The effect of high dismissal protection on bureaucratic turnover and productivity*

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Abstract

This paper studies the impact of high dismissal protection on bureaucratic turnover and productivity in the context of public school teachers in Chile. We take advantage of a law that required education administrators to grant a permanent contract to temporary teachers with a minimum seniority and implement a difference-in-differences strategy comparing eligible and ineligible teachers. We find that high dismissal protection reduces turnover by 25 percent in the first two years. The reduction is only statistically significant among teachers at the bottom and top of the distribution of baseline performance. We then examine the impact on teacher productivity and find a significant decline in the learning of students taught by teachers with low baseline performance. These findings are consistent with the hypothesis that high dismissal protection can be a double-edged sword. It can help to retain high-performing employees, but at the cost of making it more difficult to separate and motivate low-performing employees.

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1 Introduction

Job stability is a key feature of most public sector jobs. Public officials with permanent contracts typically have high dismissal protection and can only be fired under exceptional circumstances. One motivation for this arrangement is to avoid the use of public sector employment for political patronage, which can be detrimental to the quality of government and public finances, and to the well-functioning of the democratic system (OECD, 2019; CAF, 2019). Furthermore, protection against dismissal can improve the quality of public service delivery by reducing the switching costs associated with excessive turnover (Akhtari et al., 2022) and improving the composition of public sector employees. For example, job stability could help to attract and retain high performing employees with good outside options.[1] However, strict dismissal protection comes at a cost, as it eliminates employers' ability to fire workers with poor performance and weakens employees' incentives to exert effort on the job—due to the absence of a separation threat. Furthermore, the inability to dismiss workers can limit the flexibility of the public sector to adapt its workforce to changes in demand for public services across subsectors or regions. The counterproductive effects of strict dismissal protection might be particularly relevant when decisions to grant permanent contracts are not based on worker performance, but on seniority, as is the case in many contexts. Yet there is a lack of evidence on the causal effect of strict dismissal protection on bureaucratic turnover and productivity and its associated mechanisms.

This paper studies the impact on turnover and productivity of granting strict dismissal protection to civil servants, with a focus on protection conceded on the basis of seniority. We study this question in the context of public school teachers in Chile. To deal with the endogeneity of receiving dismissal protection, we take advantage of a 2015 law that required all public education administrators to grant a permanent contract to teachers who had a temporary contract and at least three consecutive years or four non-consecutive years of experience in 2014.[2]

[1]Job security may also increase employers' and workers' returns to investing in their jobs (Autor et al., 2007; Acharya et al., 2013; Griffith and Macartney, 2014; Martins, 2022).

[2]Despite regulations limiting the use of temporary contracts, the share of teachers with temporary contracts increased from 19 percent in 2003 to 59 percent in 2014 (see details in Section 2). The rise of temporary contracts in the public sector is also common to many other contexts (Grindle, 2012; Lægreid and Wise, 2015).

To carry out our empirical analysis, we use microdata on primary and secondary education teachers from the personnel registers of the Chilean Minister of Education for the 2003-2016 period. This dataset holds detailed information on the features of each teaching position in the country. It also contains several teacher characteristics and a unique employee identifier that allows to track teachers across their trajectory in the education system, and hence to identify the seniority and type of contract of teachers in the sample. Using teacher identifiers, we merge these data with student level data on math and literacy test scores from a nationwide standardized assessment of grade 6 students in 2013-2015, and also use the scores of these students in the grade 4 examination held two years before. We also use responses from a student survey with information on the pedagogical activities of their teachers in 2014-2015. We focus our analysis on public schools.

To document the effect of dismissal protection on the probability that teachers leave their school, we use a difference-in-differences estimation in which we compare the subsequent turnover of teachers with temporary contracts who had two and three years of consecutive experience in 2010-2014. Intuitively, we compare teachers who had the minimum experience required to be granted the right to a permanent contract under the policy (three years) to teachers who had one less year of experience, using the difference-in-differences variation to account for any time-invariant heterogeneity between these two groups. Two aspects of the 2015 law are worth noting for interpreting our results. First, once the law was passed, all eligible teachers had the legal right to the high dismissal protection granted by permanent contracts, even if their contractual status did not instantly change. Second, eligiblity for permanent contracts under this law was determined using the seniority obtained by teachers before the law was passed.

We find that dismissal protection reduces teacher turnover by 25 percent in the first two years after the policy is enacted. We show that our results are not biased by differential pretrends, by time varying differences in the characteristics of teachers, or by spillovers to the comparison group. To understand what type of workers are retained by dismissal protection, we test for heterogeneous effects by a measure of baseline teacher performance: scores in a nationwide teacher evaluation. We find that the reduction in turnover is only statistically significant among teachers at the bottom and top of the distribution of baseline evaluation scores, leaving the average quality of teachers unchanged.

We then examine the impact of dismissal protection on teacher productivity, using value-added to student achievement as our measure of productivity. We implement a difference–in–differences design in which we compare the performance of students taught by teachers who the previous year had a temporary contract and at least three years of consecutive experience or four years of total experience versus the performance of those taught by the other teachers. As students could sort into teachers in 2015 according to the teacher's contractual status, we take advantage of the fact that most students are taught by treated teachers in some subjects but not in others, and implement a within-student, across-subject model.

We find a small and statistically insignificant overall effect on student test scores. Importantly for identification, we show that there are no pre-trends in student learning by teacher contractual status, and no sorting of students into teacher type by their lagged test scores. When we include teacher fixed effects, thus keeping the composition of teachers fixed and isolating the effort channel from the selection channel, we observe a statistically significant decline (6 percent of a SD) in the test scores of students taught by teachers with low baseline evaluation scores. We also find a statistically significant reduction in teacher effort on several pedagogical activities as reported in a questionnaire completed by students at the time of their examination.

Summing up, we find that granting high dismissal protection to public education teachers in Chile results in a large reduction in turnover, which is only statistically significant among teachers at the bottom and top of the distribution of baseline performance. Looking at the effect of dismissal protection on productivity, we do not find an impact on average teacher productivity. However, when we keep the composition of teachers constant—isolating the effort channel from the composition channel—we observe a drop in productivity for teachers at the bottom of the quality distribution. Together, these findings are consistent with the hypothesis that high dismissal protection can be a double-edged sword. It can help to retain high-quality employees by increasing the value of staying on the job, but at the cost of making it more difficult to separate and motivate low-performing employees; particularly when it is granted on the basis of seniority—a common practice in the public sector.

This study contributes to the literature on the personnel economics of the state (see Finan et al., 2017; Besley et al., 2021) and particularly to the study of the effects of personnel policies on the profile and productivity of public sector employees. A strand of papers has focused on how recruitment methods (Estrada, 2019; Moreira and Pérez, 2021; Muñoz and Prem, 2021), wages (Dal Bo et al., 2013; Biasi, 2021; Bobba et al., 2021; Leaver et al., 2021), and career opportunities (Ashraf et al., 2020) shape the profile of those who join the public sector and the quality of public service delivery.[3] Others have analyzed how wages affect the turnover and the profile of public school servants through exit (see, for example, Clotfelter et al., 2008; Berlinski and Ramos, 2020). Our contribution is to look at both the effect on bureaucratic turnover and the productivity of public service delivery of a different policy: strict dismissal protection, a common but barely examined characteristic of public sector employment.

We also contribute to the literature on teacher effectiveness in the economics of education. Teachers are a key determinant of student learning (Rivkin et al., 2005). Hence, it is important to understand the impact of personnel policies on the capacity of education systems to attract and retain better teachers and to incentivize them properly. This is particularly relevant in contexts like Chile, in which teachers are negatively selected in terms of skills with respect to the rest of the tertiary-educated population (Estrada and Lombardi, 2020). In the closest paper to ours, Jacob (2013) focuses on the effect of dismissal protection on teacher effort.[4] This study finds that giving school principals in Chicago the possibility of firing probationary teachers reduces teacher absences, with most of the effects being driven by changes in teacher composition. The author also conducts an exploratory exercise on changes in student learning. The results suggest that reducing

[3] Another strand of this literature studies these issues in the case of elected officials, as opposed to bureaucrats. For example, Ferraz and Finan (2009), Gagliarducci and Nannicini (2013), Fisman et al. (2015), and Pique (2019) study the effects of wages on selection, while Dal Bó and Rossi (2011) study how term length affects legislative effort.

[4] In the context of the private sector, Ichino and Riphahn (2005) and Martins (2009) study the effect of high employment protection on absenteeism in Italy and turnover and wages in Portugal, respectively. There is a large literature in labor economics on the effects of employment protection legislation on labor market dynamics and firm-level outcomes—see, for example, a review in Addison and Teixeira (2003).

dismissal protection leads to higher test scores in low-achieving schools. Our contribution with respect to this work is to provide direct evidence on the impact of dismissal protection on teacher productivity (i.e., student test scores). We are also able to examine whether these effects differ by baseline teacher quality.[5]

The paper is organized as follows. Section 2 provides background on public education and teacher contracts and turnover in Chile. Section 3 describes the estimation strategy, data, and results for our analysis on the causal effect of dismissal protection on teacher turnover. Section 4 presents our analysis on the causal effect on teacher productivity. Finally, Section 5 concludes.

2 Teacher labor markets in Chile

2.1 Personnel policies in the public education system

The Chilean school system has three types of schools: public schools, subsidized private schools, and non-subsidized private schools. Public schools are locally run by each of the 346 municipal governments. The National Teacher Labor Code, which regulates the working conditions of teachers in public schools, dictates that teachers can be hired under permanent or temporary contracts. Although temporary teachers receive the same salary as permanent teachers, they are hired through a different process and enjoy substantially less protection against dismissal (Bertoni et al., 2018).

Teachers with permanent contracts are hired through public contests held by the municipalities. Candidates are selected on the basis of professional performance, seniority, and training by a jury composed of the director of the municipality's education department, the principal of the school with the position, and a randomly selected teacher with the same specialization as the va-

[5] Relatedly, Dee and Wyckoff (2015) examines the impact of lower dismissal protection in the District of Columbia, where there is already no strict protection against dismissal. The authors find that teachers who are classified as low performers in a teacher evaluation and hence face a larger dismissal threat are more likely to leave their job than teachers with a score above this threshold and, conditional on staying, to have a better performance in subsequent evaluations. An advantage of our setting is that it allows us to isolate the effect of dismissal protection from other changes in teachers' working conditions induced by the evaluation system—i.e., lower pay and a negative evaluation feedback. There is evidence that receiving negative feedback in a teaching evaluation leads to lower job satisfaction, which could in turn affect turnover and performance on the job (Koedel et al., 2017; Lombardi, 2019).

cancy (Paulo et al., 2017). Temporary positions, on the other hand, are directly filled without the need of a public contest. In theory, temporary contracts are only reserved for teachers who perform tasks that are transitory or who replace a teacher on leave.[6] However, many temporary teachers have their contracts periodically renewed, and thus hold their position for several years. In 2014, for example, 20 percent of temporary K-12 teachers had been working for the same municipality for 4 or more years. Note that these flexible contractual arrangements do not act as probationary periods, since temporary contracts cannot be converted into permanent ones.

Another important difference between temporary and permanent contracts is the job security they afford. Both types of teachers can be dismissed over a severe infringement of their duties, or after repeated bad performance in teaching evaluations, but this is uncommon.[7] The Teacher Labor Code also allows municipalities to reduce teachers' hours or dismiss them with severance pay in response to a drop in enrollment or curricular modifications, prioritizing those who are close to the retirement age or had a bad performance in their teaching evaluation, regardless of their type of contract.[8] The key difference between temporary and permanent teachers is that the contract of temporary teachers can only last up to two years (and typically lasts only one), allowing municipalities to let these teachers go without severance pay once their contract is up.

Despite regulations capping the share of hours taught by teachers with temporary contracts at 20 percent, compliance has decreased over time, as shown in Appendix Figure A.4. The share of teachers with temporary contracts increased from 19 percent in 2003 to 59 percent in 2014. The main reason behind the increasing use of temporary contracts is the drop in enrollment in public schools. Public schools used to enroll the majority of students, but this sector has been losing students to subsidized private schools over the last decades, as shown in Appendix Figure A.5. Although this drop in enrollment has not led to a reduction in the size of the teaching force

[6]Further details about the labor regulations of public sector teachers in Chile are provided in the website of the Labor Directorate, at https://www.dt.gob.cl/portal/1628/w3-propertyvalue-22160.html (last accessed June 6, 2022).

[7]By 2014, only 0.31 percent of all evaluated teachers had been removed due to poor performance in their teaching evaluations. Further details can be found at https://bibliotecadigital.mineduc.cl/bitstream/handle/20.500.12365/14547/Resultados-EvDoc2018.pdf?sequence=1&isAllowed=y (last accessed June 6, 2022).

[8]Until 2012, teachers with temporary contracts were first in line to have their hours reduced or be dismissed, and they were not entitled to any compensation.

in public schools, municipal governments started relying on temporary contracts to retain certain flexibility in response to waning enrollment.[9]

In January 2015, before the start of the 2015 school year, the Chilean Congress enacted a law requiring that municipal governments grant permanent contracts to public sector teachers that had a temporary contract (Law 20,804). The law only applied to public sector teachers that as of August 2014 worked in the same municipality for at least three consecutive years or four non-consecutive years, emulating the previous regularization process that occurred in 1999. Only years in which the teacher worked for 20 or more hours a week were considered when computing teachers' seniority. Almost a third of all temporary K-12 teachers in 2014 fulfilled these conditions (approximately 15 thousand teachers).

Once the law was passed at the beginning of 2015, teachers that fulfilled the seniority requirements had the legal right to the high dismissal protection granted by permanent contracts, even if their contractual status did not instantly change. Probably due to administrative delays, not all teachers who were entitled to permanent contracts under the law received them right away, as shown in Figure 1. By mid-2015, 25 percent of eligible teachers had been granted a permanent contract, and this figure rose to 52 percent in 2016. By 2021, almost 80 percent of these teachers had been awarded a permanent contract. Around 10 percent of eligible teachers did not receive a permanent contract because they left the public school system or took a job in a different municipality (where they are not entitled to a permanent contract under the 2015 law). The remaining 11 percent of teachers who we identify as eligible were not awarded a permanent contract by 2021. As explained in Section 3.2 below, this is likely due to measurement error in the number of years of experience that teachers have in their municipality.[10]

[9]The increase in the share of temporary teachers can also be explained by the implementation of a bonus for early retirement in 2011. In private correspondence with an education specialist from the Chilean association of municipalities we were told that teachers who were up for retirement were generally replaced with temporary teachers. We were also informed that in addition to the municipalities' desire to retain certain flexibility, the rise in the share of temporary teachers also resulted from an increase in resources channeled to municipalities for specific projects starting in 2011 (the *Subvención Escolar Preferencial*). Due to the temporary nature of these extra funds, it was reasonable to use them to hire teachers under fixed-term contracts.

[10]The database with the positions held by Chilean teachers is based on reports made at the middle of the year. We assume that a teacher's situation in the middle of the year is representative of what occurred during the entire year, but that is not necessarily true. We could be overstating the years of experience if, for example, some teachers were

2.2 Teacher turnover

Turnover among Chilean teachers is not uncommon. In 2010-2013, turnover for teachers in K-12 public schools was 18 percent—two thirds of these teachers took a job in another school, and the remaining third left the profession altogether.[11] The rate of teacher turnover is similar to that of other countries (NCES, 2019; OECD, 2021). For example, 16 percent of public school teachers in the U.S. leave their jobs every year. While some turnover is to be expected, and could even be desirable (e.g. if low performing teachers are replaced with better ones), there is evidence that teacher turnover results in lower student learning (Akhtari et al., 2022).

Turnover is significantly higher for teachers in the early stages of their careers (Appendix Figure A.1). For example, turnover for teachers in their first year in a municipality is 38 percent, compared to only 8 percent for teachers who have been in their municipality for 7 or more years. The type of contract is also a key determinant of teacher mobility. As shown in Appendix Figure A.1, turnover is higher for teachers with temporary contracts vis-à-vis teachers with permanent contracts, reflecting the higher protection against dismissal enjoyed by the latter. On average, the likelihood of not working in the same school after one year is 7 percentage points higher for teachers with temporary contracts, even after controlling for years of experience in the municipality. Turnover for permanent teachers is most likely voluntary, whereas teachers with temporary contracts may also leave if their contracts are not renewed (our data does not allow us to distinguish between quitting and firing).

The type of contract held by teachers not only determines overall turnover, but also the characteristics of teachers who leave. As shown in Appendix Figure A.2, turnover is higher for teachers with lower scores in a teaching evaluation. This association is significantly higher for teachers with temporary contracts, likely reflecting a higher likelihood of non-renewal of the contracts of temporary teachers with low baseline performance.[12]

teaching in a municipality for 20 or more hours when the reporting is made, but taught for less than 20 hours or not at all at some other moment during the year.

