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The Giving Advice Effect: Reducing Teacher Sorting Through Self-Persuasion*

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Abstract

This paper examines how the act of giving advice to others can serve as a tool for self-persuasion in high-stakes decisions. We tested this hypothesis in Peru's nationwide teacher selection process, involving over 74,000 candidates. By prompting teachers to advise peers on selecting schools for maximum educational impact, we observe a significant shift in their own choices: an increased probability of choosing and being assigned to hard-to-staff schools, institutions serving disadvantaged areas that are typically understaffed. In line with recent literature on behavioral sciences, our findings demonstrate that advising others can influence one's own consequential decisions. This insight offers a cost-effective approach to mitigating teacher sorting and reducing educational inequality. It also corroborates the validity of the *giving advice* effect in a high-stakes, real-world context using a large sample.

***JEL classification*: D91,I23, I25**

Keywords: *Teachers, teacher policy, teacher shortages, behavioral, giving advice*

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1 Introduction

A fundamental principle of public education systems worldwide is ensuring equal opportunities for students from diverse socioeconomic backgrounds. However, many countries face a pervasive challenge that threatens this objective: the phenomenon known as teacher sorting (Jackson, 2009; Lankford et al., 2002; Boyd et al., 2013; Pop-Eleches and Urquiola, 2013). This issue arises when students from low-income households are more likely to attend understaffed schools with less qualified teachers. Teachers play a crucial role in the education process (Rivkin et al., 2005); their influence is typically greater for students from hard-to-staff backgrounds (Araujo et al., 2016). The shortage of high-quality educators in more vulnerable areas has severe detrimental implications for equity, as teacher sorting tends to exacerbate potential achievement gaps (Sass et al., 2012; Thiemann, 2018).

With a few recent exceptions (Ajzenman et al., 2024a,b), policy responses have predominantly centered on increasing compensation for positions at understaffed schools to motivate teachers to include such hard-to-staff schools among their potential choices (Evans and Mendez Acosta, 2023). Although such measures may influence applicants' decisions in specific cases (Neilson et al., 2021; Pugatch and Schroeder, 2014), they often prove ineffective (Glazerman et al., 2012; Rosa, 2017) and are generally associated with substantial costs.

In this paper, we present the results of an experimental assessment of a nationwide, real-world, zero-cost nudge to mitigate teacher sorting in the Peruvian national public school teacher selection process in 2022 (CPM 2022, for its acronym in Spanish), inspired by a recent insight from the behavioral sciences literature: the power of advice-giving for self-motivation (Eskreis-Winkler et al., 2019, 2018). The intervention was fully implemented through the official teacher job application platform before applicants viewed the list of schools and vacancies for application. Our intervention asked teachers in the treatment group applying to job vacancies to (voluntarily) advise a generic fellow applicant

–a hypothetical peer– about which types of schools they should choose if they wished to have the greatest social impact on students who needed them most. Teachers in the control group were asked to (voluntarily) answer two neutral questions about the application process. To rule out (or attenuate) potential priming effects (Weingarten et al., 2016), teachers in both the treatment and control groups initially received a brief explanation that outlined the different types of schools that exist in Peru (for example, schools where teachers could significantly impact student learning or receive career advancement benefits for working in rural or remote areas). In addition, icons appeared on the platform next to eligible schools, indicating which institutions offered teacher bonuses for working in disadvantaged areas (such as rural locations).

Assuming that a considerable proportion of teachers have service-oriented or altruistic motivations –which seems reasonable, as shown by recent literature in the region (Ajzenman et al., 2024a,b)–, the advice given in this context could work through several channels. For instance, reducing cognitive dissonance (advising to apply to a hard-to-staff school while not doing so) or by prompting a concrete plan to maximize social impact (Rogers et al., 2015; Eskreis-Winkler et al., 2019).

