IDB WORKING PAPER SERIES Nº IDB-WP-1148

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Samuel Berlinski
Alejandra Ramos

Inter-American Development Bank
Department of Research and Chief Economist

August 2020

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Samuel Berlinski*
Alejandra Ramos**

* Inter-American Development Bank
** Trinity College Dublin

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Cataloging-in-Publication data provided by the
Inter-American Development Bank
Felipe Herrera Library

Berlinski, Samuel, 1970-
Peer effects in the decision to apply for a professional excellence award / Samuel Berlinski, Alejandra Ramos.
p. cm. — (IDB Working Paper Series ; 1148)
Includes bibliographic references.
1. Performance awards-Chile-Econometric models. 2. Teachers-Salaries, etc.-Chile-Econometric models. 3. Merit pay-Chile-Econometric models. I. Ramos, Alejandra. II. Inter-American Development Bank. Department of Research and Chief Economist. III. Title. IV. Series.
IDB-WP-1148

http://www.iadb.org

Abstract

We study the role social interactions at the workplace play in the decision to apply for a professional recognition program. In Chile, teachers can apply to a pedagogical excellence award. Successful applicants receive a wage increase and are publicly recognized. We exploit the quasi-random variation in the allocation of awards generated by a sharp assignment rule. We document that the success of an applicant increases her school colleagues' application rate to the program by almost 75 percent. The impact is higher for colleagues with closer interaction with a successful applicant. We speculate on social learning as a driver of this result.

Keywords: Merit award, Public recognition, Peer effects, Social learning
JEL Classification: J33, J58, M52

We thank Hugo Jales, Menno Pradhan, Mauricio Romero, Miguel Sarsoza, Laura Schechter, Sergio Urzua, Bas van der Klaauw, two anonymous referees and participants in the AASLE, RIDGE Labor Forum and the seminars of UCD and the Tinbergen Institute for their valuable comments and suggestions. We thank the Ministerio de Educación in Chile and its Centro de Estudios for sharing the data and knowledge about the program with us. The views expressed herein are those of the authors and should not be attributed to the Inter-American Development Bank, its Executive Directors, or the governments they represent. Authors' emails: samuelb@iadb.org; alejandra.ramosmoreno@gmail.com

1 Introduction

Social connections play an important role in the workplace. Neighbors and relatives affect the probability of finding a job and employment conditions (Bayer, Ross, and Topa, 2008; Pellizzari, 2010). Effort levels and pay are influenced by the workplace social network even in the absence of team-based compensation (Falk and Ichino, 2006; Mas and Moretti, 2009; Bandiera, Barankay, and Rasul, 2010; Park, 2019). The take-up of employment related benefits such as paternity leave (Dahl, Loken, and Mogstad, 2014) and retirement plans (Duflo and Saez, 2003) is also impacted by such interactions. Peer effects may also arise in occupational choices (Jones and Kofoed, 2020) and entrepreneurship (Markussen and Røed, 2017). Nonetheless, there is little evidence about the role of social interactions on other important career decisions such as applying for promotions or taking-up certification credentials that may lead to wage increases or other career prospects.

Chile's *Pedagogical Excellence Award* (Asignación a la Excelencia Pedagógica - AEP) is a program from the Ministry of Education that pays successful public school applicants a 6 percent yearly wage increase for up to 10 years in recognition for excellence in teaching practice. Applications to the program are voluntary, and around 5 percent of eligible teachers apply. To receive the award teachers must demonstrate their expertise by preparing a teaching portfolio and taking a knowledge test. The results of both assessments are combined in a final score and only those scoring above a certain threshold receive the award. About 26 percent of applicants pass and their success is publicly announced. Using a sharp regression discontinuity design, Berlinski and Ramos (2020) identify the causal effect of the program on mobility within the school system and out of the teaching career. Locally, obtaining the award does not alter transitions out of the school system in a five-year window. However, it has a positive effect on the likelihood of switching to a new public school, consistent with the idea that it provides teachers with a previously unobservable signal of quality.

In this paper, we focus on the information that a successful award conveys to colleagues. We rely on administrative records for the universe of school teachers in Chile between 2003 and 2011 to build a colleagues' network for each awardee. We estimate the causal impact of a successful AEP awardee on her colleagues' future application exploiting the quasi-random variation generated by the sharp allocation rule. Despite the fact that the AEP is a well known and established program, we find that having a successful peer applicant at the school plays a significant role in the behavior of prospective applicants. Our findings suggest that the success of an applicant increases her school

colleagues application rate to the program by almost 75 percent. We show that this result is robust to several specification checks.

We speculate on the mechanism driving this result. We show that the closeness of the social connection between award winners and prospective applicants matters. Prospective applicants in the same school district where the public announcement of the awards is made are unaffected by a successful applicant in other schools. However, colleagues teaching in the same grade as the applicant are more affected than those elsewhere in the school. This likely reflects the fact that the effect we estimate is not driven by the public information revelation of the existence of the award only but by a more subtle social interaction process. Thus, we go a step further and propose a learning model where prospective applicants learn both from successful and failed applicants. If an applicant of lower quality passes the test, her colleagues increase the likelihood of applying for the award. If an applicant of higher quality fails the test, her colleagues are less likely to apply for the award. These hypotheses are consistent with the data but are imprecisely estimated.

Our paper directly relates to the literature on peer effects and public recognition in the workplace.[1] Ashraf, Bandiera, and Lee (2014) find that public recognition increases effort in an experimental study among health workers in Zambia. Ager, Bursztyn, and Voth (2016) observe positive performance effects from public recognition among German pilots during World War II. In the context of a nation-wide and well-established public sector program, our paper supports the idea that public recognition has spillover effects in the workplace.

This paper is also a reminder that social interactions are to be considered when designing personnel policies (see Ashraf and Bandiera (2018) for an overview of this literature). When worker quality is observed imperfectly in the labor market, promotions and merit awards provide prospective employers a valuable signal of worker quality (Spence, 1973; Bates, 2020). Yet, when relevant information about the cost and benefits of seeking promotions or obtaining merit-based pay is unknown, employees may use their peers' previous experience to update their priors. As a result, employers need to internalize that publicly announcing performance awards creates a signal both for employers and other employees.

In the literature of peer effects in program take-up our approach complements those in Dahl et al. (2014) and Moreira (2019). Dahl et al. (2014) study the effect of social

[1] We refer the reader to Sacerdote (2014) for an excellent review of the existing experimental and quasi-experimental evidence of peer effects.

interactions in the take-up of paternity leave in Norway. Fathers of children born after April 1, 1993, were eligible for one month paid paternity leave, while fathers of children born before this date were not. The authors find that the coworkers and brothers of eligible fathers are more likely to take paternity leave. Their identification restricts to networks with a single peer father in the reform window as, when there is more than one peer father, it is not clear how to define the running variable. Moreira (2019) overcomes the multiple-peers issue by limiting to the peer closest to the discontinuity threshold.[2] We propose a new strategy to identify peer effects through a regression discontinuity design when there are multiple peers affecting the same individual. Instead of defining the dependent variable at the level of a prospective applicant whose behavior is affected by several applicants, we use as a dependent variable an aggregate measure of the behavior of the colleagues of each applicant. By doing so, we turn a problem of multiple applicants affecting a common prospective applicant into a problem of one applicant affecting her colleagues' behavior.

The rest of the paper is organized as follows. In Section 2, we provide background on the Chilean education system and the design of the program. In Section 3, we describe the data and the sample used. Section 4 presents our identification strategy and Section 5 tests the validity of the regression discontinuity design. Section 6 presents our main results and section 7 explores potential mechanisms underlying the estimated effects. Section 8 concludes.

2 Background

Primary and secondary education in Chile is provided by *municipal* schools, *private* schools, and *private-subsidized* schools. The public education system is characterized by a nationwide voucher scheme. Private schools receive no subsidies from the government, while private-subsidized schools and municipal schools receive the same per-student subsidy.[3] The contractual arrangements offered to teachers also differ across the three types

[2] Moreira (2019) compares classrooms with narrow winners and losers of Brazil's Math Olympiad Honorable Mention and finds that the award improves future educational outcomes of both the winner and her classmates.

[3] *Municipal* schools are non-profit institutions, administered by municipalities, that offer instruction for free and receive a per-student subsidy from the Ministry of Education. *Private* schools are for-profit institutions that charge tuition and receive no subsidies from the government. *Private-subsidized* schools are for-profit institutions administered by private corporations that charge tuition and receive a per-student subsidy from the Ministry of Education. See Hsieh and Urquiola (2006) for further description.

of providers. The employment of municipal school teachers follows a *Teacher Statute* negotiated by the teacher's union; the private sector follows the standard labor law; and private-subsidized schools retain some aspects of both (Mizala and Romaguera, 2005; Santiago, Benavides, Danielson, Goe, and Nusche, 2013).[4] The majority of the teaching labor force is distributed between private-subsidized and municipal schools. Likewise, these two types of providers capture more than 90 percent of student enrollment. Throughout the paper, we refer to municipal and private-subsidized schools as the voucher system.