[11] These figures were calculated using the sample of teachers below age 55 who were teaching 20 or more hours in a K-12 public school. If we include all K-12 public school teachers, turnover is slightly higher, at almost 20 percent.

[12] Besides from having higher turnover, teachers with low teaching evaluation scores are also more likely to leave

3 Impact of high dismissal protection on turnover

3.1 Estimation strategy

To estimate the causal effect of strict dismissal protection on teacher turnover, we take advantage of the fact that municipalities had to grant permanent contracts to teachers with at least three consecutive years of experience in that municipality in 2014, but were exempt from doing so with less experienced teachers. To make the treatment and comparison groups as similar as possible, we limit our sample to teachers that in each year of 2010–2014 had exactly two or three years of consecutive experience in a municipality under a temporary contract.[13] We thus estimate the following difference-in-differences equation, in which we compare the differences in subsequent turnover of teachers that had three years of consecutive experience (our treatment group) and two years of consecutive experience (our comparison group) in the 2014 cohort with the differences in previous cohorts:

$$Y_{imt} = \beta_0 + \beta_1 \mathit{Treated}_{imt} + \beta_2 \mathit{Treated}_{imt} \times Post_t + \gamma_t + U_{imt} \tag{1}$$

Y_{imt} are dummy variables for whether teacher *i* working in municipality *m* in cohort *t* was working in the same school after one and two years, with *t* spanning the cohorts from 2010 to 2014. $\mathit{Treated}_{imt}$ is a dummy equal to one if teacher *i* has three years of consecutive experience in municipality *m* in year *t*, and zero if the teacher has only two years of consecutive experience in that year. Our variable of interest is the interaction between $\mathit{Treated}_{imt}$ and $Post_t$, the dummy for the 2014 cohort. We also include cohort fixed effects (γ_t). We should note that around half of the teachers in our sample appear twice in our analysis. For example, the 2010 cohort includes people that had two or three years of consecutive experience in 2010, and the 2011 cohort includes

teaching altogether when they leave their school, as shown in Appendix Figure A.3.

[13] Teachers with exactly three years of experience in the same municipality account for a third of the K-12 teachers in 2014 that had the right to a permanent contract. The rest of the teachers covered by the law had four or more years of experience (consecutive or not).

people that had two or three years of consecutive experience in 2011. Therefore, teachers that had two years of experience in 2010 can also appear in the 2011 cohort if they work in the same municipality and still have a temporary contract (with three years of experience by that point). To account for serial correlation in the error term, we cluster standard errors at the teacher level.

As with any difference-in-differences estimation, the validity of our estimates relies on the assumption that in the absence of the reform, the differences in the likelihood of exiting the school for teachers that had two and three years of experience in 2014 would have been the same as the differences for teachers with two and three years of experience from previous cohorts (i.e., parallel trends). Under this assumption, β_2 measures the effect of providing strict dismissal protection on the basis of seniority. The parallel trends assumption requires that any unobservable differences between teachers with two and three years of experience are fixed over time, and that any shock in a given year that is common to all teachers has the same average impact, irrespective of the teachers' seniority. In Section 3.4, we present the results of several validity checks.

Our estimates are probably a lower bound of the effect of high dismissal protection on turnover for two reasons. First, as mentioned in Section 2, there is some measurement error in the years of experience, making approximately 10 percent of teachers in our treatment group ineligible for a permanent contract under the reform. Importantly, this measurement error is unlikely to differ between teachers in the treatment and comparison groups. Secondly, 13 percent of teachers who had two years of experience in 2014 got a permanent contract, as shown in Figure 2. While the regularization process entitles teachers in the treatment group to a permanent contract, it does not forbid municipalities from holding contests and granting permanent contracts to other teachers. Our estimates are therefore a lower bound of the impact of dismissal protection on turnover.[14]

[14]We could have performed an instrumental variables estimation using a dummy for whether the teacher was granted a permanent contract as the endogenous regressor, and the interaction between our $Treated_{imt}$ and $Post_t$ dummies as the instrument. We do not do this because our main treatment (having high dismissal protection) was already granted to eligible teachers once the 2015 law was passed, regardless of whether their contractual status had changed at that point or not.

3.2 Data and descriptive statistics

We use a public database from the Chilean Ministry of Education with every teaching position since 2003. This database, created on the basis of annual reports by schools, reflects the situation at the middle of each year, and holds detailed information on the features of the position, such as the type of school (public, private-subsidized or private-unsubsidized), the weekly number of hours teaching, the type of contract (permanent or temporary), and school and municipality identifiers. This data set also contains several teacher characteristics, such as their age, gender, and whether they have an education degree, among others. Importantly, it has unique teacher identifiers that allow tracking teachers across years and positions, allowing to compute teachers' seniority in any municipality, and identify changes in their type of contract.

We restrict our sample to teachers who in any year of 2010–2014 were working for 20 or more hours a week in a K-12 public school under a temporary contract, and had exactly two or three years of consecutive (and total) experience in that municipality. Following the regulation, we only consider that a teacher has accumulated a year of experience in a municipality if he/she works for 20 or more hours a week during that year. For teachers working in more than one school in the same municipality, we consider the total number of hours across all schools from the same district. The first cohort in our sample is that of 2010 because our measurement of the total years of experience in a municipality for teachers in previous cohorts is more prone to error, as our data on teacher positions starts in 2003.[15] To abstract ourselves from retirement decisions when studying turnover, we exclude teachers who are 55 years or older (less than 6 percent).[16]

[15]We limit our control group to teachers that have exactly two years of experience in a municipality because those with four or more years of experience were entitled to a permanent contract (the reform covered teachers with at least three consecutive years of experience or four years of total experience). As some teachers have interruptions in their tenure within a municipality, there are indeed teachers with two consecutive years and four or more total years of experience in a municipality. This is the case for 10 percent of the teachers that accumulated two years of consecutive experience in a municipality by 2014, for example. The difference between consecutive and total years of experience is harder to identify in earlier cohorts, as our data on teacher positions only starts in 2003. For example, there are no teachers with two years of consecutive experience in the 2007 cohort with four or more years of total experience (since 2003). This is probably underestimated, as we cannot observe whether the teacher was working in the same municipality before 2003. To avoid including eligible people in our comparison group, we restrict our sample to cohorts from 2010 onwards.

[16]This leaves us with 27,854 observations. We then drop 865 observations from municipalities that changed borders during our analysis period, and 5 from municipalities with missing data on teachers in some years. Of the remaining

Teachers in a given municipality can appear twice in the sample, once when they have two years of experience, and again if they accumulate three years of experience with a temporary contract in the same municipality. Approximately half of the teachers in the sample are featured twice under a particular municipality.[17] Furthermore, teachers can appear twice if they work in different municipalities and accumulate the necessary years of experience in those municipalities. This is very rare, and is only the case for less than 1 percent of the teachers in our sample.

We use the same dataset to construct our outcome variables. Our main outcomes are dummy variables for whether the teacher was not working in the same public school after one and two years. For teachers working in more than one school in the same municipality (9 percent), we focus on the school with the highest concentration of teaching hours. As the dataset has information on teacher positions in all private and public schools in the country, we also examine whether the teacher was working in a private school, was not teaching at all, or was teaching in another public school in the same or different municipality.

Table 1 reports some descriptive statistics for the 24,002 observations in the sample, separated by whether the teacher has two or three consecutive years of experience. Around 75 percent of the teachers are female, 25 percent of them work in a rural school, and 93 percent have an education degree. The average teacher works for 33 hours each week, and 9 percent work in more than one school. Most teachers (around 79 percent) work in a primary school, with the remainder working in a secondary school. The main difference between teachers with two and three years of experience is their age. Teachers with two years of experience are, on average, 33.7 years old, whereas teachers with one more year of experience are one year older. Turnover is high, with

26,984 observations, we drop 2,267 that belong to teachers who appear to have gone from a permanent to a temporary contract in the same school in consecutive years, as there must be a reporting error in the type of contract. Finally, we drop 715 observations from teachers who participated in the teacher evaluation before the first year they appear in the dataset on teaching positions—as they are missing from the dataset on teaching positions on a year in which they were teaching, this indicates that we have an error in the measurement of the years of experience for these teachers. This leaves us with a final sample size of 24,002.

[17] Teachers appear only one time if they already had three years of experience in 2010, or only had two years of experience in 2014. This is the case for 67 percent of the teachers that only appear once in the sample. The remaining teachers only appear one time because they have two years of consecutive experience in the same municipality at some point in 2010–2013, but do not fulfill the requirements to be in the sample in the following year. This can occur if they obtain a permanent contract, work less than 20 hours, take a job in another municipality, or leave the public sector.

teachers with less years of experience being more likely to leave their school. In particular, 30 percent of these teachers are not working in the same school after four years (as opposed to 25 percent of teachers with three years of experience). Turnover is quite evenly split into teachers who leave the public school system, and teachers who work in a different public school. Approximately half of the teachers who leave the public school system take a job in a private school, and half stop teaching altogether. Mobility within the public school system is mostly concentrated in the same municipality, with few teachers taking a job in a public school in a different municipality.

3.3 Results

As can be seen in Table 2, dismissal protection leads to a large and statistically significant reduction in teacher turnover. As shown in column 1, it reduces the probability that the teacher leaves the school after one year by 3.7 percentage points, a 23 percent reduction over the comparison group mean. This reduction in turnover implies closing half of the gap between temporary and permanent teachers shown in Appendix Figure A.1. After two years, the likelihood of leaving the school drops by 6.7 percentage points (column 4), a 25 percent reduction over the mean. Both of these estimates are statistically significant at the 1 percent level. As shown in Appendix Table A.1, the reduction in turnover mostly materializes after the second year, as the impact on turnover after three and four years is a reduction of 7.4 and 7.6 percentage points, respectively.

Teachers leave their school by leaving the public school system or by taking a job in another public school. As shown in columns 2-3 and 5-6 of Table 2, around two thirds of the reduction in teacher turnover is driven by a reduction in the likelihood of leaving the public school system, and the remainder comes from a lower probability of taking a job in another public school. In Table A.1, we further disaggregate these two sources of turnover. As shown in columns 1 and 2, the reduction in the likelihood of leaving the public school system is equally split into a drop in the likelihood of taking a job in a private school or not teaching. In the case of teachers who would have taken a job in another public school if it were not for their permanent contract, the decrease in this source of turnover is driven by teachers who would have taken a job in a public school in another municipality (column 4). This is expected, as permanent contracts give teachers job stability within their municipality.

To understand how dismissal protection affects the pool of teachers, it is important to examine what type of teachers are retained by it. On the one hand, granting dismissal protection to teachers might allow municipalities to retain some good teachers that would have otherwise left. In this case, dismissal protection could lead to an improvement in the average quality of teachers. But, on the other hand, the average quality of teachers could suffer if granting teachers protection against dismissal means that schools must keep ineffective teachers they would have otherwise let go at the end of their contracts.

We take advantage of the fact that Chilean public school teachers are evaluated by the Ministry of Education, and use teaching evaluation scores as a proxy for teacher quality/performance.[18] Since 2003, public school teachers in Chile are evaluated every four years using different instruments: (i) a teaching portfolio, (ii) a peer assessment, (iii) a self-assessment, and (iv) a supervisor assessment. The teaching portfolio accounts for 60 percent of the final score, and it is composed of a videotaped lesson and different elements that make up an eight-hour learning unit that teachers have to plan and implement for the grade and subject in which they conduct most of their teaching.[19] The peer assessment accounts for 20 percent of the score, and it is a scripted interview conducted by a trained teacher from a different school who instructs the same subject and grade. The remaining 20 percent of the score is evenly split between a self-assessment questionnaire and a supervisor assessment completed by the school principal. As shown in Appendix Figure A.7, there is a positive association between student test score gains in math and literacy and their teacher's nationwide ranking in terms of their teaching evaluation score. Going from a teacher in the 25th percentile to one in the 75th percentile of teacher evaluation scores is correlated with an increase in

[18]The Chilean Ministry of Education carries out annual standardized tests for students, but only certain grades are tested, and these tests are only carried out at the end of the school year. It is thus not possible to construct individual estimates of teacher value added, a common proxy for teacher performance.

[19]Teachers present a written lesson plan for each of the classes in this learning unit and an evaluation of their students. They also answer a questionnaire with several questions about their pedagogy and their students' assessment results. Portfolios are blindly graded by trained teachers with at least five years of classroom experience in the same subject area and grade level as the evaluated teacher. Further details on the portfolio grading process can be found in Taut and Sun (2014).

student test scores of 2.84 percent of a SD.[20]

In order to analyze whether the response to dismissal protection depends on baseline teacher performance, we rely on a dataset with the results of teaching evaluations. For each teacher, we use the results of his/her first evaluation. As shown in Table 1, 83 percent of teachers with three years of experience were evaluated at least once, whereas 76 percent of teachers with two years of experience went through the evaluation process. These differences arise from the fact that first-year teachers are not evaluated, and teachers can postpone their assessment if they act as peer evaluators, or for personal reasons. Importantly, these differences in the likelihood of being evaluated by experience are constant over time, as shown in Table 4, and there is no imbalance in teachers' evaluation score percentile. We split the sample of teachers that were evaluated (79 percent of our sample) into terciles according to their performance in the teaching evaluation (national ranking),[21] and estimate equation (1) fully interacted with dummies for each tercile.

The estimates of the impact of permanent contracts on the likelihood of not working in the same school after one and two years for each tercile are presented in Figure 3. Teachers in the bottom and top tercile have a 4.6 and 4.2 percentage point reduction in the likelihood of leaving the school after one year (both statistically significant at the 5 percent level). The point estimate for the middle tercile is smaller and not statistically significant. After two years, teachers in the bottom and top tercile have a 8.1 and 7.8 percentage point reduction in the likelihood of not working in the same school (both significant at the 1 percent level), whereas the reduction for teachers in the middle tercile is only 3.8 percentage points (significant at the 10 percent level).[22] While these

[20] We conduct this analysis using the same sample of students as in the analysis of Section 4. In particular, we regress students' math and literacy test scores in a nationwide evaluation conducted in grade 6 against their teachers' evaluation score percentile, subject fixed effects, student-year fixed effects, and the students' grade 4 score in that subject.

[21] For teachers that were evaluated more than once, we consider the performance in their first evaluation. Since teachers were evaluated in different years, and evaluation scores could change over time for reasons unrelated to performance/ability, we consider teachers' evaluation score percentile from the year they were evaluated, using all teachers evaluated in that year to determine teachers' place in the distribution of scores.

[22] As firing or quitting decisions might depend on teachers' relative performance within their school, we instead split teachers into terciles according to their position in the distribution of evaluation scores in the school they work for. Results are depicted in Appendix Figure A.8. Although results are similar to those in Figure 3, we observe a larger reduction in turnover for teachers in the bottom tercile (7 percentage points after one year, and 11 percentage points after two years), and can reject the null hypothesis that the impact on turnover for teachers in the bottom tercile is equivalent to that of teachers in the middle and top terciles.

results suggest that high dismissal protection increases the retention of all teachers, but especially those in the bottom and top of the distribution of teaching evaluation scores, we cannot reject that the impacts do not differ between terciles.

We also explore whether the type of turnover differs according to teacher evaluation scores. As shown in Figure 4, teachers in the bottom tercile are less likely to take a job in a public school in a different municipality when they are entitled to a permanent contract. For teachers at the top of the distribution of baseline performance, high dismissal protection not only retains them in the same school, it prevents them from leaving the teaching profession altogether, possibly because they have better employment prospects in other industries. This result highlights the potential value of job stability in keeping highly trained and well-performing teachers in the teaching career.

3.4 Validity checks

As discussed in Section 3.1, the identifying assumption is that if it were not for the reform, the difference in the subsequent likelihood of leaving the school of teachers that had two and three years of experience in 2014 would have been the same than that of teachers with two and three years of experience in previous cohorts (i.e., parallel trends). While this assumption is untestable, we explore its plausibility by analyzing whether the treatment and comparison groups of previous cohorts were on parallel trends. We estimate the following dynamic difference-in-differences estimation:

$$Y_{imt} = \beta_0 + \beta_1 \textit{Treated}_{imt} + \sum_{k=2011}^{2014} \beta_k \, \textit{Treated}_{imk} \times I[t=k] + \gamma_t + U_{imt} \tag{2}$$

Once again, Y_{imt} is a dummy for whether teacher *i* working in municipality *m* in cohort *t* is working in the same school after one and two years, with *t* spanning the cohorts from 2010 to 2014, and $I[t=k]$ are cohort dummies. We omit the 2010 cohort, thus normalizing relative to the first cohort.