We document several results closely following our preregistered plan, including a preregistered heterogeneity analysis by gender, based on previous research by Ajzenman et al. (2024a) and Bertoni et al. (2023b) in the same context, showing that female candidates have a disproportionally higher preference for working in urban and more accessible schools. Focusing on our main specification, the proportion of male teacher candidates who ranked a hard-to-staff school as their top choice increased by 2.1 percentage points (pp) (significant at 5%; control mean: 57%) while there was no effect for female candidates (the difference between male and female candidates is significant at 5%). The proportion of male teacher candidates that ranked a hard-to-staff school as their second choice increased by 2.2 pp (significant at 5%; control mean: 57%) while there was no effect for female candidates (the difference between male and female candidates is significant at 5%). These effect sizes align with previous findings in similar interventions. Ajzenman et al. (2024a) found that male teacher candidates' likelihood of applying to disadvantaged schools increased between 3 and 3.4 pp, depending on the outcome. In a separate intervention, Ajzenman et al. (2024b) found that preferences for hard-to-staff schools increased by 1.4 to 5.2 pp, depending on the outcome. Although we cannot rule out other channels, we show evidence consistent with avoidance of cognitive dissonance. We document a strong correlation in the treatment group between the specific type of school teachers recommend and the type of school they end up applying to as their first choice.

The proportion of hard-to-staff schools listed in the male teachers' choice set was 0.9 pp higher (significant at 10%; control mean: 62%), while there was no effect for female candidates (the difference between male and female candidates was nonsignificant). The probability of being offered a position in a hard-to-staff school increased by 1.5 pp for male candidates (significant at 10%; control mean: 46%), while there was no effect for female candidates (the difference between male and female candidates was non significant). The differences across genders (and the fact that the nudge was mostly ineffective among female candidates) are not surprising and align with other papers showing the same pattern in the same context (Ajzenman et al., 2024a), plausibly explained by the fact that hard-to-staff schools are relatively farther away and female candidates tend to have less geographic mobility (see Ajzenman et al. (2024a) and Bertoni et al. (2022)). To further validate these results, we use causal forest techniques with honest splitting to examine heterogeneous treatment effects and identify teachers' characteristics that maximize the effect of the intervention (Athey and Imbens, 2016, 2017)). Consistent with our manual estimates of heterogeneous effects, our analysis uncovers substantial heterogeneity in treatment effects but only in terms of gender.

We designed the intervention based on recent insight in behavioral economics and psychology: the *giving advice* effect. This effect suggests that individuals who give advice (rather than receive it) are more likely to make decisions that align with their own recommendations. In our experiment, we randomly assigned teachers to the treatment and control groups. Teachers in the treatment group were asked to recommend what types of schools their colleagues should prioritize if they wanted to maximize their impact on students' learning and explain their recommendations. Teachers in the control group answered two neutral questions. Following Eskreis-Winkler et al. (2019), we hypothesized that prompting applicants to actively think about and advise colleagues on how to maximize their impact on students would increase the likelihood of choosing a hard-to-staff school.

Our paper contributes to at least two strands of behavioral economics and education literature. First, it extends the research on teacher sorting and educational inequality. Extensive literature reveals that students from low-income backgrounds or with lower academic performance tend to be enrolled in schools with less qualified teachers (Boyd et al., 2006; Dieterle et al., 2015; Feng and Sass, 2018; Lankford et al., 2002; Jackson, 2009; Sass et al., 2012), which adversely affects their educational outcomes (Aaronson et al., 2007; Sass et al., 2012; Thiemann, 2018). Research on strategies to mitigate teacher sorting has predominantly focused on monetary incentives, which, as evidenced by numerous studies (Clotfelter et al., 2008; Falch, 2011; Glazerman et al., 2012; Springer et al., 2016; Rosa, 2017; Bueno and Sass, 2018; Feng and Sass, 2018; Elacqua et al., 2019), often yield only modest or non significant impacts on teachers' preferences for hard-to-staff schools (Neilson et al. (2021) being a noteworthy exception). In line with Ajzenman et al. (2024a) and Ajzenman et al. (2024b), we contribute to this literature by exploring a novel behavioral intervention to reduce teacher sorting at zero cost.

Second, our paper intersects with an expanding literature on advice-giving. While most papers in this literature focus on the effects of receiving advice as a way of driving behavioral changes through persuasion or information provision, a recent body of work in psychology has shown promising evidence on the power of giving, rather than receiving, advice. Most of these studies have focused on motivating students to exert higher-level efforts and thus improve their grades in school (see Schaerer et al. (2018), Eskreis-Winkler et al. (2018) and Eskreis-Winkler et al. (2019)). Our results corroborate the validity of the advice-giving effect, but in a much larger and less controlled environment and targeting a different behavior: teachers' employment choices.

