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TEACHER COMPENSATION AND STRUCTURAL INEQUALITY: EVIDENCE FROM CENTRALIZED TEACHER SCHOOL CHOICE IN PERÚ

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ABSTRACT

We exploit data on centralized teacher recruitment in Perú to establish that wage rigidity creates large urban-rural disparities in teacher effectiveness. Leveraging a teacher compensation reform, we provide causal evidence that increasing salaries in less desirable locations attracts qualified teachers and improves student learning. We estimate a model of teacher sorting and student achievement featuring rich heterogeneity in teachers' preferences and effectiveness. Substantial equity and efficiency gains arise from reallocating existing teachers to exploit match effects or attracting applicants with higher average effectiveness into public teaching. Cost-minimizing counterfactual wage schedules aimed at achieving these gains imply the latter is more cost-effective.

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A online appendix is available at http://www.nber.org/data-appendix/w29068

1 Introduction

Children born in remote or rural communities attain significantly lower academic achievement than their urban counterparts (World Bank, 2018). These disparities are particularly salient in developing countries, where remote locations suffer from a structural and historically persistent underdevelopment (Sokoloff and Engerman, 2000; Banerjee and Iyer, 2005; Dell, 2010). Designing effective policies that provide equal opportunities despite these barriers is thus a first-order concern. In this paper, we show that structural inequalities in schooling outcomes can be mitigated or reinforced by the inherent mobility of a critical factor in the production of human capital: teachers.

Teachers are a key determinant of student learning and long-run outcomes (Rivkin et al., 2005; Chetty et al., 2014b; Araujo et al., 2016; Jackson, 2018). Recent evidence further documents that teachers hold comparative advantages in teaching different types of students (Gershenson et al., 2022; Ahn et al., 2023; Graham et al., 2023), implying that teacher allocation is not only relevant for equity, but also has efficiency implications. Yet, little is known about the fundamental drivers of teacher sorting across schools. Wage rigidity – a common feature of most public schooling systems around the world – has been highlighted as an important friction that would make teachers sort on non-pecuniary aspects of employment (Rosen, 1986), potentially resulting in low-quality teachers disproportionately working in disadvantaged schools (Clotfelter et al., 2005; Mansfield, 2015). While decentralizing wage-setting has been shown to yield limited equity and efficiency gains (Biasi et al., 2021), centralized, rule-based compensation reforms may be effective in addressing these challenges.

In this paper, we conduct a comprehensive analysis of the importance of teacher compensation design in shaping teacher sorting across schools and the distribution of student achievement. Our study takes place in Perú, a large developing country featuring stark geographic disparities in student achievement, a rigid teacher compensation schedule, and a centralized teacher recruitment drive. Using rich data on the universe of public-school teachers and students, we show that wage rigidity prompts teachers to sort on non-pecuniary aspects of employment, creating large urban-rural disparities in teacher effectiveness. We leverage a teacher compensation reform to show that raising wages in disadvantaged schools is effective at attracting qualified teachers and improving student learning. We then estimate a model of teacher sorting and student achievement featuring rich heterogeneity in teachers' preferences and effectiveness. We allow for teacher-student match effects in our student achievement production function. We find that large equity and efficiency gains are attainable by either reallocating existing teachers to exploit teacher-student match effects or attracting applicants with higher average effectiveness into public teaching. Finally, we develop a cost-minimizing wage-setting procedure that seeks to achieve these gains by using information on teachers' preferences and effectiveness. We find that leveraging the extensive margin of recruitment is more cost-effective than exploiting match effects.

Our analysis draws on rich administrative panel data linking the universe of applicants and jobs posted in the nationwide centralized recruitment drive for public teachers in Perú with an array of additional data sources on public primary schools and students. All applicants take a standardized national competency test and choose their preferred position sequentially according to their test score rank. We first document that students in rural areas lack access to basic amenities and attend schools with notably inferior infrastructure. Teachers working in rural schools are significantly less qualified: they obtain 0.67 standard deviation (σ) lower scores in the national teacher competency test. Auxiliary survey evidence eliciting teachers' preferences over various non-wage job amenities suggests that the observed sorting is driven by preferences for urban amenities that are uncompensated by the rigid wage schedule. As a result, highly competent teachers disproportionately concentrate in urban areas. These inequities are associated with a staggering urban-rural difference of 0.60σ in students' standardized math scores.

We identify the causal effect of financial incentives on teacher sorting by evaluating a teacher compensation reform. This policy attributes wage bonuses to teachers working in rural schools that get increasingly large with remoteness through discrete jumps determined by arbitrary cutoffs on the school locality's population and its distance to the provincial capital. Using a regression discontinuity design around a 13% wage increase, we find that labor supply is highly elastic: higher wages raised new recruits' competency scores by 0.42σ. These gains passed through to students, raising school-level math and language scores by 0.27σ and 0.21σ, respectively. We provide evidence that these gains did not come at the expense of lowering the quality of teachers in lower-paying schools located near the threshold. Importantly, these effects are entirely driven by the selection of higher-quality new recruits rather than increased effort from incumbents.

We then build an empirical model of teacher sorting across schools and student achievement to characterize the potential equity and efficiency gains from alternative compensation policies. We allow teachers to have heterogeneous preferences over wages and non-wage job attributes. Specifically, non-pecuniary factors such as local amenities, geographical proximity, and teaching conditions induce both vertical and horizontal differentiation across jobs. In line with the institutional framework, wages are fixed, and we assume that the equilibrium teacher-school match is stable with respect to teachers' preferences over schools and school priorities. Teacher sorting maps into student achievement through a potential outcomes framework where teacher effectiveness is allowed to be heterogeneous and interact with students' prior achievement measures and gender. Teachers' absolute and comparative advantages flexibly correlate with latent teacher attributes governing their willingness to pay for non-wage amenities and their valuation of the outside option. This potentially captures intrinsic motivation that cannot be explained by observable teacher characteristics and allows for selection on unobserved teaching quality in response to counterfactual teacher compensation policies.

Stability implies that each teacher is matched to their preferred school among their feasible choice set, i.e. the set of schools that would be willing to rematch with them. As school priorities are observed, we can construct the set of feasible schools for each teacher directly from the data. This unique feature allows us to express the equilibrium teacher-school allocation as the outcome of a discrete choice problem with personalized choice sets. We leverage this insight to identify and estimate teachers' preferences from the observed teacher-school match. We relax the selection on observables assumption typically imposed in the teacher value-added literature by allowing teachers' unobserved preference for their school assignment to directly influence student outcomes. Standard selection correction recovers unbiased estimates of the teacher value-added parameters.

The estimated model closely replicates the main features of the data, including the equilibrium sorting patterns and the causal effects on teacher quality and student achievement estimated at the RD threshold, as well as moments of the distribution of matched teacher and school characteristics. Our estimates show that teachers' willingness to pay for non-wage amenities is typically large and features substantial heterogeneity. Consistent with the descriptive survey evidence, teachers have a high willingness to pay for proximity to home and better teaching conditions, driving the concentration of talent in urban areas. To fully compensate for the urban-rural amenity differential, the most remote schools would require wage premiums two to four times larger than existing levels.

We report substantial heterogeneity in teachers' absolute and comparative advantage. Moving one standard deviation up the distribution of average effectiveness implies an increase in student score of 0.38σ in math and 0.33σ in Spanish. Importantly, 22-38% of the overall variance in teacher effectiveness can be explained by differences in their comparative advantage. The variance in teacher value-added is larger for students lagging behind, implying that rural schools would highly benefit from making teachers sort based on their comparative advantage. Teachers' latent types driving their preferences over job postings correlate with their effectiveness. In particular, teachers who are more responsive to wage differences and those who have better outside options are less effective on average. Accounting for such selection is crucial to get accurate predictions of the distribution of student achievement under counterfactual compensation regimes.

Our estimates of teacher value-added further show that, even in the presence of the wage bonus policy, teacher sorting across locations remains highly unequal, favoring urban areas. In particular, we document a 0.14σ urban-rural gap in teacher value-added for math, corresponding to one quarter of the overall gap in student achievement. Teachers do not sort based on their comparative advantage, implying large potential efficiency and equity gains from counterfactual teacher assignments. We quantify these gains by characterizing the allocation that maximizes total student achievement. Efficiently reallocating the pool of assigned teachers would increase student test scores in rural areas by 0.17σ while moderately decreasing student test scores in urban areas by 0.02σ. In contrast, inducing applicants with higher average effectiveness to sort into public teaching instead of choosing the outside option could generate aggregate gains of 0.11σ.

Finally, we provide a framework to design cost-effective teacher compensation policies that seek to achieve these equity and efficiency gains by leveraging information on teachers' preferences and effectiveness. We consider the problem of setting wages in each school to ensure that each rural school is assigned a teacher with sufficiently high value-added while minimizing total cost. We leverage results from Hatfield and Milgrom (2005) to show that the solution to this problem is equivalent to the outcome of the school-proposing generalized deferred acceptance algorithm within a counterfactual economy where schools would be able to increase wages until that objective is met.

Applying this procedure, we show that the status quo policy is poorly targeted and that large equity and efficiency gains are attainable at no additional cost. Importantly, these gains mainly come from attracting applicants with higher average effectiveness into public teaching rather than reallocating existing teachers to exploit teacher-student match effects. This suggests that, despite the large potential gains from teacher reallocation, leveraging the extensive margin of recruitment is more cost-effective.

Our findings speak to several strands of literature. A large body of work studies teachers' contribution to student achievement (Rivkin et al., 2005; Chetty et al., 2014b; Araujo et al., 2016; Bau and Das, 2020). We first highlight the benefits of leveraging data on teachers' choices over schools to identify and estimate teacher value-added. As teachers' preferences over job attributes correlate with their effectiveness, this additional information is essential to capture selection on unobserved quality resulting from changes in compensation (Rothstein, 2015; Brown and Andrabi, 2020). Following Abdulkadiroğlu et al. (2020), we also show how to leverage this information to correct for selection on teachers' unobserved preference for their school assignment. Finally, we add to the literature studying teacher-student match effects (Ahn et al., 2023) by showing that efficiently reallocating teachers would largely reduce urban-rural disparities in student achievement.

This paper also contributes to a recent literature studying the link between teacher sorting and student achievement through equilibrium models of the labor market for teachers (Boyd et al., 2013; Bonhomme et al., 2016; Tincani, 2021; Biasi et al., 2021; Bates et al., 2025). In contrast to the literature, our approach utilizes panel data on a nationwide centralized allocation mechanism for public-sector teachers, providing detailed information on each applicant's choices and choice sets over several years. This dataset, combined with quasi-experimental variation in wages, allows us to identify and estimate, under minimal assumptions, a rich empirical model of teacher sorting and student achievement accounting for flexible heterogeneity in teachers' preferences and effectiveness.

Finally, our work is broadly related to a growing literature studying personnel and organizational policies in the public sector (Finan et al., 2017; Khan et al., 2019). While there is a large body of work studying the effectiveness of pay-for-performance schemes for teachers (Muralidharan and Sundararaman, 2011; Barrera-Osorio and Raju, 2017; Biasi, 2021; Leaver et al., 2021; Gilligan et al., 2022; Brown and Andrabi, 2020), there is relatively little evidence on the effects of unconditional pay increases on teacher sorting and student outcomes (Clotfelter et al., 2008; de Ree et al., 2018; Pugatch and Schroeder, 2018; Cabrera and

Webbink, 2020). In line with this literature, we find that such interventions do not prompt increased effort from incumbent teachers. However, we document that they can largely increase student achievement by attracting higher-quality teachers. We borrow insights from the empirical market design literature (Agarwal, 2017; Agarwal and Budish, 2021) to develop a cost-minimizing wage-setting procedure aimed at attaining equity and efficiency gains by leveraging information on teachers' preferences and effectiveness. This allows us to conclude that incentivizing reallocation to leverage teacher-student match effects is less cost-effective than exploiting the extensive margin of recruitment by attracting applicants with higher average effectiveness into public teaching.

2 Context, Data, and Descriptive Evidence

2.1 Institutional Background

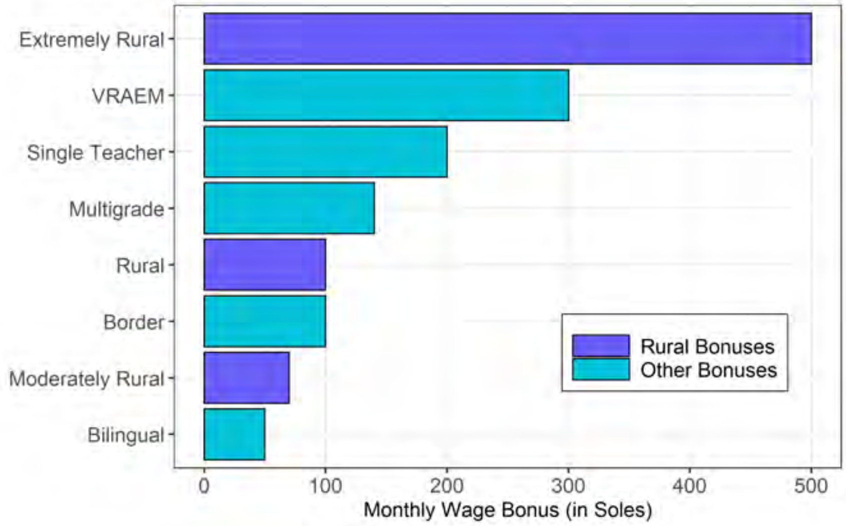
Our analysis focuses on public primary education, which offers the broadest coverage of any schooling level in Peru. Secondary schools are notably less prevalent in rural regions. Public schools constitute 75% of nationwide primary school enrollment in Peru. In rural areas, more than 26,000 public primary schools enroll 99% of school-aged children.

Public school teachers in Perú are hired under two distinct types of contracts. Permanent teachers (*docentes nombrados*) are civil servants with secure employment conditions. Contract teachers (*docentes contratados*) are hired on a fixed one-year contract by a specific school, renewable for an additional year upon approval from the school's principal.

Primary school teachers' earnings in Perú rank second to last among liberal professions (INEI, 2016). In 2016, all contract teachers were receiving a fixed base monthly wage of S/ 1,396 (US$ 402) while permanent teachers were receiving S/ 1,550 (US$ 447), irrespective of where they worked.[1] Public-sector teachers also receive wage bonuses linked to specific school appointments. Figure 1 illustrates the various wage bonuses that were in place during our analysis period, which range between 4% (for bilingual schools) and 36% (for extremely rural locations, as detailed in Section 3.1) of the monthly base wage. These bonuses are additive, such that teachers working in schools meeting multiple criteria (e.g., being both multi-grade and rural) accumulate bonuses.

[1]This figure increased to S/ 2,000 in 2018. The average wage for a primary-school teacher in the private sector was S/ 950 (US$ 274) per month (MINEDU, 2014).

Figure 1: Wage Bonuses for Teachers in Disadvantaged Schools



NOTES. This figure shows the monetary amount in Peruvian Soles (S/) for the different wage bonuses implemented by the Government during the period 2015-2018. The VRAEM indicator groups schools that are in the *Valle de los Rios Apurimac, Ene y Mantaro*, an extremely poor region that presents security concerns due to the activity of drug cartels. Border categorizes schools that are adjacent to the country's borders. See Section 3.1 for the definition of Extremely Rural, Rural, and Moderately Rural.

Historically, the recruitment of public teachers followed a decentralized approach, granting regional and local officials substantial discretion in hiring and resource allocation (Bertoni et al., 2019; Estrada, 2019). To enhance transparency and fairness, the Ministry of Education (MoE) introduced a nationwide recruitment process in 2015, which centralized all job postings and teacher applications in a unified platform. This process has taken place every other year since then. All applicants for positions in public schools are required to have a teaching certification (i.e. a teacher degree) and to undergo a national competency test.[2] Permanent positions are solely accessible to teachers scoring above selective thresholds and are assigned through a two-step matching process in which schools have a fair amount of discretionary power over hires (for more details, see Appendix A.1). In our data, only about 11% of the applicants are eligible for a permanent teaching position. Temporary positions are allocated through a serial dictatorship algorithm where applicants are ranked according to their competency score. Applicants first select a school district and then choose from the available vacancies within that district by order of their rank (for more details, see Appendix A.2).

[2]The test comprises three modules, each contributing different weights to the total score: logical reasoning (25 percent), reading comprehension (25 percent), and curricular and pedagogical knowledge (50 percent). Figure B.1 provides relevant individual-level correlates of teacher performance in each module of the national competency test.

We restrict the analysis to the recruitment of contract teachers for several reasons. First, the majority of vacancies open in rural schools are filled by teachers on a temporary contract (85%), thus making the allocation of short-term teaching positions most relevant for student outcomes in remote locations. Second, the recruitment of contract teachers is based on a "strict-priority" mechanism – schools rank candidates by their competency score. This institutional feature enables us to observe teachers' choice sets directly, which is essential to identify teachers' preferences over job postings.

2.2 Data Description and Sample Selection

Our empirical analysis draws upon comprehensive administrative records on students, teachers and schools within the Peruvian public education system. Through the centralized matching platform, we have access to information on the universe of vacancies and applicants for teaching positions at public schools in 2016 and 2018. These data include the wage associated to each job posting, applicants' scores in the national competency test, and basic demographics such as education, age, gender, and native language.

We complement this information with several auxiliary data sources providing a wide range of additional school and applicant characteristics. The school census collects yearly information on the number of students and teachers in each school, school infrastructure (libraries, computers, sports facilities, etc.), and access to basic local amenities like electricity, water, internet, banking services, and public libraries. The teachers' employment records (NEXUS, for its acronym in Spanish) track all employed public-sector teachers from 2012 to 2020 and contain information on their workplace and contract type. Finally, we obtain information on the residential location of applicants from a government-collected dataset at the household level, which is primarily utilized for targeting social programs (SISFOH, for its acronym in Spanish).

Student academic performance is obtained from a national, standardized test evaluating proficiency in math and Spanish language (ECE, for its acronym in Spanish). This assessment has near-universal coverage of the student population in the country and contains individual test scores for second- and fourth-grade students. We can link students to their respective classroom and teacher identifiers through an administrative teacher-classroom dataset held by the Ministry of Education (SIAGIE, for its acronym in Spanish).

We construct two samples for our analysis. The *choice sample*, used to analyze teachers' choices over job postings, is composed of applicants and positions that participated in the 2016 and 2018 recruitment drives for contract teachers. Considering only schools and applicants with non-missing covariates, our choice sample comprises 22,743 vacancies and 50,319 applicants.[3] Importantly, 75% of applicants participating in the 2016 recruitment drive reapply in 2018, which allows us to construct a panel tracking teachers' re-matching decisions. We label our second analysis sample as the *outcome* sample, which we use to measure teacher effectiveness and to study the effects of recruitment on student achievement. This is a subset of the choice sample composed of applicants assigned to fourth-grade classrooms for which we observe student performance at the national test. Our outcome sample comprises 4,834 teachers and 96,508 students. Table B.1 provides descriptive statistics for these two samples.

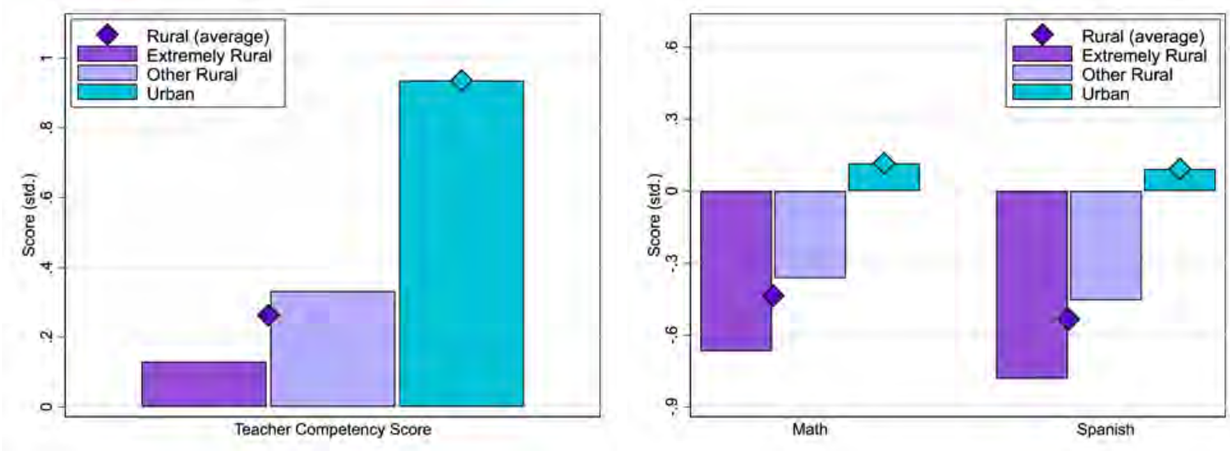
2.3 Wage Rigidity, Sorting, and Inequalities

Educational outcomes in Peru exhibit severe spatial inequality. Despite policy efforts to address historical underinvestment in disadvantaged areas (Bertoni et al., 2020), the urban-rural divide remains acute. This disparity is particularly evident in the stark gaps in both schooling inputs and student performance. While urban amenities are nearly universal, rural localities frequently lack basic facilities, such as internet access, medical clinics, or access to drinking water. School-specific infrastructure is similarly stratified; for instance, only 8% and 37% of rural schools have science-lab and computer facilities, while the corresponding shares in urban schools are 42% and 83%, respectively. These disparities, along with differences in other school characteristics, are reported in Table B.2.