The graph to the right in Figure 5 plots the coefficients for the interactions between the dummy for whether the teacher has three years of experience and the corresponding cohort dummies, as well as their 95 percent confidence intervals. We do not find any differences in the likelihood of leaving the school after one or two years in the 2011-2013 cohorts, as compared to teachers with two and three years of experience in 2010. The raw means can also be observed in Figure 6.

Identifying the causal effect of high dismissal protection on teacher turnover also requires parallel trends in the likelihood of obtaining a permanent contract. We thus estimate equation (2) with a dummy for whether the teacher has a permanent contract as the outcome. As compared to teachers with two and three years of experience in the 2010 cohort, there are no differences in the likelihood of obtaining a permanent contract in the 2011 and 2013 cohorts. The difference in the likelihood of obtaining a permanent contract between teachers with three and two years of experience in 2012 is slightly smaller than that of teachers in the 2010 cohort. While this difference is statistically significant, it is very small in comparison to the difference-in-difference estimates for the 2014 cohort (5.7 percentage points vs. 26.8 percentage points after two years), and it is almost not visible in the raw data (Figure 2).

As with any difference-in-differences analysis using repeated cross sections, an additional concern is that the differences in the characteristics of the treatment and comparison groups could vary over time. In order to examine this, we perform a balance test using our full sample of teachers. In particular, we estimate equation (1) using the baseline characteristics of teachers as dependent variables. While there are statistically significant differences in the characteristics of teachers with three and two years of experience, these differences are constant over time, as shown in Table 4. The only teacher characteristic that varies across time between teachers with three and two years of experience is the likelihood of teaching in primary school (2.1 percentage points higher). However, this coefficient is only statistically significant at the 10 percent level, which would be expected by chance given the amount of regressions we estimate in this analysis.

As discussed in Section 3.1, our estimates on the effect of granting permanent contracts would be biased if there were other policies that had differential effects on teacher turnover according to seniority. For instance, assume that teacher salaries were increased at the start of 2014, and higher salaries reduced turnover. If teachers with greater experience react more to this salary increase, the observed reduction in turnover could be driven by higher salaries and not by permanent contracts. To examine whether this concern could be driving our results, we perform two placebo exercises. In the first placebo, we perform a difference-in-differences estimation comparing the subsequent turnover after two years of teachers with permanent contracts that had two and three years of experience in 2014 to teachers with two and three years of experience and permanent contracts in previous cohorts. As shown in column 1 of Appendix Table A.2, we find no differences in turnover between teachers with two and three years of experience in 2014 as compared to previous cohorts. We perform another placebo exercise, comparing teachers with four years of consecutive experience to teachers with three years of consecutive experience (all with temporary contracts). As both groups of teachers from the 2014 cohort receive high dismissal protection, we should not expect to find a differential impact on subsequent turnover. Column 2 of Appendix Table A.2 reports the results of a difference-in-differences estimation for this sample of teachers, where the dependent variable once again is a dummy for whether the teacher is not working in the same school after two years. We find no difference in turnover of teachers with three and four years of experience in 2014 as compared to previous cohorts, as the estimates are small and not distinguishable from zero.

A final concern is the possibility that turnover in the comparison group is affected by the 2015 law (i.e., that there are spillovers). This could occur, for example, if temporary teachers with two years of experience are discouraged by not receiving a permanent contract and decide to leave the public sector, or teaching altogether. Furthermore, because of the reform, schools may have to let some of these teachers go in order to avoid increasing the size of their teaching force (although, to achieve this goal, a hiring freeze could be a more palatable alternative from the political point of view). If this were the case, our estimates would be capturing an increase in turnover in the comparison group, instead of a drop in turnover for treated teachers. Mitigating concerns about spillovers, the raw data in Figure 6 shows that there is no increase in the likelihood of leaving the school for teachers in the comparison group. This evidence indicates, hence, that our estimates are driven by a drop in the likelihood of leaving the school by treated teachers.

4 The impact of high dismissal protection on productivity

In the previous section, we showed that granting high dismissal protection to teachers on the basis of their seniority decreased turnover significantly. We turn now to study the impact of this policy on teacher productivity. We expect that the right to a permanent contract affects teacher productivity through two channels: selection and effort. In both cases, the direction of the effect is theoretically ambiguous, given that within each channel there might be counteracting forces at play.

With respect to selection, the expansion of permanent contracts could have changed—for better or worse—the composition of teachers if their effect on turnover varied by teacher quality. As reported in the previous section, we do not find direct evidence supporting the relevance of this mechanism in our context when we proxy teachers' quality by their performance in a national teacher evaluation. Yet, we investigate this channel further in the empirical analysis below.

In terms of the second channel, the absence of a separation threat could reduce teachers' incentives to exert effort, with negative consequences for productivity.[23] However, higher protection against dismissal could increase teachers' returns to invest in their jobs (i.e., in course preparation) leading to higher effort and productivity, and making the sign of the effort channel a priori ambiguous.[24]

4.1 Estimation strategy

For the empirical analysis, we proxy teacher productivity by their value-added to student achievement. More specifically, we use test scores from a national standardized assessment that all stu-

[23]Lower effort from teachers can manifest itself through more absences (Ichino and Riphahn, 2005; Jacob, 2013), lower effort in the classroom, or lower likelihood of implementing new directives (Marino et al., 2010), among others.

[24]In support of the idea that dismissal protection increases employees' returns to investing in their job, Acharya et al. (2013) and Griffith and Macartney (2014) show that job stability increases innovation by private sector employees. Along the same lines, a more stable match could incentivize employers to further invest in employee training, as documented by Autor et al. (2007) and Martins (2009, 2022).

dents take at the end of grades 6 and 4 (SIMCE, by its Spanish acronym), which we merge with the personnel data described in Section 3.2. In this way, we create a pooled-cross sectional database of grade 6 students inclusive of information on their math and language teachers' dismissal protection status.

To identify the causal effect of strict dismissal protection on student achievement, we implement a difference–in–differences design in which we compare the performance of students taught by teachers who the previous year had a temporary contract and at least three years of consecutive experience or four years of total experience in that municipality ("Treated") versus the performance of those taught by the other teachers. Hence, as in the previous analysis, we use the 2015 law as an exogenous shifter in the probability of obtaining high dismissal protection. The identification assumption is that in the absence of treatment, student outcomes by teacher's contractual status follow parallel trends. We present below supportive evidence for this assumption by showing that prior to the 2015 law there was no divergent path in student achievement by teacher's contractual status. One potential concern in this setting is the possibility that, for some reason we are not aware of, students sort into teachers in 2015 by their (last year's) contractual status—e.g., if best students are assigned to treated teachers in that year. To deal with concerns about student sorting, we take advantage of the fact that students may be taught by eligible teachers in some subjects but not in others, and that SIMCE measures yearly student performance in both math and literacy, and implement a within-student, across-subject model. In Chile, grade 6 students from the same classroom share the same teachers which makes any within-student, across-subject sorting extremely unlikely.

More precisely, we estimate the following equation:

$$Score_{ist} = \beta_1 \textbf{\textit{Treated}}_{ist} + \beta_2 \textbf{\textit{Treated}}_{ist} \times Post_t + \beta_3 Lagged\,Score_{ist} + \theta_{it} + \delta_s + U_{ist}, \quad (3)$$

where $Score_{ist}$ is the score of student i in subject s (math or literacy) in grade 6 in year t (z-score), $Treated_{ist}$ is a dummy for whether the teacher of student i in subject s had a temporary contract and least three years of consecutive experience or four years of total experience in that municipality the year before. $Post_t$ is a dummy for the year 2015 (0 in 2013-2014), $LaggedScore_{ist}$ is the score of student i in subject s (math or literacy) in grade 4 (z-score), and θ_{it} and δ_s are student-year and subject fixed effects (respectively). We report standard errors clustered at the teacher-year level, as this is the level at which the treatment is assigned—i.e., all students from a teacher in a given year have the same treatment status—(Abadie et al., 2017).

β_2 is the parameter of interest and captures the causal effect of granting teachers dismissal protection on student achievement, under the parallel trends assumption. Note that this effect is a composite of both the selection and effort channels. We also run an alternative specification of equation (3) in which we include teacher fixed effects to examine the impact of dismissal protection on teacher productivity net of changes in the pool of teachers (i.e., the effort channel).

We estimate equation (3) using a dataset of grade 6 students from public schools who took the SIMCE examination in 2013, 2014, or 2015.[25] As our estimation compares students' scores across subjects, we limit our sample to students who took the SIMCE test for both subjects (81 percent of enrolled students). We do not have a valid score in either subject for 11 percent of enrolled students, as 5 percent of students were exempt from taking the test because they have a disability or do not speak Spanish, and 6 percent were absent or were the only student in their school to take the test (these scores are not reported due to privacy issues). This does not pose a threat to the internal validity of our estimates, which compare students' scores across subjects. The remaining 8 percent of students took the test in only one subject (the tests are held on two consecutive days). Importantly, the likelihood of missing the test is not related to whether the student's teacher in that subject is entitled to tenure under the reform.[26] As our estimation controls

[25]The SIMCE exam is also taken by students in grades 2, 4, 8, and 10. However, we focus on grade 6 students because younger students have the same teacher in both subjects, which does not permit a within-student across-subject estimation. In our sample of 6$^{\text{th}}$ grade students, 95 percent have a different teacher in math and language, as shown in Table 5. Although students in grades 8 and 10 also have separate teachers for both subjects, we can only match students to their previous score in grade 4 (this was the only grade tested every year before 2013). We thus decided to focus our analysis on grade 6, as our lagged score measure is more recent, allowing us to adequately control for past achievement and mitigate any concerns about subject-specific sorting.

[26]We estimate equation (3) for the full sample of enrolled students, replacing the dependent variable with a dummy for whether the student did not take the test in that subject, and our estimate of β_2 is very small (0.0099) and not statistically significant.

for lagged scores, we also limit our sample to students who took the 4^{th} grade SIMCE in both subjects (89 percent of students who took the grade 6 test in both subjects).[27] Table 5 presents descriptive statistics for the 208,474 students in our sample. They are on average 11.7 years old. Around half of them are female, 4 percent are repeating grade 6, and 20 percent attend a rural school. Importantly for our identification strategy, 20 percent of students have math and language teachers with different eligibility status. Regarding their teachers, around 14 percent of them are treated (i.e., have a temporary contract and at least three years of consecutive experience or four years of total experience in that municipality the year before), and 52 percent have permanent contracts. The students' teachers are on average around 44 years old, and 60 percent (85 percent) of the math (language) teachers are female.

Note that while the current sample is composed of math and language teachers irrespective of their seniority or contractual status, the turnover analysis presented in the previous section included teachers from all subjects, but with only two and three years of experience, and a temporary contract. The focus of the current analysis on math and language teachers is dictated by the availability of test scores.[28] The choice of expanding the analysis beyond temporary teachers with two and three years of experience is to avoid producing and small and selected sample of students. Only 2,606 (1.2 percent) of the 208,474 students in our sample are taught by a combination of math and language teachers with temporary contracts and two and three years of experience.

4.2 Results

Table 6 presents the results of estimating equation (3). As one can observe in column 1, we find no effect on student learning (we can reject a drop in test scores larger than 4.6 percent of a SD, and an increase larger than 1.2 percent of a SD). A similar finding emerges when we include teacher fixed

[27] Nine percent of the sample does not have a grade 4 score in either subject, and 3 percent are missing the score in just one of the subjects. Importantly, the likelihood of not having a 4^{th} grade score is not related to whether the student's teacher in that subject is entitled to a permanent contract under the reform. Furthermore, when we include these students in our sample (and thus do not control for grade 4 scores), we obtain similar results (columns 1-2 of Appendix Table A.3).

[28] Students in grade 6 are also evaluated in social sciences and natural sciences, but unlike math and language, these subjects are not included in the SIMCE evaluation every year.

effects (column 2), which allows us to isolate the effort channel. We do not observe a significant average effect on student learning (we can reject a drop in test scores larger than 4.6 percent of a SD, and an increase larger than 2.6 percent of a SD). Appendix Tables A.4 and A.5 support the robustness of these results by showing evidence, respectively on the absence of pre-trends on student learning by teacher type, and student sorting into teacher type by 4 grade test scores.

Appendix Table A.6 shows that there are no observed changes in the composition of treated (vs. comparison) teachers along characteristics like age, gender, having an education degree, number of teaching hours, teaching role, etc. There is a significant decrease in the probability of being evaluated in the national teaching assessment (of around 5 percent versus the comparison group), but without a change in the average scores obtained in that evaluation. After the reform, there is an increase in the experience profile of teachers in the comparison group, which is reflected by the increase in the difference between the two groups of teachers in the likelihood of having one year of experience. This is probably the consequence of a higher separation of temporary teachers from the comparison group that have one year of experience. In principle, this pattern could potentially increase the (short-run) productivity of the comparison group due to a drop in the share of teachers with little experience, leading to an underestimation of the parameter of interest.[29] This spillover might be particularly relevant in our context because a large literature has documented the existence of a steep learning curve in the first years of teaching (Rivkin et al., 2005; Staiger and Rockoff, 2010; Araujo et al., 2016). Mitigating these concerns about spillovers, Appendix Table A.3 shows though that the results are similar if one omits students whose teachers have less than two years of experience (columns 3-4).

Going back to the main results, it is feasible that behind the average there are heterogeneous effects of opposite sign. We investigate this possibility in Table 7, which presents results by teacher quality—proxied by teachers' evaluation scores (see details in Section 3.3).[30] In the main

[29] One could also expect a reduction in the entry rates of new teachers, but as shown in Appendix Table A.6 this is not a concern in our context.

[30] We restrict our sample in this analysis to students for whom both teachers were evaluated (77 percent of the students in our main sample). As shown in columns 5-6 of Appendix Table A.3, we obtain very similar point estimates when we estimate equation (3) in this subsample.

specification (column 1), we do not find significant results for neither teachers in the top nor the bottom of the teacher evaluation score distribution (splitting the sample in terciles produces similar results). However, when we keep the pool of teachers constant (i.e., when we include teacher fixed effects)—focusing on the effort channel—we find a drop in the learning of students taught by treated teachers in the bottom of the teacher quality distribution (of around 5.7 percent of a SD, significant at the 5 percent level). The treatment effect for students taught by treated teachers in the top of the teacher quality distribution is positive (3.4 percent of a SD), but is not statistically significant (the associated p-value is 0.176). We observe a similar pattern if instead of halves we split the sample in terciles. In other words, we find evidence that granting high dismissal protection leads to a reduction in effort and productivity among teachers with lower performance at baseline, while for teachers at the top of the quality distribution the estimates show a positive but noisily estimated impact.

To explore direct evidence on changes in teacher effort, we use data from the context questionnaires given to all students who take the SIMCE test. Unfortunately, the survey questionnaires have some changes from year to year, but for 2014 and 2015 we have information on a set of six questions in which students report the frequency with which their language teacher engages in different pedagogical activities (e.g., related to addressing students' questions, providing feedback on exams, and correcting homework)—details on these questions are available in Appendix B. We use the responses to these questions to construct a Teacher Effort Index by principal component analysis (standardised with mean 0 and SD 1). The index is positively correlated with student test scores (a 1 SD increase in the index predicts an increase of 3.2 percent of a SD in student test scores, with statistical significance at the one percent, controlling for student characteristics and school fixed effects), which suggests it is a meaningful measure of teacher effort. We then adapt equation (3) and estimate a model with only one subject (there are no comparable questions on math teachers' effort on these years) and two periods (one before and one after treatment). Table 8 presents the results. As one can observe, we find evidence of a significant decline in teacher effort (both in the models with and without teacher fixed effects). When the composition of teachers is fixed, we observe that granting high dismissal protection reduces teacher effort by around 6 percent of a SD. The heterogeneous effects by baseline teacher evaluation scores are somehow noisy. In the main specification, we observe significant declines in the effort of teachers both in the bottom and top halves of the baseline teacher evaluation performance (of around 8.7 and 5.1 percent of a SD, respectively). If we keep the composition of teachers constant, only the decline in the top half is statistically significant at conventional levels, although the difference in the magnitude of the estimated effects for both groups is small (0.7 percent of a SD) and not statistically significant. Hence, using students' reports on teacher effort in several activities and a more tentative identification strategy, we find that teachers seem to react to high dismissal protection by reducing the effort they put at work. We do not observe heterogeneous effects by teacher performance at baseline, although we cannot distinguish if this is due to a potential bias in the estimated effects in this exercise or to other factor(s) like, for example, the fact that teachers from the bottom of the teacher quality distribution might be reducing their effort more in activities that we do not observe.

