification), interacted with a dummy $Post_t$ (column (2)), or interacted with year dummies $\mathbb{1}(t = k)$ (column (3)). The table reports reduced-form estimates of the effect of the REP+ treatment on exits with different choices for clustering of standard errors. Standard errors (in parentheses) clustered by teacher and school-year. Controls, fixed-effects, and clusters are measured in 2013/2014. See Section 4 for details. Underlying data: Bases Relais, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) Exit	(2) Exit	(3) Exit
Post × REP+	-0.00592**	-0.00586**	-0.00586**
	(0.00232)	(0.00234)	(0.00234)
Teacher controls	✓	✓	
School FEs	✓	✓	✓
CZ × Year FEs	✓	✓	✓
Observations	148,857	148,857	148,857
R-squared	0.018	0.018	0.018
Counterfactual exit	0.022	0.022	0.022
As perc. of exit	-0.269	-0.266	-0.266

Table A.16. DiD estimates (ITT) by TVA robust to controlling for teacher-characteristics-by-time

Note: Table reports estimates from the ITT difference-in-differences regression (3), using the 2013/2014 treatment assignment. The sample includes all teachers employed at REP or REP+ schools in 2013/2014. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) + \gamma_k \mathbf{X}_j \times \mathbb{1}(t = k) + \theta_s + \lambda_{zt} + \varepsilon_{jst}$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. The specification includes teacher-level controls (gender, qualification, experience, age, and subject taught), as well as school and commuting-zone-by-year fixed effects. Columns (1)-(3) show results for teachers with above-median value-added (High TVA), and columns (4)-(6) for those below the median (Low TVA). Teacher controls are included in levels (column (1) and (4), the baseline specification), interacted with a dummy $Post_t$ (column (2) and (5)), or interacted with year dummies $\mathbb{1}(t = k)$ (column (3) and (6)). The table reports reduced-form estimates of the effect of the REP+ treatment on exits with different choices for clustering of standard errors. Standard errors (in parentheses) clustered by teacher and school-year. Controls, fixed-effects, and clusters are measured in 2013/2014. See Section 4 for details. Underlying data: Bases Relais, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	High TVA			Low TVA		
	(1)	(2)	(3)	(4)	(5)	(6)
	Exit	Exit	Exit	Exit	Exit	Exit
Post × REP+	-0.0140***	-0.0154***	-0.0154***	-0.00470	-0.00423	-0.00422
	(0.00518)	(0.00522)	(0.00522)	(0.00514)	(0.00515)	(0.00516)
Teacher controls	✓	✓	✓	✓		
School FEs	✓	✓	✓	✓	✓	✓
CZ × Year FEs	✓	✓	✓	✓	✓	✓
Observations	24,054	24,054	24,054	22,322	22,322	22,322
R-squared	0.091	0.091	0.093	0.090	0.091	0.092
Counterfactual exit	0.023	0.023	0.023	0.017	0.017	0.017
As perc. of exit	-0.622	-0.682	-0.682	-0.270	-0.243	-0.243

Table A.17. DiD estimates (ITT): robustness to different specification choices

Note: Note: Table reports estimates from the ITT difference-in-differences regression (3), using the 2013/2014 treatment assignment. The sample includes all teachers employed at REP or REP+ schools in 2013/2014. Formally, for teachers j at school s and commuting zone z: :

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}.$$

The dependent variable $exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. Teacher-level controls include gender, qualification, experience, age, and subject, with school and commuting-zone-by-year fixed effects. Standard errors (in parentheses) are clustered by teacher and school-year. Controls, fixed effects, and clusters are measured in 2013/2014. Columns (1)–(6) vary specification choices: (1) baseline; (2) defines exit as no re-entry for at least three years; (3) restricts to schools observed 2013–2019; (4) excludes novice teachers (less than two years of experience in 2013/2014); (5)–(6) relax the retirement-age cutoff to 58 and 55 years, respectively. All estimates are relative to 2013/2014. Counterfactual exit rates and percentage changes for REP+ teachers are reported at the bottom. Source: Bases Relais, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) Baseline	(2) Exit for 3y min.	(3) Balanced sch. panel	(4) No novice	(5) Exit below 58 yo	(6) Exit below 55 yo
Post × REP+	-0.00592**	-0.00483**	-0.00598***	-0.00539**	-0.00626***	-0.00614***
	(0.00232)	(0.00211)	(0.00232)	(0.00220)	(0.00221)	(0.00226)
Constant	0.0219***	0.0179***	0.0212***	0.0192***	0.0212***	0.0212***
	(0.00108)	(0.000929)	(0.00103)	(0.000984)	(0.000997)	(0.00100)
Teacher controls	✓	✓	✓	✓	✓	✓
School FEs	✓	✓	✓	✓	✓	✓
CZ × Year FEs	✓	✓	✓	✓	✓	✓
Observations	148857	148857	148171	148500	172772	164614
R-squared	0.018	0.017	0.018	0.017	0.016	0.017
Counterfactual exit	0.022	0.020	0.022	0.022	0.023	0.022
As perc. of exit	-0.269	-0.247	-0.271	-0.244	-0.275	-0.280

Table A.18. DiD estimates (ITT): Robustness to different specification choices by TVA

Note: Note: Table reports estimates from the ITT difference-in-differences regression (3), using the 2013/2014 treatment assignment. The sample includes Math and French teachers employed at REP or REP+ schools in 2013/2014 for whom a TVA measure is available. Formally, for teachers j at school s and commuting zone z: :

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}.$$

The dependent variable $exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. Teacher-level controls include gender, qualification, experience, age, and subject, with school and commuting-zone-by-year fixed effects. Standard errors (in parentheses) are clustered by teacher and school-year. Controls, fixed effects, and clusters are measured in 2013/2014. Columns (1)–(6) vary specification choices: (1) baseline; (2) defines exit as no re-entry for at least three years; (3) restricts to schools observed 2013–2020; (4) excludes novice teachers (less than two years of experience in 2013/2014); (5)–(6) relax the retirement-age cutoff to 58 and 55 years, respectively. Panel (a) shows results for teachers with above-median value-added (High TVA), and Panel (b) for those below the median (Low TVA). All estimates are relative to 2013/2014. Counterfactual exit rates and percentage changes for REP+ teachers are reported at the bottom. Source: Bases Relais, Scolarité, Sysca, DNB, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(a) High-TVA teachers

	(1) Baseline	(2) Exit for 3y min.	(3) Balanced sch. panel	(4) No novice	(5) Exit below 58 yo	(6) Exit below 55 yo
Post × REP+	-0.0140***	-0.0107**	-0.0139***	-0.0129**	-0.0103**	-0.0116**
	(0.00518)	(0.00486)	(0.00521)	(0.00513)	(0.00505)	(0.00496)
Constant	0.0233***	0.0193***	0.0233***	0.0223***	0.0269***	0.0238***
	(0.00495)	(0.00461)	(0.00497)	(0.00493)	(0.00489)	(0.00464)
Teacher controls	✓	✓	✓	✓	✓	✓
school_id13	✓	✓	✓	✓	✓	✓
CZ13_yr	✓	✓	✓	✓	✓	✓
Sample						
Observations	24054	24054	23943	24037	27145	26188
R-squared	0.091	0.089	0.091	0.091	0.085	0.085
Counterfactual exit	0.022	0.019	0.023	0.023	0.022	0.021
As perc. of exit	-0.646	-0.569	-0.616	-0.563	-0.472	-0.555

(b) Low-TVA teachers

	(1) Baseline	(2) Exit for 3y min.	(3) Balanced sch. panel	(4) No novice	(5) Exit below 58 yo	(6) Exit below 55 yo
Post × REP+	-0.00470	-0.00387	-0.00549	-0.00548	-0.00230	-0.00393
	(0.00514)	(0.00439)	(0.00505)	(0.00508)	(0.00494)	(0.00497)
Constant	0.0327***	0.0264***	0.0330***	0.0324***	0.0297***	0.0290***
	(0.00588)	(0.00559)	(0.00588)	(0.00587)	(0.00564)	(0.00562)
Teacher controls	✓	✓	✓	✓	✓	✓
School FEs	✓	✓	✓	✓	✓	✓
CZ × Year FEs	✓	✓	✓	✓	✓	✓
Sample						
Observations	22322	22322	22210	22298	25038	24101
R-squared	0.090	0.090	0.091	0.090	0.083	0.084
Counterfactual exit	0.018	0.014	0.017	0.017	0.016	0.016
As perc. of exit	-0.265	-0.267	-0.314	-0.316	-0.147	-0.244

Table A.19. DiD estimates (ITT): Robustness to different subsamples of teachers

Note: Table reports estimates from the ITT difference-in-differences regression (3), using the 2013/2014 treatment assignment. The sample includes all teachers employed at REP or REP+ schools in 2013/2014. Formally, for teacher j in school s and commuting zone z:

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}.$$

The dependent variable $exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. Teacher-level controls $\mathbf{X}_j$ include gender, qualification, experience, age, and subject. All specifications include school fixed effects and commuting-zone-by-year fixed effects. Standard errors (in parentheses) are clustered by teacher and school-year. Controls, fixed effects, and clusters are measured in 2013/2014. Columns differ by sample restriction (All teachers, teachers with standard qualification (Capes), and teachers with an advanced qualification (Agrégation)). All estimates are relative to 2013/2014. Counterfactual exit rates and percentage changes for REP+ teachers are reported at the bottom. Source: Bases Relais, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) Baseline	(2) Capes	(3) Agregation
Post × REP+	-0.00592**	-0.00602***	-0.0132
	(0.00232)	(0.00229)	(0.0143)
Constant	0.0219***	0.0212***	0.0249***
	(0.00108)	(0.00108)	(0.00837)
Teacher controls	✓	✓	✓
School FEs	✓	✓	✓
CZ x Year FEs	✓	✓	✓
Observations	148857	140368	8199
R-squared	0.018	0.018	0.190
Counterfactual exit	0.022	0.022	0.021
As perc. of exit	-0.269	-0.272	-0.616

Table A.20. DiD estimates for the average teacher: ATT estimates are similar to LATE estimates