In the late 1990s and early 2000s, the Chilean government implemented a battery of performance incentive policies within the voucher system (Mizala and Schneider, 2014a,b). Starting in 2002, teachers of municipal and private-subsidized schools willing to demonstrate their teaching excellence can apply for the Pedagogical Excellence Award (*Asignación a la Excelencia Pedagógica*) or AEP – following its Spanish acronym. The program is available for any teacher working at least 20 hours a week at municipal or private-subsidized schools. To receive the award, teachers must prepare a teaching portfolio and take a written test in their main area of expertise.[5] In the portfolio, teachers demonstrate their teaching practices. This assessment requires a learning plan for the students, an evaluation strategy, a pedagogical reflection, and a recording of a class (Rodriguez, Manzi, Peirano, Gonzalez, and Bravo, 2015). The written test evaluates teachers on grounds of their academic knowledge.

The results of the two assessments are combined (70 percent the portfolio and 30 percent the written test) in a final score ranging from 100 to 400. Only teachers with a final score of at least 275 receive the award (see Figure 3). We identify this cut-off by inspecting the data. To the best of our knowledge, there is no official document where the threshold is stated.[6] Neither applicants nor schools are informed of the exact value of the cut-off.

Receiving the AEP entitles awardees a financial incentive equivalent to a 6 percent yearly wage increase for up to 10 years. The exact magnitude of the bonus varies at four certification tracks defined by years of experience in the education sector: 0-11 years,

[4]For instance, teachers who reach retirement age (60 years for women and 65 for men) can work at private-subsidized schools, but not at municipal schools. At the same time, the minimum wages, bonuses, and maximum working hours in private-subsidized schools follow the same standards as municipal schools and are centrally negotiated by the teachers' union.

[5]The design of the program is similar to that of the US National Board of Professional Teaching Standards Program (Elliott, Koenig, and Hakel, 2008).

[6]The information was confirmed by the *Centro de Perfeccionamiento, Experimentación e Investigaciones Pedagógica* (CPEIP) in internal correspondence.

12-21 years, 22-30 years, and 31 plus years. Within each of these levels, teachers can apply to the program at most twice. The bonus is paid by the government, irrespective of the school, as long as the teacher works a minimum of 20 hours per week in the voucher system.

In addition to the monetary reward, successful applicants receive a diploma and the award is publicly announced. The official award credibly signals teacher quality throughout the entire education system and the social recognition component makes it salient among the educational community. Social recognition takes place in regional ceremonies that present the awardees. These ceremonies are organized by the Regional Ministerial Secretaries of Education (*Secretarías Regionales Ministeriales de Educación* or SEREMIS) and have extensive local media coverage.

The AEP process begins in April, right after the beginning of the school year. Throughout the month, printed materials are disseminated across schools and teachers receive e-mails inviting them to apply (Rodriguez et al., 2015). Teachers can enroll in the program from April to May. In June, once enrollment is closed, the rubric for the teaching portfolios are distributed. The portfolios are submitted in October and in November the written examination takes place. Around March, every applicant is privately informed about her final score and her performance in the two assessments. Throughout the month, awardees are publicly announced. We present this timeline in Figure 1.

After 2011, several components of the program were restructured: the duration of the financial incentive was reduced from ten to four years, the amount of the payments and the certification tracts were adjusted by performance in the assessment rather than by experience, and the weights of the portfolio and the knowledge test were readjusted. We consider these changes a complete restructuring of the AEP and concentrate our analysis on the 2003–2011 period.

3 Data and Descriptive Statistics

We use administrative data for the universe of teachers in the school system published yearly by the Ministry of Education. The data are available starting in 2003 and contain information on basic demographics, qualifications, experience, and place and hours of work for all active teachers. We match these data with the scores and award status of teachers who applied for the award between 2003 and 2011. There were 14,562 ap-

plications to the AEP during this period.[7] We concentrate our analysis on the 12,503 first-time applicants with complete administrative records.

Our main outcome variable is colleagues' application rate to the AEP one period ahead. Therefore, we have a well defined outcome variable for each application wave by excluding 2011 from our sample of peer applicants, although they are included as prospective applicant colleagues. In our main specification, we define colleagues as the teachers working at the same school as the applicant at the time she applied for the award.[8] In other words, any prospective applicant working at a school with an AEP applicant who has never applied to the program themselves belongs to that applicant's group of colleagues.

In Table 1 we present average characteristics for all active teachers between 2003 and 2011 (column one), the colleagues of the AEP applicants that are eligible for the AEP (column two), and the AEP applicants at the time of application (column three). In columns one and two, the same teacher can appear more than once as the unit of observation is the teacher-year pair. In contrast, in column one, an AEP applicant only appears at the time of her first application.

The average Chilean teacher is a 41-year-old female with 18 years of experience working 37 hours a week in a single school (column one). Almost six out of ten of these teachers work as primary school teachers, 26 percent work at a school that is currently receiving the SNED incentive,[9] 42 percent work at a private-subsidized school and 13 percent work at a rural school. The 1,562,142 teacher-year observations in column one correspond to 278,308 unique teacher observations working at 14,076 schools between 2003 and 2011.

At the time of application, the average AEP applicant is a 39 years old female with 15 years of experience, working 38 hours a week in a single school (column three). Half of the applicants work at private-subsidized schools, 12 percent work at rural schools, and 35 percent work at a school that is receiving SNED. This means that when they apply to the program, AEP applicants are almost 2 years younger than the average eligible teacher and as a result, have less experience. The descriptive statistics for the sample of

[7] We eliminate 2002 AEP applicants because of a lack of administrative data.

[8] For teachers working at multiple schools, we restrict to the school with the largest share of hours worked. However, only 10 percent of teachers work at more than one school.

[9] The government offers a school based performance incentive, the National System for Performance Evaluation (Sistema Nacional de Evaluación del Desempeño - SNED) for all schools in the voucher school system. Teachers in selected schools receive an annual bonus equivalent to 50-70 percent of a teacher's monthly salary (Mizala and Schneider, 2014a).

colleagues are in general similar to the ones of the average teacher (column two).[10]

The number of first-time applicants to the AEP suggests that the application rate to the program is low. Only 5.3 percent of the eligible teachers applied for the program in the 2003-2011 period (top panel of Table 2). On average, one out of every four of these applicants received the award. Nonetheless, the passing rate is higher for earlier application waves.

The ratio between the number of schools with a first-time applicant relative to the overall number of schools suggests that applications are not evenly distributed across schools. In Figure 2, we present the distribution of the total number of applications in a school in the entire 2003-2011 period among schools with at least one applicant. The vast majority of schools have only one applicant, yet, other schools have as many as 30 applicants during the entire period.

The bottom panel of Table 2 documents the distribution of applicants across schools. During the period we study, 34 percent of the schools had at least one teacher applying to the program. Every year, between 5 and 10 percent of the schools have at least one applicant. Naturally, the share of first-time school applications falls over time.

4 Identification Strategy

For a given individual, the decision to apply to the award depends on several factors, including the likelihood of passing the test, the cost of preparing for it, and the benefits that she may draw from the program. While some of this information is known, other may need to be inferred from work peers that had applied to the program. Yet, the success of one's colleagues can be determined by factors that simultaneously influence the decision to apply to the program and are unobserved by the econometrician. To overcome this identification problem, we rely on the quasi-random variation in the allocation of the award around the discontinuity threshold.

Our goal is to measure the causal impact of an applicant's success in obtaining the award on her colleagues' future application to the program. We estimate our parameter

[10]About 6 percent of colleagues work at rural schools, the difference with the overall average reflects the fact that urban schools have five times as many teachers as rural schools and AEP applicants are more likely to come from urban schools.

of interest using the following sharp regression discontinuity design (RDD):

$$R^t_{jcw} = \alpha + \beta D_j + \gamma f(s_j) + \delta D_j \times f(s_j) + \lambda_w + \epsilon^t_{jcw}. \tag{1}$$

For a teacher j who applied for AEP at wave w while she was working at school c (or other relevant domain for the definition of a colleague), the outcome variable R^t_{jcw} is the share of her colleagues applying for the award at wave $w+t$; D_j is a dummy variable equal to 1 if j scored at least 275 and 0 otherwise; s_j is j's score centered around the 275 cut-off; the function $f(s_j)$ is a suitable polynomial of the score varying at both sides of the cutoff; and λ_w is a set of application wave fixed effects. Our parameter of interest is β.