Summing up, we do not find that the granting of high dismissal protection leads to changes in average teacher productivity (as proxied by value-added to student learning). Yet, when we keep the composition of teachers fixed—isolating the effort channel—we observe a significant decline in the learning of students taught by treated teachers from the bottom of the teacher quality distribution. We obtain direct measures of teacher effort—as reported by students—and find suggestive evidence of a negative impact of dismissal protection on these measures of teacher effort.

5 Conclusions

The evidence presented in this paper shows that granting high dismissal protection on the basis of seniority results in large reductions in turnover for public education teachers in Chile. Furthermore, we find suggestive evidence that the reduction in turnover is higher for teachers at the bottom and top of the teacher quality distribution—as compared to those in the middle. Although we do not find any changes in average teacher productivity, when we keep the composition of teachers constant—isolating the effort channel from the composition channel—we observe a drop in productivity for teachers at the bottom of the quality distribution. Importantly for interpretation, we study teachers who receive strict dismissal protection after they have been at least three years in their job, which means that their employers have had time to learn about their productivity and have decided to renew their (annual) contract at least a couple of times. Hence, it is reasonable to expect that our estimates are a lower bound of the effect of strict dismissal protection in contexts when it is granted earlier in the labor relationship, as the opportunities for screening on the job are reduced.

There are good reasons to promote job security in the public sector. A first order argument is that job security weakens political patronage. Politicians require control over both access and permanence in jobs in order to use them as an effective clientelist tool (Robinson and Verdier, 2013). Furthermore, our results indicate that public sector employees place a high value on job stability. Hence, by making public sector employment more appealing, dismissal protection can help to attract and retain higher quality employees and to reduce the disruptive effect of excessive turnover on the quality of public service delivery (Akhtari et al., 2022). However, strict job security might come at the cost of lower flexibility in personnel management and of weak incentives to perform on the job, as documented in this paper for employees with lower performance at baseline.

The results presented here call for a revision of the practice of granting permanent contracts to public sector employees on the basis of seniority and irrespective of performance. In many contexts, gaining access to a regular public sector job—i.e., excluding political appointments and temporary contracts—comes with a credible promise of lifetime employment irrespective of job performance. Exploring how to strengthen the use of merit on entry and dismissal protection decisions are promising avenues for personnel policy.

The law analyzed in this paper was enacted in reaction to the growing use of temporary contracts to hire teachers. The use of such contracts has become common in the public sector, despite being often inconsistent with civil service regulations (Grindle, 2012; Lægreid and Wise, 2015). In principle, public administrators might rely on temporary contracts to avoid the personnel restrictions imposed by strict employment protection regulations. For example, public education administrators in Chile faced a downward trend in student enrollment in public schools and claimed that hiring teachers under permanent contracts could lead to large personnel redundancies in the near future. However, the use of temporary contracts might be motivated by avoiding the limitations that regular civil service jobs put on political patronage—particularly, as temporary contracts are associated with higher discretion in hiring. Colonnelli et al. (2020) find that mayors in Brazil use indeed the availability of temporary positions to reward political supporters with municipal jobs. Further research is necessary for a better understanding of the motivations and consequences of the growing use of temporary contracts in the public sector and the balance between isolating public sector jobs from the electoral cycle and the overregulation of public sector employment.

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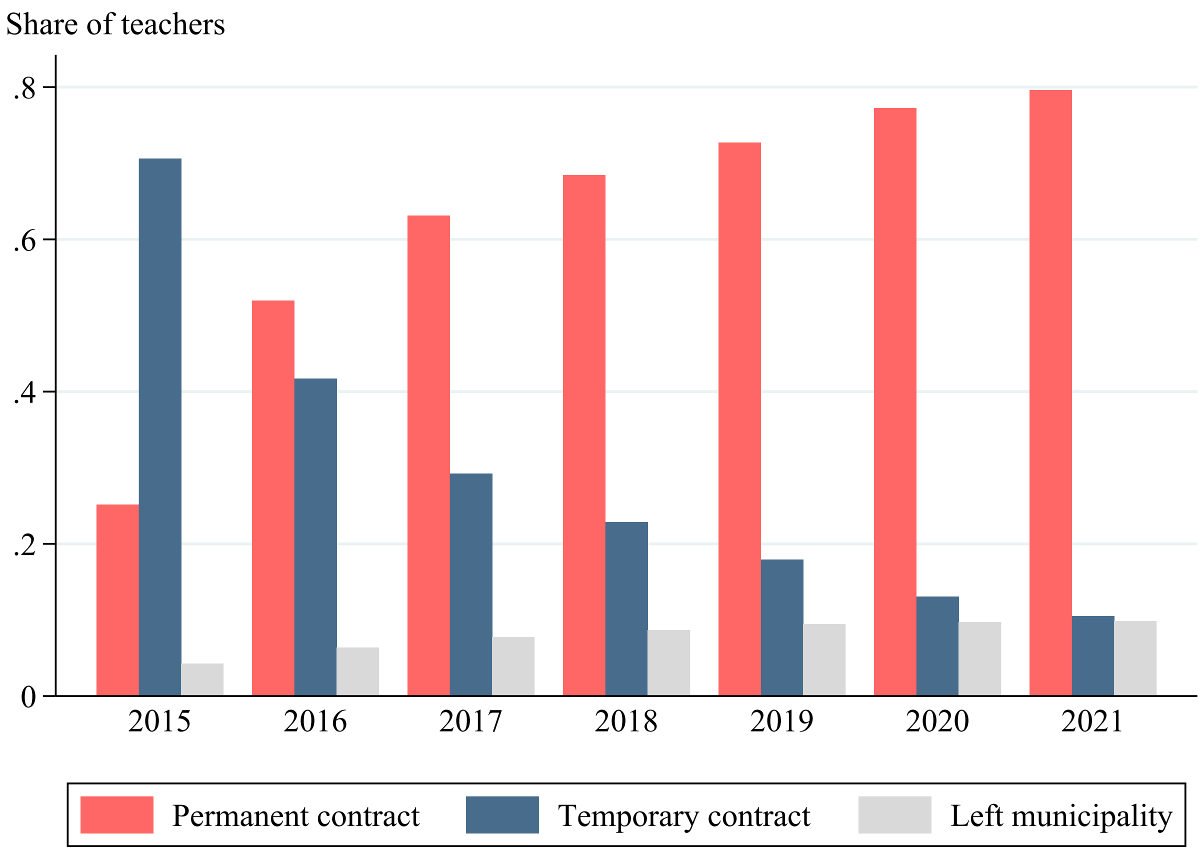
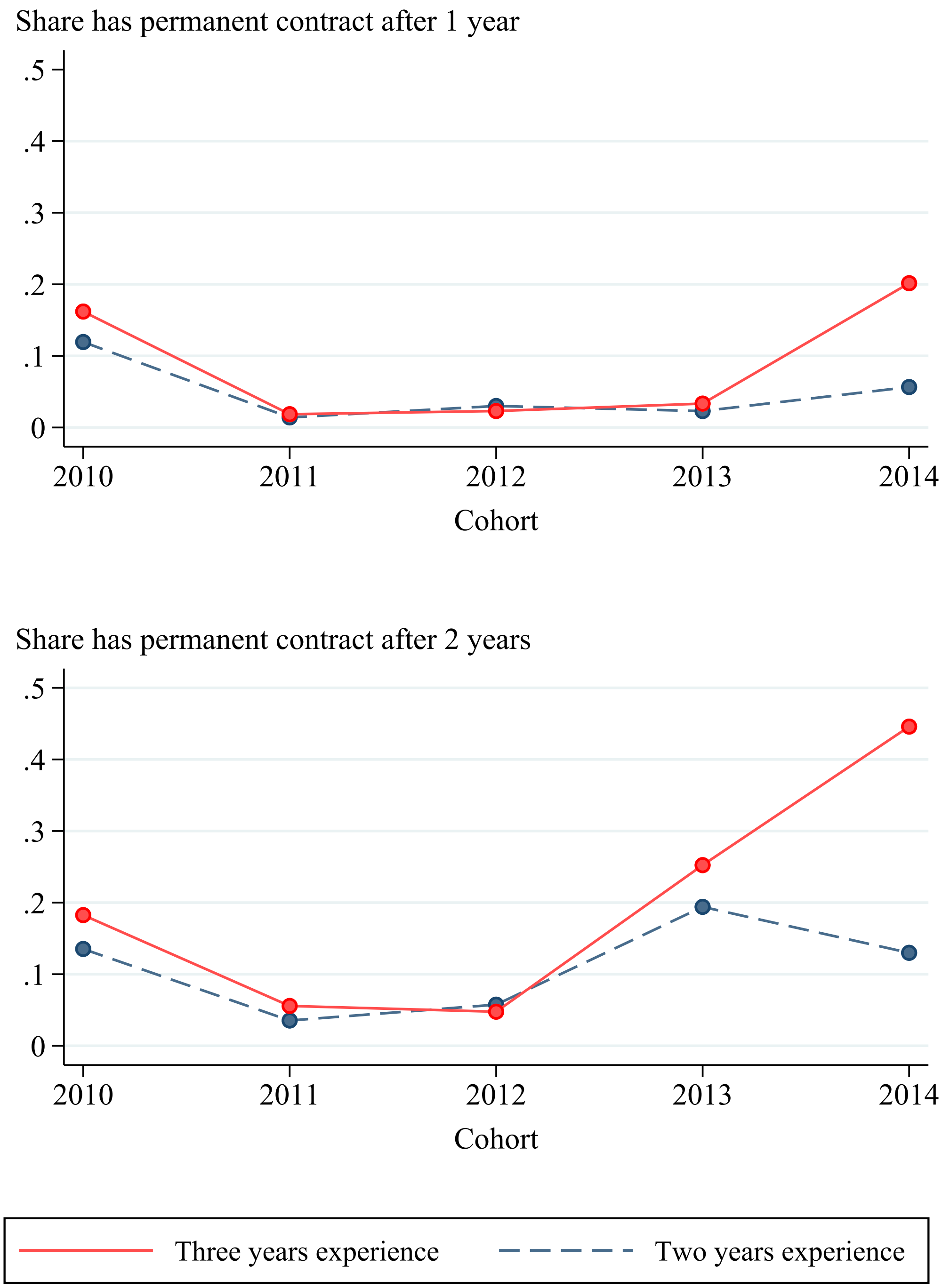
Figure 1: Implementation of the reform



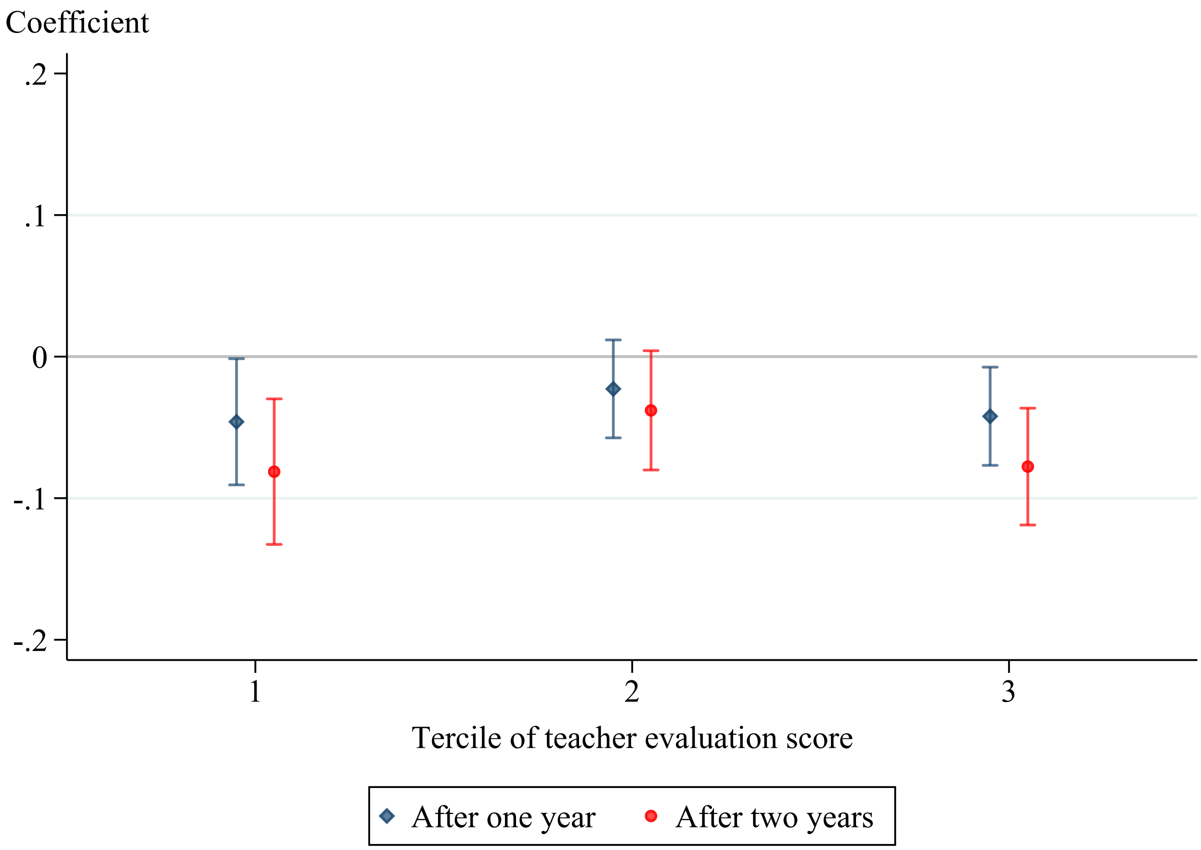
Notes: The sample is composed of all teachers who were teaching 20 or more hours in a K-12 public school under a temporary contract in 2014, and had at least three consecutive years or at least four years of experience in the municipality. This figure shows the evolution of the contractual status of these teachers in 2015-2021. The red bars show the share of these teachers who had obtained a permanent contract by each year. We consider that a teacher obtained a permanent contract if he/she had a permanent contract at some point, regardless of whether the teacher left the public school system in their municipality after that. The blue bars show the share of teachers who were still working in a public school in the same municipality under a temporary contract, and the grey bars show the share of teachers who had not received a permanent contract by that year and were not working in a public school in that municipality.

Figure 2: Share of teachers who had permanent contract



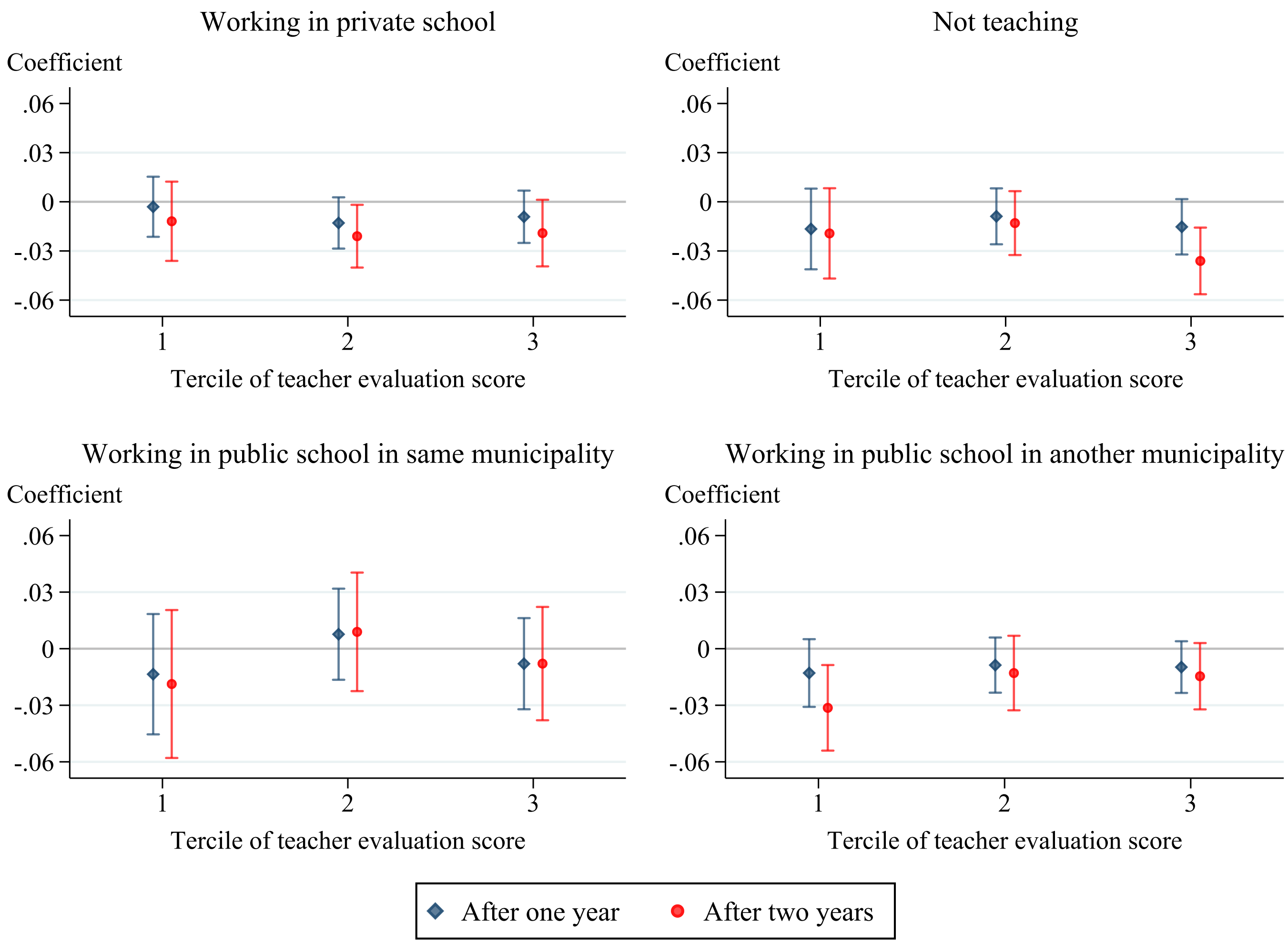
Notes: The top (bottom) figure shows the share of teachers who obtained a permanent contract in their municipality after one (two) years. We consider that a teacher obtained a permanent contract if he/she had a permanent contract at some point, regardless of whether the teacher left the public school system after that. The sample in each cohort is composed of teachers who were teaching 20 or more hours in a K-12 public school under a temporary contract, and were younger than age 55. The solid line shows the mean outcome for teachers with three years of consecutive experience in the same municipality (the treatment group), and the dotted line reports the same for teachers that had two years of consecutive experience (the comparison group).