Moreover, the policy implications of these findings are significant. The strategy we examined in this paper addresses an important problem in education and development –specifically, the shortage of qualified teachers in certain areas. From the teachers' point of view, an intervention that reduces congestion by nudging them to understaffed positions increases their likelihood of landing a stable, full-time teaching job. In a competitive job market where many teachers apply for the same positions, not all secure a job. Indeed, in 2018, out of 22,000 teachers who applied for positions after passing a qualifying exam in Peru, only 46% were hired for a full-time permanent position. Teachers without job offers often end up in temporary positions or leave the profession.

Addressing teacher sorting is also relevant for promoting equity. Effective teachers play a pivotal role in the educational process, significantly impacting student outcomes such as test scores (Rivkin et al., 2005; Kane and Staiger, 2008; Bau and Das, 2020), as well as non-cognitive factors like absenteeism and school suspension (Ladd and Sorensen, 2017; Jackson, 2018). Notably, the impact of teachers tends to be more pronounced among students with lower performance and from lower-income backgrounds (Aaronson et al., 2007; Araujo et al., 2016; Marotta, 2019; Elacqua and Marotta, 2020; Neilson et al., 2021). Despite this, hard-to-staff schools grapple with more severe teacher shortages and struggle to attract high-quality candidates (Sutcher et al., 2016; Dee and Goldhaber, 2017; Bertoni et al., 2020).

Our study demonstrates that implementing low-cost, easily scalable behavioral strategies can play a crucial role in reducing educational inequities. These strategies help alleviate teacher shortages in hard-to-staff schools and increase the influx of qualified teachers into low-performing institutions.

We proceed as follows. Section 2 describes the key features of the public school teacher selection process in Peru. Section 3 describes our experimental design, while Section 5 presents our empirical strategy, data, and balance tests. Section 6 presents our main results. Section 7 concludes.

2 Institutional Context

2.1 Teacher selection process in Peru

The teacher selection process in the Peruvian public school system is regulated by the Ministry of Education (MINEDU) and involves two stages: a centralized and a decentralized stage. At the beginning of the year, MINEDU determines and publishes all vacant teaching positions with a workload of thirty pedagogical hours. Applicants with a teaching degree or a bachelor's degree in education can voluntarily participate in this process by registering online.

The first stage consists of a standardized exam administered by MINEDU (PUN, by its acronym in Spanish), which includes two components. The first component measures general skills, and the second measures pedagogical, curricular, and disciplinary knowledge in the candidate´s specialization. These tests assess candidates' subject knowledge, teaching skills, and general education qualifications. To advance to the decentralized stage, applicants must meet the two minimum score requirements: (i) obtaining 84 points or more on the component of pedagogical, curricular, and disciplinary knowledge (from a total of 150 points), and (ii) achieving a total score of 110 points or more, which is the sum of the scores from both components (from a total maximum of 200 points).

Upon passing the qualifying exam, candidates select a region and a local education administrative unit (Unidad de Gestión Educativa Local - UGEL) for the second stage: the in-person evaluation. This evaluation assesses pedagogical competence through classroom observation, an interview, and a review of the candidate's trajectory, including academic and professional training and work experience. An evaluation committee assigned to each applicant is responsible for this assessment. To pass this final evaluation, candidates must score at least 30 points out of 50 on the classroom observation component.

Candidates who pass both stages are eligible to apply for school vacancies through a centralized system managed by MINEDU. They must log into an online platform to select and rank their preferred vacancies in their chosen region. The platform provides basic information on schools in that region. Applicants must select at least 20 educational institutions with available positions, or the maximum available for their designated certified group, which is determined by teaching level and subject area (e.g., regular primary education in mathematics). If no positions are available in that region, they may choose institutions in another region.