We compute applicant j colleagues' future application rate in school c as follows:

$$R^t_{jcw} = \frac{\sum_{i \in N_{w+t}} y_{i,w+t} \;\; g^w_{ijc}}{\sum_{i \in N_{w+t}} g^w_{ijc} \, (1 - \sum_{\tau=1}^{w} y_{i,\tau})}, \tag{2}$$

where $y_{i,w+t}$ is a dummy variable that takes the value of 1 if teacher i applied for the AEP at time $w+t$ and 0 otherwise; N_{w+t} denotes the set of teachers working for more than 20 hours a week in voucher system schools at time $w+t$; and g^t_{ijc} is a dummy variable that takes the value of 1 if the teacher i was a colleague of applicant j in school c at time w. The numerator of equation (2) is the number of j's colleagues that applied for AEP at $w+t$. The denominator is the number of applicant j's colleagues that are eligible for the program at time $w+t$ and did not apply before for the AEP.

Similar to Berlinski and Ramos (2020), our main specification estimates equation (1) using a local non-parametric approach with a triangular kernel and a first-order polynomial of the score in the optimal bandwidth of Calonico, Cattaneo, and Titiunik (2014a). We present the bias-corrected coefficients of β and the robust corrected standard errors, clustered at the school of application as prescribed by Calonico, Cattaneo, and Titiunik (2014b).

Equation (1) allows us to identify spillovers in the presence of social interactions circumventing the conventional identification problems (Manski, 1993).[11] First, as we limit our analysis to first-time applications, there is no reflection problem. Once a teacher applies for the award ($y_{j,w+t} = 1$), her application decision is deterministic ($y_{j,w+\tau} = 0$ for all $\tau > t$) and her score only affects her colleagues.

Second, because the contracts and teaching assignments are determined at the be-

[11] See Dahl et al. (2014) for detailed discussion.

ginning of the school year, prior to the start of the AEP application process (see Figure 1), groups of colleagues are formed before the announcement of the awards. As a result, even if teachers self-select into schools; at the discontinuity threshold, the allocation of the award is orthogonal to the composition of the group of colleagues.

Third, because the AEP is quasi-randomly assigned around the cut-off, the award is also orthogonal to other regressors. Therefore, correlated unobservables are unlikely to be a source of bias. This is particularly relevant for schools with several applicants.

To clarify this last point, consider the case of two applicants $j = 1, 2$, and three prospective applicants $i = a, b, c$. Assume that a, b, and c were colleagues of 1 and 2 when 1 and 2 applied for the program. The application decision of a, b, and c can be written as

$$y_i = \alpha_i + \beta_1 D_1 + \gamma_1 s_1 + \beta_2 D_2 + \gamma_2 s_2 + \epsilon_i, \;\; \forall i = a, b, c,$$

Averaging the behavior of a, b and c, the application rate among the colleagues of applicant j can be written as:

$$R_j = \frac{y_a + y_b + y_c}{3} = \overline{\alpha} + \beta_1 D_1 + \gamma_1 s_1 + \beta_2 D_2 + \gamma_2 s_2 + \overline{\epsilon}, \;\; \forall j = 1, 2$$

where $\overline{\alpha} = \frac{\alpha_a + \alpha_b + \alpha_c}{3}$ and $\overline{\epsilon} = \frac{\epsilon_a + \epsilon_b + \epsilon_c}{3}$. Assuming $\beta_j = \beta$ and $\gamma_j = \gamma$, we can estimate the regression discontinuity design on the application rate through:

$$R_j = \overline{\alpha} + \beta D_j + \gamma s_j + \nu_j, \;\; \forall j = 1, 2,$$

where $\nu_j = \beta D_{-j} + \gamma s_{-j} + \overline{\epsilon}$. Given that our estimating equation omits s_{-j}, the underlying assumption for identification must be that $D_j | s_j$ is orthogonal to s_{-j}, $\forall j$. This assumption should hold both for current applicants and previous applicants.

Finally, we can also adopt $y_{i,w+t}$ as our outcome variable. In such a case, we require additional criteria to decide whether we want to use as regressors the score of j or the scores of the other applicants. Dahl et al. (2014)'s strategy is to limit the analysis to networks with one applicant peer.[12] Moreira (2019) uses as the source of variation the applicant peer closest to the discontinuity threshold. In comparison, our strategy defines the left-hand-side variable at the applicant peer level rather than at the prospective applicant level. Thus, we turn a multiple-to-one problem into a one-to-one problem.

[12] As we show in Section 6, our results are robust to pursuing this alternative approach.

5 Validity of the Regression Discontinuity Design

There is a sharp discontinuity in the assignment rule at the 275 score. In Figure 3, we provide graphical evidence for this fact. The circles represent the mean of a variable that takes the value of 1 if a teacher receives the award, and 0 otherwise. We plot these means against their corresponding scores.[13]

The basic identifying assumption of an RDD is that, around the cut-off, there is no systematic manipulation of the running variable (in this case, the score). There are at least two strategies to test the plausibility of this assumption (Bloom, 2012; Hahn, Todd, and Klaauw, 2001; Imbens and Lemieux, 2008; Lee and Card, 2008; Lee and Lemieux, 2010). First, there should be no kinks in the density of the score around the discontinuity. Second, predetermined factors ought to vary smoothly around the 275 cut-off.

In Figure 4 we plot the histogram of the AEP score. There is no visual evidence of kinks in the density of the score around the 275 threshold. In Table 3 we formally test the no discontinuity hypothesis. We present the p-values of the Calonico et al. (2014a)'s test and Frandsen (2017)'s test for variables with discrete support. The first column presents the results of the tests in the pooled sample. The remaining columns present the corresponding p-values for each application wave. The tests do not reject the null hypothesis either yearly or pooling all the applications wave together. Thus, the estimated densities to the left and to the right of the discontinuity overlap, and we cannot reject the no-discontinuity hypothesis.

We also provide evidence on the continuity of the predetermined factors around the continuity threshold. To do so, we estimate equation (1) using as outcome variables several characteristics of the teachers at the time of application and their corresponding schools. For each predetermined outcome, we use a non-parametric RDD specification in the variable-specific Calonico et al. (2014a) optimal bandwidth, a triangular kernel and a linear polynomial of the score.

The fourth column of Table 1 presents the estimated β coefficients with their corresponding standard errors in parenthesis. There is no evidence of systematic differences in the characteristics of awardees and non-awardees around the discontinuity threshold, neither for their schools. Importantly for our identification strategy, the number of previous AEP applicants at the school and the number of previous AEP awardees at the

[13] Applicants scoring 275 or more are not entitled to receive the award if they do not satisfy the eligibility conditions, i.e., work 20 hours a week or more at voucher system schools.

school are uncorrelated around the 275 cut-off. We consider this as evidence supporting the assumption that conditional on the scores, the award status of applicants are uncorrelated.[14]

6 Results

Prospective applicants evaluate the expected costs and benefits of applying to the program using the information available to them. On the one hand, there are monetary, time, and psychological costs of preparing a portfolio and taking the exam. On the other hand, the benefits from the program are a function of the monetary reward (which is probably well known for all teachers), its career impact (such as opportunities for promotion and changing schools) and any other status elements (such as prestige, hate or envy) linked to it. These costs and benefits have associated probability distributions which depend, among other factors, on how difficult it is to pass the threshold for the award.

How does the public announcement of a peer marginally obtaining an award affect the behavior of prospective applicants? The answer to this question will depend crucially on how much news a successful award conveys to each prospective applicant. Given that teachers are heterogeneous in ability and that they may also vary on their perception of costs and benefits from the impact of the award, *a priori*, the effect of the additional information is difficult to predict. Thus, whether there are spillover effects is eminently an empirical question.

In Figure 5 we summarize the relationship between the AEP score of an applicant and her colleagues' one period ahead application rate. We define colleagues as the prospective applicants that were working at the same school as the applicant when she applied for the program. The circles represent the unadjusted mean of the colleagues' application rate within bins of size 4 of the score. The superimposed solid lines are fitted values from a piecewise linear polynomial of the score. The visual evidence suggests applicants with a higher score have higher colleagues' application rate. Moreover, the jump in the

[14]Ideally, we would saturate the model with school fixed effects. Unfortunately, this is not computationally feasible in a non-parametric setting. For further reassurance, we use the school fixed effects to predict our main outcome variable and show evidence of no discontinuity at the threshold. To generate this prediction, we estimate an ordinary least squares regression of the colleagues' application rate one period ahead on the applicant characteristics in Table 1, the average colleagues' characteristics, application wave fixed effects, and school fixed effects. The results are available from the authors upon request.

application rate at the discontinuity threshold suggests the award boosts application to the program.