Figure 3: Impact of high dismissal protection on teacher turnover – Heterogeneous effects by teacher evaluation scores



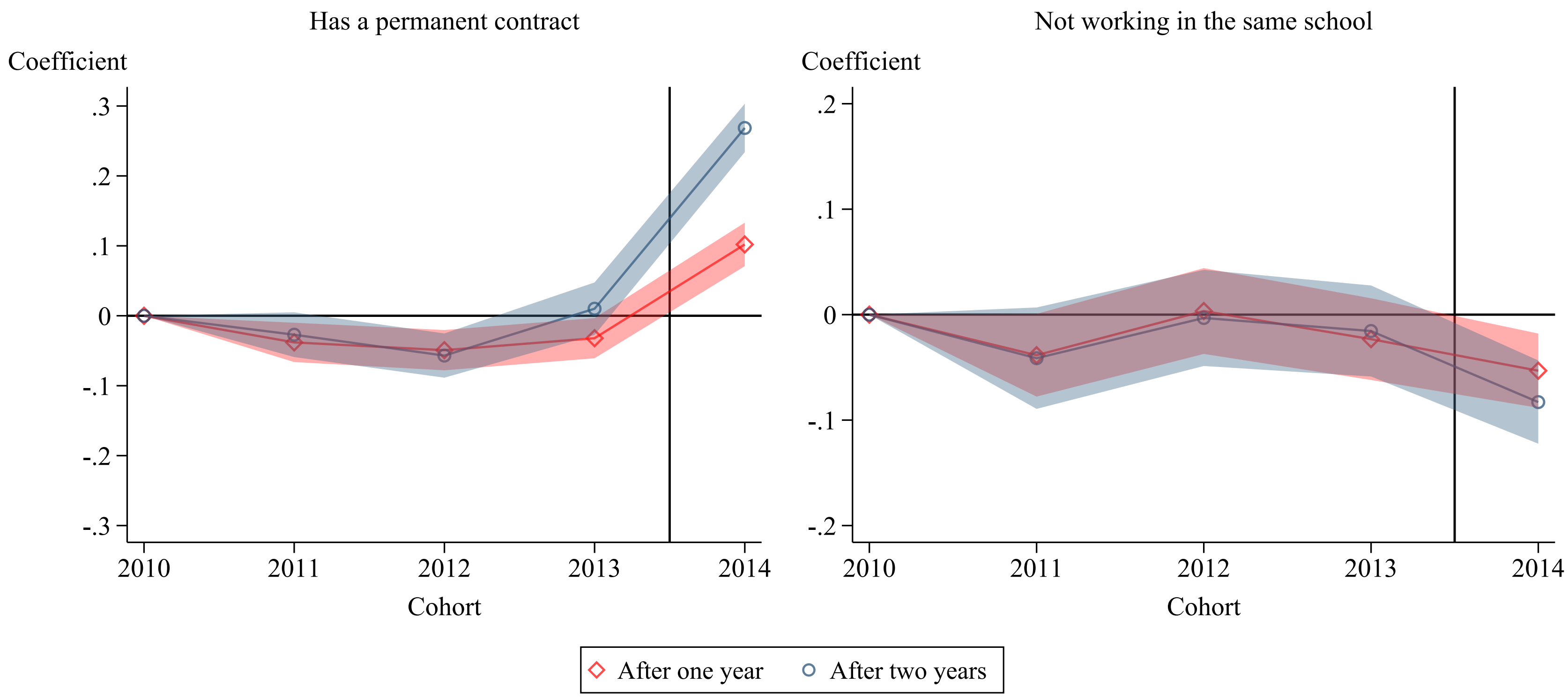
Notes: The sample is composed of teachers who were teaching 20 or more hours in a K-12 public school under a temporary contract in 2010–2014, were younger than age 55, participated in a teaching evaluation, and had two or three years of consecutive experience in the same municipality. This figure presents the main coefficients and 95 confidence interval for the estimation of equation (1) fully interacted with dummies for each tercile in the distribution of teacher evaluation scores. The estimates where the dependent variable is measured after one year are presented with a diamond, and the estimates for two years after are depicted with a circle.

Figure 4: Impact of high dismissal protection on teacher turnover by turnover type – Heterogeneous effects by teacher evaluation scores



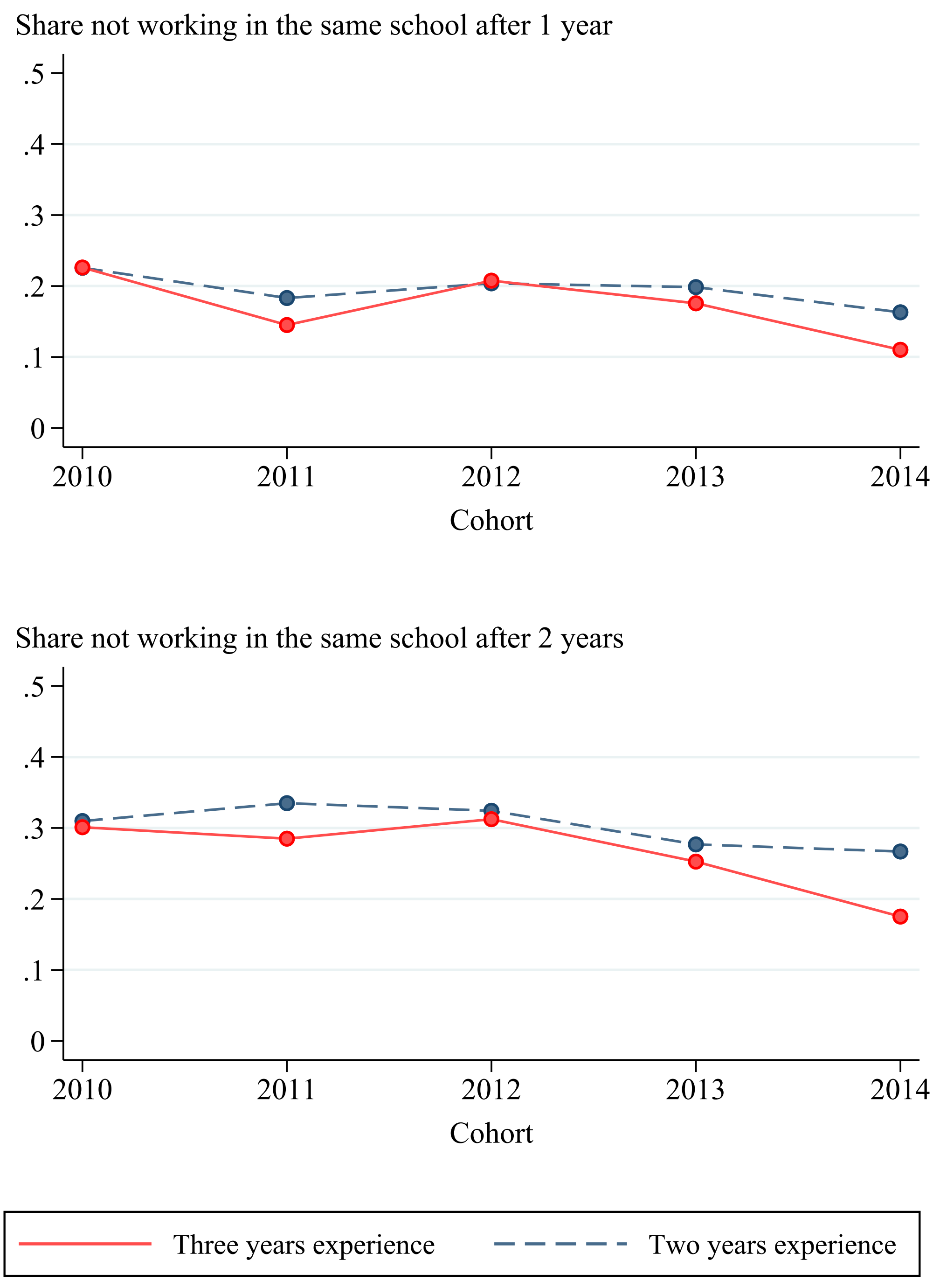
Notes: The sample is composed of teachers who were teaching 20 or more hours in a K-12 public school under a temporary contract in 2010–2014, were younger than age 55, participated in a teaching evaluation, and had two or three years of consecutive experience in the same municipality. This figure presents the main coefficients and 95 confidence interval for the estimation of equation (1) fully interacted with dummies for each tercile in the distribution of teacher evaluation scores. The dependent variable in the top left graph is a dummy for whether the teacher was working in a private school, and the dependent variable in the top right graph is a dummy for whether the teacher was not teaching in any school. The dependent variables in the two bottom graphs are dummies for whether the teacher was working after two years in another public school in the same or in another municipality, respectively. The estimates where the dependent variable is measured after one year are presented with a diamond, and the estimates for two years after are depicted with a circle.

Figure 5: Dynamic difference-in-differences estimates



Notes: These figures shows the estimates of dynamic difference-in-differences regressions where the regressors include cohort fixed effects, a dummy for whether the teacher had three years of experience, and interactions of this dummy with cohort fixed effects, with the omitted category being 2010 (the first cohort). The dependent variables in the regression results depicted in the left-hand figure are dummies for whether the teacher obtained a permanent contract after one and two years (in red and blue, respectively). In the figure to the right, the dependent variables are dummies for whether the teacher was not working in the same school after one and two years. We report the coefficients for the interactions and their 95 percent confidence intervals. The sample is composed of teachers who in 2010–2014 were teaching 20 or more hours in a K-12 public school under a temporary contract, and were younger than age 55. Standard errors are clustered at the teacher level.

Figure 6: Share of teachers not working in the same school



Notes: The top (bottom) figure shows the share of teachers who were not working in the same school after one (two) years. The sample in each cohort is composed of teachers who were teaching 20 or more hours in a K-12 public school under a temporary contract, and who were younger than age 55. The solid line shows the mean outcome for teachers with three years of consecutive experience in the same municipality (the treatment group), and the dotted line reports the same for teachers that had two years of consecutive experience (the comparison group).

Table 1: Descriptive statistics – Teachers with two and three years of experience

	Two years experience			Three years experience		
	Mean	SD	N	Mean	SD	N
Baseline characteristics						
Age	33.701	7.699	14,547	34.754	7.519	9,455
Female	0.751	0.432	14,547	0.750	0.433	9,455
Has an education degree	0.928	0.258	14,547	0.934	0.248	9,455
Rural school	0.252	0.434	14,547	0.257	0.437	9,455
Number of weekly hours teaching	33.157	7.607	14,547	33.247	7.593	9,455
Teaches primary school	0.786	0.410	14,547	0.784	0.411	9,455
Works in more than one school	0.090	0.286	14,547	0.089	0.284	9,455
Share students low SES	0.610	0.195	14,525	0.618	0.185	9,450
Was evaluated	0.761	0.427	14,547	0.831	0.375	9,455
Percentile in teaching evaluation	51.014	27.619	11,063	51.440	27.435	7,853
Outcome variables (after two years)						
Obtained a permanent contract	0.124	0.330	14,547	0.236	0.424	9,455
Not working in the same school	0.295	0.456	14,547	0.248	0.432	9,455
Left public school system	0.138	0.345	14,547	0.104	0.305	9,455
Working in private school	0.061	0.239	14,547	0.043	0.203	9,455
Not teaching	0.077	0.267	14,547	0.060	0.238	9,455
Working in a different public school	0.157	0.363	14,547	0.145	0.352	9,455
Working in a different public school in the same municipality	0.113	0.317	14,547	0.109	0.311	9,455
Working in a public school in another municipality	0.044	0.204	14,547	0.036	0.187	9,455

Notes: The sample is composed of teachers who were teaching 20 or more hours in a K-12 public school under a temporary contract in 2010–2014, and who were younger than age 55. The first three columns report statistics for teachers with two years of consecutive experience in the same municipality, and the last three columns report the same for teachers that had three years of consecutive experience.

Table 2: Impact of high dismissal protection on teacher turnover

	After one year			After two years		
	Not in same school	Left public school system	In another public school	Not in same school	Left public school system	In another public school
Treated × Post	-0.037***	-0.026***	-0.011	-0.067***	-0.045***	-0.022**
	(0.010)	(0.007)	(0.008)	(0.012)	(0.008)	(0.010)
Treated	-0.016**	-0.013***	-0.002	-0.025***	-0.018***	-0.007
	(0.006)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)
Observations	24,002	24,002	24,002	24,002	24,002	24,002
R^2	0.008	0.010	0.002	0.011	0.010	0.002
Dependent variable mean (control)	0.163	0.076	0.087	0.267	0.120	0.147

Notes: The sample is composed of teachers who were teaching 20 or more hours in a K-12 public school under a temporary contract in 2010–2014, were younger than age 55, and had two or three years of consecutive experience in the same municipality. The dependent variables in columns 1 and 4 are dummies for whether the teacher was not teaching in the same school after one and two years, respectively. In columns 2 and 5, the dependent variables are dummies for whether the teacher was not teaching in a public school after one and two years. In columns 3 and 6, the dependent variables are dummies for whether the teacher was working in a different public school after one and two years. *Treated* is a dummy for whether the teacher has three years of consecutive experience in that year, and *Post* is a dummy for the 2014 cohort. The regressions also include cohort fixed effects. Standard errors clustered by teacher are in parentheses. The dependent variable mean reported shows the average value of the dependent variable for the sample of teachers that had two years of experience in 2014. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 3: Impact of high dismissal protection on teacher turnover – Disaggregated by turnover type

	Left public school system		In another public school	
	Private school	Not teaching	Same municipality	Different municipality
Panel A: After one year				
Treated × Post	-0.010**	-0.016***	-0.003	-0.008*
	(0.004)	(0.006)	(0.007)	(0.004)
Treated	-0.007**	-0.007*	0.001	-0.003
	(0.003)	(0.004)	(0.004)	(0.002)
Observations	24,002	24,002	24,002	24,002
R^2	0.005	0.006	0.001	0.001
Dependent variable mean (control)	0.029	0.047	0.061	0.026
Panel B: After two years				
Treated × Post	-0.021***	-0.024***	-0.008	-0.013***
	(0.006)	(0.007)	(0.009)	(0.005)
Treated	-0.010***	-0.008**	-0.002	-0.004
	(0.003)	(0.004)	(0.004)	(0.003)
Observations	24,002	24,002	24,002	24,002
R^2	0.006	0.004	0.001	0.002
Dependent variable mean (control)	0.051	0.068	0.105	0.042

Notes: The sample is composed of teachers who were teaching 20 or more hours in a K-12 public school under a temporary contract in 2010–2014, were younger than age 55, and had two or three years of consecutive experience in the same municipality. The dependent variable in column 1 is a dummy for whether the teacher was working in a private school, and the dependent variable in column 2 is a dummy for whether the teacher was not teaching in any school. In columns 3 and 4, the dependent variables are dummies for whether the teacher was working after two years in another public school in the same or in another municipality, respectively. In Panel A, the outcomes are measured after one year, whereas in Panel B they are measured after two years. *Treated* is a dummy for whether the teacher has three years of consecutive experience in that year, and *Post* is a dummy for the 2014 cohort. The regressions also include cohort fixed effects. Standard errors clustered by teacher are in parentheses. The dependent variable mean reported shows the average value of the dependent variable for the sample of teachers that had two years of experience in 2014. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: Balance in baseline characteristics of teachers