Figure 2 visually portrays the striking disparities in teaching competency and student achievement between primary schools in rural and urban areas. As shown in Panel A, teachers working in rural schools score, on average, 0.67σ lower on the national competency test than teachers working in urban schools. Teachers in the most remote areas, categorized as *Extremely Rural* (see Section 3.1 for a formal definition), score 0.80σ lower than in urban schools. These spatial differences strongly correlate with inequalities in student achievement. Panel B displays the distribution of students' academic performance at the national

[3] When removing a matched applicant because of missing information, we remove the corresponding position. Similarly, when removing a matched position, we remove the corresponding applicant. Under this sampling procedure, the likelihood of removing matched vacancies and applicants is twice as large as the likelihood of removing unmatched vacancies and applicants (Menzel, 2015). To correct for this and ensure that the overall matching rate remains constant, we randomly remove unmatched applicants and vacancies accordingly.

Figure 2: Teacher Competency and Student Achievement by Remoteness



a) Teacher Competency by Urban/Rural **b) Student Achievement by Urban/Rural**

NOTES: This figure presents summary statistics for teachers and students in schools with different levels of rurality. Areas are grouped as follows: (a) urban areas, where the locality population exceeds 2,000 inhabitants; (b) extremely rural areas, where the locality population is below 500 inhabitants and the travel time to the closest provincial capital exceeds 120 minutes; and (c) other rural areas, where the locality population is either above 500 inhabitants or travel time is below 120 minutes (see Section 3.1 for details). Panel A uses our choice sample and reports the average teacher competency score in filled vacancies by rurality. Panel B uses our outcome sample and reports the average student scores in the fourth-grade Spanish and math modules of the national standardized assessment by rurality. In each panel, diamonds indicate the average for urban and rural areas, where the rural average aggregates the two rural categories.

standardized evaluation. We observe an urban-rural gap in student achievement of 0.55σ in mathematics and 0.63σ in Spanish.[4]

Several factors might explain the observed teacher sorting patterns. Teachers working in rural areas contend with several challenges, including the scarcity of basic school resources, inadequate services, limited access to public goods, and, for many, geographical distance from home. We directly elicit teachers' preferences using an online survey among applicants for permanent positions during the 2016 centralized job application process, in which we obtained a response rate of just under 20% (5,550 teachers).[5] The survey results indicate that non-monetary factors significantly influence teachers' choices over job postings. As shown in Table 1, 44% of teachers mention 'proximity to home' as a crucial factor guiding their preference

[4]Figure B.2 provides a geographical visualization of these disparities by mapping the distribution of teachers' competency scores and students' test scores across provinces. Figure B.3 shows the correlation between our proxies for rurality, teacher competency score, and student achievement, documenting that smaller/more remote localities have worse teaching inputs and lower academic outputs. The correlation between teacher competency scores and student achievement in Math (Spanish) is 0.36 (0.39).

[5]As shown in Table B.3, observable teacher characteristics of survey respondents align closely with the population of applicants for permanent positions. Test scores of survey respondents are significantly higher than those within our choice sample (see Table B.1) since the survey was sent to teachers who were eligible to apply for permanent teaching positions, i.e. who scored above 120 points in the test.

Table 1: Applicant Survey (Choice Attributes)

	Rank			
	1^{st}	2^{nd}	3^{rd}	In Top 3
Close to House	44.17	11.66	8.00	63.83
Safe	10.66	24.19	19.25	54.1
Well Connected	9.69	20.62	20.20	50.51
Prestige	17.92	14.12	12.29	44.33
Cultural Reasons	10.61	9.67	12.31	32.59
Good Infrastructure	2.02	8.40	12.86	23.28
Good Students	1.24	4.52	6.08	11.84
Possibility other Jobs	1.93	3.72	4.90	10.55
Career	1.76	3.10	4.09	8.95

NOTES. This table summarizes the answers of 5,553 survey respondents to the question "What are the most important characteristics for your ranked choices?". Survey participants are applicants for permanent positions in the 2016 application process. The first three columns show the share of respondents that ranked the corresponding answer first, second, or third. The last column shows the share of respondents who listed the corresponding choice in their top 3 reasons. For other determinants of participation in the assignment mechanism and more results on heterogeneity in responses by competency score, see Table B.4.

ranking. Likewise, attributes such as prestige, safety, and cultural considerations are also frequently cited as relevant when ranking potential teaching positions.

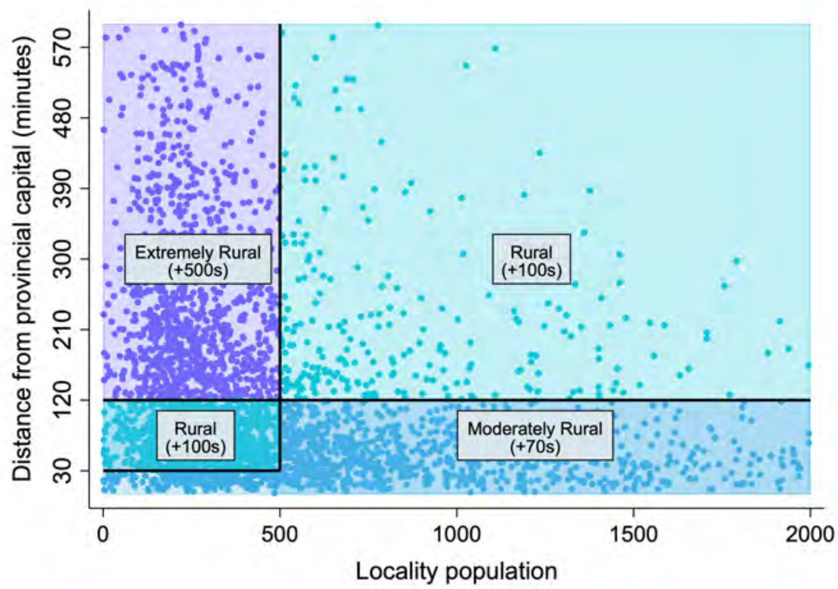
Since rigid wages fail to compensate for rural disamenities, teachers with higher priority systematically sort into urban schools. As a result, over half of urban postings (compared to a quarter in rural areas) are occupied by teachers ranked in the top 20% of the applicant pool within their school districts. This suggests that wage rigidity is an important contributor to unequal teacher sorting and the associated spatial disparities in student outcomes. In Section 3, we study the scope for teacher compensation reform to address these disparities by leveraging a policy that provided wage bonuses to teachers working in rural schools.

3 Teacher Compensation Reform

3.1 The Rural Wage Bonus Policy

Wage bonuses for contract teachers working in rural schools were introduced in August 2015, i.e., during the school year before the first wave of the centralized teacher recruitment drive. The reform was only announced briefly before being actually implemented. Both incumbent and newly assigned teachers were eligible for these bonuses. The policy established three

Figure 3: The Distribution of Rural Schools and the Wage Bonuses



Notes. This figure shows the spatial distribution of rural primary schools along the two dimensions that determine the assignment of the rural wage bonus: travel distance to the closest provincial capital (y-axis) and locality population (x-axis). The solid black lines indicate the cutoff(s) defining the rurality categories. Each category's name, together with the corresponding wage bonus (in Peruvian soles), is reported in the labeled boxes. The sample includes all schools in the choice sample.

categories of "rurality", which are defined based on two measures: (i) the school locality's population and (ii) travel time between the school and the provincial capital. The population of the locality is measured by the latest available census (2007), while travel time to the provincial capital is computed based on the school's GPS coordinates (taken on-site by government inspectors), the type of roads available, and the most frequent modes of transport.

Figure 3 shows how wage bonuses are distributed across schools based on the two measures defining the rural categories. *Extremely Rural* schools are in localities with less than 500 inhabitants and situated more than 120 minutes away from the provincial capital. Teachers in these schools receive a bonus of S/ 500 (US$ 144), representing between 25% to 36% of their base wage (depending on the year of the assignment).[6] *Rural* schools are either in localities with less than 500 inhabitants and situated between 30 and 120 minutes away from the province capital, or in localities with 500 to 2,000 inhabitants that are farther than 120 minutes from the province capital. The bonus received by teachers in these schools is S/ 100 (US$ 29). Finally, *Moderately Rural* schools are either in localities with 500 to 2,000

[6] The base monthly wage of contract teachers increased from S/ 1,396 in 2016 to S/ 2,000 (US$ 576) by the end of 2017.

inhabitants that are within 120 minutes of the province capital or in localities with less than 500 inhabitants that are within 30 minutes of the province capital. In these schools, teachers receive a bonus of S/ 70 (US$ 20). All other schools are classified as urban and are therefore not entitled to the rural wage bonus.

There is a large mass of schools around both the travel time (30 minutes and 120 minutes from the provincial capital) and the population cutoffs (500 inhabitants). As localities become more remote, they are more likely to have few inhabitants and predominantly fall into the *Extremely Rural* category. Likewise, as localities become more populated, they are less likely to be remote and fall into the *Moderately Rural* category.

3.2 Regression Discontinuity Design

To study the effects of increasing compensation in rural schools on teacher sorting and student achievement, we exploit the sharp thresholds that determine the allocation of the rural wage bonuses in a regression discontinuity (RD) design. The validity of this research design relies on two assumptions: (i) continuity of potential outcomes around the cutoffs, and (ii) independence between the potential outcomes of each unit and the treatment status of other units in a neighborhood of the cutoffs (SUTVA).

Continuity may be violated if the introduction of the rural wage bonus generated incentives for school administrators to manipulate the information used to determine eligibility for the bonus. The population cutoff of 500 inhabitants is based on census data collected before the policy was announced, and as such, is impossible to manipulate. The travel time cutoffs at 30 minutes and 120 minutes are based on GPS measures gathered periodically by government inspectors to account for possible changes in the transportation network and could be subject to manipulation. Figures C.1 and C.2 show a large and significant jump in the density of schools located just above the travel time threshold at 120 minutes in 2018 (but not for 2016) in both of our analysis samples. Instead, there are no significant jumps in the density of schools at the population threshold for either year for our two analysis samples.

SUTVA may be violated if the policy triggered spillovers through teacher sorting *around* the population cutoff – e.g. if teachers who chose a position in a high-bonus school just below the threshold would have otherwise chosen a position just above the threshold in the absence of the wage bonus policy. While we cannot entirely rule out this possibility, in Section 3.3 we report evidence that suggests that spatial spillovers of this sort are not a first-order concern in our setting.

Given the possible manipulation of the travel time threshold, we only exploit the variation in wages generated by the population threshold in this part of the analysis. The validity of the RD design around the population threshold is corroborated by covariate smoothness tests on both samples (see Table C.1).[7] Teachers recruited in localities with slightly less than 500 inhabitants earn, on average, about S/ 244 (US$ 72) more than teachers hired in localities that are just above the cutoff (corresponding to an increase of about 13%).[8]

To study the effects of the wage bonus policy on recruiting, we consider the following RD specification:

$$y_{ijt} = \gamma_0 + \gamma_1 \mathbf{1}(pop_j < pop_c) + g(pop_j, pop_c) + \delta_t + \varepsilon_{ijt}, \tag{1}$$

where y_{ijt} is the recruiting outcome for vacancy i (e.g. the competency score of the hired teacher) in school j at time t, $g(\cdot)$ is a flexible polynomial in the population of the locality of the school on both sides of the population cutoff, δ_t denotes time indicators for the specific year of the recruitment drive, and ε_{ijt} is an error term clustered at the school-year level.

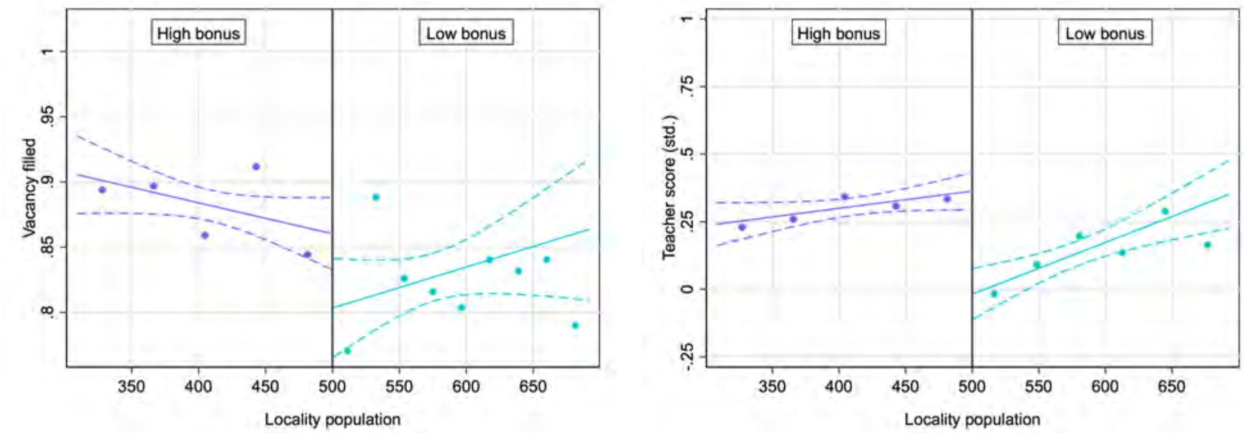
We further use Equation (1) to study the effects of the wage bonuses on student outcomes, denoted as $y_{\ell jt}$, e.g. student ℓ's standardized test score in school j at time t. In both analyses, the parameter of interest is γ_1, which represents the average outcome difference between vacancies or students in localities that are just below or above the population cutoff. We estimate γ_1 non-parametrically using the robust estimator proposed by Calonico et al. (2014) through bias-corrected local linear regressions that are defined within the mean squared error optimal bandwidths.

We exclude all urban schools and rural schools in localities within 30 minutes of the provincial capital, since crossing the population cutoff does not lead to an increase in the rural wage bonus in these schools (see Figure 3). In what follows, we refer to the observations in rural areas that are above the population cutoff of 500 inhabitants as 'Low-Bonus' and to those below the cutoff as 'High-Bonus'. The RD estimates reported in this section are robust

[7] Table C.1 shows that the indicator variable for the travel time threshold (travel time >120) does not significantly jump at the population threshold. This addresses concerns that schools might have manipulated the 120-minute threshold to a greater extent below the 500-inhabitant threshold (where the benefits from manipulating are larger), which would threaten the validity of our research design.

[8] Note that the wage increase at the threshold would be larger if we restrict our estimation sample to schools above the travel time cutoff of 120 minutes (see Table C.2). However, this alternative approach would imply conditioning on a partially manipulated variable (travel time) and decrease the sample size.

Figure 4: Teacher Choices and Sorting



a) Vacancy Filled **b) Competency Score**

NOTES. This figure shows how the probability that a vacancy is filled (Panel A) and the competency score of the assigned teacher (Panel B) vary with the population of the school's locality on either side of the 500-inhabitant cutoff. Each marker indicates the mean of the outcome variable within each bin, defined following the IMSE-optimal evenly spaced method proposed by Calonico et al. (2015). Solid lines represent predictions from linear regressions estimated separately on either side of the cutoff, assuming a triangular kernel. Dashed lines denote 95% asymptotic confidence intervals. The sample used is the choice sample. Additionally, the sample excludes urban schools (i.e., those located in areas with more than 2,000 inhabitants) and schools situated within 30 minutes of the nearest provincial capital. In Panel B, the sample is further restricted to vacancies filled through the centralized assignment process.

to alternative specifications, which we report in Figure C.4.

3.3 Teacher Sorting

We start by investigating how teachers' school choices responded to the wage increase at the threshold using our choice sample. The graphical evidence displayed in Panel A in Figure 4, along with the corresponding RD estimates reported in Column (1) of Table 2, documents that the wage increase at the threshold had a small and statistically insignificant effect on the probability that a vacancy is filled in the national recruitment drive.

We next restrict our attention to filled vacancies on either side of the threshold and consider as outcome variables a measure of teachers' preference intensity and the competency score of the assigned teachers.[9] The preference index takes the value of zero if the position is filled last and the value of one if the position is filled first in a school district. We find that high-paying vacancies were filled at a significantly higher priority order within the centralized

[9]These outcomes are observed for filled vacancies only, raising concerns about selection. We thus report in Table C.3 bounds around our RD estimates for both of these outcomes using the approach outlined in Gerard et al. (2020). The bounds are tight, suggesting that the potential censorship caused by restricting the analysis to filled vacancies is inconsequential for the results of our RD analysis.

Table 2: Teacher Choices and Sorting

	(1) Vacancy filled	(2) Preferences	(3) Teacher Score (Std.)
High Bonus	0.063	0.108	0.418
	(0.048)	(0.031)	(0.106)
Bandwidth	190.896	162.270	193.380
Schools	1478	1157	1388
Observations	3524	2575	3068

Notes. This table reports the effect of crossing the population threshold on several outcomes. In Column (1), the outcome variable is an indicator for whether the vacancy is filled in the national recruitment drive. Column (2) reports the effect on the rank at which a vacancy is chosen within a school district, normalized to range from zero to one. Column (3) reports the results on the competency score obtained by teachers in the centralized test. In all regressions, we use our choice sample and exclude urban schools (i.e., those located in areas with more than 2,000 inhabitants) and schools situated within 30 minutes of the nearest provincial capital. In Columns (2) and (3), the sample is further restricted to vacancies filled in the national recruitment drive. Cells report bias-corrected regression-discontinuity estimates obtained using the robust estimator proposed by Calonico et al. (2014). Regressions are estimated within a mean-squared-error–optimal bandwidth, reported at the bottom of the table. Standard errors are clustered at the school×year level.

assignment process than low-paying vacancies. The average vacancy in a low-bonus location is filled by a teacher ranked in the 34^{rd} percentile $(= 1 - 0.66)$ of the score distribution of applicants in that school district. Column (2) of Table 2 shows that high-bonus schools fill the position with an applicant ranked in the 23^{rd} percentile on average $(= 1 - 0.66 - 0.11)$.

Given that priority for choosing vacancies in the recruitment drive for short-term positions is determined by teachers' competency scores only, we expect that the increase in preferences for high-bonus schools documented in Column (2) translates into an increase in the quality of recruited teachers. Both the graphical evidence in Panel B of Figure 4 and the RD estimates in Column (3) of Table 2 document that teachers who sort into high-bonus schools have higher competency scores than those who choose a position in low-bonus schools. The effect is sizable, at 0.42σ of the overall distribution of the competency score. As a benchmark, this corresponds to approximately twice the gap in teacher competency between *Extremely Rural* schools and other rural schools and two-thirds of the overall urban-rural gap (see Figure 2).

In sum, increasing wages in disadvantaged locations effectively steered teachers' labor supply toward the targeted job postings. The observed change in teachers' behavior does not seem to have significantly affected the probability of creating new matches, but instead led to an inflow of more competent teachers in high-bonus schools.[10]

Additional evidence suggests a limited role of spillovers around the 500 population threshold. First, it is important to note that localities around the population threshold are not

[10] This result is consistent with recent evidence reported in Agarwal (2017), which documents that the primary effect of financial incentives was to increase the quality, not numbers, of medical residents in rural America.