Table 4 confirms the insight of Figure 5. We present the estimated β coefficients from equation 1 for the colleagues' application rate one period ahead $(t+1)$ using a non-parametric RDD specification in the Calonico et al. (2014a)'s optimal bandwidth. In column one, we present our benchmark specification. In column two, we add the applicants' background characteristics as controls. In column three, we control instead for the average colleague's characteristics. In column four, we use both sets of controls. All the specifications include application wave fixed effects.

Being publicly recognized as a teacher of excellence increases colleagues' next year's application rate to the program by 0.0124 percentage points (column one). The results are remarkably stable across specifications. Relative to an average 0.017 application rate, this constitutes almost a 75 percent boost. Alternatively, one can also think about the effect relative to the applicants who score below the threshold within the optimal bandwidth, in which case the effect constitutes almost a 90 percent increase in colleagues' application rate. Thus, having a peer marginally obtaining an AEP award positively affects the behavior of prospective applicants.

We carry out several sensitivity analyses to address the robustness of this result. Figure A1 explores how the estimated parameter (and its' 95 percent confidence intervals) fluctuates at different bandwidths. The Calonico et al. (2014a) optimal bandwidth is depicted with a solid red line as a reference. Reassuringly, the coefficients are stable at around 0.012. Not surprisingly, as we include observations further away from the 275 threshold precision increases.

Further, we look for other (fake) jumps in the dependent variable along the score distribution. As is standard in the literature (Cattaneo, Idrobo, and Titiunik, 2020), we pick as fake discontinuity thresholds each score between the 5th percentile and the 95th percentile, excluding a doughnut-hole of 10 points around the 275 cut-off. In Figure A2 we plot the histogram of the series of estimated β-coefficients, corresponding t-statistics, and p-values. The estimated coefficients look normally distributed around zero, which is expected when picking fake discontinuity points. Only three out of the 89 estimates are statistically significant at the 10 percent level. All in all, these exercises increase confidence in the estimation procedure and our results.

We also estimate the effects of the program at periods $t+2$ and $t+3$. Figure A3 presents the estimated coefficients and their corresponding 95 percent confidence intervals. The effects seem relatively short lived. After the first year $(t+1)$ the spillover

effect reduces significantly. We are cautious about the interpretation of these results as every year ahead mechanically eliminates one application wave. Moreover, the estimates are within the confidence interval of our benchmark estimate.

As a final step, we contrast our results with the well accepted methodology of Dahl et al. (2014). Unlike our main specification, their identification runs at the colleague (prospective applicant) level (i) rather than at the peer (applicant) level (j). To circumvent the problem of multiple applicants, Dahl et al. (2014) restrict the analysis to groups with only one treated peer. In Table 5 we limit the analysis to schools with only one applicant per year. Column one presents the analogous coefficients as in Table 4 (column one) but for this sub-sample. In column two, we estimate equation (1) non-parametrically using as an outcome variable a dummy taking the value of 1 if the prospective applicant i applied to the award and zero otherwise, i.e. $y_{i,w+1}$. In column three, we replicate Dahl et al. (2014) methodology and estimate equation (1) using a parametric specification with a piece-wise polynomial of order 1 in the Calonico et al. (2014a)'s optimal bandwidth and $y_{i,w+1}$ as the outcome variable. Although the coefficients are slightly lower under this restricted sub-sample, they still imply a boost of at least 50 percent in the application rate across specifications.

7 Mechanism

We have shown that a teacher who marginally obtains the award significantly increases her school colleagues' application rate to the program. A first step behind understanding the drivers of this effect is to investigate whether social proximity is important in determining its magnitude. A key difference between successful and unsuccessful applicants is the public announcement of the achievement. Announcements are made at the regional level in ceremonies organized by the Regional Ministerial Secretaries of Education (see Section 2). If the public announcement *per se* is an important driver of the effect, then we may observe that schools that belong to the school-district but that have no successful applicants are also affected by the successes in other schools.

In Panel A of Table 6, we explore whether there are spillovers at the school-district level. Our left-hand-side variable is the colleagues' application rate one period ahead, defining colleagues as the teachers working in the same school district as the applicant at the time she applied to the program. To make sure we identify school-district spillovers and not own-school effects, we exclude from our computations the teachers working in

the same school as the applicant. In column two, we present the benchmark coefficients of estimating equation (1) among the sub-sample of AEP applicants for which there is school-district data (equivalent to column one, Table 4). We observe that a successful applicant does not affect the behavior of teachers working at other schools in the same-school district (column one). In contrast, in this sub-sample, a successful applicant increases the application rate of teachers working at her same school by 0.0136 percentage points (column two).

As an extra check that the school is the relevant unit at which the spillovers take place, we randomly allocate teachers to schools within the same school district in a given year.[15] Figure A4 presents the histogram of the estimated β-coefficient, corresponding t-statistics, and p-values of equation (1) for 500 random draws. Similar to the exercise depicted in Figure A2, the estimated coefficients look normally distributed around zero. Therefore, consistent with Panel A in Table 6, if we assign applicants to groups of colleagues in the same school district at random, the public announcement of the award status no longer affects the colleagues' future application behavior.

In Panel B of Table 6, we show that not only the effect is confined to the school, but it is also stronger among those that are closer to the successful applicant. Colleagues with closer interaction are those that share at least one of the same grades as the applicant at the time she applied to the program, as opposed to those who do not share even one same grade. The estimates corresponding to these two networks are in column one. A successful applicant increases her same grade colleagues' application rate by 0.0233 percentage points and those in other grades by only (a non-statistically significant) 0.0045 percentage points. Similar to the previous exercise, because we do not have the full teaching assignments for all the AEP applicants in our original sample, in column two we present the benchmark coefficient for this sub-sample, keeping the original definition of colleagues (teachers working at the same school, regardless of the grade).[16]

The above spillover effects can be the result of two phenomena (see, for example, Bursztyn, Ederer, Ferman, and Yuchtman (2014)): social comparisons (e.g., inequality aversion or envy) or social learning (e.g., prospective applicants infer relevant information about the program from successful peers). In the presence of social learning, the added boost from proximity might be explained by a stronger informational content or by social comparisons that are more powerful closer to home. However, in a process of social

[15]Whenever the teacher is randomly assigned to her original school, we turn the observation to a missing. Yet, on average we manage to randomly assign at least 70 percent of the observations.

[16]For this exercise, we limit our analysis to primary and basic education teachers (grades 1 to 8) as after the 8th grade students can self-select into courses.

learning, we will expect that both failures and successes reveal relevant information to applicants. Whereas social comparisons are unlikely to be driven by failing applicants (besides feelings of empathy). To further investigate this mechanism, in what follows we propose a simple analytical framework that incorporates success and failure and a way of testing it with the available data.

Teacher i is characterized by her teaching quality, θ_i. Regardless of teacher quality, all teachers receive a fixed wage of w.[17] The government offers a voluntary recognition program that allows high quality teachers to differentiate themselves from less talented ones. To obtain the award, applicants must take a test and demonstrate to have a quality above a threshold $\tilde{\theta}$. Taking the tests costs c_i.[18] Passing the test entitles teachers to a bonus b. Teachers decide whether or not to apply to the program by comparing their expected utility against their current wage. π_i denotes the probability that a teacher of quality θ_i passes the test. The expected utility of applying to the program is: $\pi_i (w + b) + (1 - \pi_i)w - c_i$.[19]

The exact location of the threshold $\tilde{\theta}$ is uncertain. The only information universally available is that it follows a uniform distribution between $\underline{\theta}$ and $\overline{\theta}$. Teachers also have an additional source of information at hand: the quality of their peers. Suppose i (a prospective applicant) is a colleague of applicant j. Then, i's posterior belief about the probability of passing the test (π_i') depends on whether she is better ($\theta_i > \theta_j$) or worse ($\theta_i \leq \theta_j$) than j, and on whether j passed ($\theta_j \geq \tilde{\theta}$) or failed ($\theta_j < \tilde{\theta}$) the test.

Her peer's fortunes affect a prospective applicant's decision to apply in four possible ways: 1) the success of a worse peer ($\theta_i > \theta_j, \theta_j \geq \tilde{\theta}$) or **unexpected good news**, 2) the success of a better peer ($\theta_i \leq \theta_j, \theta_j \geq \tilde{\theta}$) or **expected good news**, 3) the failure of a worse peer ($\theta_i > \theta_j, \theta_j < \tilde{\theta}$) or **expected bad news**, and 4) the failure of a better peer ($\theta_i \leq \theta_j, \theta_j < \tilde{\theta}$) or **unexpected bad news**.