	Age	Female	Has education degree	Rural school	Weekly hours teach.	Teaches primary	Share low SES students	Main role teacher	More than one school	Was evaluated	Evaluation score percentile
Treated × Post	-0.200	0.013	-0.006	0.001	0.334	0.021*	0.001	0.003	0.001	0.018	0.016
	(0.211)	(0.012)	(0.007)	(0.012)	(0.223)	(0.011)	(0.004)	(0.007)	(0.008)	(0.012)	(0.882)
Treated	1.176***	-0.005	0.007**	0.005	0.066	-0.011***	0.004*	0.009***	-0.001	0.063***	0.299
	(0.075)	(0.004)	(0.003)	(0.004)	(0.101)	(0.004)	(0.002)	(0.003)	(0.004)	(0.004)	(0.284)
Observations	24,002	24,002	24,002	24,002	24,002	24,002	23,975	24,002	24,002	24,002	19,839
R^2	0.007	0.001	0.002	0.001	0.002	0.002	0.260	0.006	0.001	0.010	0.000
Dependent variable mean (control)	33.309	0.764	0.941	0.237	32.891	0.800	0.680	0.922	0.077	0.734	51.818

Notes: The sample is composed of teachers who were teaching 20 or more hours in a K-12 public school under a temporary contract in 2010–2014, were younger than age 55, and had two or three years of consecutive experience in the same municipality. This table presents results of regressions where the dependent variable is a teacher characteristic specified in the column header, measured in the year in which the teacher has two or three years of experience, respectively. *Treated* is a dummy for whether the teacher has three years of consecutive experience in that year, and *Post* is a dummy for the 2014 cohort. The regressions also include cohort fixed effects. Standard errors clustered by teacher are in parentheses. The dependent variable mean reported shows the average value of the dependent variable in each column, for the sample of teachers who had two years of experience in 2014. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Descriptive statistics – Students in grade 6

	Mean	SD	N
Student characteristics			
Age	11.703	0.788	207,769
Female	0.490	0.500	207,769
Repeated 6th grade	0.040	0.196	207,612
Lagged GPA (1-7)	5.637	0.598	206,935
Lagged attendance (%)	92.117	8.542	207,612
Class size	31.224	10.319	208,474
Rural school	0.203	0.402	201,622
Share students low SES in school	0.654	0.155	201,614
Different teacher for math and literacy	0.945	0.229	208,474
Variation in eligibility status between math and literacy	0.205	0.403	208,474
Math teacher characteristics			
Treated	0.137	0.344	208,474
Has permanent contract	0.514	0.500	208,474
Female	0.601	0.490	201,622
Age	44.321	12.254	201,622
Works in more than one school	0.018	0.132	201,622
Number of weekly hours teaching	38.097	5.597	201,622
Number of years of experience (since 2003)	7.370	3.857	201,622
Number of years of experience in school (since 2003)	5.142	4.159	201,622
Was evaluated	0.898	0.302	201,622
Percentile in teaching evaluation	56.212	27.936	189,763
Language teacher characteristics			
Treated	0.137	0.344	208,474
Has permanent contract	0.520	0.500	208,474
Female	0.848	0.359	201,235
Age	44.302	11.722	201,235
Works in more than one school	0.011	0.106	201,235
Number of weekly hours teaching	37.918	5.320	201,235
Number of years of experience (since 2003)	7.396	3.865	201,235
Number of years of experience in school (since 2003)	5.259	4.185	201,235
Was evaluated	0.898	0.303	201,235
Percentile in teaching evaluation	56.129	28.271	188,416

Notes: The sample is composed of 6th grade public school students in 2013-2015 who took both the math and literacy SIMCE test, and have a 4th grade SIMCE score for both subjects.

Table 6: Impact of high dismissal protection on test scores

	(1)	(2)
Treated × Post	-0.017	-0.010
	(0.015)	(0.018)
Treated	0.025***	0.023
	(0.010)	(0.020)
Lagged score	0.426***	0.417***
	(0.003)	(0.003)
Observations	416,948	416,898
R^2	0.833	0.852
Student-year FE	✓	✓
Teacher FE		✓

Notes: The sample is composed of 6th grade public school students in 2013-2015 who took both the math and literacy SIMCE test, and have a 4th grade SIMCE score for both subjects. The dependent variable is the student's score in the math or literacy SIMCE evaluation (z-score). *Treated* is a dummy for whether the student's teacher in that subject had a temporary contract and at least three years of consecutive experience (or at least four years of total experience) in that municipality the year before. We include this variable by itself, as well as interacted with *Post* (a dummy for the year 2015). *Lagged score* is the student's score in the same subject in the 4th grade SIMCE evaluation (z-score). The regressions also control for subject fixed effects and student-year fixed effects. In column 2, the regression controls for teacher fixed effects as well. Standard errors clustered at the teacher-year level are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Impact of high dismissal protection on test scores – Heterogeneous effects by teacher evaluation score

	(1)	(2)
Treated × Post	-0.034	-0.057**
	(0.023)	(0.028)
Treated × Post × Teacher evaluation above median	0.038	0.091**
	(0.032)	(0.036)
Observations	319,592	319,562
R^2	0.834	0.852
P-value: sum coefficients=0	0.888	0.176
Treated × Post	-0.003	-0.068*
	(0.029)	(0.035)
Treated × Post × Middle tercile teacher evaluation	-0.017	0.065
	(0.039)	(0.045)
Treated × Post × Top tercile teacher evaluation	-0.008	0.103**
	(0.040)	(0.046)
Observations	319,592	319,562
R^2	0.834	0.852
P-value: sum coefficients middle tercile=0	0.463	0.919
P-value: sum coefficients top tercile=0	0.672	0.276
Lagged scores and student-year FE	✓	✓
Teacher FE		✓

Notes: The sample is composed of 6th grade public school students in 2013-2015 who took both the math and literacy SIMCE test, have a 4th grade SIMCE score for both subjects, and have a teacher that was evaluated in both subjects. The dependent variable is the student's score in the math or literacy SIMCE evaluation (z-score). *Treated* is a dummy for whether the student's teacher in that subject had a temporary contract and at least three years of consecutive experience (or at least four years of total experience) in that municipality the year before, *Teacher evaluated above median* is a dummy for whether the student's teacher in that subject had an evaluation score above the median (in the year in which he/she was first evaluated), and *Post* is a dummy for the year 2015. The regression in the top panel includes these variables by themselves, their interaction with each other, and their triple interaction. We also control for the student's score in the same subject in the 4th grade SIMCE evaluation, subject fixed effects and student-year fixed effects. In column 2, the regression controls for teacher fixed effects as well. We repeat the same exercise in the bottom panel, but splitting the sample of teachers into three groups according to their evaluation scores. Standard errors clustered at the teacher-year level are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: Impact of high dismissal protection on teacher effort

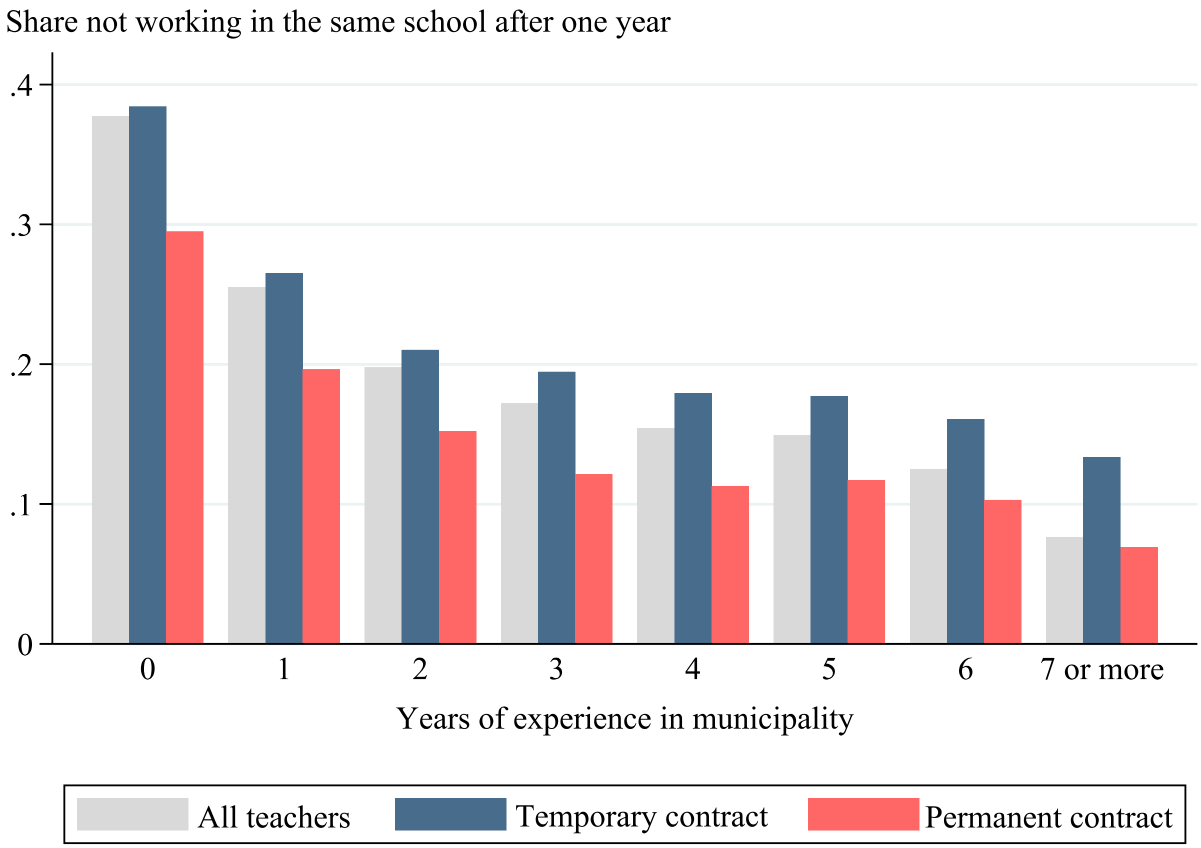
	(1)	(2)	(3)	(4)
Treated × Post	-0.068***	-0.060***	-0.087**	-0.054
	(0.022)	(0.023)	(0.034)	(0.034)
Treated	0.108***	0.073**	0.135***	0.053
	(0.023)	(0.032)	(0.036)	(0.053)
Treated × Post × Teacher evaluation above median			0.036	-0.007
			(0.045)	(0.045)
Observations	121,197	121,197	121,197	121,197
R^2	0.128	0.155	0.128	0.155
P-value: sum above median=0			0.084	0.044
Student controls	✓	✓	✓	✓
School FE	✓		✓	
Teacher FE		✓		✓

Notes: The sample is composed of 6th grade public school students in 2014-2015 who took the literacy SIMCE test and have a 4th grade SIMCE score. The dependent variable is the effort index score (z-score) of the student's teacher. *Treated* is a dummy for whether the student's language teacher had a temporary contract and at least three years of consecutive experience (or at least four years of total experience) in that municipality the year before, *Post* is a dummy for the year 2015, and *Teacher evaluated above median* is a dummy for whether the student's language teacher had an evaluation score above the median (in the year in which he/she was first evaluated). The regressions include, when appropriate, their interaction with each other and their triple interaction. We also control for the student's score in the same subject in the 4th grade SIMCE evaluation, gender, age, being above the normative age, being a repeater, and lagged GPA and attendance. In columns 2 and 4, the regression controls for teacher fixed effects as well. Standard errors clustered at the teacher-year level are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

ONLINE APPENDIX

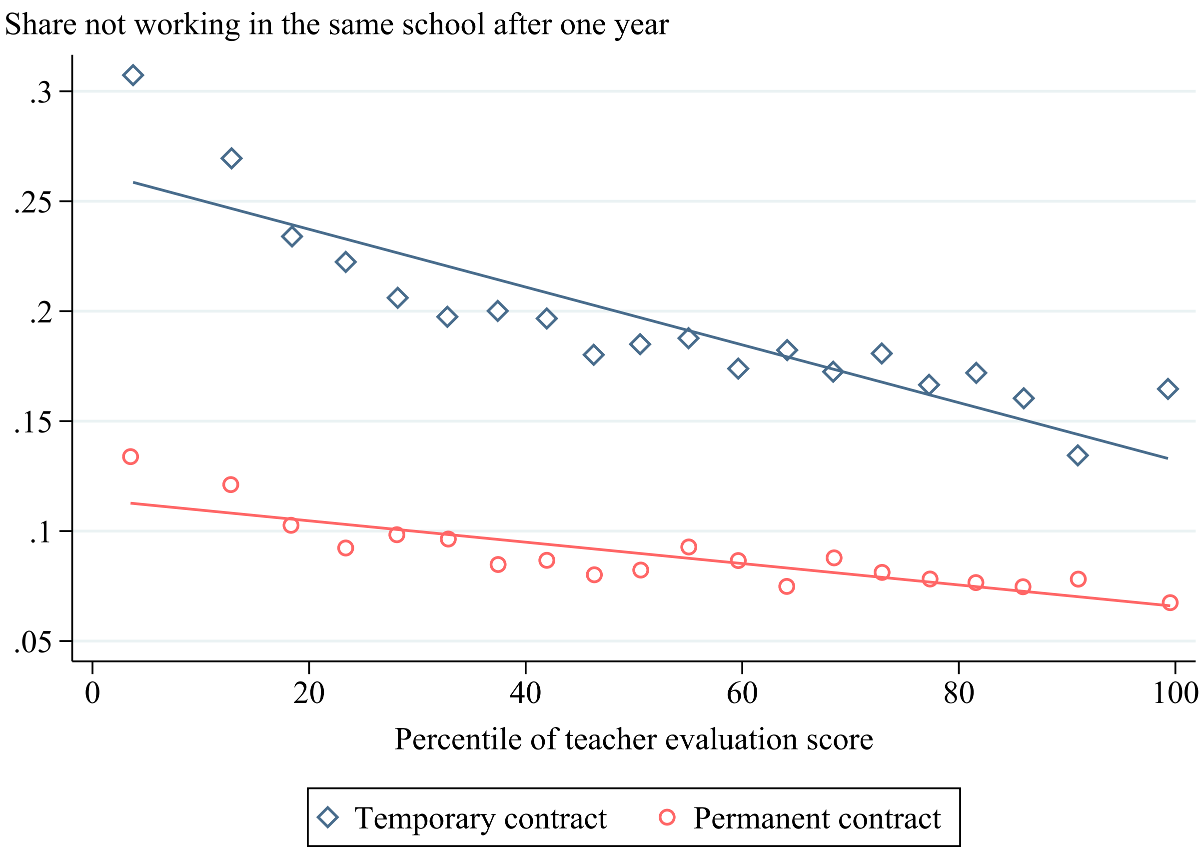
Appendix A Figures and Tables

Figure A.1: Teacher turnover by type of contract and experience, 2010-2013



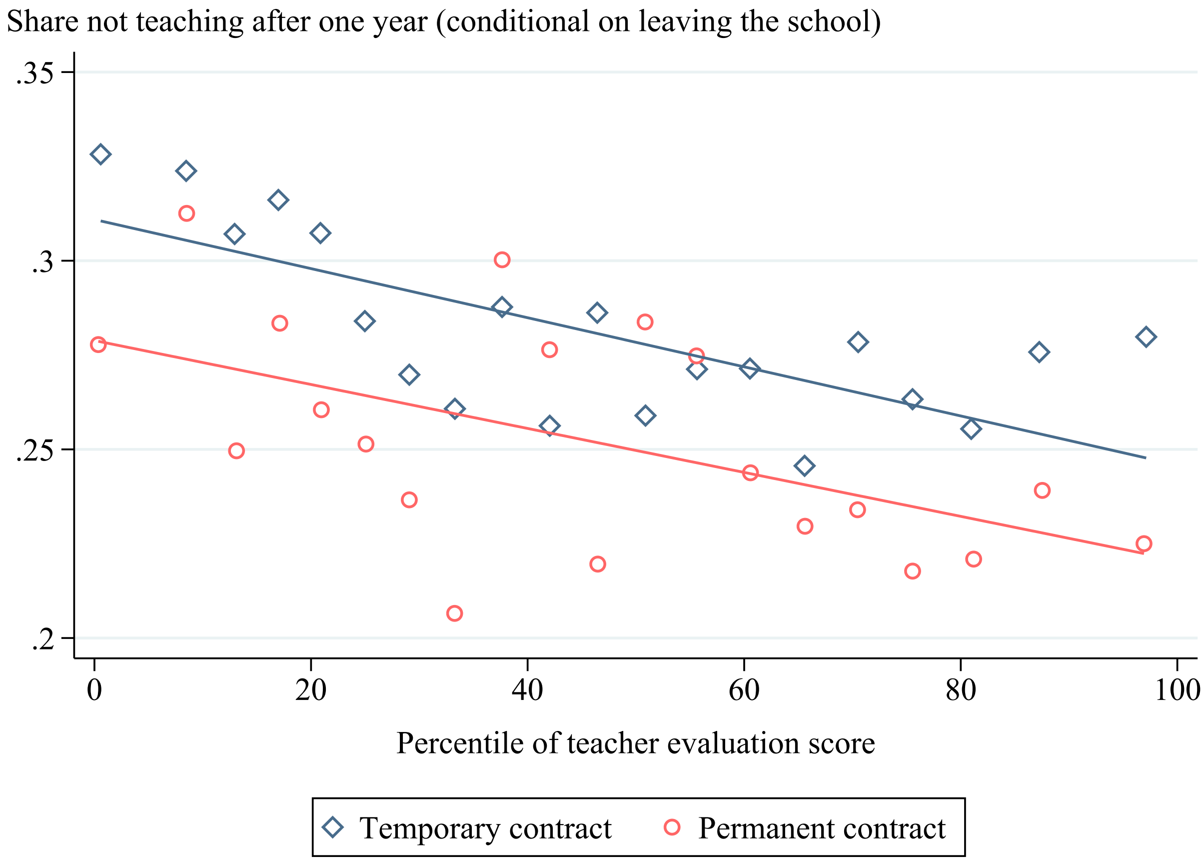
Notes: The sample is composed of teachers in 2010-2013 who were teaching 20 or more hours in a K-12 public school and were younger than age 55. This figure shows the share of teachers who were not working in the same school after one year, separated by total years of experience in the municipality. The grey bars show these figures for all teachers, the blue bars for teacher with a temporary contract, and the red bars for those with a permanent contract. We only consider that a teacher has accumulated a year of experience in a municipality if he/she works for 20 or more hours a week during that year.