Note: Table reports estimates from the ATT difference-in-differences regression (3) and (5), using the contemporaneous treatment assignment. The sample includes all teachers employed at REP or REP+ schools in a given year t. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_s = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise (except for column (6), where $Post_t$ equals 0 between 2009/2010 and 2013/2014). $\mathbb{1}(type_s = \text{REP+})$ equals 1 for schools later classified as REP+, and 0 for REP. Treated teachers are therefore teachers who are in school for which $\mathbb{1}(type_s = \text{REP+}) = 1$ at a given year t. The specification includes teacher-level controls (gender, qualification, experience, age, and subject taught), as well as school and commuting-zone-by-year fixed effects. Standard errors (in parentheses) clustered by teacher and school-year. Controls, fixed-effects, and clusters are measured in year t. Columns (1)-(3) report estimates of the effect of the REP+ treatment on exits for the short-run (until 2015/2016), and columns (4)-(7) report estimates for the medium-run (until 2017/2018). The sample period is cut prior to 2019/2020 to reflect the changing relative composition at treated schools after 2018/2019. Columns (1) and (4) report reduced-form ATT estimates, and columns (2), (5) and (6) report the 2SLS ATT estimates instrumenting for the intended or realized wage bonus. Column (6) uses a de-trended difference-in-differences method to reflect the increasing relative pre-trend in exits for treated teachers (see Section C.4 for details). The first stage relates the post-reform REP+ status to the realized wage bonus, and the second stage estimates its effect on exit probability. Columns (3) and (7) report the corresponding LATE estimates from regressions equivalent to those in Table (1) column (4) for the short- and medium-run, respectively, for comparability with the ATT estimates. The mean counterfactual exit rate and counterfactual wage for REP+ teachers are reported for each column. The corresponding relative percentage decrease in exit rates and the elasticity with respect to the wage (computed using equation (6)) are also reported. The bonus and counterfactual wage are CPI-deflated to 2024 thousands of euros. See Section 4 for details. Source: Bases Relais, 2013/2014–2017/2018. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Short-run			Medium-run			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Reduced-form (ATT)	2SLS (ATT)	2SLS (LATE)	Reduced-form (ATT)	2SLS (ATT)	2SLS de-trended (ATT)	2SLS (LATE)
Post x REP+	-0.00663***			-0.00810***			
	(0.00244)			(0.00239)			
Bonus (realized)		-0.00959***	-0.00739*		-0.00790***	-0.0101***	-0.00717**
		(0.00353)	(0.00381)		(0.00232)	(0.00145)	(0.00298)
Teacher controls	✓	✓	✓	✓	✓	✓	✓
School FEs	✓	✓	✓	✓	✓	✓	✓
CZ x Year FEs	✓	✓	✓	✓	✓	✓	✓
Sample	[2013,2015]	[2013,2015]	[2013,2015]	[2013,2017]	[2013,2017]	[2009,2017]	[2013,2017]
Observations	80378	80378	72317	135358	135358	242157	112688
R-squared	0.022	0.003	0.002	0.018	0.003	0.000	0.001
First-stage F stat		759524689	2898		4715	4775	1567
Counterfactual exit	0.032	0.032	0.024	0.032	0.032	0.035	0.023
As perc. of exit	-0.204			-0.252			
Counterfactual wage		34.458	34.979		34.545	34.520	35.327
Elasticity to wage		-10.184	-10.892		-8.487	-9.945	-11.187

Table A.21. DiD estimates by TVA: ATT estimates are similar to LATE estimates

Note: Table reports estimates from the ATT difference-in-differences regression (3) and (5), using the contemporaneous treatment assignment. The sample includes Math and French teachers employed at REP or REP+ schools in a given year t for whom a TVA measure is available. Formally, for teachers j at school s and commuting zone z: $exit_{jst} = \beta \, Post_t \times \mathbb{1}(type_s = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}$, where $exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise (except for column (6), where $Post_t$ equals 0 between 2009/2010 and 2013/2014). $\mathbb{1}(type_s = \text{REP+})$ equals 1 for schools later classified as REP+, and 0 for REP. Treated teachers are therefore teachers who are in school for which $\mathbb{1}(type_s = \text{REP+}) = 1$ at a given year t. The specification includes teacher-level controls (gender, qualification, experience, age, and subject taught), as well as school and commuting-zone-by-year fixed effects. Standard errors (in parentheses) clustered by teacher and school-year. Controls, fixed-effects, and clusters are measured in year t. Columns (1)-(3) report estimates of the effect of the REP+ treatment on exits for the short-run (until 2015/2016), and columns (4)-(7) report estimates for the medium-run (until 2017/2018). The sample period is cut prior to 2019/2020 to reflect the changing relative composition at treated schools after 2018/2019. Columns (1) and (4) report reduced-form ATT estimates, and columns (2), (5) and (6) report the 2SLS ATT estimates instrumenting for the intended or realized wage bonus. Column (6) uses a de-trended difference-in-differences method to reflect the increasing relative pre-trend in exits for treated teachers (see Section C.4 for details). The first stage relates the post-reform REP+ status to the realized wage bonus, and the second stage estimates its effect on exit probability. Columns (3) and (7) report the corresponding LATE estimates from regressions equivalent to those in Table (1) column (4) for the short- and medium-run, respectively, for comparability with the ATT estimates. Panel (a) shows results for teachers with above-median value-added (High TVA), and Panel (b) for those below the median (Low TVA). The mean counterfactual exit rate and counterfactual wage for REP+ teachers are reported for each column. The corresponding relative percentage decrease in exit rates and the elasticity with respect to the wage (computed using equation (6)) are also reported. The bonus and counterfactual wage are CPI-deflated to 2024 thousands of euros. See Section 4 for details. Source: Bases Relais, Scolarité, Sysca, DNB, 2013/2014–2017/2018. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(a) High-TVA teachers

	Short-run			Medium-run			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Reduced-form (ATT)	2SLS (ATT)	2SLS (LATE)	Reduced-form (ATT)	2SLS (ATT)	2SLS de-trended (ATT)	2SLS (LATE)
Post x REP+	-0.0123**			-0.0177***			
	(0.00519)			(0.00513)			
Bonus (realized)		-0.0177**	-0.0151*		-0.0173***	-0.0242***	-0.0161**
		(0.00752)	(0.00807)		(0.00502)	(0.00297)	(0.00665)
Teacher controls	✓	✓	✓	✓	✓	✓	✓
School FEs	✓	✓	✓	✓	✓	✓	✓
CZ x Year FEs	✓	✓	✓	✓	✓	✓	✓
Sample	[2013,2015]	[2013,2015]	[2013,2015]	[2013,2017]	[2013,2017]	[2009,2017]	[2013,2017]
Observations	12238	12238	11377	20480	20480	36779	18032
R-squared	0.117	0.003	0.000	0.094	0.002	0.002	-0.004
First-stage F stat		647748474	457		3990	4150	306
Counterfactual exit	0.023	0.023	0.020	0.025	0.025	0.031	0.022
As perc. of exit	-0.523			-0.695			
Counterfactual wage		34.567	34.847		34.772	34.712	35.230
Elasticity to wage		-26.545	-25.842		-23.726	-26.706	-26.183

(b) Low-TVA teachers

	Short-run			Medium-run			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Reduced-form (ATT)	2SLS (ATT)	2SLS (LATE)	Reduced-form (ATT)	2SLS (ATT)	2SLS de-trended (ATT)	2SLS (LATE)
Post x REP+	-0.00479			-0.00691			
	(0.00509)			(0.00510)			
Bonus (realized)		-0.00693	-0.00193		-0.00682	0.00170	-0.00728
		(0.00736)	(0.00779)		(0.00503)	(0.00329)	(0.00632)
Teacher controls	✓	✓	✓	✓	✓	✓	✓
School FEs	✓	✓	✓	✓	✓	✓	✓
CZ x Year FEs	✓	✓	✓	✓	✓	✓	✓
Sample	[2013,2015]	[2013,2015]	[2013,2015]	[2013,2017]	[2013,2017]	[2009,2017]	[2013,2017]
Observations	11878	11878	10796	19895	19895	35804	16840
R-squared	0.126	0.006	0.004	0.098	0.005	0.001	0.001
First-stage F stat		649054803	415		3972	4235	327
Counterfactual exit	0.021	0.022	0.015	0.021	0.021	0.018	0.017
As perc. of exit	-0.227			-0.324			
Counterfactual wage		34.420	34.791		34.585	34.584	35.158
Elasticity to wage		-11.068	-4.611		-11.054	3.193	-15.076

Table A.22. Small reallocation of teaching hours for REP+ teachers (vs. REP)

Note: Table reports estimates from the reduced-form estimates of regressions using the 2013/2014 treatment assignment and restricting the sample of teachers to those who remain at the treatment or control group in year t, for teachers j at school s and commuting zone z::

$$TeachingHours_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}.$$

where $TeachingHours_{jst}$ is the number of hours a teacher spends in teaching, $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. The specification includes teacher-level controls (experience and age at year t), as well as school, commuting-zone-by-year and teacher fixed effects. Standard errors (in parentheses) are clustered by teacher and school-year. Fixed effects, and clusters are measured in 2013/2014. Column (1) reports the estimates for the average teacher, while columns (2) and (3) split this sample into Capes-certified and Agrégation-certified teachers. Columns (4) and (5) focus on the high- and low-TVA sample of teachers, respectively. The mean number of hours taught in 2013 for the respective subsample of REP+ teachers are reported for each column. Source: Bases Relais, Scolarité, Sysca, DNB, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

		All		High TVA	Low TVA
	(1)	(2)	(3)	(4)	(5)
	All	Capes	Agregation		
Post × REP+	-0.779***	-0.755***	-0.443	-0.770***	-0.760***
	(0.137)	(0.137)	(0.391)	(0.219)	(0.233)
Teacher controls	✓	✓	✓	✓	✓
CZ × Year FEs	✓	✓	✓	✓	✓
School FEs	✓	✓	✓	✓	✓
Teacher FEs	✓	✓	✓	✓	✓
Observations	113290	106557	6142	18437	16588
R-squared	0.599	0.594	0.670	0.595	0.601
Mean hours for REP+ in 2013	18.579	18.698	16.126	18.516	18.741

Table A.23. DiD estimates (ITT): Robustness to inclusion of school-level controls

Note: Table reports estimates from the ITT difference-in-differences regression (3), using the 2013/2014 treatment assignment, adding contemporaneous or lagged school ("bad") controls. The sample includes all teachers employed at REP or REP+ schools in 2013/2014. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \delta \mathbf{X}_{st} + \theta_s + \lambda_{zt} + \varepsilon_{jst}$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. School-level controls include the average share of disadvantaged students, need-based scholarship holders, female students, and primary-school repeaters. The specification includes teacher-level controls (gender, qualification, experience, age, and subject taught), as well as school and commuting-zone-by-year fixed effects. Standard errors (in parentheses) clustered by teacher and school-year. Controls, fixed-effects, and clusters are measured in 2013/2014. Column (1) reports the baseline reduced-form estimates of the effect of the REP+ treatment on exits. Columns (2)–(4) present the reduced-form estimates when contemporaneous or lagged school controls are added (as indicated at the bottom of each column). School controls are computed for the school in which a teacher is assigned to in 2013/2014. All estimates are relative to 2013/2014. The mean counterfactual exit rate for REP+ teachers and the corresponding relative percentage decrease in exit rates are reported for each column. See Section 4 for details. Source: Bases Relais, Scolarité, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) Exit	(2) Exit	(3) Exit	(4) Exit
Post × REP+	-0.00592**	-0.00571**	-0.00616***	-0.00597**
	(0.00232)	(0.00232)	(0.00233)	(0.00235)
Constant	0.0219***	0.0160	0.0236	0.0311*
	(0.00108)	(0.0172)	(0.0169)	(0.0161)
Teacher controls	✓	✓	✓	✓
School controls	–	✓	–	–
School controls lag1	–	–	✓	–
School controls lag2	–	–	–	✓
School FEs	✓	✓	✓	✓
CZ x Year FEs	✓	✓	✓	✓
Observations	148857	148839	148857	148822
R-squared	0.018	0.018	0.018	0.018
Counterfactual exit	0.022	0.022	0.022	0.022
As perc. of exit	-0.269	-0.259	-0.279	-0.271