In case 1, the prospective applicant i has a higher ability than applicant j and j has passed the test. Therefore, if i was considering not to apply to the program, she will apply upon observing the results of her peer ($\pi_i' = 1$). In case 2, i has a lower ability than j and j passed the test. As a result, i beliefs about the probability of passing increase, but to a lower extent ($\pi_i' = \frac{\theta_i - \underline{\theta}}{\theta_j - \underline{\theta}}$). In case 3, i has a higher ability than the j and j failed

[17] This could be, for instance, because even if teachers know their own ability, they cannot credibly signal it to potential employers. See Berlinski and Ramos (2020) for a description of such a setting.

[18] The cost is the sum of a deterministic component common for all teachers and an idiosyncratic component orthogonal to teacher quality. At the beginning of any period, a prospective applicant privately observes the realization of her cost and decides whether or not to apply. As a result, the decision of whether or not to take the test is not deterministic on quality.

[19] A teacher of ability θ_i will apply to the program if $\pi_i \geq \frac{c_i}{b}$.

the test, so that i's beliefs about the probability of passing decreases $\left(\pi_i' = \frac{\theta_i - \theta_j}{\bar{\theta} - \theta_j}\right)$.[20] In case 4, since i is of lower ability than j and j failed the test, if i was considering applying for the award, upon observing the results of j she will revert her decision ($\pi_i' = 0$).

Posterior Beliefs about the Probability of Passing

	Worse Colleagues $\theta_i \leq \theta_j$	Better Colleagues $\theta_i > \theta_j$
Pass ($\theta_j \geq \tilde{\theta}$)	Expected good news **Case 2**: $\pi_i' = \frac{\theta_i - \underline{\theta}}{\theta_j - \underline{\theta}}$	Unexpected good news **Case 1**: $\pi_i' = 1$
Failure ($\theta_j < \tilde{\theta}$)	Unexpected bad news **Case 4**: $\pi_i' = 0$	Expected bad news **Case 3**: $\pi_i' = \frac{\theta_i - \theta_j}{\bar{\theta} - \theta_j}$

This analysis suggests that the reaction of applicants is stronger in cases of unexpected failures and successes of peers. To test these hypotheses, we can estimate equation (1) among the colleagues of higher quality than the applicant and the colleagues of lower quality, separately.[21]

A requirement for the estimation is to have data on teacher quality to rank teachers of the same school. To build such a measure, we link first to fourth grade teachers to students' 4th grade math standardized tests. Then, we recover teacher quality through a battery of teacher fixed effects. In Appendix B, we provide details of how we construct this measure. Overall, we have 3,236 AEP applicants between 2003 and 2010 for which we can recover a measure of teacher quality. In Figure 6 we summarize the relationship between the AEP score and the one period ahead application rate among colleagues of higher quality than the applicant, colleagues of lower quality than the applicant, and all

[20] In case 3, not every teacher i that was considering applying for the program will desist upon observing the results of her colleague j. For the peer's results to change her decision, the peer should have a relatively high quality. The required condition is

$$\theta_i \geq \frac{c_i}{b}(\bar{\theta} - \theta_j) + \theta_j.$$

[21] The following two models entail:

$$R_{jcw}^{t,H} = \alpha + \beta^H D_j + \gamma f(s_j) + \delta D_j \times f(s_j) + \lambda_w + \epsilon_{jcw}^t \text{ if } \theta_j < \theta_i, \quad (3)$$

$$R_{jcw}^{t,L} = \alpha + \beta^L D_j + \gamma f(s_j) + \delta D_j \times f(s_j) + \lambda_w + \epsilon_{jcw}^t \text{ if } \theta_j \geq \theta_i. \quad (4)$$

Equation (3) is analogous to equation (1) among the colleagues of higher quality than the applicant and it provides a test of whether **unexpected successes** increase the application rate. Equation (4) is analogous to equation (1) among the colleagues of lower quality than the applicant, and it provides a test of whether **unexpected failures** decrease the application rate.

colleagues.

Unexpected good news seems to alter the application rate. To the left of the discontinuity threshold, the application rate remains at the average level suggesting that expected bad news does not provide enough information to change application behavior (consistent with **Case 3**). To the right of the discontinuity threshold, however, we detect a jump in the application rate. For prospective applicants of higher quality than the applicant, the fact that their peer passed the tests provides new information that effectively alters their behavior and makes them more likely to apply (consistent with **Case 1**). For colleagues of lower quality than the applicant, there is some evidence that unexpected bad news decreases the application rate. To the left of the discontinuity threshold we observe that the fact that their peer failed the tests discourages them, to some extent, from applying (consistent with **Case 4**). Yet a regression discontinuity design that does not distinguish across relative quality, hardly captures the two different channels, even if the two effects combined visually suggest a rather large effect. In Panel C of Table 6, we present the estimation results. Unfortunately, the sample of teachers is small, and although the estimates go in the expected direction they are imprecise.

8 Conclusions

In this paper, we study the role social interactions at the workplace play in the decision to apply for a professional recognition program. In Chile, public school teachers can apply to a pedagogical excellence award. Successful applicants receive a wage increase and are publicly recognized. We exploit the quasi-random variation in the allocation of awards generated by a sharp assignment rule.

Our findings suggest that the success of an applicant increases her school colleagues' application rate the next year by almost 75 percent. This effect is limited to the school and is stronger among those colleagues with closer interaction with the applicant peer. However, without imposing some theoretical structure to the problem, we cannot disentangle whether this is due to social comparisons or social learning.

To delve into this issue, we rely on the fact that, only under social learning both success and failures matter. In our analytical framework, the effect of the news depends on the relative quality between the applicant peer and the prospective applicants. When colleagues of a high-quality applicant observe that she failed, they are less likely to apply. In contrast, when colleagues of a low-quality applicant observe her success, they are more

likely to apply.

Our results provide a clear message regarding the role of information in voluntary participation professional excellence award programs. When participation is voluntary and the awards are publicly announced, the award provides new information both to employers and co-workers. To employers, the award signals teacher quality. To co-workers, the award reveals previously unknown information relevant to the decision to apply to the program.

Because the applicants to a voluntary recognition program tend to have higher quality than the average teacher, the public announcement of the awards can induce higher application rates at the top of the quality distribution. Nonetheless, it can also deter program take-up to the left of the distribution. This is an externality that should be internalized in the design of merit-based programs. If the aim of the policy is to attain a critical mass of workers engaging with the assessment, publicly announcing the awards may simply work against this objective.

Finally, from 2003 to 2011, almost six out of every ten schools had no AEP applicants among their teaching staff. Our result points to a profitable strategy to increase applications: channel more resources to schools without applicants and use the experience of previous applicants as ambassadors for the program.

Figures

Figure 1: Timeline

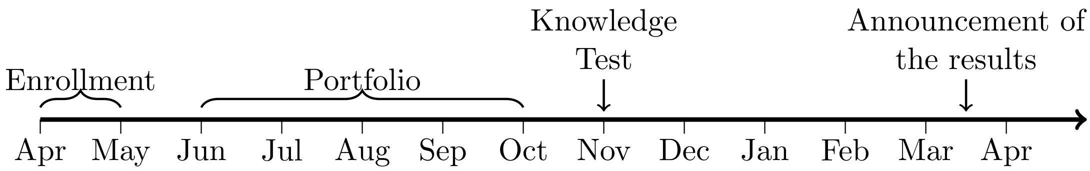
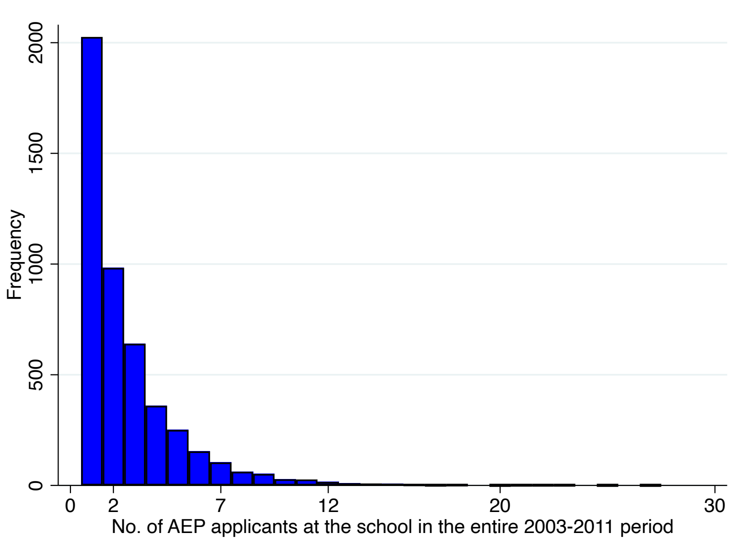
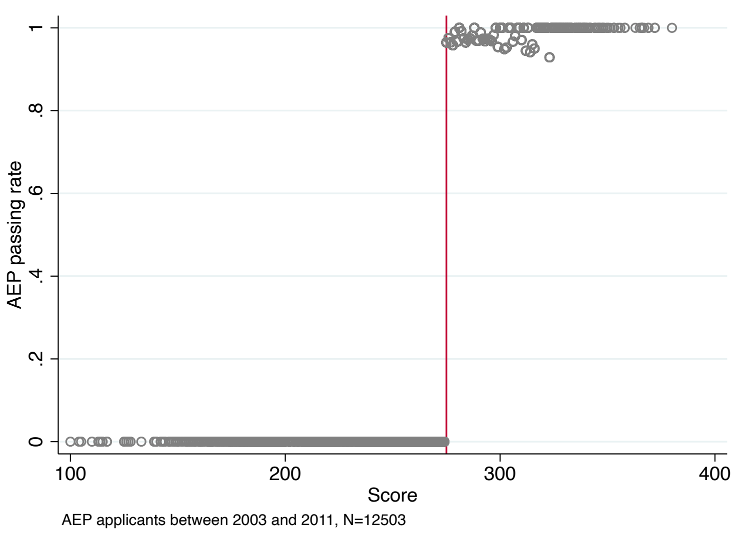