MINEDU uses an algorithm that considers the final score, which is the sum of scores from both the centralized and decentralized stages, along with the candidates' ranked preferences to assign vacancies.[1]

Candidates who passed both stages but either missed the deadline for the first round of vacancy selection or were not awarded a position can participate in a second round. In this second round, MINEDU presents the applicants with one or more alternative positions. The applicants must then log into the MINEDU platform within a specified period and indicate their acceptance or rejection of the offer. In this study, we focus solely

[1] The assignment process consists of a single stage that considers both the final score (PUN) and the demonstrative class conducted at the UGEL level (not at the school level). The Deferred Acceptance (DA) algorithm selects the candidate with the highest score among the applicants, regardless of their ranked preferences

on participants from the first round of selection.

2.2 Hard-to-staff schools

Teacher understaffing remains a persistent global concern for governments and contributes to unequal access to high-quality educators (UNESCO, 2023). This exacerbates socioeconomic achievement gaps among students and strains teacher well-being, as overcrowded systems often leave many qualified candidates jobless (Ajzenman et al., 2024b; Sass et al., 2012; Thiemann, 2018). Peru is no exception to this issue: hard-to-staff schools are typically less frequently selected in the teacher application process (Ajzenman et al., 2024a; Bobba et al., 2021).

In 2013, the Peruvian government introduced a monetary incentive program to attract teachers to hard-to-staff schools, referred to as schools with "bonificaciones." These incentives are linked to the location and type of the school, encompassing rural schools, those in the VRAEM area[2], frontier regions, bilingual institutions, single-teacher, and multi-grade schools. Alongside the monetary rewards, the government introduced non-monetary incentives for permanent teachers employed in these hard-to-staff schools. For instance, permanent teachers in rural or frontier areas receive two key career benefits: preferential consideration for school transfer requests and shortened waiting periods for salary scale advancement. These incentives are designed to accelerate career progression for teachers willing to work in these challenging locations (see Ajzenman et al. (2024a)).

However, these efforts have proven insufficient. According to Ajzenman et al. (2024a), out of 12,300 primary schools with vacancies across Peru's 24 regions in 2019, 6,424 (52%) were not chosen by any candidate in the national stage. These unselected vacancies were notably more rural, farther from provincial capitals, had limited access to basic services,

[2]The VRAEM refers to the *Valle de los Ríos Apurímac, Ene y Mantaro*, a geopolitical area in Peru, located in specific areas in the departments of Ayacucho, Cusco, Huancavelica, and Junin.

and had a higher proportion of underperforming students.

Throughout this paper, we refer to schools that receive monetary incentives as "hard-to-staff" (HTS) schools. Given the criteria used for these benefits, most of the government-targeted schools are situated outside Peru's largest metropolitan areas, Lima Metropolitana and Callao. In 2022, virtually all the schools in these areas were non-hard-to-staff (and thus did not receive any incentive), with specific exceptions. Therefore, we exclude Lima Metropolitana and Callao from our sample.

As shown in Figure 1, hard-to-staff schools generally have students with lower academic performance, both in mathematics and language. Additionally, these schools tend to serve students from lower socioeconomic backgrounds.

3 Experimental design

The experiment was implemented in the Peruvian national public school teacher selection process *Concurso de Ingreso a la Carrera Pública Magisterial* (CPM) 2022.[3] The intervention was embedded in the application platform that teachers used to apply to job vacancies. It involved 74,692 teacher candidates who successfully passed both the national exam and the in-person evaluation, and selected schools on the platform.[4]

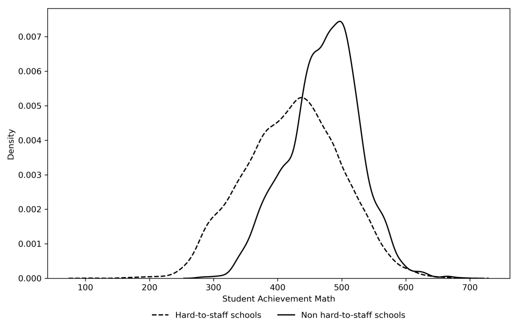
We randomly assigned these candidates into two groups, stratified by 6 groups, determined by teaching level and subject area: a treatment group and a control group.[5] The intervention took place on the online platform immediately before candidates ranked

[3]The Peruvian national public school teacher selection process was originally scheduled for 2022 but had to be postponed until 2023/24.