Table A.24. DiD estimates (ITT): Robustness to inclusion of school-level controls by TVA

Note: Table reports estimates from the ITT difference-in-differences regression (3), using the 2013/2014 treatment assignment, adding contemporaneous or lagged school ("bad") controls. The sample includes all Math and French teachers employed at REP or REP+ schools in 2013/2014 with measured TVA. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) + \gamma \mathbf{X}_j + \delta \mathbf{X}_{st} + \theta_s + \lambda_{zt} + \varepsilon_{jst}$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. School-level controls include the average share of disadvantaged students, need-based scholarship holders, female students, and primary-school repeaters. The specification includes teacher-level controls (gender, qualification, experience, age, and subject taught), as well as school and commuting-zone-by-year fixed effects. Standard errors (in parentheses) clustered by teacher and school-year. Controls, fixed-effects, and clusters are measured in 2013/2014. Column (1) reports the baseline reduced-form estimates of the effect of the REP+ treatment on exits. Columns (2)–(4) present the reduced-form estimates when contemporaneous or lagged school controls are added (as indicated at the bottom of each column). School controls are computed for the school in which a teacher is assigned to in 2013/2014. Panel (a) shows results for teachers with above-median value-added (High TVA), and Panel (b) for those below the median (Low TVA). All estimates are relative to 2013/2014. The mean counterfactual exit rate for REP+ teachers and the corresponding relative percentage decrease in exit rates are reported for each column. See Section 4 for details. Source: Bases Relais, Scolarité, Sysca, DNB, 2013/2014–2019/2020. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(a) High-TVA teachers

	(1) Exit	(2) Exit	(3) Exit	(4) Exit
Post × REP+	-0.0140***	-0.0142***	-0.0134**	-0.0140***
	(0.00518)	(0.00520)	(0.00519)	(0.00524)
Constant	0.0228***	0.0215	0.0207	0.0167
	(0.00509)	(0.0346)	(0.0355)	(0.0342)
Teacher controls	✓	✓	✓	✓
School controls	–	✓	–	–
School controls lag1	–	–	✓	–
School controls lag2	–	–	–	✓
School FEs	✓	✓	✓	✓
CZ x Year FEs	✓	✓	✓	✓
Observations	24054	24049	24054	24050
R-squared	0.091	0.091	0.091	0.091
Counterfactual exit	0.023	0.023	0.023	0.023
As perc. of exit	-0.622	-0.629	-0.593	-0.620

(b) Low-TVA teachers

	(1) Exit	(2) Exit	(3) Exit	(4) Exit
Post × REP+	-0.00470	-0.00562	-0.00429	-0.00452
	(0.00514)	(0.00522)	(0.00519)	(0.00519)
Constant	0.0343***	0.00774	-0.0379	-0.0285
	(0.00597)	(0.0462)	(0.0467)	(0.0458)
Teacher controls	✓	✓	✓	✓
School controls	–	✓	–	–
School controls lag1	–	–	✓	–
School controls lag2	–	–	–	✓
School FEs	✓	✓	✓	✓
CZ x Year FEs	✓	✓	✓	✓
Observations	22322	22319	22299	22315
R-squared	0.090	0.090	0.091	0.091
Counterfactual exit	0.017	0.017	0.017	0.017
As perc. of exit	-0.270	-0.323	-0.247	-0.260

Table A.25. Correlation of the wage gap with observable teacher characteristics

Note: Table reports estimates from a regression for teachers j at school s:

$$WageGap_{jst} = \beta X_j + \theta_{st} + \varepsilon_{jst}.$$

where X_j is: a dummy for being in the top age tercile (column (1)), a dummy for being female (column (2)), a dummy for being in the top "echelon" (i.e. position on the wage scale) tercile, or a dummy for being in the top tercile in experience in the educational sector (column (4)). The sample includes all teachers employed at a REP+ or REP school prior to 2013/2014. The specification includes school-year fixed effects. Standard errors reported in parentheses. The wage gap is CPI-deflated to 2024 thousands of euros. Source: Bases Relais, BTS-Postes, Teacher wage data (Chancel), 2009/2010–2013/2014. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) Wage gap	(2) Wage gap	(3) Wage gap	(4) Wage gap
Age terc.=3	2.129***			
	(0.0341)			
Female		-5.104***		
		(0.0183)		
Echelon terc.=3			-0.495***	
			(0.0316)	
Experience terc.=3				-0.484***
				(0.0364)
Constant	2.434***	6.596***	3.479***	3.344***
	(0.0167)	(0.0142)	(0.0158)	(0.0166)
School × Year FEs	✓	✓	✓	✓
Observations	61,033	91,167	78,415	65,492
R-squared	0.553	0.745	0.514	0.528

Table A.26. Triple DiD estimates by other sources of heterogeneity (ITT)

Note: Table reports estimates from the ITT difference-in-differences regression (3), using the 2013/2014 treatment assignment, and augmented with triple interactions. Sample includes teachers employed at REP or REP+ schools in 2013/2014. Formally, for teacher j at school s and teacher characteristic dummy X_j:

$$exit_{jst} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) \times X_j + \cdots + \gamma \mathbf{X'}_j + \theta_s \times X_j + \theta_{st} + \theta_{zt} \times X_j + \theta_m \times X_j + \varepsilon_{jst}.$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $Post_t$ equals 1 for years after 2013/2014, and 0 otherwise. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. X_j is either age tercile (column (1)), female (column (2)), "echelon" tercile (column (3)), experience tercile (column (4)), agrégation (column (5)), or all of the above combined (column (6)). The specification includes the residual teacher-level controls (gender, qualification, experience tercile, age tercile, and subject taught), interacted with X_j, as well as fixed effects as indicated in the table. Lower-level interaction terms are not reported for exposition. Standard errors (in parentheses) are clustered by teacher and school-year. All controls, fixed effects, and clusters are measured in 2013/2014. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Underlying data: Bases Relais, 2013/2014–2019/2020.

	(1) Exit	(2) Exit	(3) Exit	(4) Exit	(5) Exit	(6) Exit
Post x REP+ x Age terc.=2	-0.00399					-0.00687
	(0.00492)					(0.00675)
Post x REP+ x Age terc.=3	-0.0119**					-0.0141
	(0.00569)					(0.00877)
Post x REP+ x Female		-0.00123				-0.000910
		(0.00351)				(0.00594)
Post x REP+ x Echelon terc.=2			-0.000900			-0.00823
			(0.00449)			(0.0107)
Post x REP+ x Echelon terc.=3			-0.00809*			-0.00646
			(0.00487)			(0.00956)
Post x REP+ x Experience terc.=2				0.00362		0.0151
				(0.00595)		(0.0121)
Post x REP+ x Experience terc.=3				-0.00697		0.0111
				(0.00604)		(0.0120)
Post x REP+ x Agregation					-0.00168	-0.000621
					(0.0103)	(0.0143)
Teacher controls	✓	✓	✓	✓	✓	✓
School x Year FEs	✓	✓	✓	✓	✓	✓
School x Characteristic FEs	✓	✓	✓	✓	✓	✓
CZ x Year x Characteristic FEs	✓	✓	✓	✓	✓	✓
Subject x Characteristic FEs	✓	✓	✓	✓	✓	✓
Observations	148777	148849	148234	148780	148483	147578
R-squared	0.076	0.063	0.076	0.077	0.060	0.165

B. Figures

B.1. Descriptive figures

Figure B.1. Rates of teacher exit from the profession by experience group

Note: The figure plots the share of teachers in middle school exiting the profession every school year, restricted to teachers below the age of 50 in order to reduce noise from retirement decisions, for the sample of all tenured teachers. School year $t/t+1$ is denoted as t. Exit is defined as 1 if a teacher in school year t is not observed in secondary school teaching during at least school year $t+1$ and school year $t+2$, such that exit shares at t are defined as the teachers last observed in school year t, as a share of all teachers at t. The last school year in graph is 2019-2020, as it is the last year for which this horizon of two years is possible. Exit shares are split in different experience groups, indicated in the labels of each line. Source: Bases Relais, 2007-2008/2019-2020.

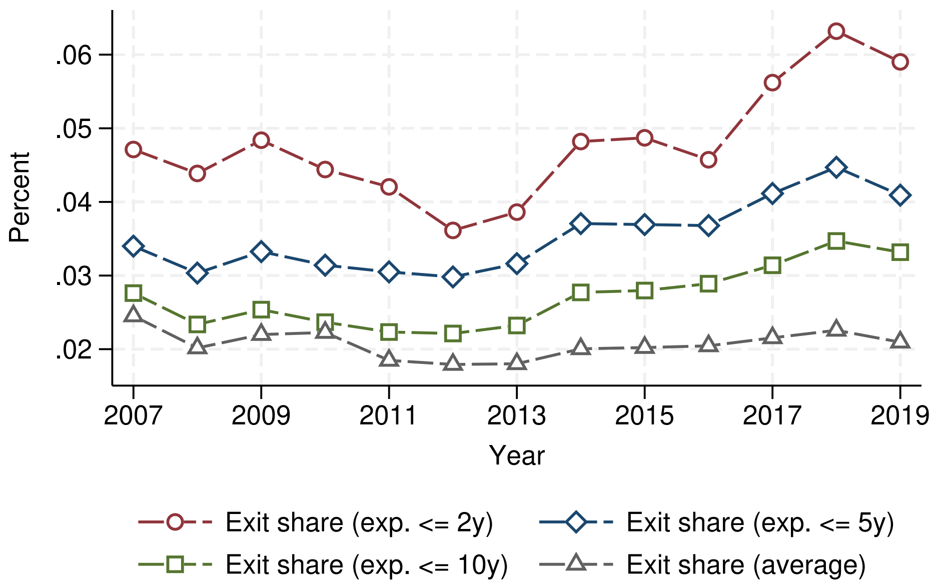


Figure B.2. Occupational transitions of exiting teachers after exit

Note: The figure is constructed based on the 15 most popular occupations towards which teachers transition between 2009 and 2021. The shares of exiters represent national shares, based on the number of exiting teachers being found at a given occupation at, respectively, year $t+1$, $t+3$ or $t+5$ after an exit from teaching at year t. This aggregation is based on either occupation level 2 or level 3 and industry level 2 only to distinguish between the education industry and the rest. The aggregation is not used for creating the outside option salaries panel, and is only useful for providing an aggregate idea of outside options. Source: BTS Postes, 2009-2021.