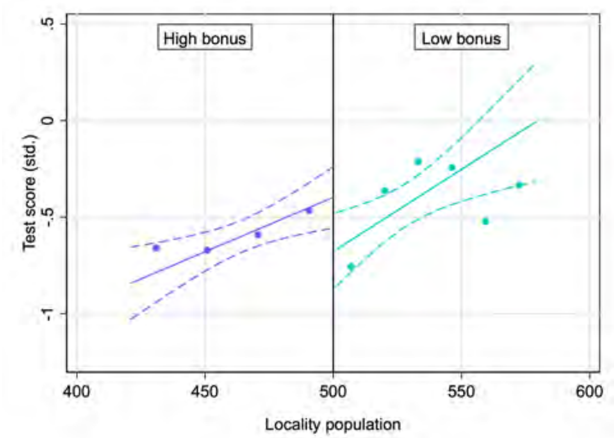
necessarily geographically close to each other. In fact, for any given school below the cutoff, the median (geodesic) distance to its first, second, and third closest school above the cutoff is approximately 10km, 20km, and 30km, respectively. Second, we use information on teachers' previous assignments to quantify potential reallocation between low-bonus and high-bonus schools around the threshold induced by the policy. Figure C.3 shows no evidence that teachers working in high-bonus schools are more likely to have previously worked in low-bonus schools close to the threshold. The estimated effect for schools just above the threshold is negative, suggesting that high-bonus schools are less likely to draw teachers from schools located just above the population cutoff relative to the rest of the locality-size distribution. Finally, Table C.4 reports the results of a difference-in-discontinuity analysis, which suggests that the increase in teacher competency in high-bonus schools did not come at the expense of decreasing the competency scores of teachers assigned in low-bonus schools near the threshold.

3.4 Student Achievement

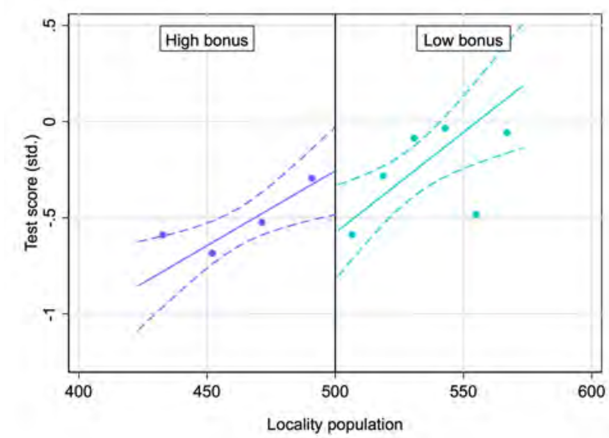
Offering higher wages for positions in rural locations could potentially improve student outcomes through two mechanisms: (i) the recruitment of higher-quality teachers, and (ii) an increased effort from incumbent teachers. To parse out the effort margin from the recruitment margin, we leverage matched teacher-classroom data (i.e. our outcome sample) and run the analysis separately for classrooms taught by newly recruited teachers and for those taught by incumbent teachers. We use fourth-grade student test scores in math and Spanish in 2016 and 2018 as measures of academic achievement.

Figure 5 plots the threshold crossing effects on students' academic achievement in classrooms taught by newly recruited teachers. Students exposed to higher-quality teachers, attracted by the wage increase at the threshold, perform significantly better on standardized tests. Column (1) in Table 3 shows sizable effects ranging from 0.34σ in Spanish to 0.54σ in math. We find no evidence of an increased effort from incumbent teachers in response to the wage increase at the threshold. Column (2) of Table 3 shows very small effect sizes of the wage bonus when restricting the sample to schools with no open vacancies. This result mirrors recent findings that establish little or no effects of unconditional wage increases in contexts where most teachers are public servants with permanent contracts (de Ree et al., 2018). Finally, Column (3) displays the aggregate effect on the overall sample, including

Figure 5: Wage Bonus and Student Achievement



a) **Spanish Test Scores**



b) **Math Test Scores**

Notes. This figure shows how student achievement varies with the population of the school's locality on either side of the 500-inhabitant cutoff. Panel A reports standardized test scores in Spanish, while Panel B reports standardized test scores in mathematics. Each marker indicates the mean of the outcome variable within each bin, defined following the IMSE-optimal evenly spaced method proposed by Calonico et al. (2015). Solid lines represent predictions from linear regressions estimated separately on either side of the cutoff, assuming a triangular kernel. Dashed lines denote 95% asymptotic confidence intervals. The sample used is the outcome sample, focusing on students taught by newly recruited teachers (corresponding to Column (1) of Table 3). Additionally, the sample excludes urban schools (i.e., those located in areas with more than 2,000 inhabitants) and schools situated within 30 minutes of the nearest provincial capital.

Table 3: Wage Bonus and Student Achievement

Panel A: Dependent Variable is Spanish Test (z-score)			
	(1) New teacher	(2) No vacancy	(3) All
High Bonus	0.340	-0.080	0.206
	(0.174)	(0.124)	(0.084)
Bandwidth	92.657	143.045	114.510
Schools	221	436	931
Observations	3830	6578	18828
Panel B: Dependent Variable is Math Test (z-score)			
	(1) New teacher	(2) No vacancy	(3) All
High Bonus	0.543	-0.040	0.265
	(0.221)	(0.143)	(0.099)
Bandwidth	79.168	144.756	110.039
Schools	192	442	889
Observations	3408	6699	18083

Notes. This table reports the effect of crossing the population threshold on student achievement in Math and Spanish. The outcome variables are standardized test scores in Spanish (Panel A) and Math (Panel B) for fourth-grade students. Column (1) considers the sample of fourth-grade students in our outcome sample (i.e. taught by newly recruited contract teachers). Column (2) considers schools with no open vacancies for contract teaching positions. Column (3) considers the overall sample of students, irrespective of whether they are taught by a newly assigned or an incumbent teacher. In all columns, the sample excludes urban schools (i.e., schools located in areas with more than 5,000 inhabitants) and schools situated within 30 minutes of the nearest provincial capital. Each cell reports bias-corrected regression-discontinuity estimates obtained using the robust estimator proposed by Calonico et al. (2014). Regressions are estimated within a mean-squared-error–optimal bandwidth, reported at the bottom of the table. Standard errors are clustered at the classroom×year level.

students taught by new recruits and incumbents. Students in high-bonus schools perform overall better in Spanish and math, with effect sizes of 0.20-0.26σ. This evidence shows that the teacher compensation reform triggered an inflow of more effective teachers, which in turn improved students' learning outcomes.[11]

We find limited empirical support for alternative mechanisms through which the rural wage bonus may affect student outcomes. Table C.5 documents no evidence of composition effects at the population cutoff along teachers' observable characteristics like gender, age, experience, native mother tongue, or having a university degree. We do not find any significant effect on the size and composition of the teaching staff (see Table C.6), nor on teacher retention (see Table C.7).

4 An Empirical Model of Teacher Sorting and Student Achievement

The previous section established that teacher compensation reform can effectively influence teacher sorting and reduce spatial gaps in student achievement. We now build a model of teacher sorting and student achievement to quantify the equity and efficiency gains attainable by optimal teacher compensation design.

4.1 Wages, Preferences, and Equilibrium

We consider a population of N teachers indexed by i, J schools indexed by j, and L students indexed by ℓ. Time is indexed by t. We denote the realized school assignment of teacher i in year t as $D_{it} \in \{1, ..., J\}$.

Wages are set by the government through a known, deterministic, rule. They are posted ex-ante for each available vacancy, observed by applicants before making their choices, and they cannot be renegotiated. Teachers receive the same fixed baseline wage, irrespective of the school they work in, and a set of wage bonuses, which vary with pre-determined locality and school characteristics (see Figure 1). We denote the monthly wage (with eventual bonuses)

[11] In Appendix Table C.8 we replicate the evidence presented in Table 3 using the universe of students, including those who are not in our matched teacher-classroom dataset (see Section 2.2). The estimates are in line with the main findings, although smaller in magnitude as we are pooling together classrooms within the same school with both incumbent teachers and new recruits.

in school j in year t as w_{jt}.

We define the indirect utility that teacher i gets from being matched with school j in year t as:

$$\begin{aligned} U_{ijt} &= u_{ijt} + \eta_{ijt} \\ &= \alpha_i w_{jt} + u(x_i, z_{jt}, d_{ijt}) + \kappa_{c(j)} + \eta_{ijt}, \end{aligned} \tag{2}$$

where $u(x_i, z_{jt}, d_{ijt})$ is a flexible function of observed school characteristics (z_{jt}), teacher characteristics (x_i) and measures of geographical proximity or familiarity between school j and teacher i (d_{ijt}) giving rise to vertical and horizontal differentiation between jobs offering the same wage, $\kappa_{c(j)}$ are province fixed effects capturing vertical differences in unobserved non-pecuniary factors across locations, and η_{ijt} is an unobserved taste shock introducing further horizontal differentiation in teachers' preferences. The parameter α_i controls teacher i's taste for wages relative to other non-pecuniary factors.

We specify the function u as follows:

$$u(x_i, z_{jt}, d_{ijt}) = x_i' \Theta_1 z_{jt} + x_i' \Theta_2 d_{ijt}$$

where the vector z_{jt} contains an amenity/infrastructure index,[12] indicators for whether school j belongs to specific categories that are eligible for other wage bonuses such as multi-grade, single-teacher, bilingual, and/or specific geographic areas (see Figure 1), polynomials of the locality's population and travel time (in hours) to the provincial capital,[13] and school averages of student characteristics including their 2^{nd} grade test scores in math and Spanish, gender, age, and ethnolinguistic background. The vector d_{ijt} contains linear splines of the geodesic distance between school j and teacher i's home location, and an indicator for whether teacher i was working in school j in the previous year. The vector x_i contains an intercept, gender, competency score, indicators for experience in the public and private sector, and ethnolinguistic background.

Around two-thirds of the applicants remain unassigned at the end of the centralized

[12]The amenity index is constructed by aggregating different indicators measuring the quality of infrastructure in the locality (see Panel D of Table B.2) as well as an asset-based measure of poverty at the individual level that we aggregate at the locality level (which enters with a negative sign in the overall index) by PCA.

[13]Conditional on z_{jt}, the residual variation in wages w_{jt} stems from the discrete changes at the thresholds induced by the rural wage policy (see Section 3.1).

assignment procedure. Among the unassigned applicants, half find alternative teaching positions within the public sector in later decentralized rounds. We denote this alternative as $j = J + 1$ and normalize its utility as follows:

$$u_{iJ+1t} = \gamma_1 \mathbb{1}\{t = 2018\} + x_i' \gamma_2, \tag{3}$$

where $x_i'\gamma_2$ captures heterogeneity across teachers in the value of participating in the secondary decentralized market.

The remaining unassigned applicants seek opportunities outside the public sector (e.g., teaching in a private school, or any other occupation, or staying outside of the labor force). We denote this alternative as $j = 0$ and normalize its utility as follows:

$$u_{i0t} = \beta_1 \mathbb{1}\{t = 2018\} + \beta_i, \tag{4}$$

where β_i is a teacher-specific random coefficient capturing observed and unobserved heterogeneity in the pecuniary and non-pecuniary benefits of choosing to leave the public sector. We assume that options $j \in \{J + 1, 0\}$ are both in the choice sets of all teachers.

We define $\theta_i = (\log \alpha_i, \beta_i)$ and assume that

$$\theta_i | x_i \sim \mathcal{N}(\mu_\theta' x_i, \Sigma_\theta).$$

These correlated random coefficients capture time-invariant idiosyncratic factors that affect how teachers substitute non-pecuniary benefits against wages, as well as how they value the outside option. Allowing for these coefficients to be correlated is crucial to correctly pin down substitution patterns from the outside option in response to changes in wages across job postings. Observing two choices per teacher and having variation in choice sets within and across teachers helps in the identification of the distribution of random coefficients (Berry et al., 2004). For example, teachers choosing the outside option (4) in one year and a public school in another year provide essential variation to identify the off-diagonal elements of Σ_θ.

As described in Section 2.1, applicants are ranked based on their competency score within school districts and choose, by order of priority, their preferred school among those that still have open vacancies. This precludes the existence of blocking pairs, i.e. teachers would not be accepted by a school they strictly prefer to the one they choose (Roth and Sotomayor, 1992). As teachers choose their district before knowing which options are available to them, they might realize ex-post that they would have preferred a feasible school in another district. However, only 0.60% of unassigned applicants chose a position in another district in later decentralized rounds. We take this as evidence that justified envy is minimal in this setting and thus assume that the realized teacher-school assignment $D_{1t}, ..., D_{Nt}$ is stable both within and across school districts. As a result, we can characterize the equilibrium teacher-school match as follows:

$$D_{it} = \underset{j \in \Omega(s_{it}) \cup \{0, J+1\}}{\arg\max} \; U_{ijt}, \quad i = 1, ..., N, \tag{5}$$

where $\Omega(s_{it})$ is teacher i's feasible choice set: the set of schools that still have remaining vacancies after all applicants with a score larger than s_{it} made their choice. As $\Omega(s_{it})$ is directly observed in the data, the matching equilibrium can be rewritten as a multinomial logit model with personalized choice sets (Fack et al., 2019).

We estimate the preference parameters $(\Theta_1, \Theta_2, \gamma_1, \gamma_2, \beta_1, \boldsymbol{\kappa})$ as well as the distribution of the random coefficients $(\mu_\theta, \Sigma_\theta)$ by maximizing the log-likelihood of observing the matching history $(D_{it})_{i=1}^{T}$ for $i = 1, ..., N$

$$\sum_{i=1}^{N} \log \int \prod_{t=1}^{T} \frac{\exp\{u_{iD_{it}t}\}}{\exp\{u_{i0t}\} + \exp\{u_{iJ+1t}\} + \sum_{k \in \Omega(s_{it})} \exp\{u_{ikt}\}} \phi(\theta_i | \mu_\theta' x_i, \Sigma_\theta) d\theta_i, \tag{6}$$

where we approximate the integral using 50 draws from a Halton sequence (Judd, 1998).

4.2 Student Achievement

We specify an education production function that maps potential teacher-student matches to student outcomes. In particular, we define $Y_{\ell i}$ the potential outcome of student ℓ when taught by teacher $i \in \{1, ..., N\}$ such that

$$Y_{\ell i} = \delta_{0i} + X_{\ell 1}' \delta_{1i} + X_{\ell 2}' \delta_2 + \epsilon_{\ell i}, \tag{7}$$

where $\mathbb{E}[\epsilon_{\ell i}] = 0$, $\mathbb{E}[X_{\ell 1} \epsilon_{\ell i}] = 0$ and $\mathbb{E}[X_{\ell 2} \epsilon_{\ell i}] = 0$ by construction. The vector $X_{\ell 1}$ includes student ℓ's lagged math test scores (second grade) and gender, and $X_{\ell 2}$ includes the variables in $X_{\ell 1}$ averaged at the classroom level, as well as squared lagged test scores, ethnicity, and age both at the student level and averaged at the classroom level. The vector $X_{\ell 2}$ also includes the school characteristics $z_{j(\ell)}$ entering teachers' preferences (2), where $j(\ell)$ denotes the identity of the school where student ℓ is enrolled.[14] We normalize $X_{\ell 1}$ to be mean zero such that δ_{0i} can be interpreted as the average treatment effect of teacher i, while δ_{1i} captures the returns to $X_{\ell 1}$ when being exposed to teacher i. This allows teachers to have comparative advantages in their ability to teach students with different demographics or backgrounds.[15] Accounting for such teacher-student match effects is particularly important for our counterfactual analysis, as it enables us to quantify the potential efficiency gains from alternative teacher assignments.

We denote the realized assignment of student ℓ to teachers as $S_\ell \in \{1, ..., N\}$ and define $Y_\ell = \sum_i Y_{\ell i} \mathbb{1}\{S_\ell = i\}$ as the observed realized test score of student ℓ. A standard approach in the teacher value-added literature is to assume selection on observables such that

$$\mathbb{E}[Y_{\ell i} | X_{\ell 1}, X_{\ell 2}, S_\ell] = \delta_{0i} + X'_{\ell 1}\delta_{1i} + X'_{\ell 2}\delta_2, \quad i = 1, ..., N. \tag{8}$$

Under this assumption, and provided that there is sufficient within-teacher variation in $X_{\ell 1}$, a regression of Y_ℓ on teacher fixed effects, $X_{\ell 1}$ interacted with teacher fixed effects, and $X_{\ell 2}$ yields unbiased estimates of $(\delta_{0i}, \delta_{1i}, \delta_2)$. Although value-added methods relying on selection on observables have been shown to yield forecast unbiased estimates of teacher effects in various settings (Chetty et al., 2014a; Bacher-Hicks et al., 2014), this assumption remains contested (Rothstein, 2017).

To relax the selection on observables assumption of standard value-added models, we employ an alternative approach that exploits the data and structure imposed on teacher school choices to recover unbiased estimates of the value-added parameters under weaker assumptions. Following Abdulkadiroğlu et al. (2020), we adapt the control function approach of Dubin and McFadden (1984) to correct for selection on teachers' unobserved preference for their assigned school:

$$\mathbb{E}[Y_{\ell i} | X_{\ell 1}, X_{\ell 2}, S_\ell, \eta_{ij(\ell)}] = \delta_{0i} + X'_{\ell 1}\delta_{1i} + X'_{\ell 2}\delta_2 + \varphi \times (\eta_{ij(\ell)} - \mu_\eta), \quad i = 1, ..., N, \tag{9}$$

[14]We exclude indicators for VRAEM and Border (see Figure 1), due to a lack of within-teacher variation in the data.

[15]This approach follows recent work in the school/teacher value-added literature (Abdulkadiroğlu et al., 2020; Ahn et al., 2023). This specification for match effects nests other approaches used in the teacher value-added literature, which either assume constant effects (Chetty et al., 2014b) or constant effects within student sub-populations (Biasi et al., 2021; Bates et al., 2025).

where μ_η is Euler's constant. The parameter φ captures selection on unobserved teacher-school match effects that would make teachers more or less productive in schools that give them larger utility. Note that any selection on other teacher-specific unobserved preferences over schools is captured by δ_{0i}. Applying the law of iterated expectations, we can write

$$\mathbb{E}[Y_{\ell i}|X_{\ell 1}, X_{\ell 2}, S_\ell, D_i, x_i, d_i, s_i] = \delta_{0i} + X'_{\ell 1}\delta_{1i} + X'_{\ell 2}\delta_2 + \varphi\lambda_{j(\ell)}(D_i, x_i, d_i, s_i), \quad i = 1, ..., N, \tag{10}$$

where λ_j denotes the control function corresponding to the expected unobserved preference shock for school j conditional on the teacher characteristics (x_i, d_i, s_i) and school j being the chosen alternative of teacher i

$$\begin{aligned}\lambda_j(D_i, x_i, d_i, s_i) &:= \mathbb{E}[\eta_{ijt} - \mu_\eta | x_i, d_i, s_i, j = D_{it}] \\ &= \int \left[\log\left(\sum_{k\in\Omega(s_{it})\cup\{0,J+1\}} \exp(u_{ikt})\right) - u_{iD_{it}t}\right]\phi(\theta_i|\mu_\theta, \Sigma_\theta)d\theta_i\end{aligned}$$

Under this assumption, OLS yields unbiased estimates of $(\delta_{0i}, \delta_{1i}, \delta_2, \varphi)$ after plugging-in the control function term. Note that we exclude $(d_{ij(\ell)})$ and $(w_{j(\ell)})$ from directly entering students' potential outcomes. This provides important variation necessary to identify φ. Distance to home has been argued to be a valid preference shifter in school choice environments (Walters, 2018; Agarwal and Somaini, 2020). We also find no evidence of a direct effect of wage increases on student achievement when the recruitment channel is muted (see Column (2) in Table 3), supporting the validity of this exclusion restriction.