Figure A.2: Teacher turnover by type of contract and teacher evaluation scores, 2010-2013



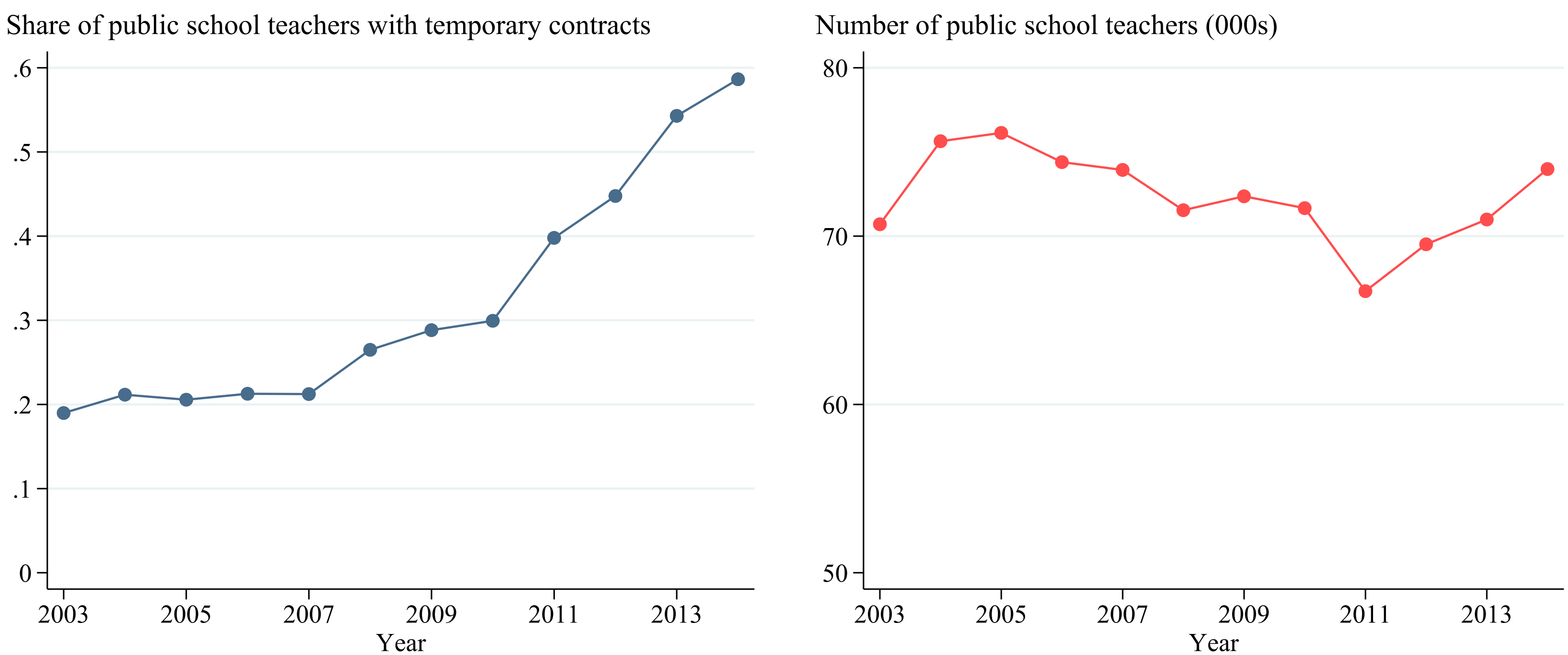
Notes: The sample is composed of teachers in 2010-2013 who were teaching 20 or more hours in a K-12 public school, were younger than age 55 and participated in a teaching evaluation. This figure plots the share of teachers who were not working in the same school after one year against the evaluation score percentile, using the score in the first evaluation the teacher participated in. We split the sample by whether the teacher has a permanent or a temporary contract. The lines plot the predicted values of a linear regression controlling for year and municipality fixed effects. The markers plot the average residuals (with the mean added back) of a regression of a dummy for whether the teacher is not in the same school after one year against year and municipality fixed effects. These means are computed for equal-sized bins of percentiles. This figure was constructed using the *binscatter* command.

Figure A.3: Teachers who are not teaching after one year (conditional on turnover) by type of contract and teacher evaluation scores, 2010-2013



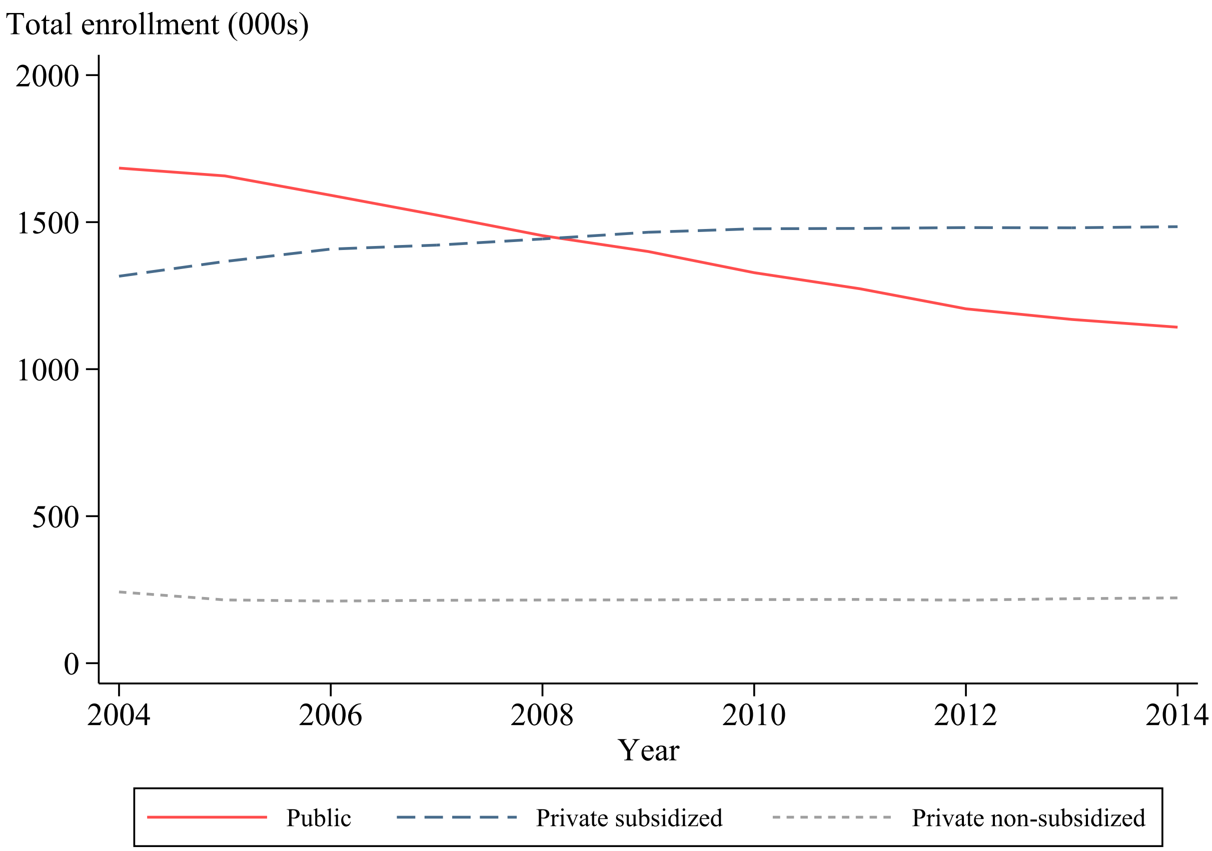
Notes: The sample is composed of teachers in 2010-2013 who were teaching 20 or more hours in a K-12 public school, were younger than age 55, participated in a teaching evaluation, and were not working in the same school in the following year. This figure plots the share of teachers who were not teaching after one year against the evaluation score percentile, using the score in the first evaluation the teacher participated in. We split the sample by whether the teacher has a permanent or a temporary contract. The lines plot the predicted values of a linear regression controlling for year and municipality fixed effects. The markers plot the average residuals (with the mean added back) of a regression of a dummy for whether the teacher is not teaching after one year against year and municipality fixed effects. These means are computed for equal-sized bins of percentiles. This figure was constructed using the *binscatter* command.

Figure A.4: Evolution of share of K-12 teachers with permanent contracts and total number of K-12 teachers in public schools



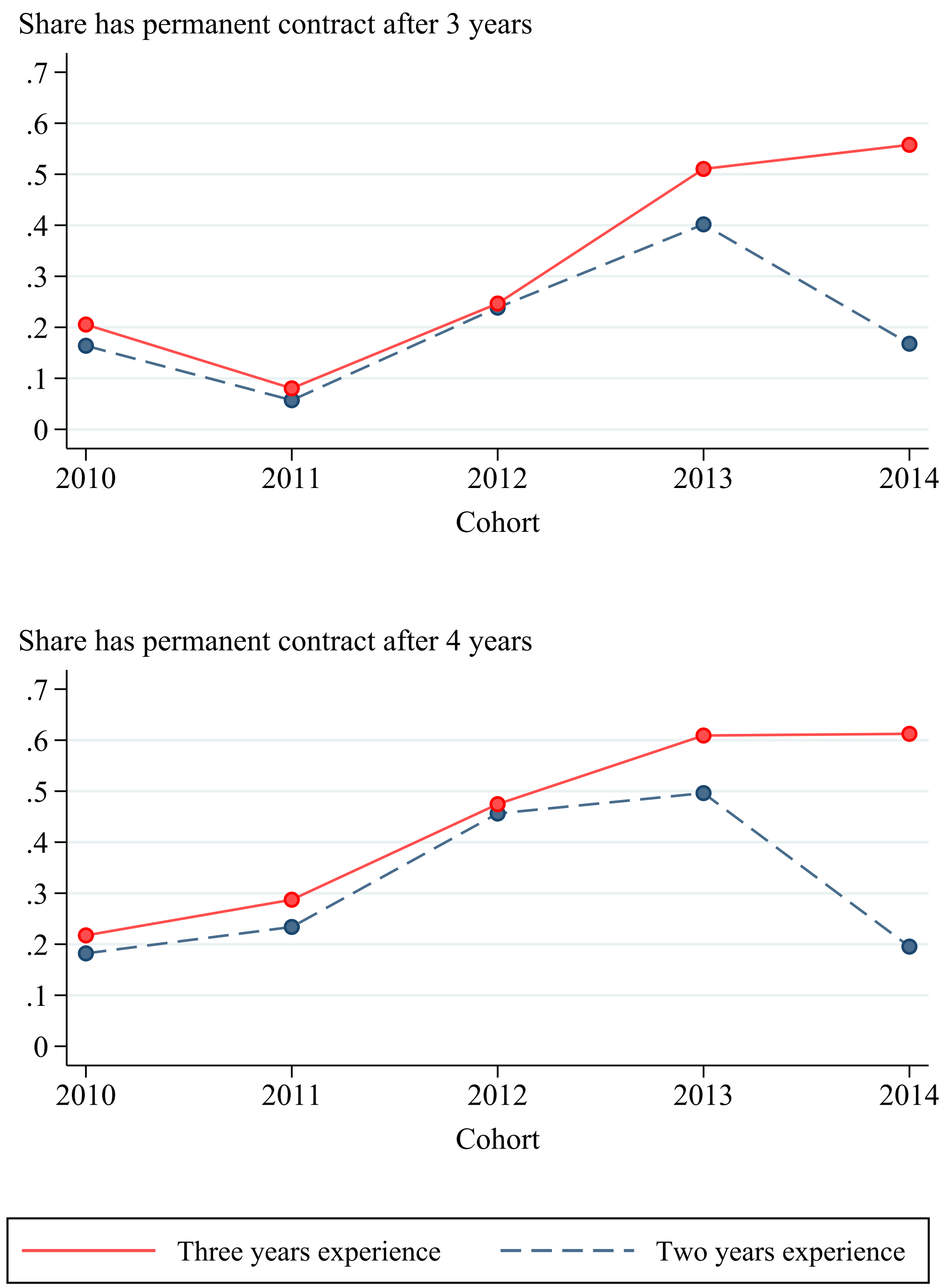
Notes: The figure to the left shows the share of K-12 teachers in public schools that had a temporary contract, and the figure to the right shows the total number of K-12 teachers employed in public schools in each year, regardless of their contract type.

Figure A.5: Evolution of enrollment in K-12 in public and private schools



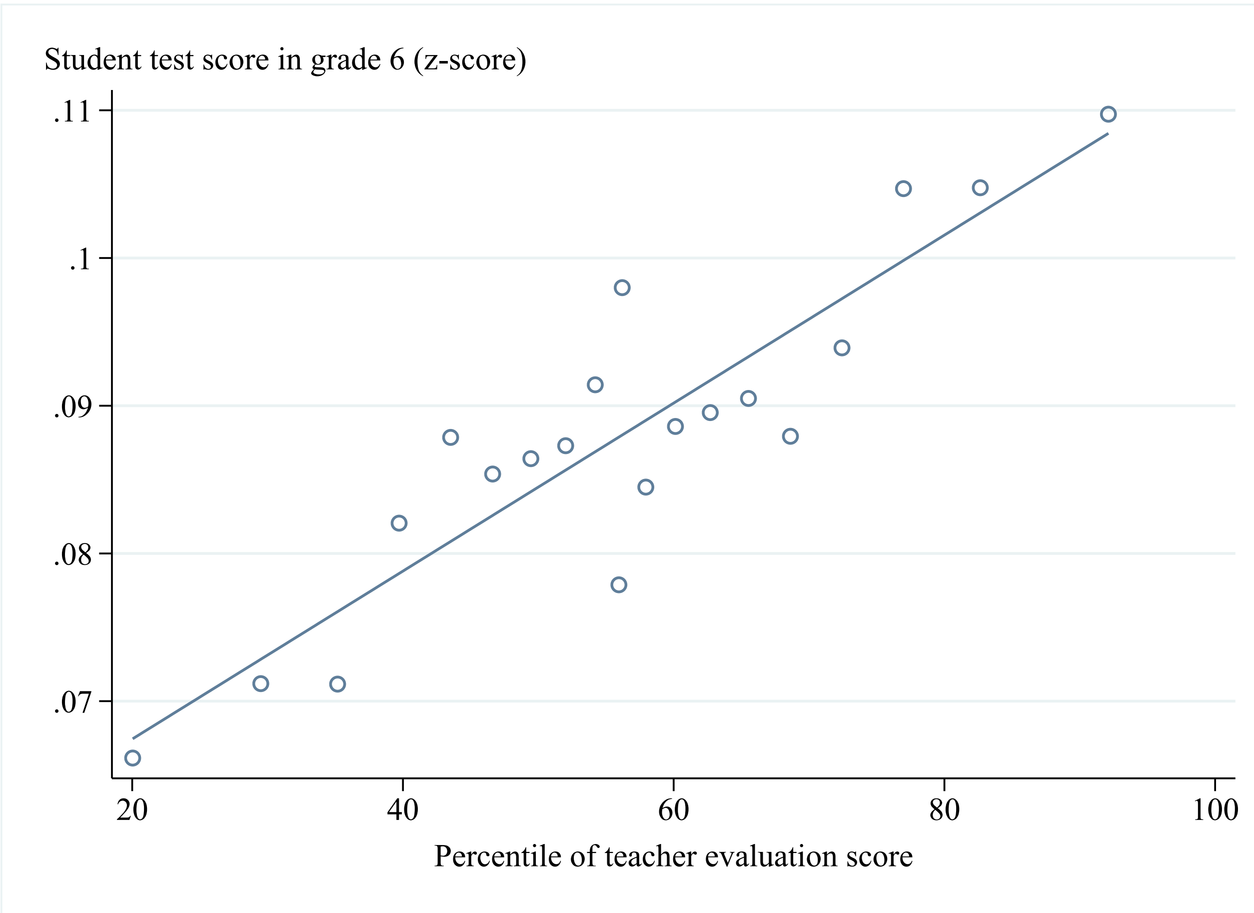
Notes: This figure shows the total number of students enrolled in K-12 by school type.