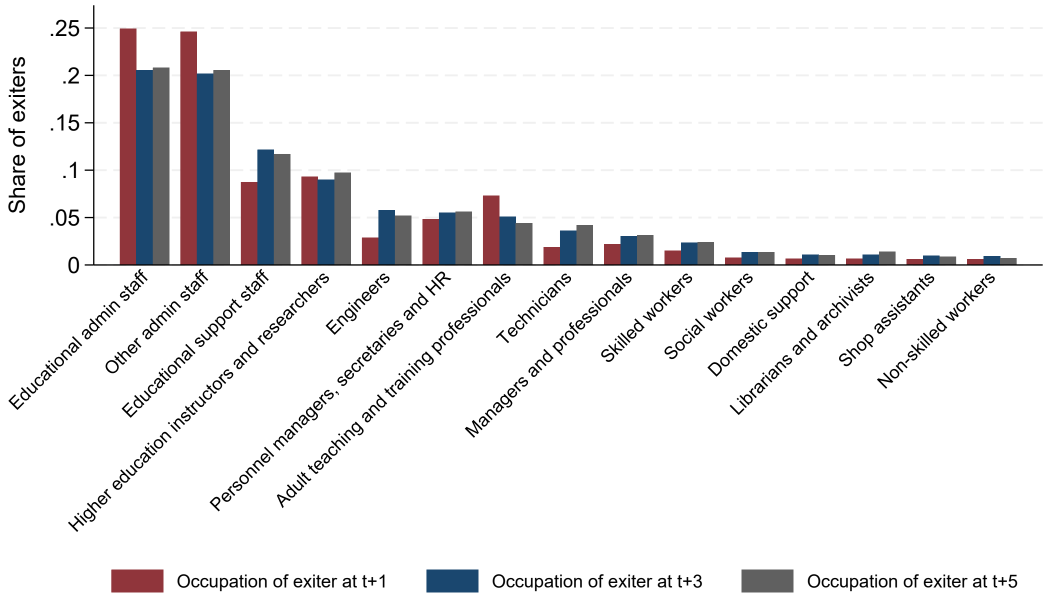


Figure B.3. Correlation between TVA and observable teacher characteristics

Note: The graph shows the estimates of individual regressions of the standardized TVA estimates in either Math or French on standardized observable teacher characteristics. In cases of non-dummy variables (such as experience), I average the variables at the teacher level, such that the dataset used has one observation per teacher. This choice reflects the fact that TVA is not measured within year. The dummy Agrégation represents having a high qualification. Pedagogical score represents the score given to teachers from external inspectors to evaluate a teacher's pedagogical practices during a class. Source: Bases Relais, Scolarité, Sysca, DNB, 2007-2008 to 2021-2022.

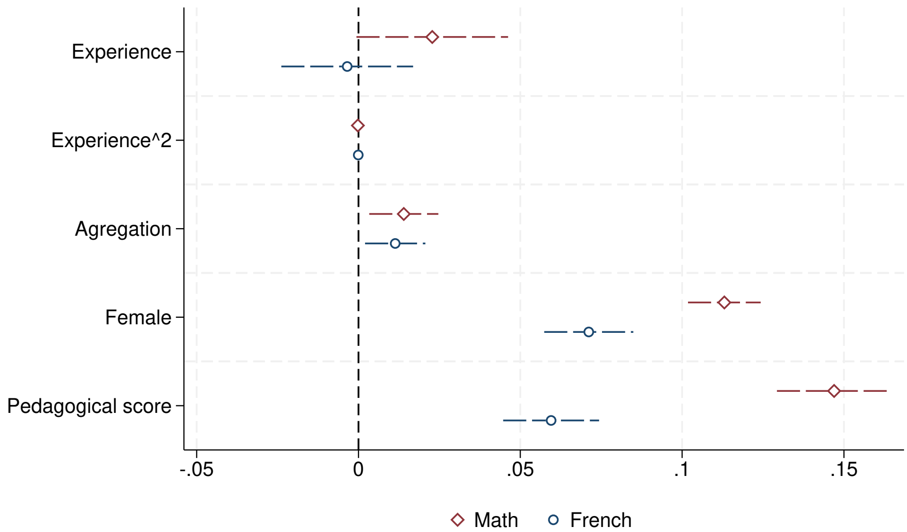


Figure B.4. Encoding of the reform-induced wage bonus treatment variable

Note: The figures show the yearly bonus treatment variable for teachers at REP and REP+ schools implied by the reform, CPI-deflated to 2024 thousands of euros. The bonus treatment variable shifts the bonus by a year in order to align the timing of the bonus with the definition of the exit variable. Source, DEPP, 2009-2010 to 2019-2020.

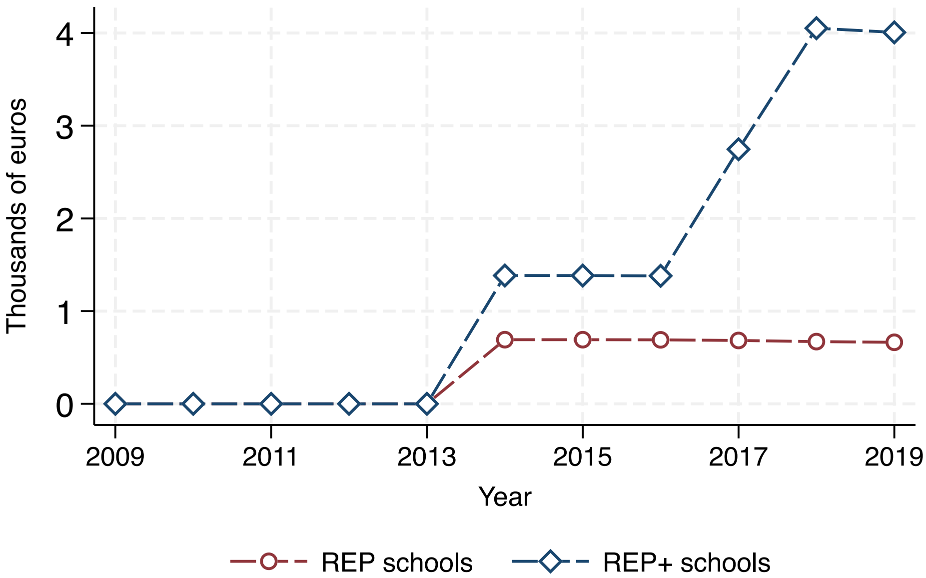
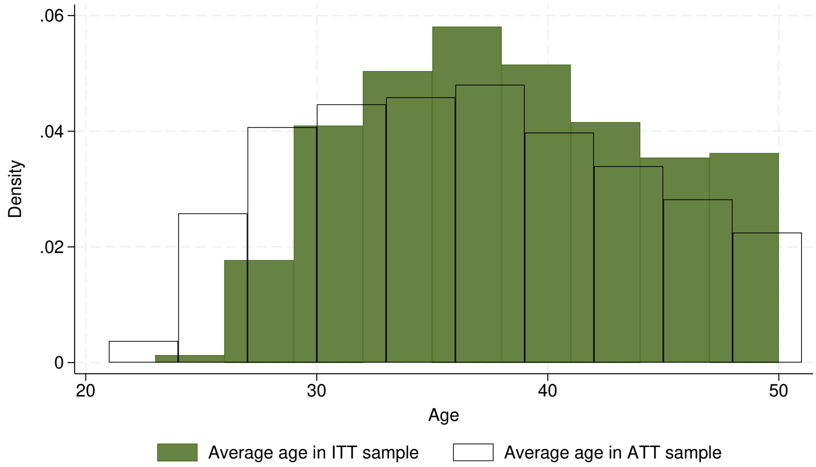
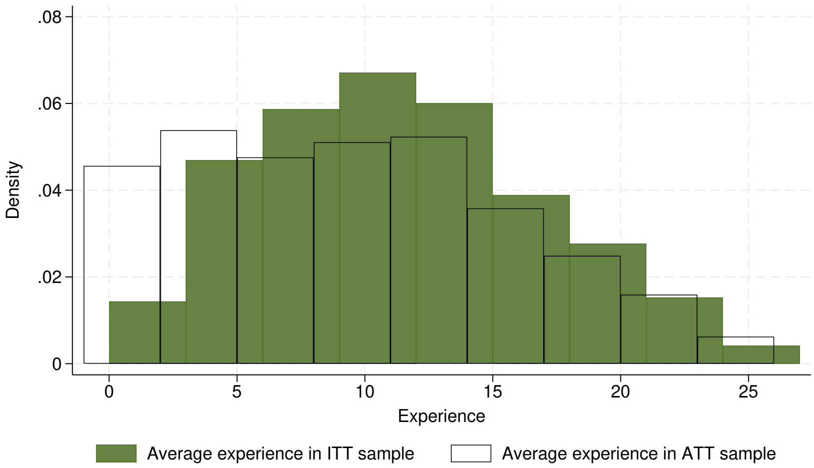


Figure B.5. Post-reform age and experience distributions: ITT vs. ATT samples

Note: The figures present histograms of the (a) age and (b) experience distributions in the post-reform period for the teachers identified as treated or control groups under the ITT approach, versus those identified as treated or control groups under the ATT approach. Specifically, the former are the groups of teachers assigned to REP+ or REP schools in 2013. The latter are the groups of teachers effectively at REP+ or REP schools. Source: Bases Relais, 2014-2015 to 2019-2020.



(a) Age



(b) Experience

Figure B.6. Correlation between wages of exiting teachers and incumbent workers at destination occupation

Note: The figure is presents the binscatter between (i) the mean wage in a given occupation o for non-teachers of a given sex g, age group a, commuting zone z and year t, and (ii) the mean wage that teachers exiting to an occupation o obtain 5 years after exit, within the same sex g, age group a, commuting zone z and year t. The red line is the 45-degree line. Underlying data source: Insee, BTS Postes, 2009-2021.