The resulting estimates of the teacher specific coefficients $(\hat{\delta}_{0i}, \hat{\delta}_{1i})$ are unbiased but noisy. Recent work has highlighted the benefits of using Empirical Bayes (EB) shrinkage when making decisions involving ranking and selection in the presence of such statistical uncertainty (Gu and Koenker, 2023; Walters, 2024). We denote $\delta_i = (\delta_{0i}, \delta_{1i})$ and treat $\hat{\delta}_i$'s as normally distributed with variance estimates denoted as $\hat{\Omega}_i$. We assume that the δ_i's are drawn i.i.d from a common prior $\delta_i|x_i, \hat{\Omega}_i \sim \mathcal{N}(\mu'_\delta x_i, \Sigma_\delta)$. We estimate $(\mu_\delta, \Sigma_\delta)$ by MLE and construct the estimated posterior means for the teacher value-added coefficients δ_i

$$\hat{\delta}_i^* = (\hat{\Omega}_i^{-1} + \hat{\Sigma}_\delta^{-1})^{-1}(\hat{\Omega}_i^{-1}\hat{\delta}_i + \hat{\Sigma}_\delta^{-1}\hat{\mu}'_\delta x_i).$$

Throughout the rest of the analysis we use $\hat{\delta}_{0i}^*$ as a measure of teacher i's average effectiveness (ATE) and $X'_{\ell 1}\hat{\delta}_{i1}^*$ as a measure of teacher i's comparative advantage with students with characteristics $X_{\ell 1}$ (match effect). We refer to the sum of the ATE and the match effect components as the value-added of teacher i for student ℓ.

Finally, we allow the teacher value-added coefficients δ_i to correlate with the random coefficients θ_i. This captures the link between teacher effectiveness and latent factors affecting their preferences over schools that could reveal their intrinsic motivation. It also captures any potential selection on teacher effectiveness that would occur as a result of counterfactual compensation policies. To identify and estimate the covariance matrix $\Sigma_{\theta,\delta}$, we leverage within-teacher variation in the characteristics of the chosen school. Intuitively, when teachers with similar observables choose the outside option or low/high wage positions at a higher frequency, this reveals information about their latent type θ_i. As we have an unbiased estimate of the teacher value-added coefficients $\hat{\delta}_i$, we can then identify and estimate the distribution of θ_i conditional on δ_i, which pins down $\Sigma_{\theta,\delta}$. A formal proof of identification and details on estimation can be found in Appendix E.1.

5 Estimation Results

5.1 Teacher Preferences

We report the full set of preference estimates in Tables D.1, D.2, and D.3. Table 4 reports the bottom decile, median, and top decile of the implied distribution of willingness-to-pay, measured in percentage of the monthly base wage, for various non-wage attributes contained in the vectors z_{jt} and d_{ijt} entering (2). Consistent with the survey evidence displayed in Table 1, teachers value local amenities, good teaching conditions, and geographical proximity. The median teacher would be willing to give up 11% of her baseline wage for a one-standard-deviation increase in the amenity/infrastructure quality index. The median willingness to pay to avoid teaching in multi-grade, single-teacher, or bilingual schools ranges between 43% and 103% of the base wage. The median cost of traveling one kilometer further away from home is decreasing with distance and ranges from 12% to 1% of the baseline wage.[16] The median teacher has a preference for schools enrolling relatively lower shares of female and Quechua students. Importantly, Columns (1)-(3) of Table 4 show that these valuations

[16]One kilometer measured in geodesic distance may entail substantial travel time in some regions of Perú due to poor road infrastructure quality. This may partly explain the large magnitudes of the estimated moving costs.

Table 4: Willingness to Pay for Non-Pecuniary Job Attributes

	Bottom Decile	Median	Top Decile
	(1)	(2)	(3)
Amenity Index	0.028	0.114	0.450
Multigrade	-1.880	-0.435	-0.075
Single Teacher	-4.263	-1.031	-0.193
Bilingual	-3.270	-0.747	0.338
Share of Female Students	-0.790	-0.103	0.156
Share of Quechua Students	-4.011	-0.898	1.003
Average Math Score	-0.293	0.001	0.236
Average Spanish Score	-0.018	0.015	0.110
Distance Spline: 0-20km	-0.451	-0.124	-0.034
Distance Spline: 20-100km	-0.232	-0.065	-0.018
Distance Spline: 100km+	-0.039	-0.010	-0.003

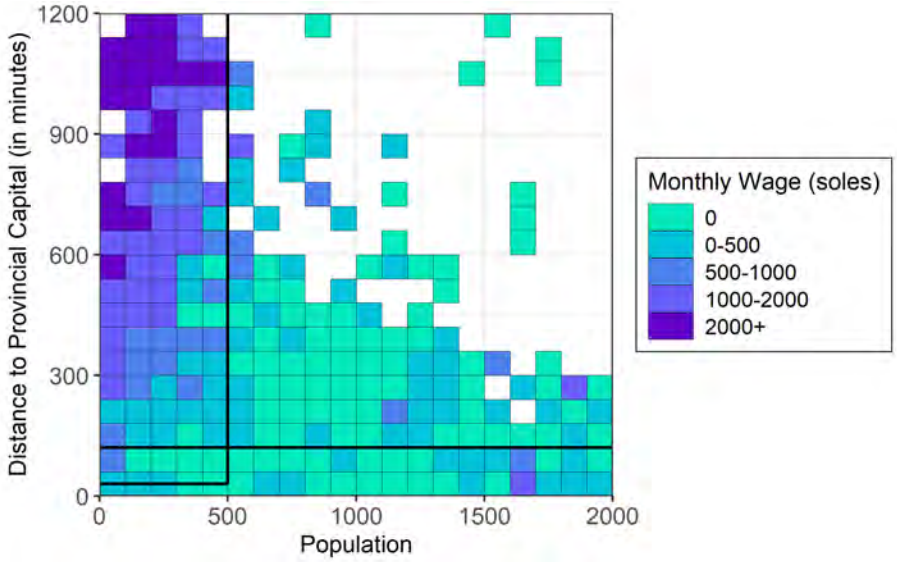
NOTES. This Table displays the median, bottom decile, and top decile of the distribution of the estimated preference parameters across teachers, expressed in percentage points relative to the baseline monthly wage in 2016 (S/ 1,396).

are highly heterogeneous across teachers. Non-pecuniary attributes thus induce substantial vertical and horizontal differentiation between school vacancies offering the same wage.

Schools located in rural areas tend to have worse teaching conditions, worse local amenities, and can potentially be very remote, compared to urban schools. This translates into large spatial differences in the utility of accepting a job. Figure 6 plots cell-average differences in the non-pecuniary components of the indirect utility, measured in monthly wages (in Peruvian Soles, S/), between rural schools and the average urban school, for teachers who score above the 25% percentile in the competency test. The vast majority of cells categorized as extremely rural would need to offer wage bonuses that range from two to four times the actual S/ 500 monthly wage bonus in order to be as attractive as the average urban school.

Overall, preference estimates suggest that the current wage bonus policy merely compensates for urban-rural differences in non-pecuniary utility. The current teacher allocation is thus likely to remain highly unequal, favoring urban schools. Figure 6 also shows substantial variation in non-pecuniary utility within geographic areas that receive the same bonus, suggesting that the rural wage bonus may be better targeted across schools and locations. This information, along with the substantial heterogeneity of preferences over non-pecuniary job characteristics (as shown in Table 4), may be leveraged to lower these utility differences and

Figure 6: Rural vs. Urban Differences in Non-Pecuniary Utility



Notes. To construct this figure, we restrict the sample to teachers in the upper quartile of the score distribution and simulate the non-pecuniary part of utility, as measured in wages: $\frac{U_{ijt}-\alpha_i w_{jt}}{\alpha_i}$ for each teacher-school pair using the estimated parameters from equation (2) and fixed draws of ϵ_{ijt} and θ_i. Then, we compute the median of the difference in those utilities for each rural school with the average urban school. Finally, we average across schools at the level of equally spaced cells of dimension 100×60 in the population-distance (in minutes) space.

help design better targeted teacher compensation policies, an issue we later return to.

5.2 Teacher Value Added

Table 5 displays our estimates of the distribution of the teacher value-added coefficients δ_i and the coefficient φ capturing selection on unobserved preferences.[17] As shown in Column (2), average effectiveness (ATE) varies substantially across teachers. Moving one standard deviation up the distribution of average effectiveness implies an increase in student score of 0.38σ in math and 0.33σ in Spanish.

Column (1) shows that the control function model attributes very little variation in student outcomes to match-specific utility that teachers get from their school assignment. As a result, shrunken estimates of the ATE derived from the control function model and the standard value-added model are very close to each other (see Figure D.1). As evidenced in prior work, this suggests that the selection on observables assumption in test score value-added models is reasonable when controlling for lagged outcomes (Chetty et al., 2014a).

Teachers widely differ in their comparative advantage in teaching students with different

[17] Column (1) of Table 5 only shows estimates of the mean of δ_i for the baseline category. Estimates of the interaction effects for the conditional mean of δ_i are displayed in Table D.5.

Table 5: Distribution of Value-Added Parameters

			Correlation		Covariance RC	
	Mean Baseline	SD	Lagged Score	Female	Wage	Outside Option
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Math test scores						
ATE	0	0.384 (0.006)	0.078 (0.031)	0.252 (0.082)	-0.049 (0.019)	-0.089 (0.046)
Lagged Score	0.498 (0.011)	0.150 (0.004)		0.021 (0.082)	-0.019 (0.011)	0.001 (0.023)
Female	-0.072 (0.014)	0.097 (0.011)			-0.008 (0.015)	-0.006 (0.030)
Unobserved Preferences (φ)	-0.007 (0.006)	-	-	-	-	-
Panel B: Spanish test scores						
ATE	0	0.329 (0.007)	0.279 (0.029)	-0.035 (0.101)	-0.062 (0.019)	-0.105 (0.043)
Lagged Score	0.549 (0.011)	0.169 (0.004)		-0.150 (0.102)	-0.005 (0.011)	0.026 (0.026)
Female	0.009 (0.014)	0.074 (0.014)			-0.021 (0.015)	-0.025 (0.029)
Unobserved Preferences (φ)	-0.007 (0.006)	-	-	-	-	-

NOTES. This table displays estimates of the mean of the value-added coefficients δ_i for the baseline category and the coefficient φ capturing selection on unobserved preferences through the control function term (Column 1), estimates of the variance-covariance matrix of δ_i (Columns 2 to 4), and estimates of the covariances between the teacher value added coefficients δ_i and the random coefficients θ_i (Columns 5 and 6). Standard errors are in parentheses.

characteristics. Students with second-grade test scores one standard deviation below average can experience fourth-grade test score gains of 0.15-0.17σ by being matched with a teacher with similar average effectiveness but with a one standard deviation higher match effect. Teachers who are effective in math, on average, tend to be particularly effective with female students, whereas teachers who are effective in Spanish, on average, tend to be particularly effective with students having higher initial levels of achievement (Columns 3 and 4). This evidence is consistent with findings in Ahn et al. (2023); Graham et al. (2023), which document that students lagging behind have the largest potential gains from teacher reallocation based on comparative advantage. It also points to large potential gains for rural areas from attracting better teachers along *both* absolute and comparative advantages.

Finally, Columns (4) and (5) of Table 5 display the estimated covariance matrix between the random coefficients (θ_i) and the value-added coefficients (δ_i). Teachers who are less

Table 6: Variance Decomposition of Student Outcomes

	Math	Spanish
	(1)	(2)
Total variance teacher value-added	0.087	0.042
Variance ATE	0.068	0.026
Variance match effects	0.011	0.015
2 × Cov(ATE,match effects)	0.007	0.001

Notes. This table displays the variance of student-level outcomes explained by teacher value-added, decomposed as the variance of the ATE, the match effects, and the covariance term. Variance components are derived using shrunken estimates of the teacher value-added coefficients.

responsive to wage differences are more effective, on average, and have a higher comparative advantage with students with lower prior achievement. Teachers with a higher outside option are less effective, on average. These findings play an important role in the counterfactual analysis that we present in Section 6, as they show that teachers attracted by wage increases are likely to be negatively selected. Not accounting for this correlation would bias predictions of teacher-school sorting under counterfactual compensation schedules.

Overall, depending on the subject taught, Table 6 shows that 62-78% of the total variance in student outcomes can be explained by differences in average effectiveness, while the remaining 22-38% is explained by differences in teachers' comparative advantage. Table D.6 in the Appendix shows that our observed teacher characteristics x_i explain 16-25% of the overall variance in the ATE, but do not explain variation in the match effect coefficients.[18] Conditioning on the random coefficients θ_i on top of the observed characteristics x_i slightly lowers the variance of the ATE but does not affect the variance of the match effect coefficients.

We use our teacher value-added estimates to study how teacher sorting contributes to the observed spatial gaps in student outcomes. Column (1) of Table 7 reports the average teacher effectiveness in urban and rural schools in the status quo scenario. Despite the large effects around the population threshold documented in the RD analysis of Section 3, teacher sorting remains highly unequal. Teachers working in rural schools have 0.14σ lower value-added than in urban schools. Match effects are uniformly unimportant, indicating that teachers do not sort based on their comparative advantages in the status quo (see Panel B of Table 7).

To characterize the potential gains from teacher reallocation, we consider the set of

[18]Table D.5 shows that teacher competency scores, experience in the private sector and gender strongly associate with their average effectiveness in math and Spanish.

Table 7: Teacher Sorting and Inequality

	Data	TVA Maximization
	(1)	(2)
Panel A: ATE Math		
Urban	0.112	0.053
Rural	-0.029	0.050
Other Rural	0.001	0.056
Extremely Rural	-0.085	0.038
Total	0.052	0.052
Panel B: Match Effects Math		
Urban	0.004	0.044
Rural	0.003	0.091
Other Rural	0.002	0.075
Extremely Rural	0.005	0.122
Total	0.003	0.064

NOTES. This table summarizes the spatial distribution of teacher effectiveness decomposed into average effectiveness (Panel A) and match effects (Panel B) in the teacher-school match observed in the data (Column 1) and in the student achievement maximization counterfactual (Column 2).

matched teacher-classrooms observed in the data and compute the teacher allocation that would maximize total student math test scores.[19] We report the distribution of teacher competency score and value-added in this counterfactual in Column (2) of Table 7. Leveraging match effects by reallocating the pool of existing teachers would increase student achievement in rural schools by 0.17σ ($=0.08\sigma$ in ATE + 0.09σ in Match Effects) and moderately decrease value-added in urban schools by 0.02σ. This would translate into aggregate gains in teacher value-added of 0.06σ.

We next quantify the potential gains from leveraging the extensive margin of recruitment by attracting applicants with higher average effectiveness into public teaching. Namely, we enlarge our pool of teachers by adding applicants that chose the outside option in the status quo, and select those with the largest ATE.[20] We find potential aggregate gains of up to 0.11σ, suggesting that large efficiency and equity gains could be attained by leveraging this extensive margin rather than by reallocating the existing pool of teachers to exploit match effects. We develop in Section 6 a wage-setting procedure that leverages information on teachers' preferences and effectiveness to achieve these gains in a cost-effective way.

[19]We weight by class size in the objective function to avoid mechanically penalizing schools that enroll fewer students.

[20]Note that for these applicants to be in our outcome sample, they would have needed to teach for at least one year in our data (i.e. in 2016 and 2018) and would have chosen the outside option in the other year.

5.3 Model Fit

We assess whether the estimated model replicates the observed teacher sorting patterns through several exercises. To simulate an equilibrium teacher-school match, we take a random draw of type-I extreme value (EV) shocks η_{ijt}, and a random draw of the random coefficients θ_i from its distribution conditional on x_i and $\hat{\delta}_i$. We then construct indirect utilities for every teacher-school pair and run the serial dictatorship algorithm to simulate the assignment mechanism (see Section 2.1).

We first test whether the model can replicate the threshold-crossing effects on teacher sorting discussed in Section 3. This provides a direct assessment of the validity of the wage elasticities implied by our estimates. Panel A of Figure 7 shows that the RD estimates derived from simulated teacher-school matches fall within the 95% confidence interval of the threshold-crossing effects estimated from the data (see Table 2). We then assess whether the model can replicate the overall sorting patterns observed in the data. Panel B of Figure 7 shows that the estimated model accurately replicates the urban-rural gap in teacher value-added and competency scores. Table D.7 in the Appendix further shows that model predictions closely match a wide range of additional moments of the distribution of matched teacher and school characteristics.

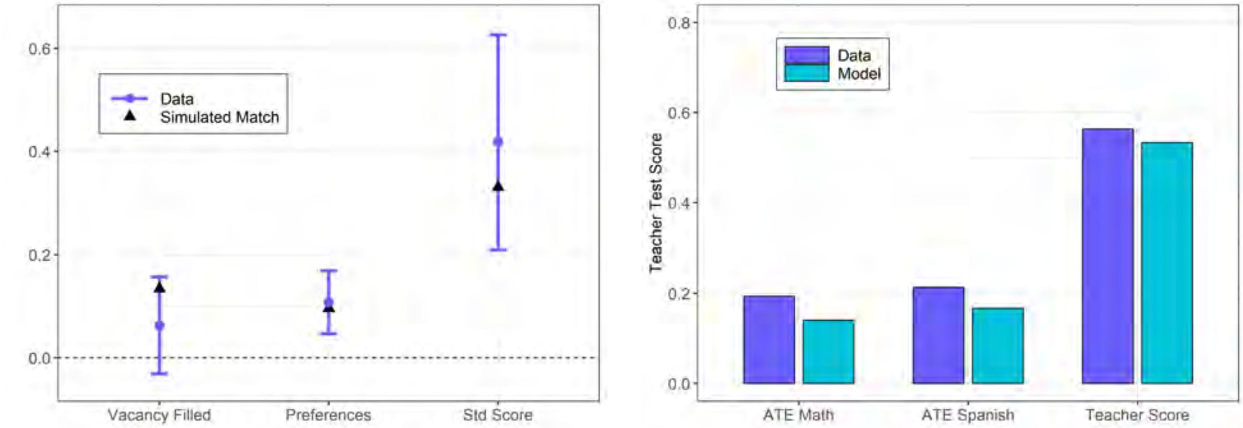
We further leverage our RD design to assess the robustness of our estimates of teacher effectiveness. Using the same sample as that in Column (1) of Table 3, we replicate our RD analysis on residualized student test scores constructed by differencing out the teacher value-added components (see Table D.8 in Appendix). Estimated threshold-crossing effects are still positive, but substantially muted and statistically insignificant. As teacher effectiveness is the only input in the student achievement production function that varies non-smoothly at the population cutoff, these results provide additional support for the validity of our value-added estimates.

6 Optimal Teacher Compensation Design

6.1 Framework

We seek to design a wage-setting protocol that systematically leverages information on teacher effectiveness and preferences over schools to achieve more equitable and efficient outcomes.

Figure 7: Model Fit – Sorting and Inequality



a) Threshold-Crossing Effects **b) Urban-Rural Inequality**

NOTES. Panel A compares estimates of the threshold crossing effects displayed in Table 2 with the corresponding simulated threshold-crossing effects estimates averaged over 10 simulated teacher-school matches. To simulate an equilibrium teacher-school match, we take a random draw of type-I EV shocks η_{ijt}, and a random draw of the random coefficients θ_i from its distribution conditional on x_i and $\hat{\delta}_i$. We then construct indirect utilities for every teacher-school pair and run the serial dictatorship algorithm to simulate the assignment mechanism (see Section 2.1). When using priority indices and test scores as outcomes, we exclude simulated matches where the threshold-crossing effect on the probability of filling vacancies is statistically significant at the 95% level to ensure validity of the RD design. The reported confidence intervals have 95% coverage. Panel B shows the urban-rural gap in teacher average effects in math and Spanish, and in teacher competency scores in the data, along with the simulated teacher-school match.

We assume that we have two policy levers: (i) setting the priorities used to rank teachers in the centralized assignment mechanism, and (ii) setting the wages offered in each school w_j. As schools can open more than one vacancy, we state our objective function as ensuring that each school j in a predefined set $\mathcal{S} \subset \{1, ..., J\}$ is assigned at least one teacher of effectiveness (value-added) Y_{ij} above a predefined threshold $\bar{c}$. We formalize the problem as follows:

$$\min_{w_1,...,w_J} \sum_j w_j, \text{ s.t. } \begin{cases} \max_{\{i:D_i=j\}} Y_{ij} \geq \bar{c}, \ \forall j \in \mathcal{S} & \text{(C1)} \\ D_i \text{ is stable given } (w_1, ..., w_J) \text{ and using } Y_{ij} \text{ as priorities} & \text{(C2)} \end{cases} \tag{11}$$

Condition (C1) imposes that our objective is attained under the resulting teacher-school match $(D_i)_{i=1}^N$. Condition (C2) imposes that the teacher-school match $(D_i)_{i=1}^N$ would result from the Deferred Acceptance algorithm if we set wages to $(w_1, ..., w_J)$ and priorities to Y_{ij}. We do not allow for teachers working in the same school to be paid differently.