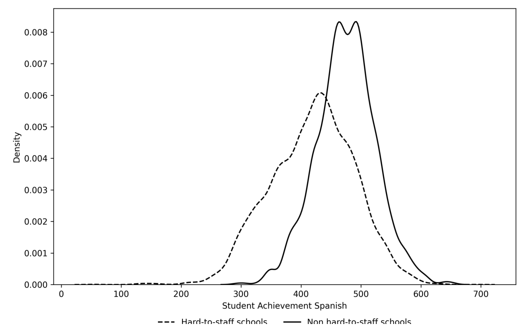
[4]We deployed two experiments: one for teachers in the countryside - mostly rural - and another for the metropolitan area of Lima and Callao. We implemented the experiments differently because that there were virtually no hard-to-staff schools as defined by the government criteria in metropolitan Lima and Callao. Thus, for those regions, we selected *ad hoc* some institutions that we considered relatively hard-to-staff, although they are different in nature and thus incomparable to those in this experiment.

[5]The six groups were categorized as regular track preschool, primary, secondary STEM, secondary Communications, secondary English, and secondary Other Subjects.

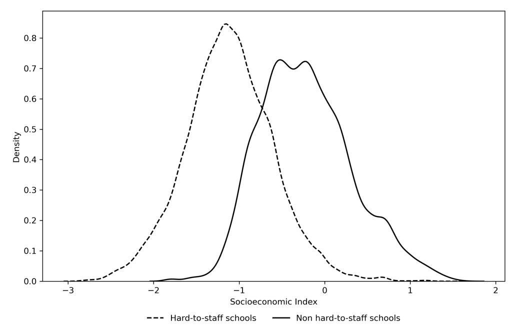
Figure 1: Hard-to-Staff versus Non-Hard-to-Staff schools



(a) School-level student achievement in Math



(b) School-level student achievement in Spanish



(c) Student socioeconomic level

Notes: All graphs show a kernel estimation of the distribution of different outcomes for hard-to-staff and non-hard-to-staff schools. Graph (a) shows the distribution of average student achievement levels in math for 4th grade (ECE 2018). Graph (b) shows the distribution of average student achievement levels in spanish for 4th grade (ECE 2018). Graph (c) shows the distributions of the average student socioeconomic level by school

their preferred school vacancies. At this critical point, both groups were prompted to participate in a voluntary exercise. While the exercise's format remained consistent across both groups, the specific questions varied. The exercise consisted of two questions: one multiple-choice and one open-ended.

In both groups (treatment and control), when teachers entered the platform, they saw a webpage with a brief paragraph (two short sentences) describing the next steps and a large picture of the three icons that identified schools as hard-to-staff (Figure A1).

The objective was twofold: to inform teachers about the meaning of the icons and to prime all participants (treated and control teachers) to think about hard-to-staff and remote schools. These icons were universally displayed to all teachers, irrespective of their treatment condition, as shown in Figure A2. In our sample, hard-to-staff schools were designated by two of these three icons: one for monetary bonuses and another for accelerated career progression due to disadvantaged conditions. This arises because hard-to-staff schools in regional areas receive both benefits, while the vast majority of schools in Lima and Callao do not. However, to encourage teachers to apply to certain urban schools, the government needed a signaling mechanism, even if these schools did not receive any bonus. Since, in theory, teachers could potentially apply to positions in Lima and Callao under specific exceptions, we used the same initial icon screens across the country for consistency.

After teachers passed the first page, the system displayed two voluntary questions. Teachers in the treatment group were asked to give advice to a hypothetical colleague who wants to maximize their impact on students' learning. The first multiple choice question specifically asked what type of school they would recommend from a list of five: Vulnerable urban, VRAEM, Intercultural bilingual, Rural, or Non-vulnerable urban. In the open-ended question teachers were asked to justify their answer. The first four categories (Vulnerable urban, VRAEM, Intercultural bilingual, Rural) are considered hard-to-staff and thus, in our sample, receive a bonus and are identified with the relevant icons. Importantly, those characteristics (not only the icons, but also the type of schools) were visible in the list of schools in both groups, so teachers could recognize them easily.

Teachers in the control group received two neutral questions. In the first (multiple choice), they selected the outlet through which they received the most information about the process, from a list of five: Text messages, Email, Helpline, UGEL. In the second (open-ended), teachers wrote about the best outlet to receive information related to the process.