Figure 2: Distribution of AEP Applicants Across Schools



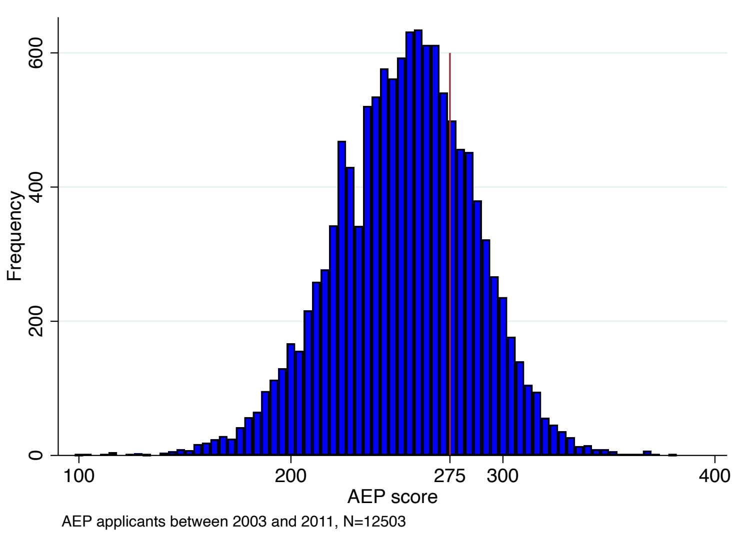
Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: Schools with at least one AEP applicant in the entire 2003 and 2011.

Figure 3: AEP Allocation Rule



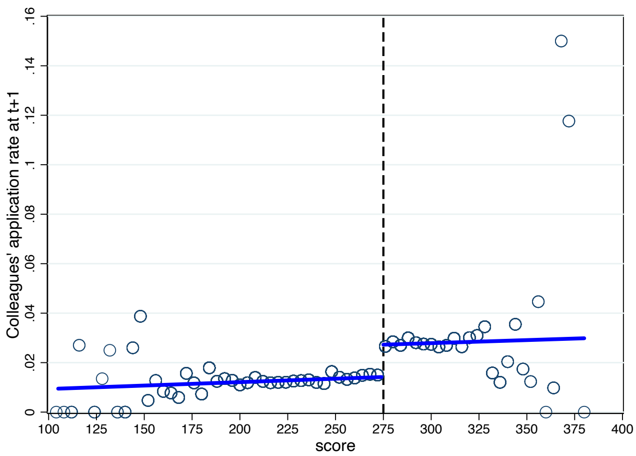
Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: The circles represent the share of applicants passing the exam within each score cell.

Figure 4: Distribution of the AEP Score



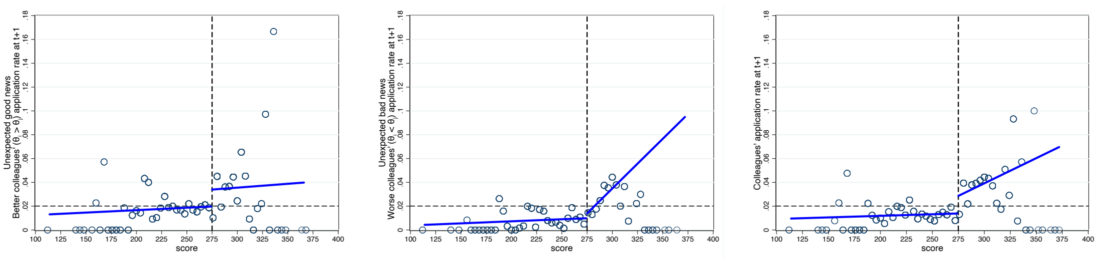
Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: AEP first time applicants between 2003 and 2011.

Figure 5: AEP Effects on Colleagues' Future Application Rate



Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: The circles represent the mean of the outcome variable within bins of size 4 of the score. The solid lines show fitted values of a piecewise linear polynomial of the score. Colleagues defined as teachers working at the same school as the applicant at the time of application.

Figure 6: AEP Effects on Colleagues' Future Application Rate by Relative Quality



Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: The circles represent the mean of the outcome variable within bins of size 4 of the score. The solid lines show fitted values of a piecewise linear polynomial of the score. Colleagues defined as teachers working at the same school as the applicant at the time of application. The left-most panel computes the application rate among the colleagues of higher quality than the applicant ($\theta_i > \theta_j$), i.e. **Unexpected good news**. The middle panel computes the application rate among the colleagues of lower quality than the applicant ($\theta_i \leq \theta_j$), i.e. **Unexpected bad news**. The right-most panel computes the application rate regardless of relative quality, i.e. the overall effect.

Tables

Table 1: Descriptive Statistics

	Eligible teachers (1)	AEP applicants' colleagues (2)	AEP applicants (3)	$\hat{\beta}$ (4)
Teacher characteristics:				
Male	0.29	0.31	0.27	0.03
	(0.45)	(0.46)	(0.44)	(0.03)
Age	41.07	41.66	39.01	-0.24
	(11.20)	(10.99)	(9.03)	(0.62)
Degree in education	0.92	0.92	0.97	0.01
	(0.27)	(0.27)	(0.17)	(0.01)
Years of experience	17.57	18.27	15.09	-0.71
	(12.33)	(12.30)	(9.54)	(0.67)
Hours per week	36.59	38.08	38.47	-0.44
	(10.43)	(8.52)	(8.39)	(0.60)
Working at more than one school	0.11	0.12	0.15	-0.01
	(0.31)	(0.33)	(0.35)	(0.03)
Primary school teacher	0.56	0.53	0.55	-0.05
	(0.50)	(0.50)	(0.50)	(0.04)
School characteristics:				
SNED	0.26	0.36	0.35	0.00
	(0.44)	(0.48)	(0.48)	(0.03)
Private-subsidized school	0.42	0.49	0.50	0.03
	(0.49)	(0.50)	(0.50)	(0.04)
Rural school	0.13	0.06	0.12	0.02
	(0.33)	(0.23)	(0.33)	(0.03)
No. of previous AEP applicants	1.08	1.86	1.53	-0.16
	(2.09)	(2.70)	(2.47)	(0.21)
No. of previous AEP awardees	0.32	0.64	0.58	0.00
	(0.83)	(1.18)	(1.16)	(0.11)
N	1,562,142	265,051	12,503	

Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: Column 1 corresponds to the universe of all active teachers between 2003 and 2011. Column 2 presents the characteristics of colleagues of the AEP applicants that work at least 20 hours a week in voucher system schools between 2003 and 2011. In columns 1 and 2 the same teacher can appear more than once, once in each teacher-year observation. Column 3 presents the characteristics of first time AEP applicants between 2003 and 2011, at the time of application. Column 4 reports the β coefficients of estimating equation (1) with the descriptive as the outcome. All teacher characteristics specifications in column 4 include application wave fixed effects. Specifications for school characteristics in column 4 do not include application wave fixed effects.
* Indicates statistical significance at 10%.
** Indicates statistical significance at 5%.
*** Indicates statistical significance at 1%.