We show that the solution of this problem is the school-optimal stable outcome of a "matching with contracts" economy (Hatfield and Milgrom, 2005) in which schools and teachers are allowed to propose different wages. Formally, we define teachers' preferences over school-wage contracts according to the indirect utility specified in (2), and we define schools' preferences over teacher-wage contracts as follows:

Assumption 1 *(i). For a fixed wage, schools preferences over contracts are responsive to the ranking induced by the priority index Y_{ij}.*
(ii). Any contract satisfying (C1) is strictly preferred to a contract not satisfying (C1). Among contracts satisfying (C1), or among contracts not satisfying (C1), the allocation with the lowest wage is always strictly preferred.

Assumption 1 (i) implies that for a given fixed wage, schools would prefer to hire the teachers with the highest priority Y_{ij} (Roth and Sotomayor, 1992). Assumption 1 (ii) implies that schools are willing to increase wages until (C1) is satisfied. We show in Appendix E.2 that this preference ordering satisfies the substitutes condition of Hatfield and Milgrom (2005). We can then establish the following result.

Proposition 1 *Under Assumption 1, the school-optimal stable set of contracts is the solution to* (11).

See Appendix E.3 for a proof. This result stems from Theorem 3 in Hatfield and Milgrom (2005) showing that a stable set of contracts always exists in this counterfactual economy. Intuitively, the proof shows that, since Assumption 1 (ii) implies that schools are willing to increase wages until (C1) is satisfied, stability implies that (C1) is satisfied. However, Assumption 1 (ii) also implies that schools strictly prefer allocations with lower wages meaning that the school-optimal stable allocation will satisfy (C1) while minimizing wages. Assumption 1 (i) implies that, for fixed wages, the allocation is stable with respect to school priorities which satisfies the implementability constraint (C2). The school-optimal stable set of contracts can be reached through the school-proposing generalized Deferred Acceptance algorithm introduced in Hatfield and Milgrom (2005), which delivers the wage schedule $(w_1, ..., w_J)$ solving (11).

6.2 Application

We apply the wage-setting protocol outlined in Proposition 1 to derive a set of cost-effective counterfactual compensation policies. We use teacher value-added for math as our outcome of interest and define $\mathcal{S}$ as the set of rural schools with at least one filled vacancy in the

Table 8: Counterfactual Teacher Compensation Policies

	Status Quo (1)	Optimal Policy (2)
Panel A: ATE Math		
Urban	0.112	0.138
Rural	-0.029	0.089
Other Rural	0.001	0.093
Extremely Rural	-0.085	0.079
Total	0.052	0.119
Panel B: Match Effects Math		
Urban	0.004	0.005
Rural	0.003	0.009
Other Rural	0.002	0.005
Extremely Rural	0.005	0.022
Total	0.003	0.007
Panel C: Monthly Wage Bonus		
Urban	0	0
Rural	235	227
Other Rural	86	153
Extremely Rural	500	365
Total	122	120

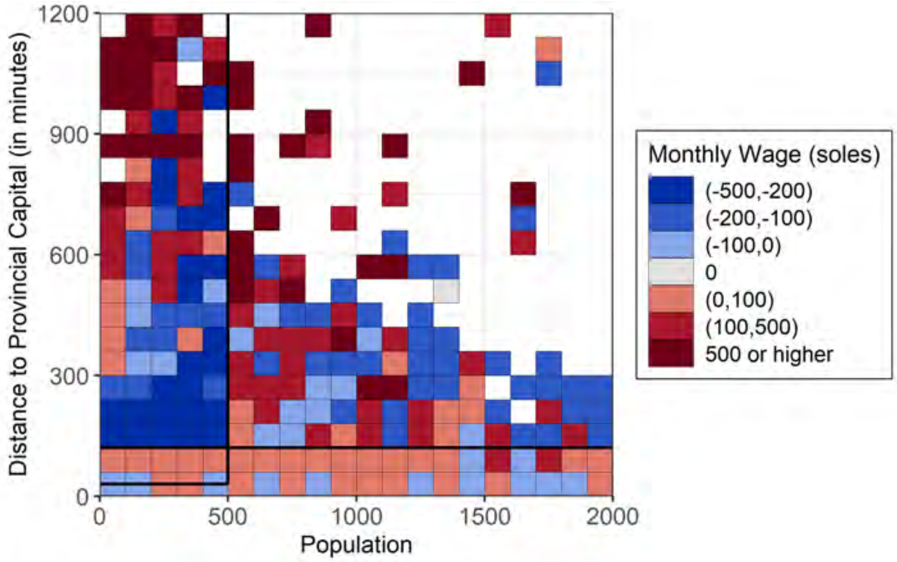
NOTES. This table summarizes the distribution of teacher average effectiveness (Panel A) and match effects (Panel B) under the simulated equilibrium teacher-school matches resulting from different wage bonus policies. Column (1) corresponds to the simulated match in the status quo while Column (2) corresponds to the simulated match under the wage policy solving (11) when setting Y_{ij} as math test score value-added and setting the threshold $\bar{c}$ such that the policy is budget neutral relative to the status quo policy. To simulate the equilibrium teacher-school match in each scenario, we construct indirect utilities for every teacher-school pair by taking a unique random draw of type-I EV shocks η_{ijt}, and a unique random draw of the random coefficients θ_i from its distribution conditional on x_i and $\hat{\delta}_i$.

status quo. We then run the generalized DA algorithm for different threshold values $\bar{c}$ and select the counterfactual compensation policy that is budget neutral relative to the status quo policy.[21] We report the distribution of teacher value-added under this counterfactual compensation policy in Column (2) of Table 8.

Our results show that large equity and efficiency gains are attainable under the proposed allocation mechanism. Teacher value added increases by 0.12σ in rural schools and by 0.03σ in urban schools. In contrast with the optimal reallocation counterfactual shown in Column (2) of Table 7, teacher value-added increases in the aggregate mostly through an increase in average effectiveness (ATE), i.e. by attracting higher quality teachers that chose the outside

[21] We set the step size for wage increases in the generalized DA algorithm to S/ 20.

Figure 8: Distribution Optimal Wage Bonuses Relative to Status Quo



NOTES. This figure plots cell-averaged differences between the monthly wage bonuses attributed to each school in the optimal policy described in column 3 of Table 7 and the status quo policy. Cells are equally spaced with dimension 100×60 in the population-travel time (in minute) space.

option in the status quo in the overall pool of assigned teachers. This suggests that inducing teacher reallocation to exploit match effects is not cost-effective relative to leveraging the extensive margin of recruitment by attracting applicants with higher average effectiveness into public teaching.

We further characterize how our counterfactual wage bonuses are distributed across schools. Panel C of Table 7 shows that the optimal policy reduces the overall weight put on extremely rural schools and redistributes it partly to other rural schools. Figure 8 shows a more disaggregated version of these figures by plotting cell-average differences between our counterfactual wages and the status quo policy. This uncovers substantial heterogeneity within the extremely rural category. Schools located close to the 120 travel-time threshold are substantially over-subsidized in the status quo policy, while the most remote schools are under-subsidized. Taken together, this evidence highlights the benefits of acquiring information on teachers' preferences and effectiveness to design more effective teacher compensation policy.

7 Conclusion

This paper establishes that the design of teacher compensation can largely exacerbate or alleviate structural inequalities in schooling outcomes. We assemble rich data on applicants and jobs posted in the nationwide centralized recruitment drive for public teachers in Perú. Wage rigidity induces teachers to sort on non-pecuniary aspects of employment, resulting in school choices that are skewed towards urban areas. We leverage a policy reform that increased teacher compensation in rural schools and show that it was effective at attracting higher quality teachers and improving student learning.

To go beyond the local estimated effects of the policy and understand the potential equity and efficiency gains that can be achieved through alternative teacher compensation schemes, we build and estimate a model of teacher sorting across schools and student achievement. Teachers are heterogeneous in their preferences over wage and non-wage attributes, which induces vertical and horizontal differentiation across jobs. Teacher sorting maps into student achievement through a teacher value-added model in which teacher effectiveness is heterogeneous and interacts with students' prior achievement and gender. We find that large equity and efficiency gains are attainable by either reallocating the existing pool of teachers to leverage match effects or by attracting applicants with higher average effectiveness into public teaching.

In the last part of our analysis, we develop a wage-setting procedure that seeks to achieve equity and efficiency gains at a minimal cost by leveraging information on teachers' preferences and effectiveness. We find that a substantial reduction in the urban-rural gap in teacher effectiveness can be achieved at the same cost as the status quo policy. These gains are achieved by increasing the average effectiveness of teachers working in rural areas, instead of leveraging match effects. This suggests that improving teacher effectiveness through the extensive margin of recruitment is more cost-effective than inducing teachers to sort based on their comparative advantages.

Stretching beyond the specific setting of our analysis, our findings suggest that incorporating measures of preferences and productivity in the design of worker compensation is a promising alternative to rigid wage schedules or market-based wage setting in a variety of other labor markets. Aligning the private and social returns of worker-firm matches through informed compensation design can result in more equitable and efficient allocations.

We believe that this approach is increasingly relevant from a policy perspective, given the widespread availability of administrative data on centralized labor markets as well as recent developments in the tools that enable researchers and practitioners to leverage such data to infer the preferences of participating agents (Roth, 2018; Agarwal and Budish, 2021).

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Appendices

A Institutional Details

A.1 The Assignment Mechanism for Permanent Vacancies

Every permanent position across all education levels is posted on a centralized platform. The opening of each position depends on previous retirements and transfers, as well as the ability of local governments to secure permanent funding for the position. Applicants are required to have a teaching accreditation (i.e., a teaching degree). They must also correctly answer at least 60 percent of the questions in each of the three parts (reading comprehension, logic reasoning, and curricular knowledge) of the national competency evaluation.

Eligible applicants can indicate their preferred school district and submit a rank order list of schools within that district. Once preferences are submitted, teachers move on to a decentralized stage of evaluation and enter a shortlist for their top three choices (top two in 2016). This shortlist has a maximum length of 10 (20 in 2016). For schools that are oversubscribed, test scores are used to prioritize candidates. In this second evaluation round, teachers are given another score based on a direct evaluation of their performance in teaching a typical class and an in-person interview with the principal and other school stakeholders. Points can also be assigned based on their CV. Finally, schools make offers sequentially to the applicants ranked according to the overall score that comprises the competency test and the decentralized evaluation. Unassigned applicants can then participate in an exceptional stage that allocates the remaining unfilled slots. At the end of this round, unassigned teachers can decide to participate in the allocation of temporary positions, which takes place shortly after.

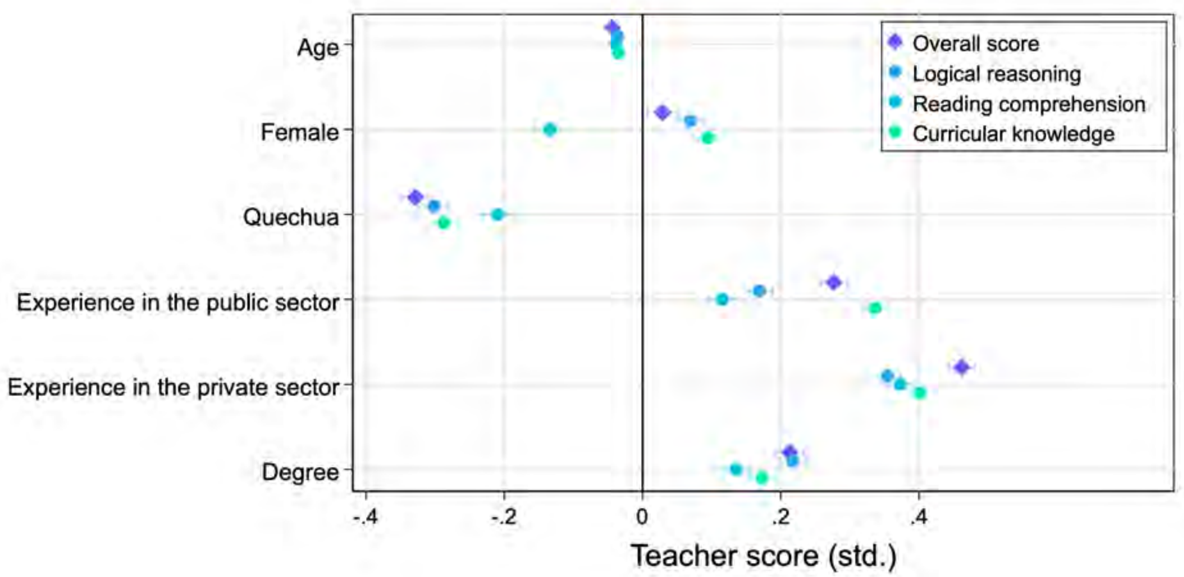
A.2 The Assignment Mechanism for Temporary Vacancies

Contract teacher positions are posted on the website of each school district. The list of vacancies includes both standard positions – that are based on fixed term one year contracts, with the possibility of renewal for a second year upon approval from the school's director – and occasional positions catering to short-term extra needs, such as covering maternity leaves and lasting up to one year. The list of contract teacher vacancies also includes positions that are not filled through the assignment mechanism for permanent teachers, and are later posted as contract teacher vacancies.

Participants in the assignment mechanism for contract teachers are asked to indicate a preferred school district when applying. School districts are administrative units corresponding roughly to Peruvian provinces. As of 2016, there are 226 school districts in Peru. Vacancies are assigned based on a serial dictatorship mechanism. All applicants to the assignment mechanism in a given school district and specialization are ranked based on the score they got on the national competency test, with bonus points awarded to those with recognized disabilities or who served in the Peruvian army. The assignment procedure works as follows: the highest-scoring teacher chooses their preferred position, which is thus removed from the list and thus is not available to the subsequent lower-ranked applicant. This procedure continues until all positions in the list are filled or the lowest-ranked applicant makes her choice. Vacancies that remain unfilled are made available to other groups of (unmatched) applicants who initially indicated a different school district or specialization. Specifically, they are first made available to those who initially indicated a different school district within the same region. Second, if vacancies still remain, they are made available to applicants who initially indicated a different region or stage/subject specialization. Any positions not filled through this procedure are then offered to non-certified teachers – who did not participate in the competency test – based on a committee evaluation of their curricula.

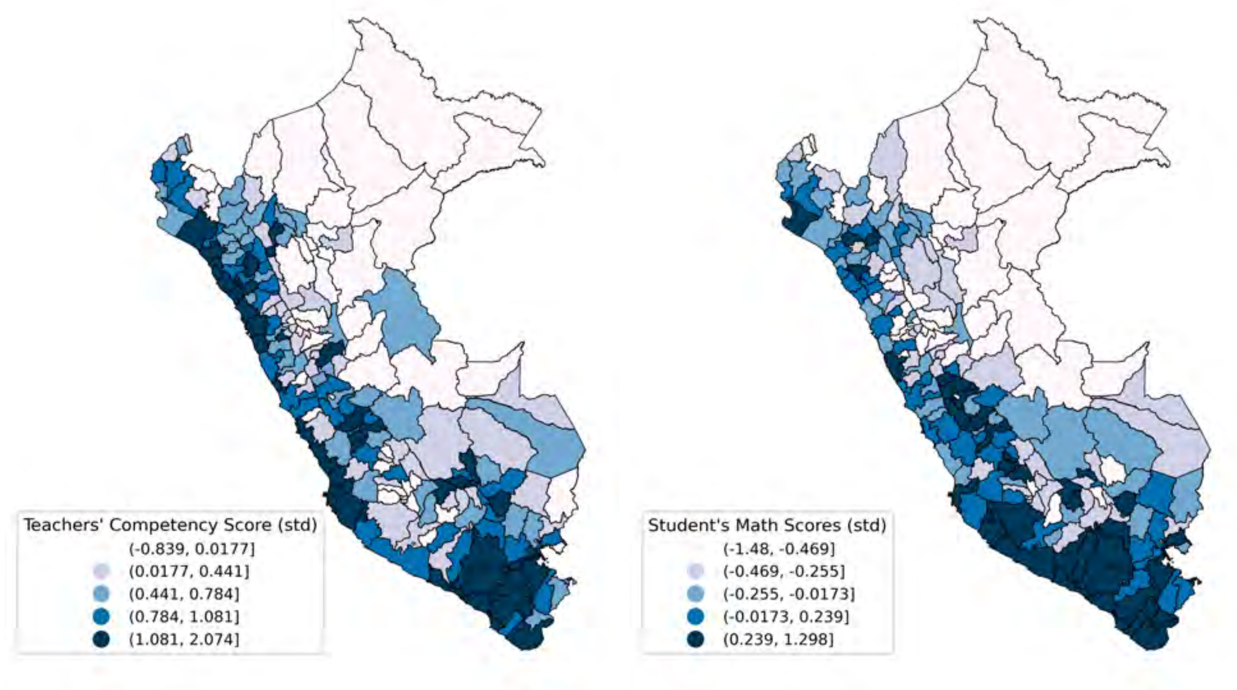
B Additional Descriptive Evidence

Figure B.1: Correlates of Performance in the Teacher Competency Test



NOTES: This figure reports OLS coefficient estimates with associated 95 percent confidence intervals from a multivariate regression of teacher competency scores on individual teacher characteristics. The covariates include age; indicators for female applicants and Quechua speakers; dummies for having at least three years of teaching experience in the public sector and at least one year of experience in the private sector; and an indicator for having a university education. The sample includes all applicants in the choice sample.

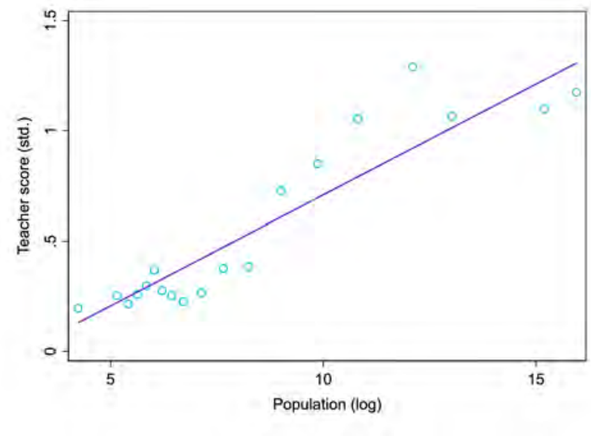
Figure B.2: Geographic Distribution of Teacher Competency and Student Achievement



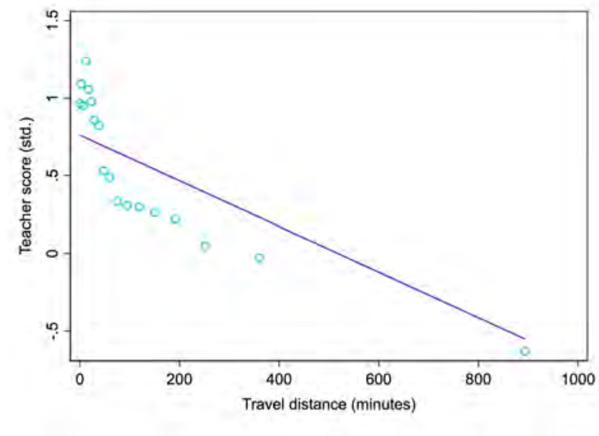
a) Teacher Competency by Province **b) Student Achievement (Math) by Province**

NOTES: This figure depicts the geographical variation in teachers' competency score (Panel A) and students' test scores in Math (Panel B) across provinces of Perú. In both panels, darker colors indicate higher average scores, with class intervals defined based on quintiles of the score distributions.