By design, teachers in both groups received exactly the same information and, within region, had access to the same list of vacancies and relevant information about schools. Section A1 of the Appendix provides a detailed description of the questions and illustrates how they were presented on the platform.

The treatment draws inspiration from behavioral economics studies examining the impact of giving advice (primarily Eskreis-Winkler et al. (2018), Eskreis-Winkler et al. (2019)). Our hypothesis posits that by explicitly outlining strategies for teachers to make a social impact, particularly in teaching hard-to-staff students, those with prosocial tendencies—teachers who prioritize making a societal difference—would be inclined toward consistent behavior. Consequently, they would be more likely to apply to schools where their influence could be maximized.

4 Data

This paper uses administrative data from the 2022 public school teacher selection process in Peru. The data include a comprehensive database of the vacancies offered in the teacher selection process. This database contains various school characteristics, such as location (region, province, district, UGEL), area (urban/rural), school type (multi-teacher, multi-grade, or single-teacher), and indicators showing whether the school is bilingual, located in the VRAEM area or a frontier region, and whether it is classified as hard-to-staff.

The dataset also contains information on the candidates evaluated in the centralized stage, specifically their performance on the standardized written exam. The dataset includes details about those assessed in the decentralized stage, including their results on both the pedagogical competence and professional trajectory assessment. Additionally, it contains information on candidates who successfully passed both stages, including their ranked preferences within their selected region and the school where they were ultimately assigned.

We restrict the sample to candidates applying for positions in the regular education track, known as *Educación Basica Regular* (EBR). Throughout the analysis, we focus on two samples. The first is the ”full sample,” which includes all candidates who selected vacancies through the government platform in any region of Peru (except Lima Metropolitana and Callao, where there were virtually no hard-to-staff schools, as defined by the government). This sample consists of 74,692 individuals.

Although the experiment was successfully implemented, many teachers were likely exposed to no variation in vacancy type, rendering the treatment ineffective. Teacher labor markets tend to be highly localized (Boyd et al., 2005; Reininger, 2012; Bertoni et al., 2020). This is because teacher candidates likely select vacancies near their area of residence. Many candidates live in areas where there is no variation in the type of vacancy within their regions: either all vacancies were in hard-to-staff schools or non-hard-to-staff schools. We anticipated this problem, as it had occurred in the previous (Ajzenman et al., 2024a) and similar (Ajzenman et al., 2024b) contexts. Thus, we pre-registered the results of restricting the full sample to increase power, although we did not detail a specific procedure.

This lack of variation in vacancy types arose because more than 20% of all district-subject specialty area pairs had no hard-to-staff schools, and over half of district-specialty area pairs had only hard-to-staff schools. Teachers searching for vacancies within their district or nearby districts must select the available vacancy type based on their subject area, which made the treatment ineffective in cases where all vacancies were either hard-to-staff or non-hard-to-staff. For instance, 10.9% of teachers only had access to hard-to-staff schools in their residence district or nearby districts, based on their subject area. Similarly, 15.6% only had access to non-hard-to-staff schools.

Hence, following Ajzenman et al. (2024b), the second sample consists of a 'likely treated sample'. This sample includes teachers who have at least one hard-to-staff and one non-hard-to-staff school available in their district of residence (where they took the qualifying exam) or nearby districts. Given that the area of residence is pre-determined at the moment of entering the platform for the first time, this restriction is exogenous to the treatment, thus preserving the causal interpretation of the results. This likely treated sample consists of 45,578 individuals. Table 1 presents a descriptive summary of the applicants in both our restricted (i.e., sample with local supply restriction) and full samples.