Table 2: AEP Application Rates

	All	2003	2004	2005	2006	2007	2008	2009	2010	2011
Teachers:										
Application rate	5.28	0.59	0.92	0.97	1.17	0.86	0.88	0.92	0.73	0.60
Passing rate	26.57	42.44	32.37	33.73	28.03	20.43	19.26	17.86	17.47	21.07
School application rate:										
At least one applicant	33.72	5.79	8.78	9.69	10.49	8.36	8.64	8.54	7.45	6.36
First time			86.82	67.96	50.51	37.23	44.15	35.26	36.22	25.56

Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: The application rate in column one (*All*) is the ratio between the first time AEP applicants between 2003 and 2011 and the number of unique teacher observations ever eligible for the AEP between 2003 and 2011.

Table 3: Density Test

	All	2003	2004	2005	2006	2007	2008	2009	2010	2011
Calonico et al. (2014a)	0.816	0.167	0.499	0.193	0.962	0.455	0.982	0.985	0.707	0.721
Frandsen (2017)	0.540	0.192	0.251	0.575	0.832	0.786	0.646	0.941	0.932	0.786

Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: Calonico et al. (2014a) test selects the optimal bandwidth independently for each wave.

Table 4: AEP Effects on Colleagues' Future Application Rate

	(1)	(2)	(3)	(4)
β	0.0124***	0.0120***	0.0119***	0.0120***
	(0.0035)	(0.0036)	(0.0033)	(0.0034)
p-value	0.0004	0.0008	0.0004	0.0004
BW	26	25	28	28
N	5,597	5,246	6,113	6,037
Applicant controls	No	Yes	No	Yes
Colleagues' controls	No	No	Yes	Yes

Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: Each cell reports the β coefficient of estimating equation (1) with a local non-parametric regression-discontinuity design specification in the Calonico et al. (2014a) optimal bandwidth, with a triangular kernel, and a linear polynomial of the score. The outcome variable is the colleagues' application rate one period ahead. Robust standard errors clustered at school of application in parentheses. Colleagues defined as teachers working at the same school as the applicant at the time of application and currently eligible for the AEP. Controls include gender, age, years of experience, hours worked, degree in education, working at more than one school status, and primary school teacher status of the applicant's colleagues, as well as the school's SNED award at the time of application, private-subsidized school and rural school. Applicant controls vary at the level of the applicant. Colleagues' controls are the average of each of the control variables among the colleagues. All specifications include application wave fixed effects.
* Indicates statistical significance at 10%.
** Indicates statistical significance at 5%.
*** Indicates statistical significance at 1%.

Table 5: AEP Effects on Colleagues' Future Application Rate, Schools with only One Applicant

	(1)	(2)	(3)
β	0.0100*	0.0072***	0.0077***
	(0.0057)	(0.0025)	(0.0021)
p-value	0.0757	0.0045	0.0002
BW	24	18	18
N	2,655	95,497	95,497
Outcome variable	R^1_{jw}	$y_{i,w+1}$	$y_{i,w+1}$
Estimation	Non-parametric	Non-parametric	Parametric

Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: Columns 1 and 2 report the β coefficient of estimating equation (1) with a local non-parametric regression-discontinuity design specification in the Calonico et al. (2014a) optimal bandwidth, with a triangular kernel, and a linear polynomial of the score. Columns 3 reports the β coefficient of estimating equation (1) with a parametric specification using a piece-wise polynomial of order 1. In column 1 the outcome variable is the colleagues' application rate one. In columns 2 and 3 the outcome variable is a dummy taking the value of 1 if a prospective applicant i, colleague of applicant j, applied for the AEP the year after the applicant and 0 otherwise. Robust standard errors clustered at school of application in parentheses. Colleagues defined as teachers working at the same school as the applicant at the time of application and currently eligible for the AEP. All specifications include application wave fixed effects.
* Indicates statistical significance at 10%.
** Indicates statistical significance at 5%.
*** Indicates statistical significance at 1%.

Table 6: Mechanism

	Mechanism	Benchmark
Panel A: *Sub-sample with school-district data*		
School-district spillovers	0.0003	0.0136***
	(0.0011)	(0.0042)
p-value	0.7653	0.0011
BW	20	25
N	3,677	4,321
Panel B: *Sub-sample with grade assignment data*		
Closer interaction (at least one same grade)	0.0233***	0.0145***
	(0.0086)	(0.0053)
p-value	0.0065	0.0058
BW	32	21
N	3,827	3,629
Limited interaction (no same grade)	0.0045	
	(0.0057)	
p-value	0.4253	
BW	29	
N	4,249	
Panel C: *Sub-sample with teacher quality data*		
Better colleagues ($\theta_i > \theta_j$)	0.0246	0.0195
	(0.0192)	(0.0133)
p-value	0.1985	0.1439
BW	22	27
N	1,093	1,684
Worse colleagues ($\theta_i \leq \theta_j$)	0.0110	
	(0.0079)	
p-value	0.1624	
BW	17	
N	945	

Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: Each cell reports the β coefficient of estimating equation (1) with a local non-parametric regression-discontinuity design specification in the Calonico et al. (2014a) optimal bandwidth, with a triangular kernel, and a linear polynomial of the score in different samples. Robust standard errors clustered at school of application in parentheses. The outcome variable is the colleagues' application rate one period ahead. Column 2 presents the benchmark coefficient within the sub-sample (equivalent to column 1, Table 4). All specifications include application wave fixed effects.
* Indicates statistical significance at 10%.
** Indicates statistical significance at 5%.
*** Indicates statistical significance at 1%.

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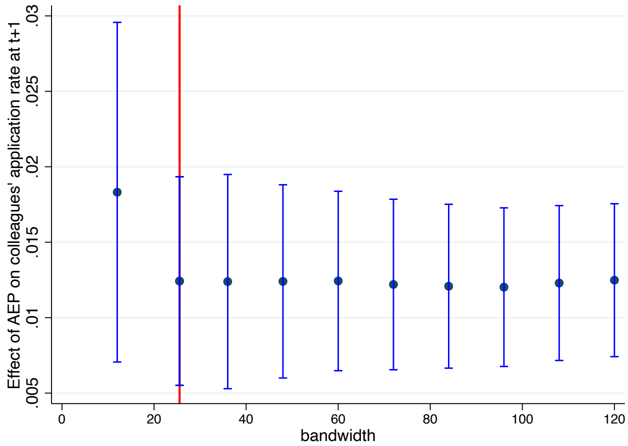
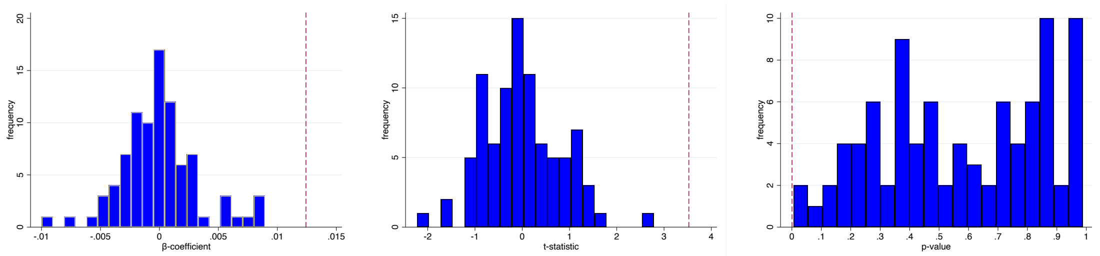
A Figures

Figure A1: Alternative Bandwidth: AEP Effects on Colleagues' Future Application Rate



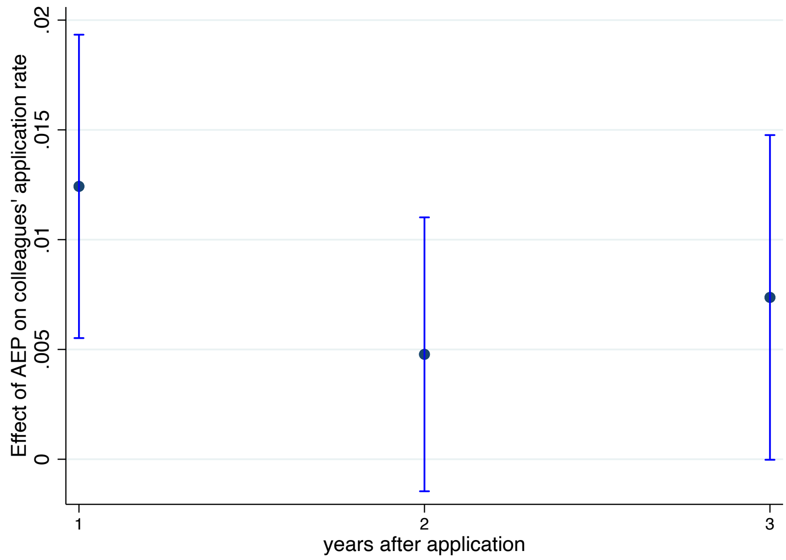
Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: Each point represents the estimated β coefficient from a local non-parametric regression-discontinuity design specification with a triangular kernel, and a linear polynomial of the score, in the bandwidth displayed in the x-axis. The brackets represent the 95 percent confidence intervals from robust corrected standard errors clustered at school of application. All of the specifications include application wave fixed effects. The Calonico et al. (2014a) optimal bandwidth is depicted by the solid red line.