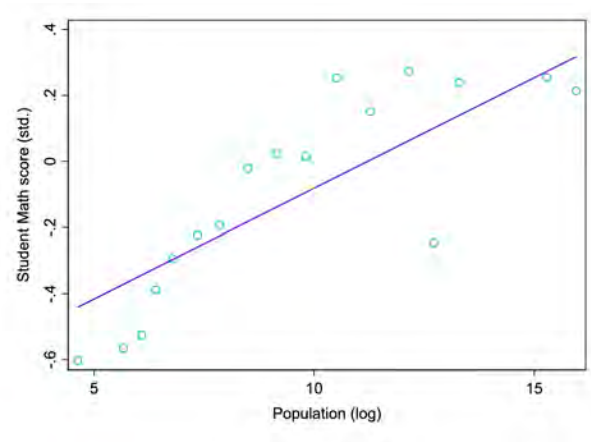
Figure B.3: Correlation between Rurality, Teacher Competency, and Student Achievement



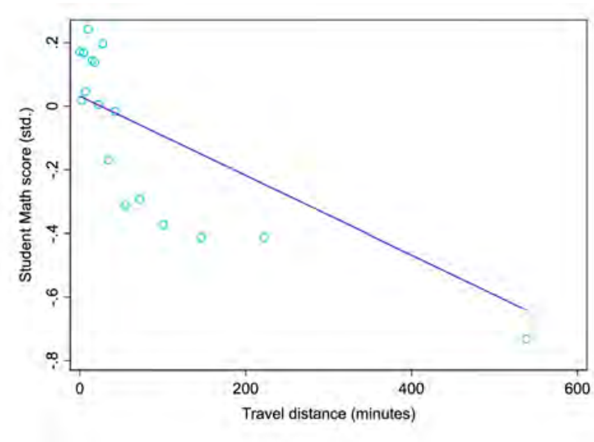
a) Teacher Competency *vs.* Population



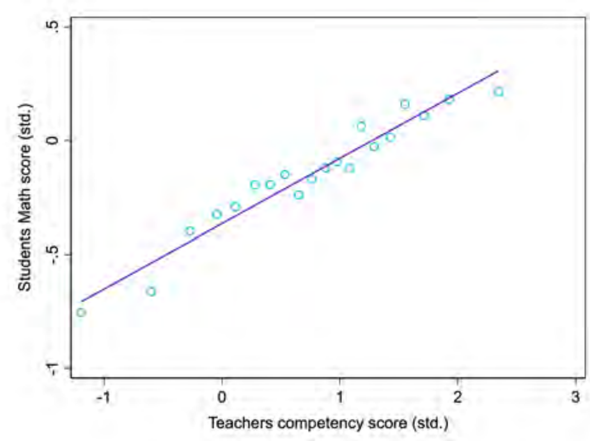
b) Teacher Competency *vs.* Travel Time



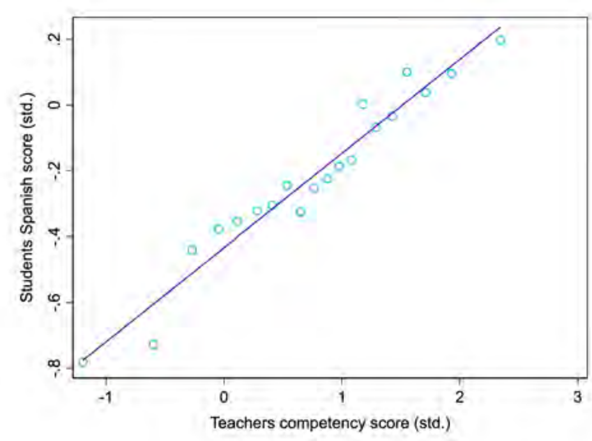
c) Student Achievement (Math) *vs.* Population



d) Student Achievement (Math) *vs.* Travel Time



e) Student Achievement (Math) *vs.* Teacher Competency



f) Student Achievement (Spanish) *vs.* Teacher Competency

NOTES: This figure illustrates the relationship between teacher competency, student achievement, and different dimensions of rurality. Rurality is proxied using two measures: the logarithm of the population of the village in which the school is located (Panels A and C) and the distance from the school to the nearest provincial capital (Panels B and D). Teacher competency (Panels A and B) is measured by the average score obtained by teachers in the 2016 and 2018 centralized assignment processes, while student achievement (Panels C and D) is measured by the average student score on the math module. Panels E and F show the relationship between teacher competency and student achievement in math and Spanish, respectively. Each circle represents the mean value within a percentile bin (ventile).

Table B.1: Descriptive Statistics for Analysis Samples

	Mean (1)	SD (2)
Panel A: Applicant characteristics (choice sample)		
Age	37.734	(6.922)
Female	0.742	(0.438)
Quechua	0.212	(0.409)
Experience in the public sector ($\geq$ 3 yrs.)	0.573	(0.495)
Experience in the private sector ($\geq$ 1 yr.)	0.479	(0.500)
Degree	0.297	(0.457)
Overall competency score (std)	-0.062	(1.013)
N. of teachers	36,698	
N. of applications	50,319	
Panel B: Vacancy characteristics (choice sample)		
Monthly Wage (soles)	1,843.162	(394.779)
Single Teacher	0.009	(0.097)
Multigrade	0.200	(0.400)
Urban	0.522	(0.500)
Extremely Rural	0.164	(0.371)
Other Rural	0.314	(0.464)
Bilingual	0.108	(0.310)
Border	0.065	(0.247)
VRAEM	0.037	(0.189)
Time-to-travel to provincial capital	127.371	(285.476)
Locality population (log)	9.061	(3.674)
Amenity/infrastructure index	0.090	(0.981)
N. of school	9,951	
N. of vacancies	37,864	
Panel C: Student characteristics (outcome sample)		
Age	9.202	(0.666)
Female	0.496	(0.500)
Quechua	0.122	(0.328)
Spanish test scores (std)	-0.097	(0.988)
Math test scores (std)	-0.051	(1.001)
N. of teachers	4,834	
N. of students	96,508	

Notes. This table reports summary statistics for the characteristics of applicants (Panel A) and vacancies (Panel B) that were part of the assignment mechanism for temporary positions in 2016 and 2018 and constitute our choice sample. Panel C reports the characteristics of 4th-grade students in our outcome sample.

Table B.2: School and Locality Characteristics by Rurality

	Rural		Urban	
	Mean	SD	Mean	SD
Panel A: School characteristics				
Single Teacher	0.0142	(0.118)	0	(0)
Multigrade	0.404	(0.491)	0.00579	(0.0759)
Bilingual	0.193	(0.395)	0.0196	(0.139)
Border	0.0701	(0.255)	0.0541	(0.226)
VRAEM	0.0462	(0.210)	0.0194	(0.138)
Panel B: Locality amenities				
Amenity/infrastructure index	-0.385	(0.932)	0.544	(0.774)
Poverty index	0.964	(1.424)	0.00995	(0.887)
Electricity	0.909	(0.287)	0.997	(0.0509)
Drinking water	0.674	(0.469)	0.941	(0.236)
Sewage	0.429	(0.495)	0.925	(0.263)
Water tower	0.256	(0.436)	0.597	(0.491)
Medical clinic	0.638	(0.480)	0.916	(0.277)
Meal center	0.251	(0.434)	0.584	(0.493)
Community phone	0.0910	(0.288)	0.122	(0.327)
Internet access point	0.181	(0.385)	0.883	(0.321)
Bank	0.0587	(0.235)	0.628	(0.483)
Public library	0.0389	(0.193)	0.488	(0.500)
Police	0.199	(0.399)	0.598	(0.490)
Panel C: School infrastructure				
Teachers room	0.134	(0.341)	0.324	(0.468)
Sport pitch	0.167	(0.373)	0.325	(0.468)
Courtyard	0.174	(0.379)	0.376	(0.484)
Auditorium	0.0868	(0.282)	0.195	(0.396)
Administrative office	0.484	(0.500)	0.666	(0.472)
Courtyard	0.0108	(0.103)	0.0777	(0.268)
Computer lab	0.371	(0.483)	0.834	(0.372)
Workshop	0.0490	(0.216)	0.304	(0.460)
Science lab	0.0766	(0.266)	0.420	(0.494)
Library	0.433	(0.495)	0.721	(0.448)
Personal computer	0.785	(0.411)	0.916	(0.278)

Notes. This table reports the average and standard deviation of various school characteristics by rurality. The sample includes all vacancies in the choice sample.

Table B.3: Survey Respondents Characteristics

	All applicants (2016) (1)	Survey respondents (2)
Age	34.546	35.201
	(6.184)	(6.546)
Female	0.725	0.723
	(0.447)	(0.448)
Quechua	0.109	0.084
	(0.312)	(0.277)
Experience in the public sector ($\geq$ 3 yrs.)	0.621	0.614
	(0.485)	(0.487)
Experience in the private sector ($\geq$ 1 yr.)	0.674	0.709
	(0.469)	(0.454)
Degree	0.559	0.563
	(0.497)	(0.496)
Overall competecy score (std)	1.593	1.608
	(0.390)	(0.457)
N. of teachers	22,784	5,550

Notes. This table reports average teacher characteristics for the sample of survey respondents (Column 2) and the sample of applicants in the 2016 recruitment drive for permanent teaching positions (Column 1). Standard deviations are reported in parenthesis.

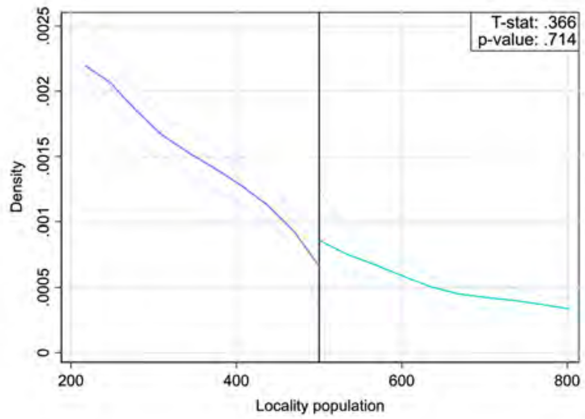
Table B.4: Applicant Survey (Participation and Choice Attributes)

	All Teachers Rank				Score in Top Quartile Rank			
	1^{st}	2^{nd}	3^{rd}	In Top 3	1^{st}	2^{nd}	3^{rd}	In Top 3
Panel A: Why did you apply to the centralized assignment mechanism? (% of respondents)								
Career	33.77	30.35	20.57	84.69	33.73	29.97	21.35	85.05
Stability	51.08	17.04	14.76	82.88	50.66	18.26	13.92	82.84
Formation Opportunities	9.63	29.15	21.81	60.59	9.57	26.73	20.32	56.62
Better Wage Opportunities	2.08	9.51	23.84	35.43	2.14	11.41	22.75	36.3
Social Benefits	1.04	7.78	7.96	16.78	1.10	7.00	7.58	15.68
Prestige	1.71	4.28	7.19	13.18	1.62	3.24	7.73	12.59
18 mil Soles Incentive	0.69	1.89	3.87	6.45	1.18	3.39	6.33	10.9
Panel B: What are the most important characteristic for your ranked choices? (% of respondents)								
Close to House	44.17	11.66	8.00	63.83	49.77	13.22	8.76	71.75
Safe	10.66	24.19	19.25	54.1	7.65	24.50	19.35	51.5
Well Connected	9.69	20.62	20.20	50.51	8.23	18.70	19.67	46.6
Prestige	17.92	14.12	12.29	44.33	21.13	15.77	12.68	49.58
Cultural Reasons	10.61	9.67	12.31	32.59	7.58	9.45	12.61	29.64
Good Infrastructure	2.02	8.40	12.86	23.28	1.81	7.23	11.83	20.87
Good Students	1.24	4.52	6.08	11.84	0.84	4.36	5.95	11.15
Possibility other Jobs	1.93	3.72	4.90	10.55	1.62	4.10	4.71	10.43
Career	1.76	3.10	4.09	8.95	1.36	2.67	4.44	8.47

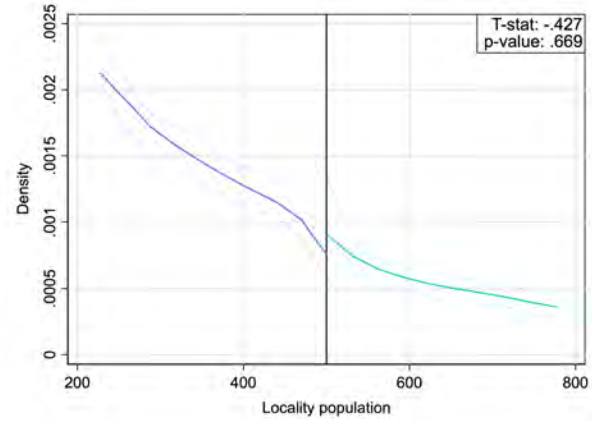
Notes. This table displays the share of the 5,553 survey respondents that chose the corresponding answers to Question A and B. The first three columns show which answer they chose and how they ranked them (by order of importance) while column (4) shows the share of respondents that listed the corresponding choice in their top 3 reasons. The last four columns display the same results for respondents that scored above the top quartile of the test score distribution for tenured teachers.

C Regression Discontinuity Analysis

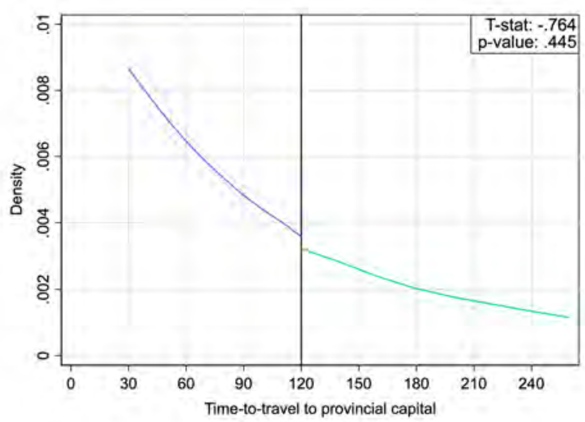
Figure C.1: Densities Running Variables: Choice Sample



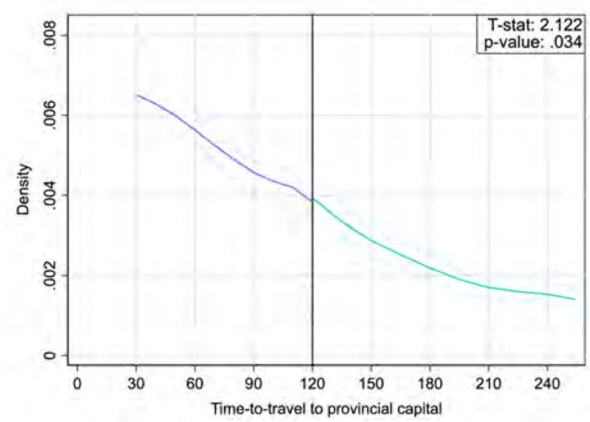
a. Population (2016)



b. Population (2018)



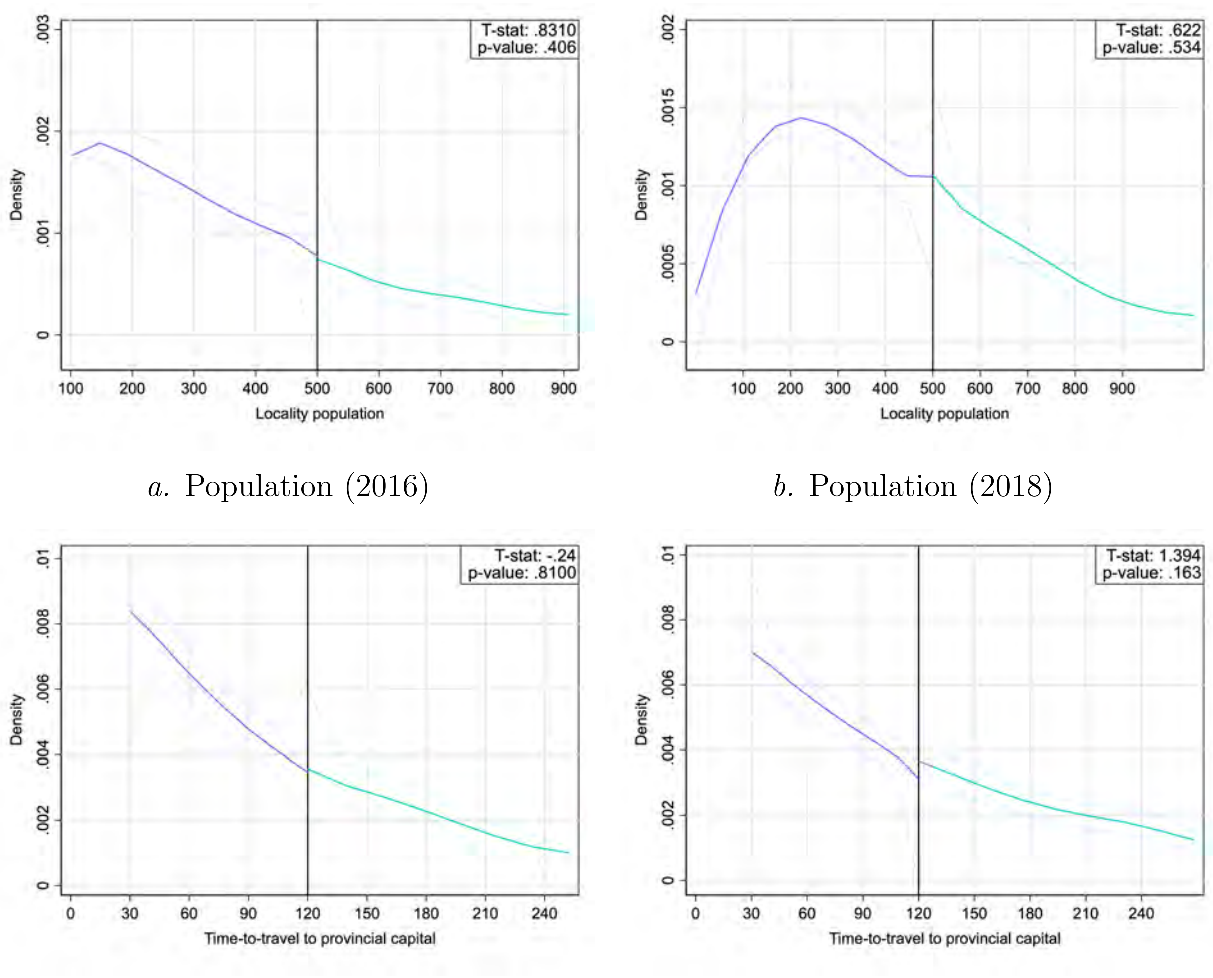
c. Travel Time (2016)



d. Travel Time (2018)

NOTES. This figure displays the empirical densities with the corresponding confidence intervals for two running variables (population and travel time) for both rounds of the teacher recruitment drive (2016 and 2018). The density is computed using the local-polynomial estimator proposed in Cattaneo et al. (2020), and the figures show the 95% confidence intervals. The sample considers all schools in the choice sample.

Figure C.2: Densities Running Variables: Outcome Sample



a. Population (2016)

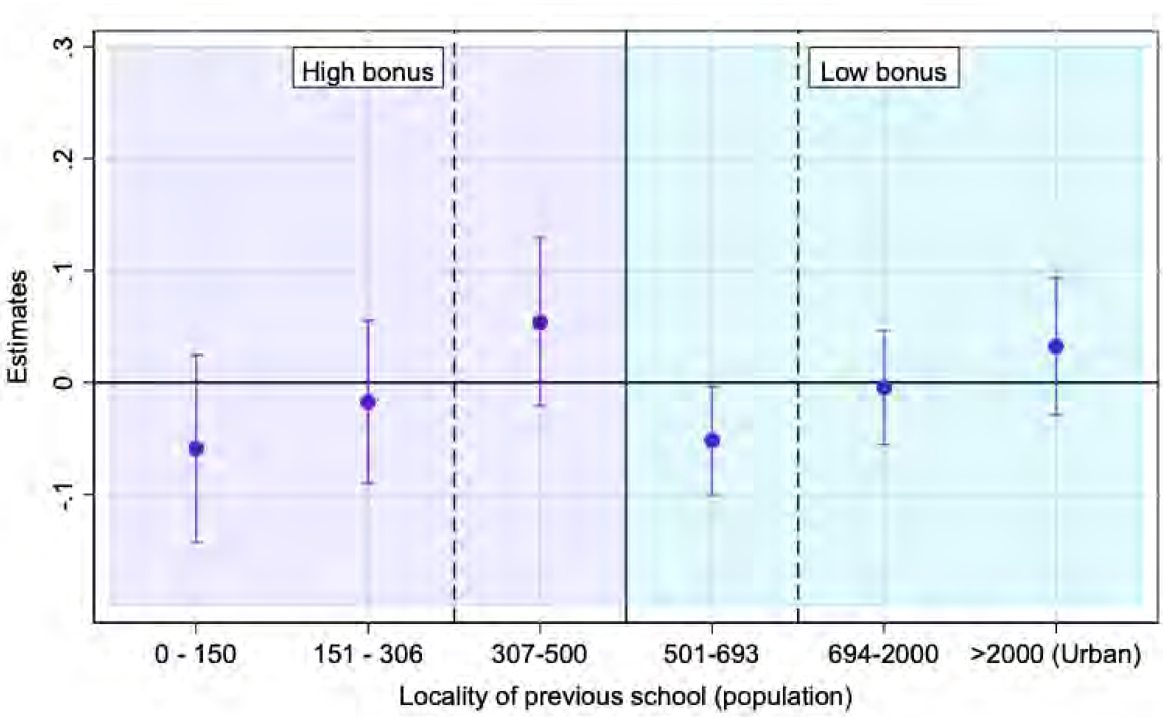
b. Population (2018)

c. Travel Time (2016)

d. Travel Time (2018)

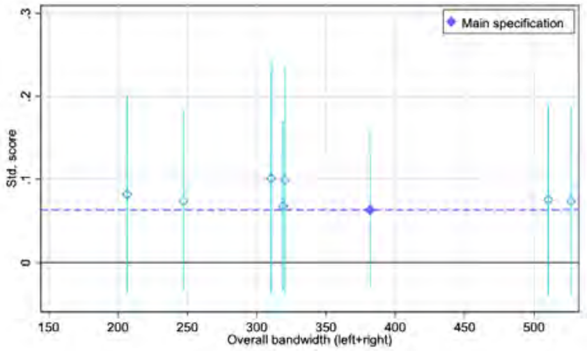
Notes. This figure displays the empirical densities with the corresponding confidence intervals for two running variables (population and travel time) for both rounds of the teacher recruitment drive (2016 and 2018). The density is computed using the local-polynomial estimator proposed in Cattaneo et al. (2020), and the figures show the 95% confidence intervals. The sample considers all schools in the outcome sample.