Table 1: Summary of model variables

	Sample with local supply restriction						**Sample without local supply restriction**					
	Mean	SD	p10	p50	p90	Count	Mean	SD	p10	p50	p90	Count
Woman	0.77	0.42	0.00	1.00	1.00	45,578	0.71	0.46	0.00	1.00	1.00	74,692
Age	39.83	7.40	30.00	40.00	49.00	45,578	39.84	7.48	29.00	40.00	49.00	74,692
Disabled	0.01	0.12	0.00	0.00	0.00	45,578	0.02	0.12	0.00	0.00	0.00	74,692
Total score of centralized stage	140.87	17.51	119.00	139.00	166.00	45,578	140.72	17.59	119.00	139.00	166.00	74,692
Total score of decentralized stage	62.20	11.33	47.50	62.00	75.50	45,578	62.37	11.50	47.74	62.50	76.00	74,692
Selects top 1	0.58	0.49	0.00	1.00	1.00	45,578	0.63	0.48	0.00	1.00	1.00	74,692
Selects top 2	0.66	0.47	0.00	1.00	1.00	45,578	0.70	0.46	0.00	1.00	1.00	74,692
Selects top 3	0.72	0.45	0.00	1.00	1.00	45,578	0.75	0.44	0.00	1.00	1.00	74,692
Selects top 4	0.75	0.43	0.00	1.00	1.00	45,578	0.78	0.42	0.00	1.00	1.00	74,692
% of HTS until top 2	0.58	0.45	0.00	0.50	1.00	45,578	0.63	0.44	0.00	1.00	1.00	74,692
% of HTS until top 3	0.58	0.43	0.00	0.67	1.00	45,578	0.63	0.43	0.00	1.00	1.00	74,692
% of HTS until top 4	0.58	0.42	0.00	0.75	1.00	45,578	0.63	0.41	0.00	0.75	1.00	74,692
Assigned	0.71	0.46	0.00	1.00	1.00	45,578	0.68	0.47	0.00	1.00	1.00	74,692
Assigned HTS	0.46	0.50	0.00	0.00	1.00	45,578	0.48	0.50	0.00	0.00	1.00	74,692

NOTES: **Woman**: Takes 1 if the teacher is a woman. **Age**: Age of the teacher. **Disabled**: Indicates whether the teacher has a disability.**Total score of centralized stage**: Score of the centralized stage(PUN exam).**Total score of decentralized stage**: Score of the decentralized stage. **Selects top i**: Takes 1 if at least one HTS is selected between ranking 1 and ranking i. **% of HTS until top i**: % of HTS selected until ranking i. **Assigned**: Takes 1 if the teacher was assigned to a vacancy. **Assigned to HTS**: Takes a 1 if the teacher was assigned to a vacancy on a HTS school. The description of the sample with local supply restriction is in Section 4

5 Empirical strategy

To measure the overall impact of advice-giving on different teachers' selection outcomes, we run regressions of the following form:

$$Y_i = \alpha + \beta T_i + \gamma X_i + \epsilon_i \tag{1}$$

were Y_i is the preference or assignment outcome for teacher candidate i. T_i is a dummy that indicates whether candidate i received the treatment, and X_i is a vector including candidate control variables, such as age, gender, region, grupo de inscripcion, and PUN score.

Table 2 compares candidate characteristics in the between treatment and control groups for both samples. Most of the variables do not show any significant difference, and even those that do are quantitatively almost identical.

5.1 Main Measures

Our analysis includes our (pre-registered) outcomes and pre-registered heterogeneity by gender. The outcomes relating to teacher choices are (i) if their first choice was a hard-to-staff school; (ii) if their first two choices included a hard-to-staff school; and (iii) the proportion of hard-to-staff schools (as defined by the government) in teachers' choice sets. Importantly, our main outcomes related to choice reflect high-stakes decisions; out of the teachers who are assigned to a school, 40.9% get their first preference and 51.7% get their top-2 preferences.

We complement our analysis with an "assignment" outcome that captures whether a candidate was offered a teaching job at a hard-to-staff school. The outcome "Assigned to hard-to-staff school" takes the value of one if the candidate was assigned to work in a school categorized as "hard-to-staff" by the algorithm. The assignment outcome should