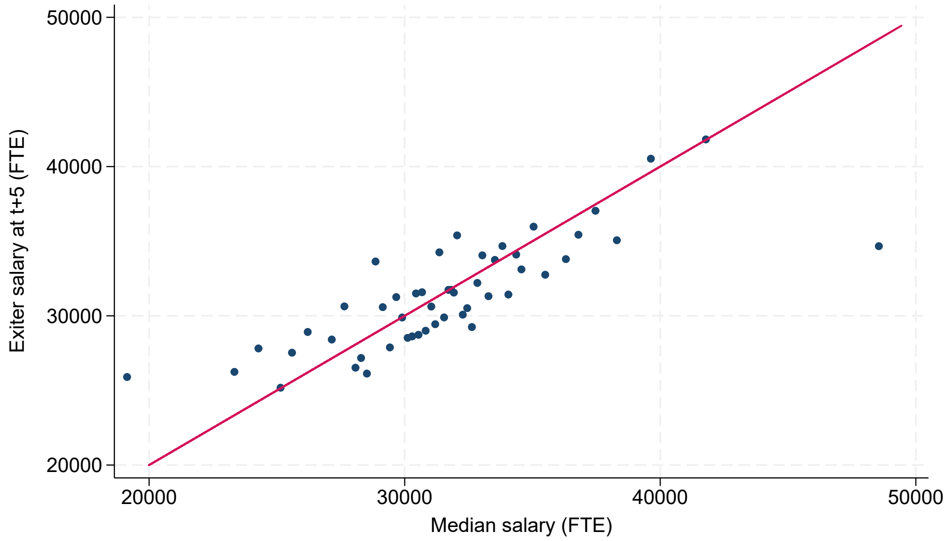


Figure B.7. Average annual wage gap between outside-option and teaching wages

Note: The figure presents the average annual wage gap in my sample per school type (computed in thousands of euros), as

$$WageGap_{gazest} = OutsideOptionWage_{gazt} - TeachingWage_{est}$$

for each age group a, gender g, commuting zone z, echelon e, school type s and year t. $OutsideOptionWage_{gazt}$ is the weighted-average wage of non-teachers in occupations identified as outside options for teachers, using teacher exits. It is defined as

$$OutsideOptionWage_{gazt} = \frac{\overline{S_o} \cdot D_{ogazt}}{\sum_o \overline{S_o} \cdot D_{ogazt}} \cdot OutsideOptionWage_{ogazt}$$

where occupation $o \in \mathcal{O}$, $\overline{S_o}$ is the average share of teachers exiting to o over the period 2009-2021, and D_{ogazt} is the share of workers at o in a given cell $gazt$. $TeachingWage_{est}$ is the teacher wage for a given echelon e, school s and year t. School year $t/t+1$ is denoted as t. School year $t—t+1$ is denoted as t. Both teaching wages and outside option wages are CPI-deflated to 2024 thousands of euros. Source: Bases Relais, BTS-Postes, Teacher wage data (Chancel), 2009/2010-2020/2021.

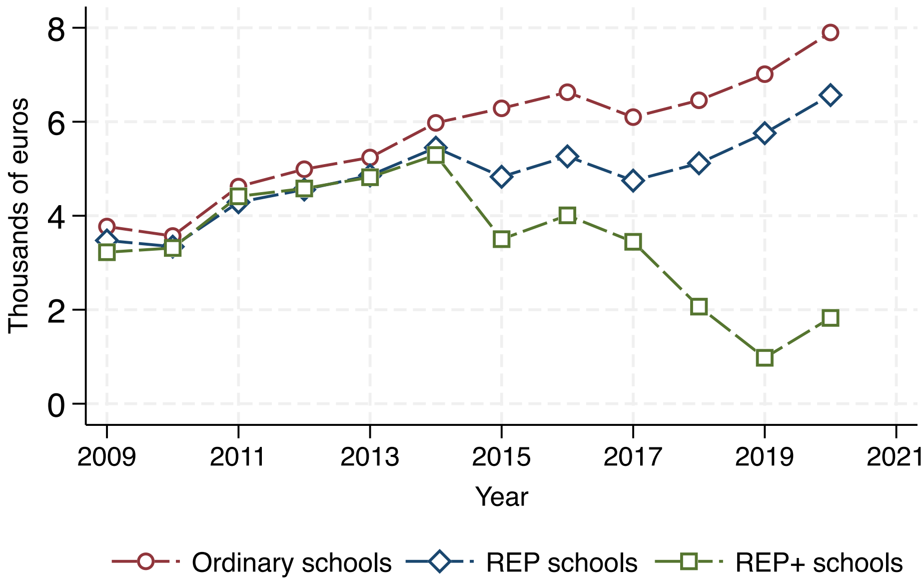


Figure B.8. Average annual teaching wage by school type

Note: The figure presents the teaching wage in my sample per school type (computed in thousands of euros), averaged over a given echelon e, school s and year t. School year $t/t+1$ is denoted as t. School year t—$t+1$ is denoted as t. Wages are CPI-deflated to 2024 thousands of euros. Source: Bases Relais, Teacher wage data (Chancel), 2009/2010-2020/2021.

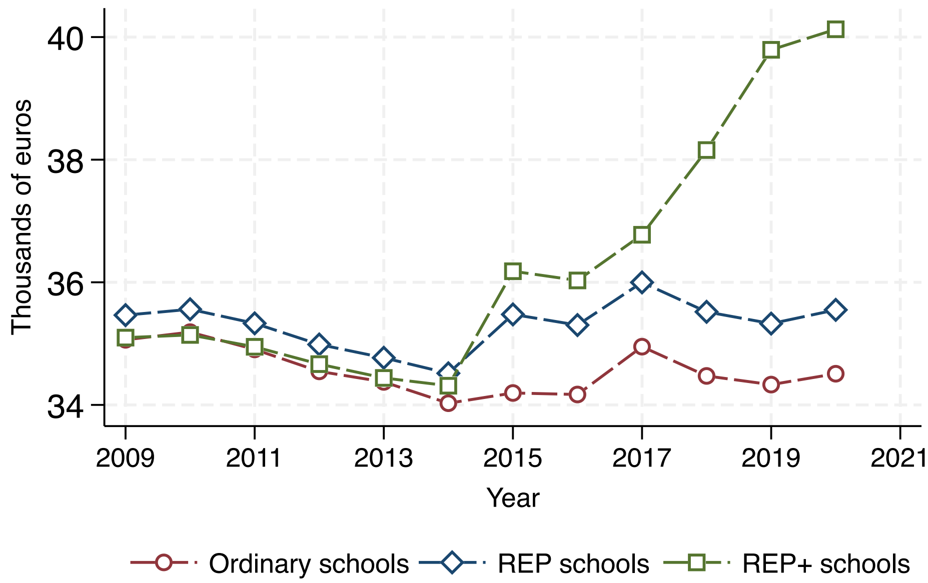
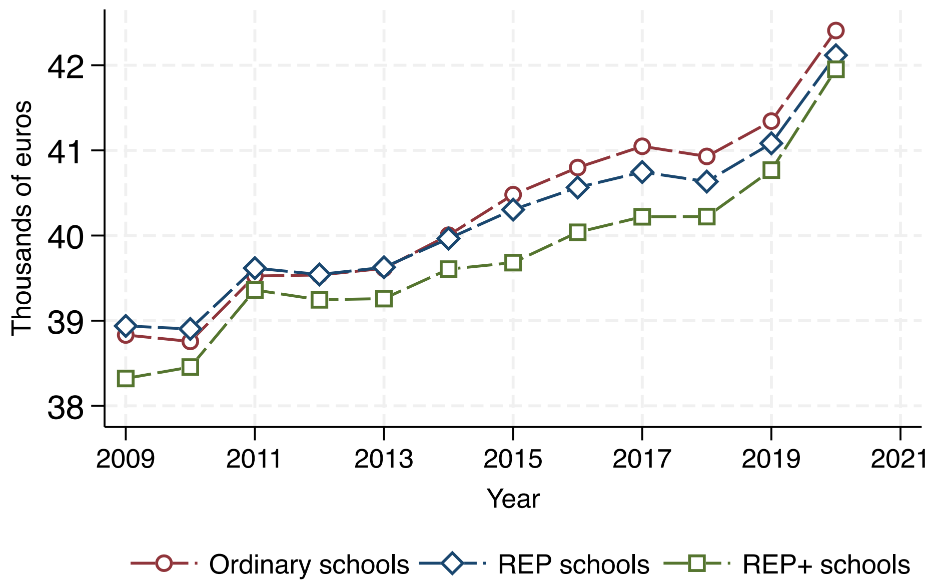


Figure B.9. Average annual outside-option wage by school type

Note: The figure presents the average annual outside option wage in my sample per school type (computed in thousands of euros), as

$$OutsideOptionWage_{gazt} = \frac{\overline{S_o} \cdot D_{ogazt}}{\sum_o \overline{S_o} \cdot D_{ogazt}} \cdot OutsideOptionWage_{ogazt}$$

where occupation $o \in \mathcal{O}$, $\overline{S_o}$ is the average share of teachers exiting to o over the period 2009-2021, and D_{ogazt} is the share of workers at o in a given cell $gazt$. Wages are CPI-deflated to 2024 thousands of euros. Source: Bases Relais, BTS-Postes, 2009/2010-2020/2021.



B.2. Analysis

Figure B.10. Event-study triple DiD estimates (ITT): High-TVA teachers respond more

Note: Figures show the estimated coefficients η_k from the event-study ITT difference-in-differences regression (3), extending the baseline specification to include heterogeneity by teacher value-added (TVA), using the 2013/2014 treatment assignment.

The sample includes all Math and French teachers employed at REP or REP+ schools in 2013/2014 with measured TVA. Formally, for teachers j at school s and commuting zone z:

$$exit_{jstm} = \beta\, Post_t \times \mathbb{1}(type_j = \text{REP+}) \times HighTVA_{jmz} + \cdots + \delta \mathbf{X}_j + \theta_{s \times TVA} + \lambda_{zt \times TVA} + \varepsilon_{jstm}.$$

$exit_{jstm}$ equals 1 in the last year a teacher appears in the system. $\mathbb{1}(t = k)$ indicates school year k/k+1. $\mathbb{1}(type_j = \text{REP+})$ equals 1 for teachers in schools later classified as REP+, and 0 for REP. $HighTVA_{jmz}$ equals 1 for teachers with TVA above the median within commuting-zone–subject cells in 2013/2014, and 0 otherwise. The figures report five specifications: (1) the baseline specification (school-by-high TVA and commuting-zone-by-high-TVA fixed effects); (2) adding teacher controls (gender, qualification, experience, age, subject taught), interacted with the high-TVA dummy; (3) adding subject-by-high-TVA fixed effects; (4) adding school-by-year fixed effects. Pre-trends are normalized to zero, as the ITT sample includes teachers present in 2013/2014. 95% confidence intervals are shown. Standard errors are clustered by teacher and school-by-year-by-TVA. Controls, fixed-effects, and clusters are measured in 2013/2014. Estimates are relative to 2013/2014. TVA is defined relative to the median TVA within the commuting zone in 2013. See Section 4 for details. Underlying data: Bases Relais, Scolarité, Sysca, DNB, 2013/2014–2019/2020.