Figure C.3: Rural Bonus and the Origin of Newly Recruited Teachers

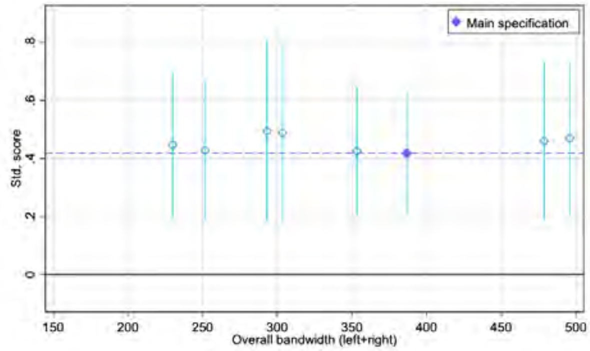


NOTES. This figure plots regression-discontinuity estimates of the effect of crossing the population threshold on the probability that newly recruited contract teachers come from schools located in different population bins. Each dot corresponds to an RD coefficient from a regression in which the outcome is an indicator equal to one if the teacher was employed in 2015 in a public school located in a village whose population falls within the bin shown on the x-axis, and zero otherwise. The bins are mutually exclusive; applicants who were employed in 2015 in the same school to which they are assigned in the 2016 and/or 2018 recruitment drive, or who were not employed in any public school in 2015, take the value zero in all regressions. The sample consists of vacancies in the choice sample that were filled through the centralized assignment process. Additionally, the sample excludes urban schools (i.e., those located in areas with more than 2,000 inhabitants) and schools situated within 30 minutes of the nearest provincial capital. The figure reports bias-corrected RD estimates with 95% confidence intervals, computed using the robust estimator of Calonico et al. (2014). Dashed vertical lines indicate the population bins that lie within the optimal bandwidth used in Column (3) of Table 2.

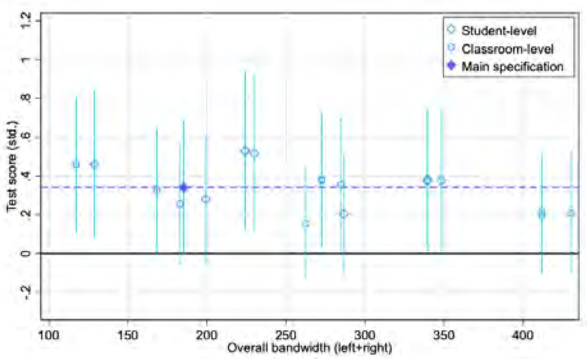
Figure C.4: Robustness to Alternative RD Specifications



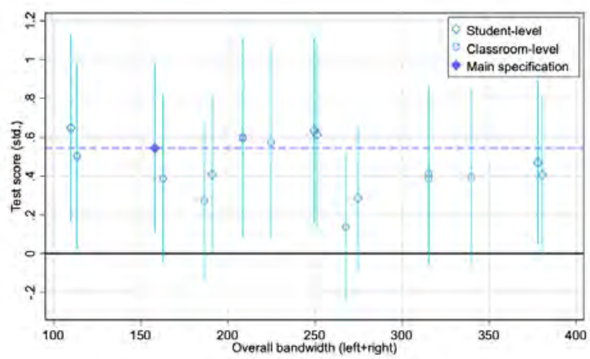
a. Vacancy filled



b. Teacher scores (std)



c. Spanish scores



d. Math scores

Notes. These figures illustrate the robustness of the regression-discontinuity estimates reported in Tables 2 and 3 to alternative specifications and estimation choices. In Panel A, the outcome variable is an indicator for whether the vacancy is filled in the national recruitment drive, while in Panel B the outcome is the teacher competency score (corresponding to Columns (1) and (3) of Table 2, respectively). In Panels C and D, the outcome variables are standardized fourth grade test scores in Spanish and Math (corresponding to Column (1) in Panels A and B of Table 3, respectively). Markers indicate how the robust bias-corrected regression-discontinuity estimates—computed using the robust estimator proposed by Calonico et al. (2014)—vary with: (i) the bandwidth choice, including the common mean-squared-error (MSE)–optimal bandwidth, the MSE-optimal bandwidth for the sum of regression estimates, the common coverage-error-rate (CER)–optimal bandwidth, and the CER-optimal bandwidth for the sum of regression estimates; (ii) the order of the local polynomial used to construct the point estimator (1 or 2); and, limited to Panels C and D, (iii) the unit of observation, which is either the student or the classroom (in the latter case, the outcome is the classroom-level average test score). The horizontal dashed line indicates the estimate obtained under the main specification reported in the corresponding table, which uses the common MSE-optimal bandwidth, a first-order polynomial, and takes the unit of observation to be the vacancy in Panels A and B and the student in Panels C and D.

Table C.1: Covariate Smoothness around the Population Cutoff

	Choice Sample			Outcome Sample		
	Mean (BW)	RD estimate		Mean (BW)	RD estimate	
	(1)	(2)	(3)	(4)	(5)	(6)
School characteristics						
Distance to the provincial capital	163.021	5.468	(43.241)	139.613	30.572	(36.632)
Distance >120 min.	0.384	-0.004	(0.068)	0.378	0.207	(0.123)
Other wage bonuses (S/.)	72.187	0.643	(14.152)	53.482	16.806	(22.907)
Single Teacher	0.000	0.000	(0.002)	0.008	0.004	(0.008)
Multigrade	0.312	-0.075	(0.075)	0.162	0.128	(0.117)
Bilingual	0.205	0.064	(0.051)	0.175	-0.089	(0.070)
Border	0.057	-0.116	(0.056)	0.050	-0.100	(0.058)
VRAEM	0.045	0.054	(0.031)	0.042	-0.010	(0.051)
Number of students	114.153	0.322	(6.342)	115.502	-5.331	(10.140)
Indigenous language students	0.268	-0.021	(0.064)	0.209	0.011	(0.082)
% indigenous language students	0.171	-0.019	(0.048)	0.138	-0.095	(0.068)
% proficient students (math)	9.739	1.547	(3.053)	10.648	3.788	(4.530)
% proficient students (spanish)	13.152	1.075	(3.520)	15.189	2.356	(3.826)
Village amenities						
Amenity/infrastructure index	-0.219	0.029	(0.124)	-0.119	-0.038	(0.148)
Poverty index	0.684	0.353	(0.217)	0.733	0.330	(0.319)
Electricity	0.960	0.068	(0.047)	0.964	0.058	(0.037)
Drinking water	0.756	0.008	(0.074)	0.784	0.076	(0.116)
Sewage	0.506	0.047	(0.080)	0.531	0.013	(0.112)
Water tower	0.246	-0.044	(0.075)	0.374	-0.251	(0.144)
Medical clinic	0.779	0.042	(0.066)	0.767	0.110	(0.120)
Meal center	0.262	0.109	(0.069)	0.315	-0.014	(0.100)
Community phone	0.078	0.018	(0.044)	0.068	0.037	(0.055)
Internet access point	0.144	0.004	(0.050)	0.183	-0.044	(0.082)
Bank	0.036	-0.001	(0.027)	0.035	0.001	(0.035)
Public library	0.023	-0.023	(0.023)	0.025	-0.018	(0.028)
Police	0.194	0.017	(0.061)	0.178	-0.178	(0.109)
School amenities						
Teachers room	0.184	-0.044	(0.052)	0.221	-0.136	(0.078)
Sport pitch	0.201	-0.037	(0.065)	0.218	-0.066	(0.117)
Courtyard	0.182	-0.106	(0.074)	0.189	-0.027	(0.090)
Auditorium	0.102	-0.005	(0.044)	0.076	0.053	(0.092)
Administrative office	0.530	0.024	(0.084)	0.534	-0.034	(0.117)
Courtyard	0.010	0.005	(0.003)	0.017	-0.058	(0.044)
Computer lab	0.436	0.067	(0.082)	0.470	-0.056	(0.126)
Workshop	0.071	-0.027	(0.030)	0.047	-0.000	(0.022)
Science lab	0.081	0.074	(0.041)	0.116	0.043	(0.073)
Library	0.494	0.013	(0.083)	0.494	-0.086	(0.139)
At least a personal computer	0.793	0.132	(0.064)	0.781	0.171	(0.073)

Notes. This table examines whether schools located just above or just below the population threshold differ in village and school amenities (measured in 2013). Columns (1)–(3) focus on the choice sample, while columns (4)–(6) focus on the outcome sample. The table reports the mean of each variable for control schools (i.e., schools located to the right of the population cutoff within the chosen bandwidth) and the robust bias-corrected regression-discontinuity estimates, together with their standard errors, obtained using the estimator proposed by Calonico et al. (2014).

Table C.2: Decomposition of wage increases around the population cutoff

	(1) Overall	(2) Time < 120	(3) Time > 120
Bonus	244.205	26.503	410.654
	(26.528)	(12.818)	(22.795)
Mean dep. var. (Lower Bonus)	1844.006	1800.000	1901.101
Bandwidth	143.908	142.384	210.283
Schools	1081	671	715
Observations	2600	1301	1627

Notes. This table reports the effect of crossing the population threshold on teacher wages. In all columns, the dependent variable is defined as the sum of the base wage and the wage bonuses described in Figure 1 (for schools that satisfy the criteria). The sample used is the choice sample. In Column (2), the sample is limited to schools that comply with the travel time cutoff, that is, farther than 120 minutes from the provincial capital. Column (3) only considers schools that do not comply with the travel time cutoff (closer than 120 minutes). In all columns, the sample excludes urban schools (i.e., those located in areas with more than 2,000 inhabitants) and schools situated within 30 minutes of the nearest provincial capital. Cells report bias-corrected regression-discontinuity estimates obtained using the robust estimator proposed by Calonico et al. (2014). Regressions are estimated within a mean-squared-error–optimal bandwidth, reported at the bottom of the table. Standard errors are clustered at the school×year level.

Table C.3: Sharp RD Bounds Under Potential Manipulation

	(1) Preferences	(2) Teacher Score (std.)
Upper bound	0.142	0.503
Lower bound	0.083	0.275
CI	[0.047 - 0.177]	[0.150 - 0.628]
Bandwidth	162.27	193.38
Schools	1247	1494
Observations	2575	3068

Notes. This table reports the RD bounds (Gerard et al., 2020) for the threshold crossing effect on two outcomes that are subject to potential censorship due to the assignment of applicants to vacancies. In Column (1) the outcome variable is the rank in which a vacancy was chosen in the assignment mechanism (normalized so that it takes values from zero to one); in Column (2) it is the standardized competency score obtained by the teachers in the centralized test. The 95% confidence intervals are obtained through 1000 bootstrap replications. The sample used is the choice sample, further restricted to vacancies filled in the national recruitment drive. Additionally, the sample excludes urban schools (i.e., those located in areas with more than 2,000 inhabitants) and schools situated within 30 minutes of the nearest provincial capital.

Table C.4: Teacher Score (Std.)–Difference-in-discontinuity

	(1) 2016	(2) 2018	(3) Pooled
Post-policy	-0.024	0.172	0.064
	(0.064)	(0.062)	(0.054)
Post-policy × High Bonus	0.214	0.272	0.254
	(0.079)	(0.079)	(0.068)
Bandwidth	158.717	221.494	173.569
Schools	1112	1776	1625
Observations	3997	5817	7279

Notes. This table reports the effect of crossing the population threshold on the standardized competency score of contract teachers across different school years. The sample includes all contract teachers employed in schools with an open vacancy in the 2016 or 2018 recruitment drives, observed in three school years: 2015, 2016, and 2018. The sample excludes urban schools (i.e., those located in areas with more than 2,000 inhabitants) and schools situated within 30 minutes of the nearest provincial capital. In all columns, the outcome is the teacher's standardized competency score from the centralized recruitment exams held in 2016 or 2018. For contract teachers observed in 2015 (pre-policy), the outcome is defined as their score in the 2016 recruitment exam, conditional on participation. Post-policy is an indicator equal to one in the post-treatment period—that is, 2016 in Column (1), 2018 in Column (2), and both years in Column (3)—and zero in the pre-policy year (2015). Post-policy × High Bonus is the interaction between the former and an indicator for municipalities with a population below 500 inhabitants, which, from 2016 onward are eligible for the higher wage bonus. All regressions include school fixed effects. Cells report conventional regression-discontinuity estimates obtained using a triangular kernel function and the optimal bandwidth selected from a standard RD specification without interaction terms, estimated on the corresponding sample. Standard errors are clustered at the school level.

Table C.5: Monetary Incentives and Teacher Characteristics

	(1) Female	(2) Age	(3) Exp. public	(4) Exp. private	(5) Quechua	(6) Degree
High Bonus	0.038	-1.885	-0.091	-0.005	-0.048	0.132
	(0.055)	(0.975)	(0.052)	(0.055)	(0.062)	(0.051)
Mean dep. var. (Low Bonus)	0.623	36.730	0.715	0.355	0.273	0.254
Bandwidth	151.707	121.048	132.436	166.104	168.171	174.157
Schools	1076	837	929	1183	1196	1237
Observations	2401	1879	2081	2622	2649	2730

Notes. This table reports the effect of crossing the population threshold on several teachers' characteristics. These are a female dummy (column 1), age (column 2), a dummy taking value 1 for teachers with at least 3 years of teaching experience in the public sector (column 3) or at least 1 year of experience in the private sector (column 4), a dummy equal to 1 if the teacher speaks Quechua (column 5), an indicator for university education (column 6). The sample used is the choice sample, further restricted to vacancies filled in the national recruitment drive. Additionally, the sample excludes urban schools (i.e., those located in areas with more than 2,000 inhabitants) and schools situated within 30 minutes of the nearest provincial capital. Cells report bias-corrected regression-discontinuity estimates obtained using the robust estimator proposed by Calonico et al. (2014). Regressions are estimated within a mean-squared-error–optimal bandwidth, reported at the bottom of the table. Standard errors are clustered at the school×year level.

Table C.6: Monetary Incentives and Teaching Staff Composition

	(1) N. of teachers	(2) Student/Teacher	(3) % Contract teachers
High Bonus	0.126	-0.174	-0.053
	(0.284)	(0.190)	(0.038)
Bandwidth	169.202	183.345	196.363
Schools	1299	1406	1517
Observations	1873	2030	1796

Notes. Notes: This table reports the effect of crossing the population threshold on the composition of teaching staff in schools. Column (1) uses the number of teachers as the outcome variable, Column (2) the student–teacher ratio, and Column (3) the percentage of teachers hired under temporary contracts. The sample includes all schools in the choice sample. Cells report bias-corrected regression-discontinuity estimates obtained using the robust estimator proposed by Calonico et al. (2014). Regressions are estimated within a mean-squared-error–optimal bandwidth, reported at the bottom of the table. Standard errors are clustered at the school level.

Table C.7: Monetary Incentives and Teacher Retention

	(1) Within-year retention	(2) Same school in t+1	(3) Same school in t+2
High Bonus	0.014	-0.036	-0.017
	(0.030)	(0.049)	(0.027)
Bandwidth	183.542	156.290	113.351
Schools	1313	1108	775
Observations	2900	2461	1750

Notes. This table reports the effect of crossing the population threshold on several measures of teacher retention. These are a set of binary indicators for whether a contract teacher assigned to a certain school through the centralized process is also observed in the same school at the end of the school year (Column 1), at the beginning of the following school year (Column 2), or at the beginning of the two-years-after school year. The sample used is the choice sample, further restricted to vacancies filled in the national recruitment drive. Additionally, the sample excludes urban schools (i.e., those located in areas with more than 2,000 inhabitants) and schools situated within 30 minutes of the nearest provincial capital. Cells report bias-corrected regression-discontinuity estimates obtained using the robust estimator proposed by Calonico et al. (2014). Regressions are estimated within a mean-squared-error–optimal bandwidth, reported at the bottom of the table. Standard errors are clustered at the school×year level.

Table C.8: School level Estimates of the Effect of the Wage Bonus on Student Achievement

Panel A: Dependent Variable is Spanish Test (z-score)			
	(1)	(2)	(3)
	New teacher	No vacancy	All
High Bonus	0.186	-0.124	0.065
	(0.087)	(0.098)	(0.065)
Bandwidth	123.514	128.634	138.888
Schools	1106	774	1867
Observations	24003	11299	41278
Panel B: Dependent Variable is Math Test (z-score)			
	(1)	(2)	(3)
	New teacher	No vacancy	All
High Bonus	0.260	-0.111	0.094
	(0.108)	(0.107)	(0.077)
Bandwidth	100.631	151.555	122.870
Schools	893	936	1613
Observations	19863	13518	36010

Notes. This table reports the effect of crossing the population threshold on student achievement in Math and Spanish. The outcome variables are standardized test scores in Spanish (Panel A) and Math (Panel B) for fourth grade students. The table is the counterpart of Table 3, but it does not rely on the matched teacher–classroom dataset described in Section 2.2; instead, it considers the universe of fourth grade students in public schools in Peru. Column (1) considers students enrolled in schools that have at least one newly recruited contract teacher in the corresponding school year. Column (2) considers schools with no open vacancies for contract teaching positions. Column (3) considers the overall sample of fourth grade students. In all columns, the sample excludes urban schools (i.e., schools located in areas with more than 5,000 inhabitants) and schools situated within 30 minutes of the nearest provincial capital. Each cell reports bias-corrected regression-discontinuity estimates obtained using the robust estimator proposed by Calonico et al. (2014). Regressions are estimated within a mean-squared-error–optimal bandwidth, reported at the bottom of the table. Standard errors are clustered at the classroom×year level.

D Model Estimation Results

Table D.1: Estimates Teacher Preferences – μ_θ

		× Female	× Teacher Score	× Quechua	× Exp Public > 3	× Exp Private > 0
	(1)	(2)	(3)	(4)	(5)	(6)
Wage	-0.394 (0.130)	-0.237 (0.049)	0.176 (0.030)	-0.154 (0.060)	-0.028 (0.046)	-0.059 (0.048)
Outside Option ($j = 0$)	2.895 (0.450)	0.306 (0.264)	-1.250 (0.139)	-0.385 (0.312)	-1.448 (0.258)	0.647 (0.263)

NOTES. This table displays the estimates of the parameters of the conditional mean of θ_i. Standard errors are in parentheses.