Figure A2: Fake-cutoff



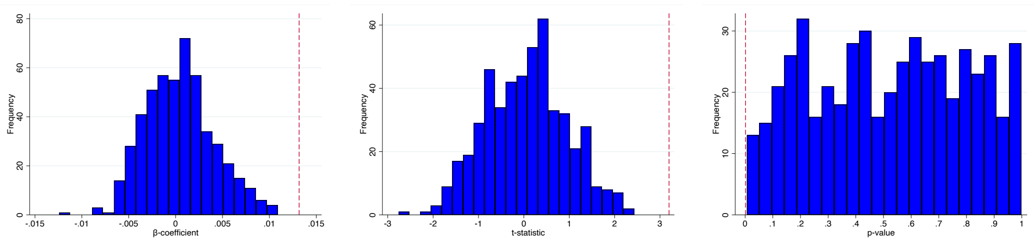
Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: Histogram of the estimated β-coefficient, t-statistics, and p-values of estimating equation (1) using as discontinuity threshold each of the scores between 5th percentile and the 95th percentile of the score, excluding a doughnut-whole of 10 points around the 275 cutoff. The dashed lines correspond to the true discontinuity cut-off (275).

Figure A3: AEP Effects on Colleagues' Future Application Rate over Time



Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: Each point represents the estimated β-coefficient from a local non-parametric regression-discontinuity design specification with a triangular kernel, and a linear polynomial of the score, on the effect of the AEP in the periods ahead displayed in the x-axis. The brackets represent the 95 percent confidence intervals from robust corrected standard errors clustered at school of application. All of the specifications include application wave fixed effects.

Figure A4: Random Peer, School-District Level



Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: Histogram of the estimated β-coefficient, t-statistics, and p-values of equation (1) for 500 random draws. At each draw an applicant is randomly assigned to any school in the school district at a given year. The dashed lines correspond the true allocation.

B Measure of Teacher Quality

To build our measure of teacher quality, we link teachers to students' standardized test, and then, recover teacher quality through a battery of teacher fixed effects.

For students' standardized test scores, we use Chile's Education Quality Measurement System (*Sistema de Medición de la Calidad de la Educación* or SIMCE). Although the national standardized test is available for 4th, 8th and 10th grade, only up to 4th grade students are assigned a general teacher for most subjects. After 4th grade, students have a different teacher for each subject and in secondary school they can self-select into courses. We restrict our analysis to 4th grade results and combine these data with teaching assignments. This produces a panel with the math teacher of each student in each of the first four years of primary school and the student's 4th grade SIMCE test scores.

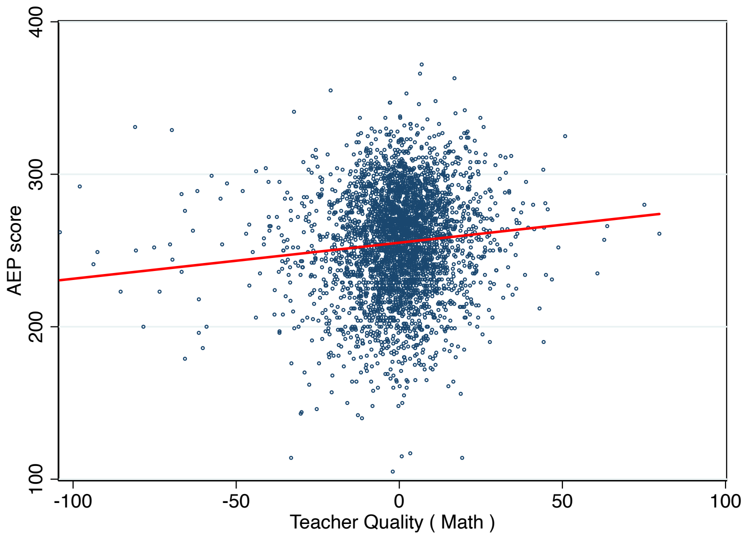
We estimate teacher quality using the following specification:

$$SIMCE_{kjgst} = c + \theta_j + \gamma_g + \lambda_s + \tau_t + \varepsilon_{kjsgt},$$

where k denotes the student, j teacher, g grade, s school, and t year. θ_j recover the teachers' fixed effects and provide an intra-school measure of teacher quality. The implicit assumption is that teacher quality is fixed over time and remains unaffected by the AEP application process.

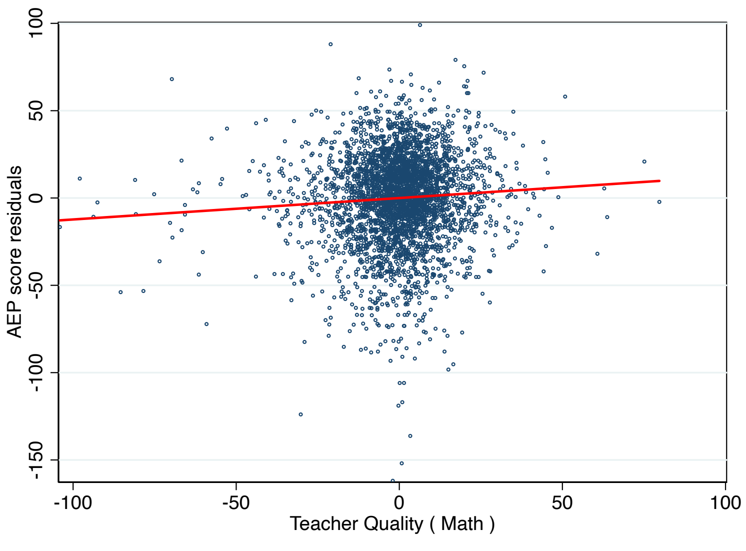
There are 3,236 AEP applicants between 2003 and 2010 for which we can recover a measure of teacher quality. In Figure B1, we document a positive the correlation between the AEP score and our measure of quality. Our measure of teacher quality already nets out both application wave fixed effects and school fixed effects. In Figure B2, we present the correlation between the residualized AEP scores (netting out application wave fixed effects) and our measure of quality. For further reassurance in Figure B3, we plot the distribution of our measure of intra-school teacher quality across the teachers eligible to apply for the AEP. As expected, the AEP requirement is to the right of the distribution of teacher quality.

Figure B1: Correlation between Teacher Quality and AEP Score



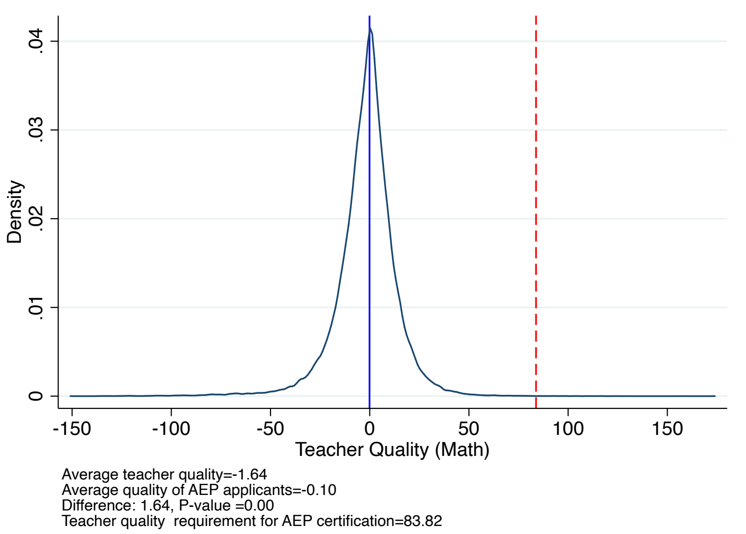
Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: Measure of teacher quality based on students' fourth grade national standardized math test.

Figure B2: Correlation between Teacher Quality and Residualized AEP Score



Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: Measure of teacher quality based on students' fourth grade national standardized math test. AEP score residuals from regressing of the AEP score against a battery of application wave fixed effects.

Figure B3: Distribution of Teacher Quality



Source: Own calculations based on data from the Ministry of Education (Chile).
Notes: Intra-school measure of teacher quality using students' math standardized tests in 4th grade. The blue solid line represents the average teacher quality for all the AEP applicants. The red dashed line represents the average teacher quality at the 275 AEP score.