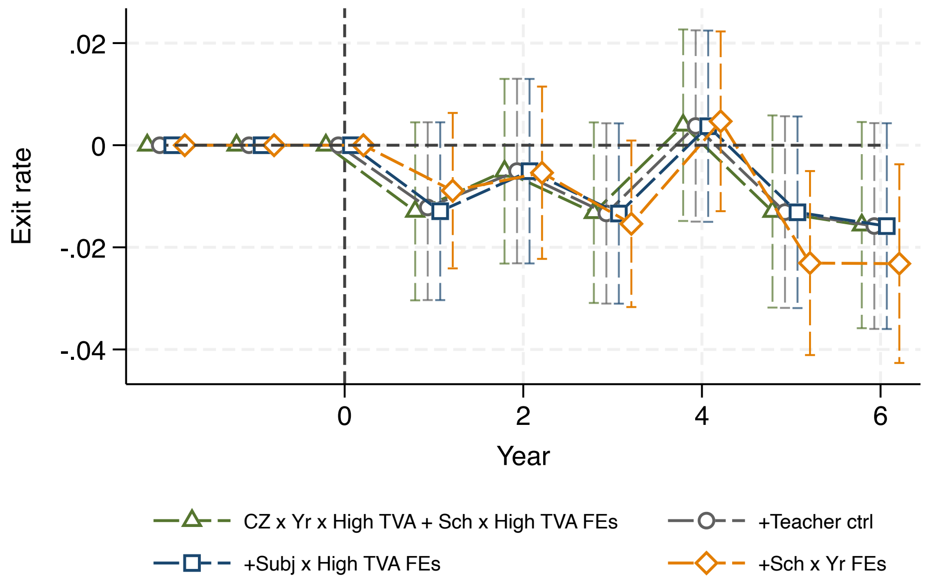
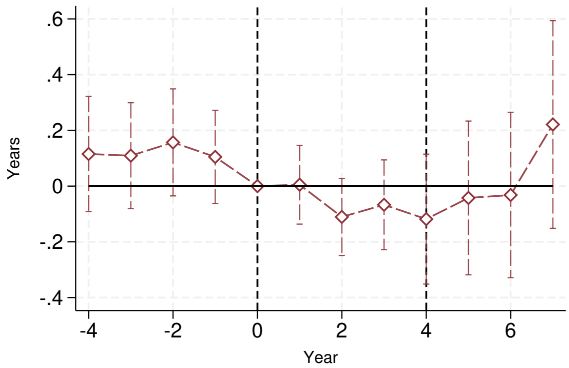


Figure B.11. Event-study DiD (ATT): compositional changes at REP+ schools (vs. REP)

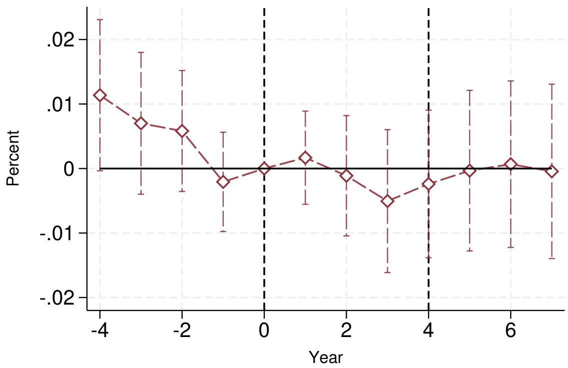
Note: Table reports estimates from the ATT difference-in-differences regression (3) and (5), using the contemporaneous treatment assignment. The sample includes all teachers employed at REP or REP+ schools in a given year t. Formally, for teachers j at school s and commuting zone z:

$$\overline{Y}_{st} = \sum_{k=2007}^{2020} \eta_k \mathbb{1}(t = k) \times \mathbb{1}(type_s = REP+) + \theta_s + \lambda_{zt} + \varepsilon_{st}$$

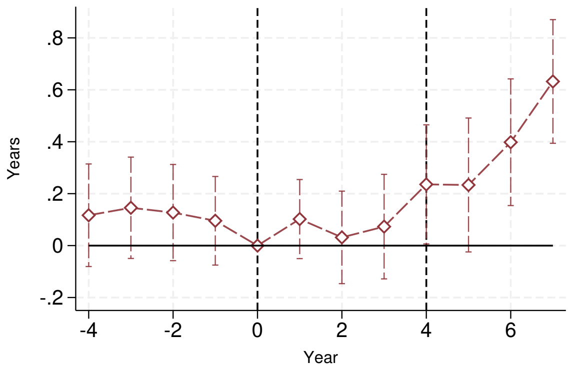
$\overline{Y}_{st}$ variable of each regression is specified below each graph. $\mathbb{1}(t = k)$ indicates school year k/k+1. $\mathbb{1}(type_s =$ REP+) equals 1 for schools later classified as REP+, and 0 for REP. Treated teachers are therefore teachers who are in school for which $\mathbb{1}(type_s =$ REP+) = 1 at a given year t. Standard errors (in parentheses) clustered by teacher and school-year. Fixed-effects, and clusters are measured in year t. 95% confidence intervals are shown. All estimates are relative to 2013/2014. See Section 4 for details. Source: Bases Relais, 2009/2010–2020/2021.



(a) Average pedagogical score



(b) Share female



(c) Average experience

Figure B.12. Event-study DiD (ATT): Larger drop of exit rates for REP+ teachers

Note: Figure shows the estimated coefficients η_k from the event-study ATT difference-in-differences regression (3), using contemporaneous treatment assignment. The sample includes all teachers employed at REP or REP+ schools in year t. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \sum_{k=2013}^{2019} \eta_k \, \mathbb{1}(t = k) \times \mathbb{1}(type_s = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $\mathbb{1}(t = k)$ indicates school year $k/k{+}1$. $\mathbb{1}(type_s = \text{REP+})$ equals 1 for schools later classified as REP+, and 0 for REP. The figure reports five specifications: (1) no controls; (2) adding school and year fixed effects; (3) adding teacher controls (gender, qualification, experience, age, subject taught); (4) replacing year effects with académie-by-year fixed effects; and (5) replacing the latter with commuting-zone-by-year fixed effects. 95% confidence intervals are shown. Standard errors are clustered by teacher and school-year. Controls, fixed-effects, and clusters are measured in year t. Estimates are relative to 2013/2014. See Section 4 for details. Source: Bases Relais, 2009/2010–2017/2018.

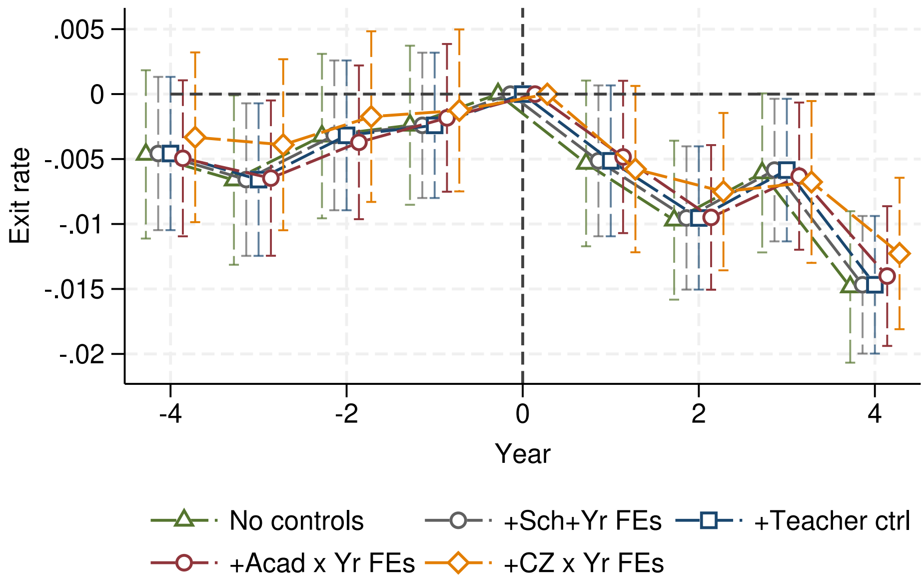
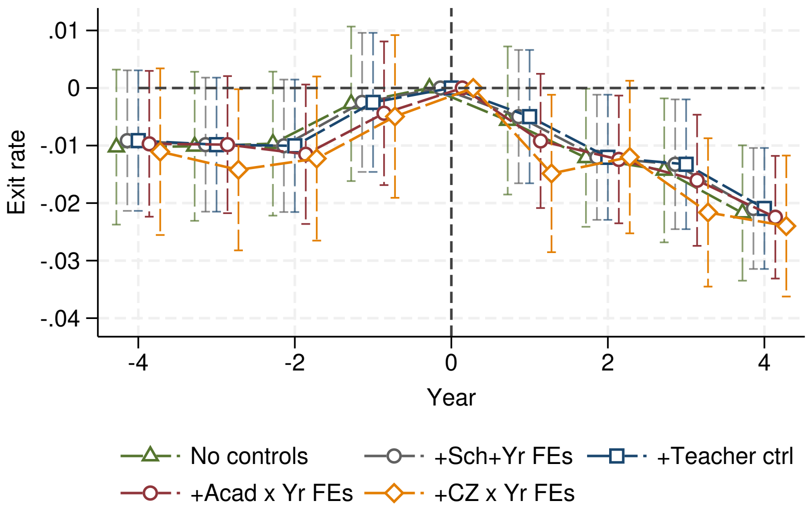


Figure B.13. Event-study DiD by TVA (ATT): High-TVA teachers respond more

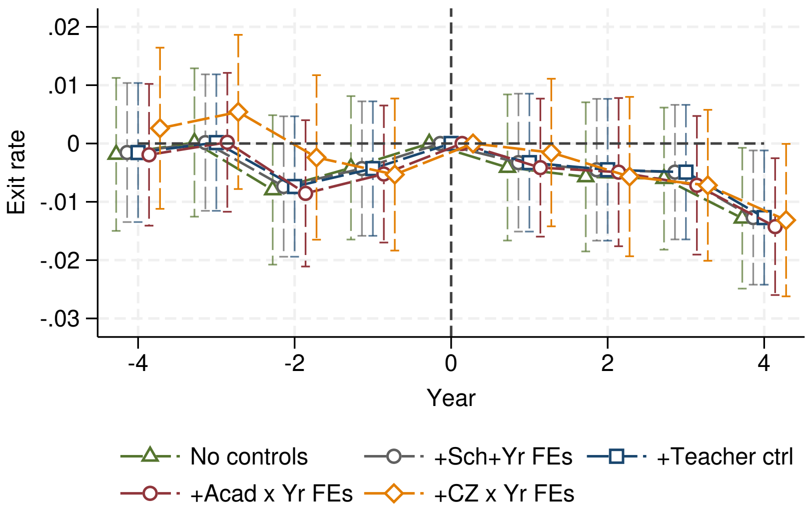
Note: Figure shows the estimated coefficients η_k from the event-study ATT difference-in-differences regression (3), using contemporaneous treatment assignment. The sample includes Math and French teachers employed at REP or REP+ schools in a given year t for whom a TVA measure is available. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \sum_{k=2013}^{2019} \eta_k \, \mathbb{1}(t = k) \times \mathbb{1}(type_s = \text{REP+}) + \gamma \mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $\mathbb{1}(t = k)$ indicates school year $k/k{+}1$. $\mathbb{1}(type_s = \text{REP+})$ equals 1 for schools later classified as REP+, and 0 for REP. The figure reports five specifications: (1) no controls; (2) adding school and year fixed effects; (3) adding teacher controls (gender, qualification, experience, age, subject taught); (4) replacing year effects with académie-by-year fixed effects; and (5) replacing the latter with commuting-zone-by-year fixed effects. 95% confidence intervals are shown. Standard errors are clustered by teacher and school-year. Panel (a) shows results for teachers with above-median value-added (High TVA), and Panel (b) for those below the median (Low TVA). Controls, fixed-effects, and clusters are measured in year t. Estimates are relative to 2013/2014. See Section 4 for details. Source: Bases Relais, Scolarité, Sysca, DNB, 2009/2010–2017/2018.