Figure A.6: Share of teachers who had a permanent contract after 3-4 years



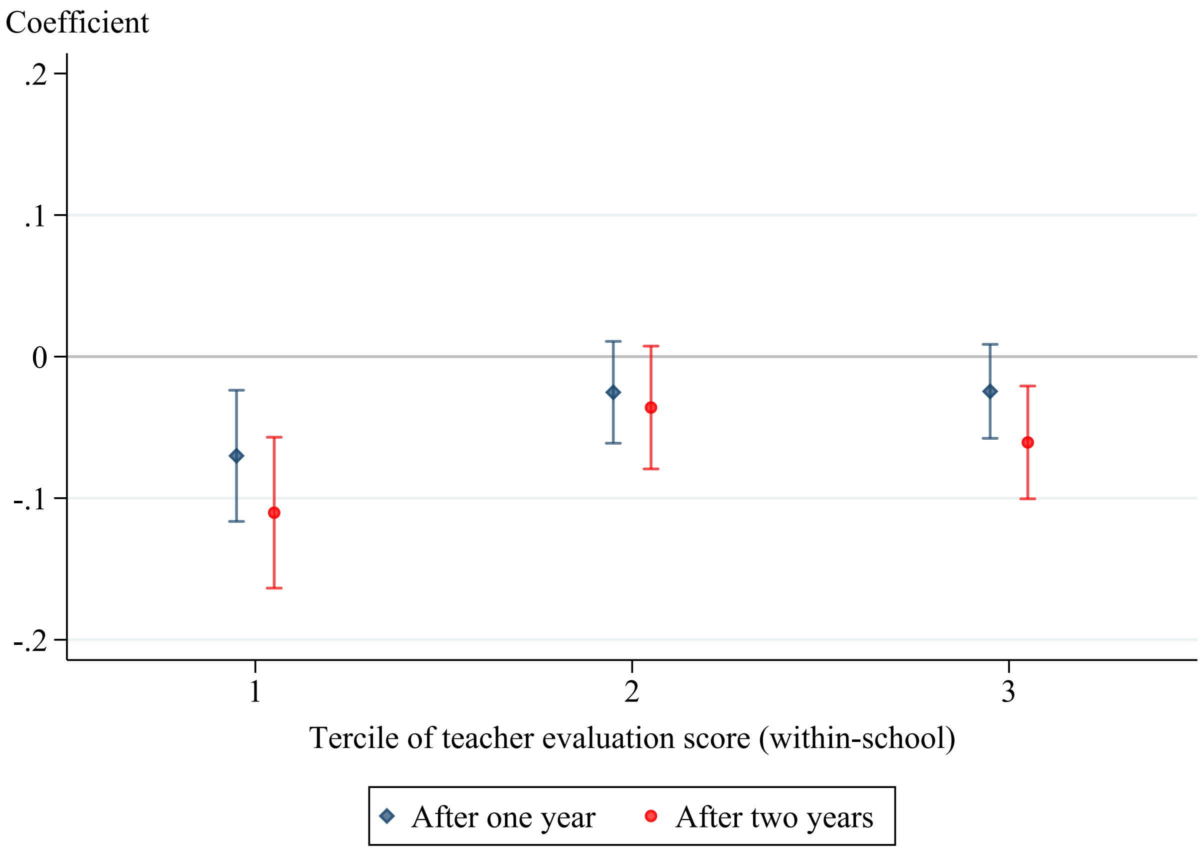
Notes: The top (bottom) figure shows the share of teachers who obtained a permanent contract after three (four) years. We consider that a teacher obtained a permanent contract if he/she had tenure at some point, regardless of whether the teacher left the public school system after that. The sample in each cohort is composed of teachers who were teaching 20 or more hours in a K-12 public school under a temporary contract, and were younger than age 55. The solid line shows the mean outcome for teachers with three years of consecutive experience in the same municipality (the treatment group), and the dotted line reports the same for teachers that had two years of consecutive experience (the comparison group).

Figure A.7: Conditional correlation between student test scores and teacher evaluation scores



Notes: The sample is composed of 6^{th} grade public school students in 2013-2015 who took both the math and literacy SIMCE test, have a 4^{th} grade SIMCE score for both subjects, and have a teacher that was evaluated in both subjects. The line plots the predicted values of a linear regression where the dependent variable is the student's score in the math and literacy SIMCE evaluation (z-score) and the main regressor is the percentile in a nationwide teaching evaluation of their teacher in that subject. The regression controls for the student's score in that same subject in the 4^{th} grade SIMCE evaluation, subject fixed effects, and student-year fixed effects. The markers plot the average residuals (with the mean added back) of this regression. These means are computed for equal-sized bins of percentiles. This figure was constructed using the *binscatter* command.

Figure A.8: Impact of high dismissal protection on teacher turnover – Heterogeneous effects by teacher evaluation scores (within-school distribution)



Notes: The sample is composed of teachers who were teaching 20 or more hours in a K-12 public school under a temporary contract in 2010–2014, were younger than age 55, participated in a teaching evaluation, and had two or three years of consecutive experience in the same municipality. This figure presents the main coefficients and 95 confidence interval for the estimation of equation (1) fully interacted with dummies for each tercile in the within-school distribution of teacher evaluation scores. The estimates where the dependent variable is measured after one year are presented with a diamond, and the estimates for two years after are depicted with a circle.

Table A.1: Impact of high dismissal protection on teacher turnover after three and four years

	After three years			After four years		
	Not in same school	Left public school system	In another public school	Not in same school	Left public school system	In another public school
Treated × Post	-0.074***	-0.046***	-0.029***	-0.076***	-0.057***	-0.019*
	(0.013)	(0.009)	(0.011)	(0.013)	(0.010)	(0.011)
Treated	-0.026***	-0.022***	-0.004	-0.031***	-0.022***	-0.009*
	(0.006)	(0.005)	(0.005)	(0.006)	(0.004)	(0.005)
Observations	24,002	24,002	24,002	24,002	24,002	24,002
R^2	0.015	0.012	0.003	0.015	0.010	0.004
Dependent variable mean (control)	0.335	0.145	0.190	0.395	0.189	0.206

Notes: The sample is composed of teachers who were teaching 20 or more hours in a K-12 public school under a temporary contract in 2010–2014, were younger than age 55, and had two or three years of consecutive experience in the same municipality. The dependent variables in columns 1 and 4 are dummies for whether the teacher was not teaching in the same school after three and four years, respectively. In columns 2 and 5, the dependent variables are dummies for whether the teacher was not teaching in a public school after three and four years. In columns 3 and 6, the dependent variables are dummies for whether the teacher was working in a different public school after three and four years. *Treated* is a dummy for whether the teacher has three years of consecutive experience in that year, and *Post* is a dummy for the 2014 cohort. The regressions also include cohort fixed effects. Standard errors clustered by teacher are in parentheses. The dependent variable mean reported shows the average value of the dependent variable for the sample of teachers that had two years of experience in 2014. * significant at 10%; ** significant at 5%; *** significant at 1%

Table A.2: Impact of high dismissal protection on turnover — Placebo exercises

	(1)	(2)
Three years × Post	0.032	
	(0.033)	
Four years × Post		0.017
		(0.015)
Observations	6,409	15,357
R^2	0.004	0.016
Dependent variable mean (control)	0.205	0.175
Sample: type of contract	Permanent	Temporary
Sample: years of experience	2-3	3-4

Notes: The sample in column (1) is composed of teachers who were teaching 20 or more hours in a K-12 public school under a permanent contract in 2010–2014, were younger than age 55, and had two or three years of consecutive experience in the same municipality. The sample in column (2) is composed of teachers who were teaching 20 or more hours in a K-12 public school under a temporary contract in 2010–2014, were younger than age 55, and had three or four years of consecutive experience in the same municipality. In both columns, the dependent variable is a dummy for whether the teacher was not working in the same school after two years. The regressors in column (1) are a dummy for whether the teacher has three years of consecutive experience in that year (*Three years*), cohort fixed effects, and the interaction between *Three years* and a dummy for the 2014 cohort. The regressors in column (2) are a dummy for whether the teacher has four years of consecutive experience in that year (*Four years*), cohort fixed effects, and the interaction between *Four years* and a dummy for the 2014 cohort. Standard errors clustered by teacher are in parentheses. The dependent variable mean reported shows the average value of the dependent variable for the sample of teachers that had two years of experience in 2014 (column 1) and three years of experience in 2014 (column 2). * significant at 10%; ** significant at 5%; *** significant at 1%

Table A.3: Impact of high dismissal protection on test scores – Robustness checks

	Including students w/o lagged score		Dropping teachers with < 2 years of exp.		Sample with evaluated teachers	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-0.012	-0.002	-0.017	-0.016	-0.015	-0.005
	(0.016)	(0.018)	(0.016)	(0.020)	(0.017)	(0.020)
Treated	0.021**	0.026	0.025**	0.032	0.020*	0.027
	(0.010)	(0.020)	(0.010)	(0.022)	(0.011)	(0.021)
Lagged score			0.427***	0.417***	0.430***	0.420***
			(0.003)	(0.003)	(0.003)	(0.003)
Observations	470,630	470,622	342,016	341,976	319,592	319,562
R^2	0.806	0.827	0.834	0.853	0.834	0.852
Student-year FE	✓	✓	✓	✓	✓	✓
Teacher FE		✓	✓	✓		

Notes: The sample is composed of 6th grade public school students in 2013-2015 who took both the math and literacy SIMCE test. In columns (3)-(6), the sample is also restricted to students that have a 4th grade SIMCE score for both subjects. In columns (3)-(4), we drop students who in one of the subjects have a teacher with less than two years of teaching experience. In columns (5)-(6), we restrict the sample to students for whom both the math and language teachers were evaluated. The dependent variable is the student's score in the math or literacy SIMCE evaluation (z-score). *Treated* is a dummy for whether the student's teacher in that subject had a temporary contract and at least three years of consecutive experience (or at least four years of total experience) in that municipality the year before. We include this variable by itself, as well as interacted with *Post* (a dummy for the year 2015). *Lagged score* is the student's score in the same subject in the 4th grade SIMCE evaluation (z-score). The regressions also control for subject fixed effects and student-year fixed effects. When indicated, the regression controls for teacher fixed effects. Standard errors clustered at the teacher-year level are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table A.4: Impact of high dismissal protection on test scores – Pretrends

	(1)	(2)
Treated × Post	-0.001	0.012
	(0.017)	(0.022)
Treated × 2014	0.029	0.032
	(0.019)	(0.020)
Treated	0.009	0.001
	(0.013)	(0.023)
Observations	416,948	416,898
R^2	0.833	0.852
Lagged scores and student-year FE	✓	✓
Teacher FE		✓

Notes: The sample is composed of 6^{th} grade public school students in 2013-2015 who took both the math and literacy SIMCE test, and have a 4^{th} grade SIMCE score for both subjects. The dependent variable is the student's score in the math or literacy SIMCE evaluation (z-score). *Treated* is a dummy for whether the student's teacher in that subject had a temporary contract and at least three years of consecutive experience (or at least four years of total experience) in that municipality the year before. We include this variable by itself, as well as interacted with *Post* (a dummy for the year 2015), and with a dummy for the year 2014. We also control for the student's score in the same subject in the 4^{th} grade SIMCE evaluation, subject fixed effects, and student-year fixed effects. In column 2, the regression controls for teacher fixed effects as well. Standard errors clustered at the teacher-year level are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table A.5: Impact of high dismissal protection on test scores – Placebo using 4th grade scores as outcome

	(1)	(2)
Treated × Post	0.002	0.004
	(0.012)	(0.014)
Treated	0.002	0.006
	(0.007)	(0.016)
Observations	416,948	416,898
R^2	0.838	0.849
Student-year FE	✓	✓
Teacher FE		✓

Notes: The sample is composed of 6^{th} grade public school students in 2013-2015 who took both the math and literacy SIMCE test, and have a 4^{th} grade SIMCE score for both subjects. The dependent variable is the student's score in the math or literacy SIMCE evaluation in grade 4 (z-score). *Treated* is a dummy for whether the student's teacher in that subject had a temporary contract and at least three years of consecutive experience (or at least four years of total experience) in that municipality the year before. We include this variable by itself, as well as interacted with *Post* (a dummy for the year 2015). The regressions also control for subject fixed effects and student-year fixed effects. In column 2, the regression controls for teacher fixed effects as well. Standard errors clustered at the teacher-year level are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table A.6: Impact of high dismissal protection on teacher characteristics

	Age	Female	Has education degree	Weekly hours teach.	Main role teacher	More than one school	New to school	Was evaluated	Evaluation score percentile	Total years of teaching experience: 0	1	2	3	4 or more
Treated × Post	-0.531	-0.004	0.006	0.410	0.001	0.002	0.026	-0.041**	0.427	0.015	0.047***	-0.005	-0.016	-0.041*
	(0.849)	(0.032)	(0.006)	(0.336)	(0.010)	(0.010)	(0.024)	(0.018)	(2.071)	(0.012)	(0.014)	(0.014)	(0.016)	(0.025)
Treated	-4.117***	0.027	0.002	0.107	0.008*	-0.006	-0.123***	0.122***	-2.337*	-0.056***	-0.090***	-0.082***	-0.078***	0.306***
	(0.521)	(0.020)	(0.002)	(0.214)	(0.005)	(0.007)	(0.018)	(0.013)	(1.233)	(0.008)	(0.011)	(0.009)	(0.009)	(0.016)
Observations	390,576	390,576	390,576	390,576	390,576	390,576	390,576	390,576	347,370	390,576	390,576	390,576	390,576	390,576
R^2	0.588	0.588	0.514	0.710	0.726	0.540	0.558	0.571	0.611	0.532	0.542	0.539	0.554	0.580
Dependent variable mean (control)	45.220	0.718	0.994	38.575	0.966	0.011	0.120	0.908	56.194	0.030	0.049	0.077	0.085	0.758

Notes: The sample is composed of 6^{th} grade public school students in 2013-2015 who took both the math and literacy SIMCE test, and have a 4^{th} grade SIMCE score for both subjects. The dependent variable is the characteristic of their math or literacy teachers in that year indicated in the column header. *Treated* is a dummy for whether the student's teacher in that subject had a temporary contract and at least three years of consecutive experience (or at least four years of total experience) in that municipality the year before. We include this variable by itself, as well as interacted with *Post* (a dummy for the year 2015). We control for the student's score in the same subject in the 4^{th} grade SIMCE evaluation, subject fixed effects and student-year fixed effects. Standard errors clustered at the teacher-year level are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Appendix B Direct measurement of teacher effort

Table B.1: Questions used to measure teacher effort

Source:	Student survey questionnaire, SIMCE 2014 and 2015.
Instructions:	For each statement, mark the alternative that best describes what your Language and Communication teacher does in class.
Questions:	The teacher reviews the homework assignments of all students. The teacher explains in class the correct answers to homework assignments. The teacher explains further when a student asks for it. The teacher explains a concept until all students understand it. The teacher explains in class the correct answers to exams. The teacher explains in class the correct answers to study guides and exercises that he/she distributes among students.
Response options:	1. Never 2. Almost never (A few times) 3. Many times 4. Always
Notes:	The 2014 questionnaire used the "2. Almost never" response option, while the 2014 questionnaire used "2. A few times".