Table D.2: Estimates Teacher Preferences – Σ_θ

	Wage	Outside Option ($j = 0$)
	(1)	(2)
Wage	0.986 (0.073)	0.647 (0.095)
Outside Option ($j = 0$)		2.036 (0.052)

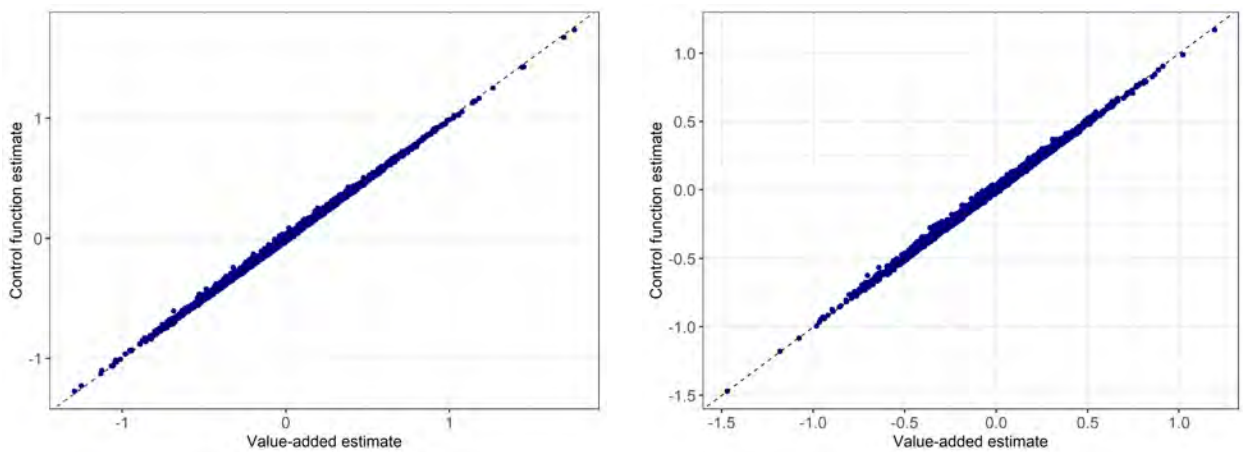
NOTES. This table displays the estimates of the variance covariance matrix of θ_i. The diagonal elements are the standard deviations of θ_i while the off diagonal elements display corr(θ_i). Standard errors are in parentheses.

Table D.3: Estimates Teacher Preferences – (Θ, γ)

		$\times$ Female	$\times$ Teacher Score	$\times$ Quechua	$\times$ Exp Public > 3	$\times$ Exp Private > 0
	(1)	(2)	(3)	(4)	(5)	(6)
Amenity Index	0.043	0.014	-0.015	-0.002	0.025	0.034
	(0.027)	(0.022)	(0.011)	(0.030)	(0.023)	(0.024)
Border	0.155	-0.194	0.084	-0.156	0.017	-0.072
	(0.100)	(0.085)	(0.049)	(0.135)	(0.086)	(0.089)
VRAEM	-0.121	-0.285	0.200	-0.147	0.292	-0.009
	(0.176)	(0.115)	(0.072)	(0.132)	(0.129)	(0.122)
Multigrade	-0.250	-0.234	0.026	-0.131	0.190	0.027
	(0.068)	(0.055)	(0.032)	(0.065)	(0.058)	(0.058)
Single Teacher	-0.192	-0.631	-0.147	-0.064	-0.229	-0.048
	(0.239)	(0.199)	(0.129)	(0.304)	(0.223)	(0.245)
Bilingual	-1.042	0.113	-0.155	0.972	0.199	0.056
	(0.105)	(0.077)	(0.046)	(0.082)	(0.083)	(0.082)
Time	-0.225	-0.046	-0.148	0.040	0.080	-0.136
	(0.039)	(0.028)	(0.017)	(0.044)	(0.038)	(0.041)
Time^2	-0.002	0.002	0.001	0.000	0.000	-0.003
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
log(Population)	0.026	0.045	0.039	-0.093	-0.026	-0.004
	(0.061)	(0.048)	(0.024)	(0.057)	(0.045)	(0.046)
log(Population)^2	-0.016	-0.000	0.001	0.002	0.001	0.003
	(0.003)	(0.002)	(0.001)	(0.003)	(0.002)	(0.002)
Time $\times$ log(Pop)	0.047	-0.002	0.010	-0.006	-0.013	0.029
	(0.006)	(0.004)	(0.002)	(0.007)	(0.005)	(0.006)
School Lagged Math Score	0.019	-0.016	0.081	0.106	-0.076	0.049
	(0.072)	(0.061)	(0.034)	(0.071)	(0.063)	(0.063)
School Lagged Spanish Score	-0.006	0.018	-0.009	-0.020	-0.006	0.028
	(0.077)	(0.064)	(0.036)	(0.076)	(0.068)	(0.068)
School Female	-0.174	0.019	0.081	0.171	0.184	-0.145
	(0.150)	(0.125)	(0.063)	(0.149)	(0.125)	(0.126)
School Quechua	-0.884	0.081	-0.020	1.525	-0.158	-0.125
	(0.120)	(0.084)	(0.050)	(0.095)	(0.087)	(0.086)
School Age	0.080	-0.080	-0.092	-0.053	-0.005	-0.002
	(0.077)	(0.061)	(0.034)	(0.078)	(0.068)	(0.068)
Previous School	4.508	0.048	-0.259	-0.696	-0.256	0.748
	(0.070)	(0.055)	(0.027)	(0.073)	(0.056)	(0.053)
Distance Spline: $[0, 20)$	-0.095	-0.001	-0.025	-0.013	0.006	-0.001
	(0.004)	(0.003)	(0.002)	(0.004)	(0.003)	(0.003)
Distance Spline: $[20, 100)$	-0.048	0.000	-0.006	0.002	-0.003	0.003
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Distance Spline: ≥ 100	-0.007	0.001	0.001	0.000	-0.001	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Outside Option ($j = J + 1$)	2.122	0.472	-1.184	-0.398	0.122	0.287
	(0.447)	(0.259)	(0.135)	(0.307)	(0.252)	(0.258)

Notes. This table displays our estimates of the parameters Θ_1, Θ_2, γ_2. Standard errors are in parentheses.

Figure D.1: Control Function and OLS Estimates



a. Math *a.* Spanish

NOTES. This figure plots shrunken estimates of the ATE coming from the value-added estimates (x-axis) and the control function estimates (y-axis) for math test scores (Panel A) and Spanish test scores (Panel B).

Table D.4: Estimates Teacher Value-Added – δ_2

	Math	Spanish
	(1)	(2)
Lagged Score^2	-0.001 (0.003)	-0.011 (0.003)
Quechua	-0.047 (0.018)	-0.023 (0.018)
Age	-0.132 (0.006)	-0.095 (0.006)
School Lagged Score	0.306 (0.056)	0.422 (0.061)
School Lagged Score^2	0.056 (0.051)	0.198 (0.048)
School Female	-0.228 (0.265)	-0.725 (0.268)
School Quechua	0.268 (0.299)	0.250 (0.303)
School Age	-0.759 (0.140)	-0.419 (0.144)
Classroom Lagged Score	-0.162 (0.036)	-0.203 (0.039)
Classroom Lagged Score^2	-0.038 (0.030)	-0.019 (0.027)
Classroom Female	0.655 (0.150)	0.600 (0.152)
Classroom Quechua	-0.349 (0.312)	-0.236 (0.312)
Classroom Age	0.076 (0.080)	0.031 (0.078)
Class Size	0.020 (0.008)	0.010 (0.008)
Class Size^2	-0.000 (0.000)	-0.000 (0.000)
Amenity Index	0.042 (0.021)	0.045 (0.022)
Border	0.017 (0.137)	0.005 (0.141)
Multigrade	0.198 (0.090)	0.140 (0.091)
Single Teacher	0.083 (0.229)	-0.280 (0.235)
Travel Time	0.118 (0.080)	0.056 (0.081)
log(Pop)	-0.019 (0.012)	-0.029 (0.012)
Time $\times$ log(Pop)	-0.015 (0.011)	-0.004 (0.011)
Control Function Missing	-0.039 (0.046)	-0.012 (0.046)

Notes. This table displays the estimates of the parameters associated with school and classroom characteristics in the student achievement production function. Standard errors are in parentheses.

Table D.5: Estimates Teacher Value-Added – μ_δ

	× Female	× Quechua	× Teacher Score	× Exp Public > 3	× Exp Private > 0
	(1)	(2)	(3)	(4)	(5)
Panel A: Math					
ATE	0.074	0.058	0.107	-0.003	0.096
	(0.017)	(0.022)	(0.009)	(0.017)	(0.017)
Lagged Score	0.007	-0.013	0.027	-0.011	-0.001
	(0.009)	(0.011)	(0.004)	(0.008)	(0.009)
Female	0.004	-0.002	-0.002	0.003	-0.014
	(0.012)	(0.015)	(0.006)	(0.011)	(0.011)
Panel B: Spanish					
ATE	0.125	-0.065	0.077	-0.015	0.098
	(0.016)	(0.021)	(0.008)	(0.015)	(0.016)
Lagged Score	0.018	-0.011	0.020	-0.017	0.009
	(0.010)	(0.012)	(0.005)	(0.009)	(0.009)
Female	-0.010	-0.011	0.006	0.009	-0.018
	(0.012)	(0.015)	(0.006)	(0.011)	(0.011)

Notes. This table displays the estimates of the parameters of the interaction effects in the conditional mean of the distribution of the teacher value-added coefficients δ_i. Standard errors are in parentheses.

Table D.6: Teacher Characteristics and Teacher Value Added

	$\text{Var}(\cdot\|\theta_i, x_i)$	$\text{Var}(\cdot\|x_i)$	$\text{Var}(\cdot)$
	(1)	(2)	(3)
Panel A: Math			
ATE	0.112	0.116	0.139
Lagged Score	0.022	0.023	0.024
Female	0.009	0.009	0.009
Panel B: Spanish			
ATE	0.043	0.047	0.063
Lagged Score	0.028	0.029	0.030
Female	0.005	0.005	0.006

Notes. This table displays the variance of the value-added coefficients conditional on x_i and θ_i (Column 1), conditional on x_i only (Column 2) and unconditional (Column 3).

Table D.7: Model Fit – Distribution of Matched Teacher-School Characteristics

	Mean		SD	
	Data	Model	Data	Model
	(1)	(2)	(3)	(4)
Wage	1.876	1.880	0.382	0.387
Amenity Index	0.057	0.049	0.967	0.968
Border	0.060	0.060	0.237	0.237
VRAEM	0.029	0.031	0.169	0.172
Multigrade	0.222	0.224	0.415	0.417
Single Teacher	0.007	0.007	0.085	0.085
Bilingual	0.108	0.108	0.311	0.310
Time	1.970	1.978	3.989	3.894
log(Pop)	8.783	8.789	3.628	3.628
School Lagged Math Score	-0.034	-0.035	0.683	0.683
School Lagged Spanish Score	-0.185	-0.187	0.697	0.701
School Female	0.494	0.493	0.141	0.144
School Quechua	0.153	0.154	0.329	0.331
School Age	0.064	0.063	0.326	0.335
Distance	69.387	72.183	155.083	124.889
Previous School	0.140	0.133	0.347	0.340
Other Public School	0.305	0.310	0.460	0.463
Outside Option	0.351	0.357	0.477	0.479

Notes. This table displays moments of the distribution of matched teacher and school characteristics in the data and in a simulated teacher-school status quo match. To simulate an equilibrium teacher-school match, we take a random draw of type-I EV shocks η_{ijt}, and a random draw of the random coefficients θ_i from its distribution conditional on x_i and $\hat{\delta}_i$. We then construct indirect utilities for every teacher-school pair and run the serial dictatorship algorithm to simulate the assignment mechanism (see Section 2.1)

Table D.8: Model Fit – Threshold Crossing Effects on Residualized Test Scores

	(1)	(2)
	Math	Spanish
High Bonus	0.187	0.121
	(0.134)	(0.136)
Bandwidth	122.514	134.728
Observations	3547	3843

Notes. This table reports regression-discontinuity estimates of the effect of crossing the population threshold on students' test scores in math and Spanish, after residualizing outcomes by differencing out the teacher value-added components (see Equation (7)). The sample does not consider schools closer than 30 minutes from the provincial capital and schools located in urban areas (population $\geq$ 2,000 inhabitants). Cells report the bias-corrected regression-discontinuity estimates obtained using the robust estimator proposed in Calonico et al. (2014). Regressions are defined within a mean-square error optimal bandwidth, reported at the bottom part of the table. Standard errors are clustered at the school$\times$year level.

E Technical Appendix

E.1 Identification and Estimation of $\Sigma_{\delta,\theta}$

We can write the probability of observing the matching history $\{D_{it}\}_{t=1}^{T}$ conditional on observed teacher and school characteristics, teachers' choice sets and their value-added coefficients δ_i:

$$\mathbb{P}(\{D_{it}\}_{t=1}^{T}|x_i, s_i, d_i, \Omega(s_{it}), \delta_i) = \int \prod_{t=1}^{T} \frac{\exp\{u_{iD_{it}t}\}}{\exp\{u_{i0t}\} + \exp\{u_{iJ+1t}\} + \sum_{k\in\Omega(s_{it})} \exp\{u_{ikt}\}}$$
$$\times\, \phi(\theta_i|\mu_\theta' x_i + \Sigma_{\theta,\delta}\Sigma_\delta^{-1}(\delta_i - \mu_\delta' x_i), \Sigma_\theta - \Sigma_{\theta,\delta}\Sigma_\delta^{-1}\Sigma_{\delta,\theta})d\theta_i$$

Fox et al. (2012) show that this relationship can be inverted to identify the mean and variance of the mixing distribution. The following function $\tilde{\mu}$ is thus identified:

$$\tilde{\mu}(x_i, \delta_i) = \mu_\theta' x_i + \Sigma_{\theta,\delta}\Sigma_\delta^{-1}(\delta_i - \mu_\delta' x_i)$$

From there, we can use variation in δ_i to identify $\Sigma_{\theta,\delta}\Sigma_{\delta,\delta}^{-1}$. Conditional on knowing Σ_δ, we can recover $\Sigma_{\theta,\delta}$.

We estimate $\Sigma_{\theta,\delta}$ by maximizing the joint log-likelihood of observing the matching history $(D_{it})_{i=1}^{T}$ and the value-added estimates $\hat{\delta}_i$ for $i = 1, ..., N$ after plugging-in estimates of the preference parameters $(\Theta_1, \Theta_2, \gamma_1, \gamma_2, \beta_1, \boldsymbol{\kappa})$, the distribution of the random coefficients $(\mu_\theta, \Sigma_\theta)$ and the distribution of value-added coefficients $(\mu_\delta, \Sigma_\delta)$. The log-likelihood has the following expression

$$\mathcal{L}(\Sigma_{\theta,\delta}) = \sum_{i=1}^{N} \log \int \prod_{t=1}^{T} \frac{\exp\{u_{iD_{it}t}\}}{\exp\{u_{i0t}\} + \exp\{u_{iJ+1t}\} + \sum_{k\in\Omega(s_{it})} \exp\{u_{ikt}\}}$$
$$\times\, \phi(\hat{\delta}_i|\mu_\delta' x_i + \Sigma_{\delta,\theta}\Sigma_\theta^{-1}(\theta_i - \mu_\theta' x_i), \Sigma_\delta - \Sigma_{\delta,\theta}\Sigma_\theta^{-1}\Sigma_{\theta,\delta} + \hat{\Omega}_i)\phi(\theta_i|\mu_\theta' x_i, \Sigma_\theta)d\theta_i,$$

where we approximate the integral using 50 draws from a Halton sequence Judd (1998).

E.2 School Preferences Satisfy the Substitutes Condition

We show that the preference ordering described by Assumption 1 satisfies the substitutes condition defined in Hatfield and Milgrom (2005). Denote the set of all possible contracts

$X = S \times T \times W$ where S is the set of schools, T the set of teachers we consider and W the set of wages that schools can propose. We assume that wages range discretely from the minimum wage proposed to teachers in Perú to an arbitrarily large upper bound. Define $C_s(X)$ and $R_s(X)$ the chosen set and the rejected set of school s from the set of contracts X. Elements of X are substitutes for school s if for all subsets $X' \subset X'' \subset X$ we have $R_s(X') \subset R_s(X'')$.

Consider X' a subset of X. Define w^* the wage offered in $C_s(X')$, $\underline{t}^*$ as the teacher with the lowest value added in $C_s(X')$ and $\bar{t}^*$ as the teacher with the highest value added in $C_s(X')$. Consider that we add an additional contract to X' such that $X'' = X' \cup \{(s,t,w)\}$.

We first look at the case where $Y_{\bar{t}^*s} < \bar{c}$. If $Y_{ts} \geq \bar{c}$ then $C_s(X'') = \{(s,t,w)\}$ and $R_s(X'') = C_s(X') \cup R_s(X')$ for any w. If $Y_{ts} < \bar{c}$ and $w > w^*$ then $C_s(X'') = C_s(X')$ and $R_s(X'') = R_s(X') \cup \{(s,t,w)\}$. If $Y_{ts} < \bar{c}$ and $w < w^*$ then $C_s(X'') = \{(s,t,w)\}$ and $R_s(X'') = C_s(X') \cup R_s(X')$. Finally, if $Y_{ts} < \bar{c}$ and $w = w^*$ two cases may arise:

- If the size of $C_s(X')$ is strictly smaller than school s capacities, under Assumption 1 (i), we have that $C_s(X'') = C_s(X') \cup \{(s,t,w)\}$ and $R_s(X') = R_s(X'')$.
- If the size of $C_s(X')$ is equal to school s capacities (school s is at max capacity), under Assumption 1 (i) we have: (i) $C_s(X'') = C_s(X')$ and $R_s(X'') = R_s(X') \cup \{(s,t,w)\}$ if $Y_{ts} < Y_{\underline{t}^*s}$, or (ii) $C_s(X'') = C_s(X') \setminus \{(s,\underline{t}^*,w)\} \cup \{(s,t,w)\}$ and $R_s(X'') = R_s(X') \cup \{(s,\underline{t}^*,w)\}$ if $Y_{ts} > Y_{\underline{t}^*s}$.

In any case, $R_s(X') \subseteq R_s(X'')$.

We then look at the case where $Y_{\bar{t}^*s} \geq \bar{c}$. If $w > w^*$ or if $w < w^*$ and $Y_{ts} < \bar{c}$, then $C_s(X'') = C_s(X')$ and $R_s(X'') = C_s(X') \cup \{(s,t,w)\}$. If $Y_{ts} \geq \bar{c}$ and $w < w^*$ then $C_s(X'') = \{(s,t,w)\}$ and $R_s(X'') = C_s(X') \cup R_s(X')$. Finally, if $w = w^*$ two cases may arise:

- If the size of $C_s(X')$ is strictly smaller than school s capacities, under Assumption 1 (i), we have that $C_s(X'') = C_s(X') \cup \{(s,t,w)\}$ and $R_s(X') = R_s(X'')$.
- If the size of $C_s(X')$ is equal to school s capacities (school s is at max capacity), under Assumption 1 (i) we have: (i) $C_s(X'') = C_s(X')$ and $R_s(X'') = R_s(X') \cup \{(s,t,w)\}$ if $Y_{ts} < Y_{\underline{t}^*s}$, or (ii) $C_s(X'') = C_s(X') \setminus \{(s,\underline{t}^*,w)\} \cup \{(s,t,w)\}$ and $R_s(X'') = R_s(X') \cup \{(s,\underline{t}^*,w)\}$ if $Y_{ts} > Y_{\underline{t}^*s}$.

In any case, $R_s(X') \subseteq R_s(X'')$.

E.3 Proof Proposition 1

Let us denote the school-optimal stable set of contracts given Assumption 1 as (D^*, w^*). We first show that condition (C1) is satisfied under (D^*, w^*). Assume that (C1) does not hold, this implies that there would exist a school k such that $\max_{\{i:D_i^*=k\}} Y_{ik} < \bar{c}$ which would be a direct contradiction of stability given that school k would be willing to keep increasing w_k above w_k^* until $\max_{\{i:D_i^*=k\}} Y_{ik} \geq \bar{c}$. A violation of (C2) would be a direct violation of stability as there would exist a teacher-school pair that would prefer to rematch given w^* under Assumption 1. This implies that (C2) holds under (D^*, w^*). Finally, we know that the school-optimal stable set of contracts is unanimously preferred by all schools conditional on stability (Hatfield and Milgrom, 2005). This implies that, conditional on stability, the sum of the wages offered is minimal, which finishes to prove Proposition 1.