(c) High TVA



(d) Low TVA

C. Technical appendix

C.1. Merge of student and teacher data

I construct my student database relying on three sources—Scolarité, Sysca and DNB. Scolarité and Sysca contain very similar personal information on students (e.g. birth date, gender, parental occupation, need-based scholarship, nationality), but come with a caveat. Scolarité does not have student identifiers but has reliable class identifiers, whereas Sysca has student identifiers but does not have class identifiers prior to 2014. While I do not use student panel data and therefore do not need to be able to track students across years, I require student identifiers to merge the data to the database DNB, which contains students' scores in the Math and French DNB exams. I use observable student characteristics to perform a fuzzy match between the two datasets and obtain a panel of students covering about 95% of all students, which I merge with exam scores. The merge between student and teacher data leads to the loss of 13.1% of 9th-grade Math and French teachers, due to missing or wrongly encoded classroom identifiers on either the teacher- or the student-side of the data. These issues are more pronounced at REP and REP+ schools: respectively, 17.9% and 22.5% 9th-grade Math teachers and 18.7% and 22.4% 9th-grade French teachers are lost in the merge.

Finally, the identification requirements of the method for TVA estimation (specifically, the requirement of belonging to the closed network of teachers, see Section 3 for more details), leads to a further reduction of the sample of Math and French teachers with a TVA measure, as more than 98% of the sample of teachers merged to student test scores belongs to the complete network. Overall, the panel of middle school tenured teachers with a TVA measure comprises 67.9% of Math teachers and 68.8% of French teachers (Table A.2). These numbers are lower at REP+ (57.4% of Math teachers and 58.3% of French teachers) and REP (62.7% and 62.6% of Math and French teachers, respectively). This issue arises because the merge between teacher and student data performs much less well in REP and REP+ schools than in ordinary schools.

C.2. Creating an exhaustive panel of teachers from tax returns data

To extend the BTS-Postes into a longitudinal dataset covering the full 2009–2021 period, I apply the methodology proposed by Godechot et al. (2023).

The database has a built-in two-year panel structure: for each yearly file, information is reported not only for the current year but also retrospectively for the previous year. This design enables one to follow individuals over short horizons, but does not directly produce a long panel. Having a longer panel than two years is particularly useful in this context, since teachers who change careers may pass through temporary or transitional jobs before settling into more permanent employment. Focusing only on the first post-teaching job could, therefore, potentially misrepresent the types of occupations teachers ultimately move into.

To extend the BTS-Postes into a longitudinal dataset covering the full 2009–2021 period, I apply the methodology proposed by Godechot et al. (2023). Their approach exploits the overlap in variables reported across consecutive yearly files. Specifically, observations for a given individual in year t of file $y-1$ can be matched to the corresponding records in year $t-1$ of file y using a set of common identifiers, including establishment ID, demographic information (gender and age), and job characteristics (e.g., hours worked, job duration, start and end dates, municipality of residence and workplace, and salary). This matching procedure achieves a unique link for the vast majority of individuals—around 98% over my sample period, while unmatched cases typically reflect either data modifications (which reduced the number of matches to 92% for 2016 and 2017), or employment interruptions exceeding one year (as the dataset does not contain unemployment spells). The latter are kept in the data but recorded as distinct individuals (who then appear under multiple identifiers).

Using 4-digit occupation codes and NAF industry classifications, I identify secondary school teachers in general education, and I narrow the sample of interest to public school teachers, using the legal status of each employer. I use the national registry of businesses and establishments (Sirene) in order to obtain the names of secondary education establishments and identify those marked as middle schools (collèges), from the set of secondary schools (which include high schools).

To define the set of outside option occupations to which middle school teachers transition, I restrict the sample of individuals to those identified for at least a year as teachers in public middle schools between 2009 and 2021, who are below 50 years old, who work full-time, and for whom teaching is a main job, based on the number of hours spent in each occupation at a given year. I identify as "exits" observations of individuals who are public middle school teachers in year t and who switch out of teaching in year $t+k$ (i.e. are not observed as either pre-primary, primary or secondary school teacher, and are not observed as a headmaster of a school, which can be considered as promotion rather than exit). I consider different $k = \{1, 3, 5\}$ in order to show how the target teachers switch across occupations (defined at an occupation-level 4) over time, allowing for the possibility that the job they take at $t+1$ is a temporary job.[62] The extended panel data allows me to confirm that transitions to an occupation generally tend to persist, with little support for the idea of observing transitory jobs.

Teachers transition into a wide array of sectors and skill levels (see Figure B.2). The largest shares move into administrative jobs, either education-related (≈20% by $t+3$ post-exit) or not (≈20%). Within administration, they take either public-sector positions (executive or management roles in government, financial services staff, or

[62] The method resembles the identification of outside options for teachers in Tsao (2025).

technical work for the State) or private-sector roles (general or sales administration, litigation, or personnel management). Within the category educational support staff, many become senior education counselors, supervising students without teaching duties in public or private institutions. About 10% move into higher education teaching or research, 5% into engineering, 5% into lower-level personnel management/secretarial/HR jobs, and 5% into adult training, typically in corporate settings. The remaining exits are dispersed across technician roles, managerial and other professional jobs, skilled labor, social work, domestic support, and other occupations.

The analysis reveals that a large share of exits (≈60%) are into other public-sector jobs. This pattern reflects the civil-servant status of French teachers, which facilitates mobility within the public sector subject to openings. Many transitions are therefore to occupations that, like teaching, are governed by rigid pay scales.

C.3. Bayesian shrinkage of TVA estimates

An Empirical Bayes shrinkage method outlined in Tartova (2023) is then needed to deal with the fact that teachers are often observed in few classrooms, making estimates likely to be noisy. The shrinkage method is inspired by the work of Kane and Staiger (2008) and Chetty et al. (2014a). Specifically, to compute the TVA estimates for Math teachers, I first take the residual from the regression 2 that purges the effect of the French teacher effects and observed covariates from ΔA_i^{*MF}, and denote it $\widehat{A}_i^{MF}$:

$$\widehat{A}_i^{MF} \equiv \widehat{\mu}_m + \widehat{\varepsilon}_i$$

To then get from these student-level estimates to TVA, first create teacher-year averages of $\widehat{A}_i^{MF}$ which are the minimum variance unbiased estimates of μ_m for each teacher m:

$$\widehat{A}_m = \sum_t w_{mt}\widehat{A}_{mt} \text{ where } w_{mt} = \frac{h_{mt}}{\sum_t h_{mt}} \text{ and } h_{mt} = \frac{1}{Var(\widehat{A}_{mt}|\mu_m)}$$

where $Var(\widehat{A}_{mt}|\mu_m)$ is the conditional variance of $\widehat{A}_{mt}$.

To estimate the variance of the teacher-specific fixed effect, σ_μ^2, Tartova (2023) first calculates the covariance of a teacher's residualized performance across two randomly chosen years, applying weights based on the number of students taught in each year:

$$Cov(\widehat{A}_{mt}, \widehat{A}_{mt'})$$

To isolate and subtract any residual variance attributable to the link teacher, the paper then computes the covariance of the link teacher's residual across the same two years:

$$\sigma_f^2 = Cov(\widehat{A}_{ft}, \widehat{A}_{ft'})$$

The variance of the fixed teacher effect is then identified as the difference between the two covariances:

$$\sigma_\mu^2 = Cov(\widehat{A}_{mt}, \widehat{A}_{mt'}) - \sigma_f^2$$

Finally, using $\widehat{A}_m$ and σ_μ^2, I construct an empirical Bayes estimator for each teacher's TVA by multiplying this weighted average residual by an estimate of its reliability (the signal-to-noise ratio):

$$\widehat{TVA}_m = \widehat{A}_m \left(\frac{\sigma_\mu^2}{\sigma_\mu^2 + 1/\sum_t h_{mt}} \right)$$

where σ_μ^2 is the variance of the Math teacher effect. The estimates $\widehat{TVA}_m$ are forecast unbiased predictors of the true Math teacher effects. The same method can be performed to estimate the TVA of French teachers.

C.4. De-trended difference-in-difference estimation

The ATT event study difference-in-differences regressions depict a strong decrease in exit rates for REP+ teachers relative to REP teachers following the introduction of the wage increase, after a clear structural break of the previously increasing relative average exit rate at REP+ schools at the time of the reform (Figures B.12 and B.13). Specifically, there exists, if anything, an initial persistent linear increase in the relative exit rates at REP+ prior to the reform, which is interrupted in the year the reform was announced, followed by a significant decrease in the relative exit rate at REP+ schools after the introduction of the reform.

The structural break in the relative probability of exit around the time of the reform indicates a causal impact of the policy. To account for increasing differential pre-trend, I cut the pre-period to 2013-2014 under the assumption that the relative evolution of exits would have stopped in 2013-2014 had it not been for the reform. This assumption is conservative if one believes that school conditions at REP+ schools were likely on a deteriorating trend, prompting an increasing relative exit rate.

An alternative assumption that the relative pre-trends observed in the data would have continued in the post-period in the absence of the reform prompts the use of a de-trended event-study difference-in-differences strategy, following Bhuller, Havnes, Leuven, and Mogstad (2013), Goodman-Bacon (2018, 2021), and Dobkin, Finkelstein, Kluender, and Notowidigdo (2018). Specifically, I estimate group-specific linear trends using only pre-2013 observations and subtract these from post-2013 outcomes. This approach relaxes the conventional parallel trends assumption by allowing REP+ and REP teachers to differ along a linear pre-trend, and interprets post-2013 coefficients as deviations from this extrapolated trajectory. Concretely, I first estimate a model which includes a linear differential trend in exit probabilities for REP+ relative to REP schools in the pre-reform sample period (2009–2013):

$$exit_{jst} = \alpha + \lambda\,\tilde{t} + \zeta\left(\tilde{t} \times \mathbb{1}\{type_s = REP+\}\right) + \mathbf{X}'_{jt}\gamma + \theta_s + \lambda_{zt} + \varepsilon_{jst}, \qquad t \in [2007, 2013] \tag{C.1}$$

in order to predict counterfactual exit rates in the post-period had this differential linear pre-trend continued (denoted as $\widehat{exit}^{T}_{jst}$), that is then used to define the de-trended exit variable as $\widehat{exit}^{DT}_{jst} \equiv exit_{jst} - \widehat{exit}^{T}_{jst}$. Using this definition, I run:

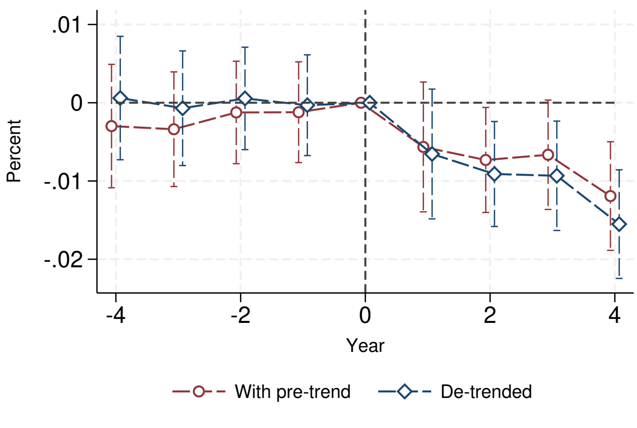
$$\widehat{exit}^{DT}_{jst} = \sum_{2009}^{2017} \eta^{DT}_{k}\mathbb{1}(t = k) \times \mathbb{1}(type_s = REP+) + \mathbf{X}_{jt}\gamma + \theta_s + \lambda_{zt} + \varepsilon_{jst} \tag{C.2}$$

Figure C.14. Differential exit probability for REP+ compared to REP teachers (de-trended DID)

Note: Figure shows the estimated coefficients η_k from the event-study ATT difference-in-differences regression (3), using contemporaneous treatment assignment. The sample includes all teachers employed at REP or REP+ schools in year t. Formally, for teachers j at school s and commuting zone z:

$$exit_{jst} = \sum_{k=2013}^{2019} \eta_k\,\mathbb{1}(t = k) \times \mathbb{1}(type_s = \text{REP+}) + \gamma\mathbf{X}_j + \theta_s + \lambda_{zt} + \varepsilon_{jst}$$

$exit_{jst}$ equals 1 in the last year a teacher appears in the system. $\mathbb{1}(t = k)$ indicates school year k/k+1. $\mathbb{1}(type_s = \text{REP+})$ equals 1 for schools later classified as REP+, and 0 for REP. The blue line shows the corresponding de-trended coefficients from regression (C.2). Teacher controls include gender, qualification, experience, age, subject taught. 95% confidence intervals are shown. Standard errors are clustered by teacher and school-year. Controls, fixed-effects, and clusters are measured in year t. Estimates are relative to 2013/2014. See Section 4 for details. Source: Bases Relais, 2009/2010–2017/2018.



(a) All

Counterfactual exit rate The counterfactual exit rate in the de-trended difference-in-differences is defined as the average exit rate that REP+ teachers would have had over the period 2014-2017, had they not experienced a differential increase in wages compared to REP teachers, under the assumption that the differential trend in exit rates between REP and REP+ teachers would have continued on the same linear trend. Specifically, the counterfactual exit rate (hereafter $\overline{exit}^{CF2}_{REP+}$) under this assumption is estimated in three steps. First, I predict the counterfactual exit rate ($\widehat{exit}^{T}_{jst}$) had the differential pre-trend (obtained by running regression C.1 without controls or fixed effects) continued. Second, I recover the counterfactual post-reform for REP+ teachers had there been no reform, by running $\widehat{exit}^{T}_{jst} = \alpha + \beta_1 Post_t \times REP +_s + \beta_2 Post_t + \beta_3 REP +_s + \varepsilon_{jst}$ over the full sample period $t \in [2007, 2017]$, as $\overline{exit}^{P}_{REP+} \equiv \alpha + \beta_1 + \beta_2 + \beta_3$. Third, I run regression C.2 without controls and fixed effects in order to obtain the REP+ post level in exit rate one would expect after de-trending, if REP+ and REP evolved the same way post-reform ($\overline{exit}^{P,DT}_{REP+} \equiv \alpha' + \beta'_2 + \beta'_3$), and compute the counterfactual REP+ exit rate as $\overline{exit}^{CF2}_{REP+} \equiv \overline{exit}^{P}_{REP+} + \overline{exit}^{P,DT}_{REP+}$.

C.5. Proof of sign of effect of changes to the wage gap for teaching quality

Given the formulas for the semi-elasticities for high- and low-TVA types, I show that raising teacher pay increases average teacher quality if high-type teachers are more responsive to wages than low-type teachers (see Appendix C.5 for proof).

Average teacher quality can be written as the weighted average of the two types in the workforce,

$$Q = \frac{\nu_H T_H + \nu_L T_L}{T_H + T_L},$$

where ν_c is the average TVA of teachers of type c, and T_c is the number of employed teachers of that type. This can be expressed more simply as

$$Q = \nu_L + (\nu_H - \nu_L) s_H,$$

where s_H is the share of high-type teachers among those who remain in teaching.

Because the number of employed teachers of each type equals the number of potential teachers of that type times their probability of staying, $T_c = m_c P_0^c$, the share of high types can be written as

$$s_c = \frac{m_c P_0^c}{m_H P_0^H + m_L P_0^L},$$

where P_0^c is the probability of staying in teaching (one minus the exit probability P_1^c).

Define the semi-elasticity of exit for type c as:

$$\eta_c \equiv \frac{1}{P_1^c}\frac{\partial P_1^c}{\partial \Delta w^c} = \frac{1}{\sigma_e^c}(1 - P_1^c) = \frac{P_0^c}{\sigma_e^c}$$

Differentiating average quality Q with respect to the wage gap yields

$$\frac{dQ}{d\Delta w} = (\nu_H - \nu_L)\frac{ds_H}{d\Delta w} = (\nu_H - \nu_L) s_H P_1^H s^L P_1^L (\eta_L - \eta_H)$$

As high-TVA teachers have by definition a higher average TVA than low-TVA teachers, ($\nu_H > \nu_L$) the sign of the quality change depends on the sign of the relative exit responsiveness of the two types to the wage gap, $\eta_L - \eta_H$. Because an increase in the teaching wage reduces the wage gap ($d\Delta w = -dw_0$), the sign of the quality change following in the teacher wage depends on the sign of the $\eta_H - \eta_L$.

Hence, raising teacher pay increases average teacher quality if high-type teachers are more responsive to wages than low-type teachers.

C.6. Explaining the teaching hours leg of the reform

In this section, I explain in more detail how the part of the reform which aimed to reduce the number of mandatory teaching hours and redistribute them towards time spent with students and parents operates and how it translated into more working hours and therefore overtime pay (*Heure Supplémentaire Annuelle*, or HSA). I do this by providing an example for a Capes-certified teacher with a statutory 18-hour teaching obligation who, prior to the reform, teaches 18.7, on average. This is in line with the findings in Table A.22, which show that the average REP+ Capes teacher taught 18.7 prior to the reform.

Prior to the reform, a teacher who taught 18.7 hours per week received annual overtime pay equivalent to 0.7 HSA. In addition to teaching hours, the 17 additional hours of work of the 35-hour working week were spent in other activities than teaching. It follows that the average REP+ Capes teacher was working on 35.7 hours per week.

After the reform, each hour of teaching in a REP+ school counted as 1.1 hours for the calculation of service. Thus, if the teacher continued to do the same amount of overtime, his hours would be counted as

$$18/1.1 + 0.7 = 17.06 \text{ hours.}$$

The 1.65 hours were to be redistributed to tasks such as liaison with families, which was one of the reform's goals. Such a redistribution would have entailed no additional working hours compared to before the reform, with a total of 35.7 hours per week worked.

If, in practice, the teacher's teaching load was reduced by only 0.76 hours instead of the full 1.65 (see Table A.22), if the teacher does not take any additional overtime work, they would in reality be teaching:

$$18/1.1 + (1.65 - 0.76) = 17.26 \text{ hours.}$$

which would entail 17.26+1.65+17 = 35.91 hours of work per week. It follows that the teacher is teaching more than their pre-reform preferred optimum of 35.7 hours.

If incentives to work overtime remained unchanged at the time of the reform, these additional hours can be regarded as involuntary, since teachers who preferred to work longer hours could already do so before the reform by voluntarily taking on overtime. Hence, the post-reform wage increase due to this overtime pay could not be viewed as a pure or unconditional bonus—it required teachers to work beyond their pre-reform preferred workload. This interpretation implies that the estimated exit elasticity with respect to the wage increase may represent a lower bound (in absolute terms) of the true elasticity, as some teachers may have been pushed out of REP+ schools due to the increased number of hours they had to work.

For the number of teaching hours, his teaching hours would be counted as:

$$17.26 \times 1.1 = 189.8 \text{ hours},$$

which entails the teacher would be earning 1 HSA after the reform—their work would be counted as 35.98 hours in total, for the 35.91 hours of actual work they do.

If this teacher was to work the same amount of hours pre-reform, therefore teaching for 18.91 hours, they would have got 0.91 HSA in overtime pay.

It follows that the reform introduced a small monetary incentive to work more, equivalent to 35.98 (hours remunerated) - 35.91 (hours actually worked) = 0.08 HSA. Between 2014 and 2019, each first HSA was paid around 1,339 (in 2024 euros), with each subsequent reduced to 1,116. For the average teacher, this would entail between

$$0.08 \times 1,339 \approx 107.1 \text{ euros more annually.}$$

This positive difference suggests that the reform slightly increased monetary incentives to take on additional teaching hours, even in the absence of institutional constraints requiring teachers to do so. Specifically, if a teacher could decide to work more in response to the reform, every overtime hour they would would be remunerated slightly more highly than it would have been pre-reform. In the case of someone who works 35.91 hours in total, they would get 107.1 euros more annually than they would have for the same 0.91 number of hours of overtime work prior to the reform.

If incentives to work overtime changed at the time of the reform, one cannot rule out teachers may have decided to work more hours voluntarily. Detailed computation of the overtime pay rule points that the reform led to a small increase in monetary incentives for the average Capes teacher, of approximately 107.1 euros annually, in 2024 euros. Assuming that teachers voluntarily decided to work more to obtain these 107.1 euros, this amount can be considered an additional effective bonus for REP+ teachers. Under this assumption, the baseline LATE exit elasticity to the wage of -8.1 computed using the effective bonus excluding this extra incentive would instead be -6.2.[63] This is a lower bound of the elasticity in absolute terms, under the assumption the average teacher decides to work more rather than being obligated to.

C.7. Computing the wage gap

I match the outside-option wage defined in equation 1, $OutsideOptionWage_{gazt}$, and the teaching wage that varies by a teacher's position on the wage scale e, school type s (REP/REP+/ordinary), and year t, to my teacher panel data, in order to obtain teacher-specific wage gaps that vary by e, sex g, age group a, commuting zone z and year t:

$$WageGap_{gazest} = OutsideOptionWage_{gazt} - TeachingWage_{est} \tag{C.3}$$

Figure B.7 depicts the estimated average wage gap per type of school over time, underlining the increasing trend of the wage gap prior to the reform and the importance of the treatment introduced by it (see Figures B.8 and B.9 below for the trend in each of the two components of the wage gap by school type). The figure shows that during the school year 2013-2014, the average wage gap was roughly 4,800 euros (CPI-deflated to 2024 euros). It continued to increase thereafter for ordinary-school teachers, despite a small increase in all novice teacher wages during the 2017-2018 school year, reaching roughly 8,000 euros by 2020-2021. For REP-schools teachers, the wage gap also continued to increase, reaching 6,500 euros by 2020-2021. For REP+-school teachers, the average wage gap dropped to 1,800 euros by 2020-2021, representing a sizable drop in the wage gap for treated teachers.

[63] To get to this back-of-the-envelope computation, note that the first stage LATE coefficient in Table 1 would change to 1.185+0.1071 = 1.2921, leading to a second-stage η coefficient of -0.00592/1.2921 = -0.00458, which is then substituted in Equation 7.