Table 2: Balance test

	Sample with local restriction			Sample without local restriction		
	Control	Treatment	Treatment - Control	Control	Treatment	Treatment - Control
Woman	0.767	0.777	0.010**	0.702	0.708	0.006*
	(0.422)	(0.416)	[0.016σ]	(0.457)	(0.454)	[0.010σ]
Age	39.816	39.840	0.024	39.841	39.834	-0.008
	(7.381)	(7.409)	[0.002σ]	(7.477)	(7.485)	[-0.001σ]
Disabled	0.013	0.015	0.001	0.015	0.016	0.000
	(0.115)	(0.120)	[0.007σ]	(0.123)	(0.124)	[0.001σ]
General skills exam score	35.774	35.706	-0.068	35.931	35.814	-0.117**
	(7.417)	(7.435)	[-0.006σ]	(7.416)	(7.415)	[-0.011σ]
Specialization and pedagogical exam score	105.213	105.054	-0.159	104.946	104.754	-0.192*
	(14.572)	(14.586)	[-0.008σ]	(14.537)	(14.510)	[-0.009σ]
Total score of centralized exam	140.987	140.760	-0.227	140.877	140.568	-0.309**
	(17.488)	(17.523)	[-0.009σ]	(17.591)	(17.582)	[-0.012σ]
Specialty knowledge score	41.534	41.562	0.028	41.527	41.538	0.011
	(6.186)	(6.197)	[0.003σ]	(6.149)	(6.142)	[0.001σ]
Personal interview score	8.217	8.244	0.027*	8.186	8.207	0.020*
	(1.568)	(1.563)	[0.012σ]	(1.560)	(1.552)	[0.009σ]
Total score of decentralized stage	62.182	62.216	0.034	62.373	62.359	-0.014
	(11.259)	(11.404)	[0.002σ]	(11.471)	(11.537)	[-0.001σ]
Observations	22,796	22,782	45,578	37,296	37,396	74,692

NOTES: **Woman**: Takes 1 if the teacher is a woman. **Age**: Age of the teacher. **Disabled**: Indicates whether the teacher has a disability. **General skills exam score**: Score from the first part of the centralized stage, related to general skills. **Specialization and pedagogical exam score**: Score from the second part of the centralized stage, focused on pedagogical, curricular, and disciplinary knowledge. **Total score of centralized stage**: Total score from the centralized stage (PUN exam). **Specialty knowledge score**: Score from the first part of the decentralized stage, which involves classroom observation. **Personal interview score**: Score from the second part of the decentralized stage, based on a personal interview. **Total score of decentralized stage**: Total score from the decentralized stage. The description of the sample with local supply restriction is in Section 4. *** p<0.01, ** p<0.05, * p<0.1. Standard errors are reported in parentheses below each mean, while standardized differences are reported below the difference columns.

be interpreted with caution, as the effect of the experiment on teachers' preferences in the treatment group could influence the outcome allocation of the control group in equilibrium (see Ajzenman et al. (2024a,b) for a discussion).

Consistent with other papers in the same context (Ajzenman et al., 2024a) we use teachers' PUN score (their grade on the qualifying exam) as a proxy for teacher quality or ability. While we acknowledge that teachers' value-added is a better predictor of teachers' performance (and, more generally, that teachers' effectiveness depends on a variety of traits as shown by Rockoff et al. (2011); Jacob et al. (2018)), we were not able to use this indicator because most of the teacher candidates were new to the school system.

Reassuringly, Bertoni et al. (2023a) shows that, in Peru, PUN scores are significantly correlated with measures of value-added.

6 Results and Interpretation

We display our main results in Table 3. For each outcome, we show two panels. Panel A - our preferred specification - shows the results using the likely treated sample (as defined in section 4) and Panel B uses the full sample. Columns labeled with odd numbers show the results for the main regression without interactions, and columns labeled with even numbers show the results including a gender dummy that takes the value of one for female candidates and zero otherwise. This heterogeneity was pre-registered and is based on previous research in the same context, documenting disproportionally higher preferences of female candidates for working in urban areas (see Ajzenman et al. (2024a) and Bertoni et al. (2022)).

As columns (1) and (2) show in panel A, the intervention successfully increased the proportion of hard-to-staff schools included in teachers' choice sets. For male and female candidates pooled, the effect is 0.4 percentage points (significant at 10%; mean in the control group 62.5%). Column (2) shows that the effect seems to be driven by male candidates: 0.7 pp (significant at 10%; mean in the control group 70.3%), although the gender interaction is not significant. Columns (4) to (6) confirm these results and emphasize the role of male teachers in driving the main effects.

While there is no treatment effect on the probability of ranking a hard-to-staff school in the top-1 or top-2 positions for female and male candidates pooled, the effect for male candidates is significant and large. They are 2.1 pp. (significant at 5%; mean in the control group 64.6%) and 2.2 pp (significant at 5%; mean in the control group 65.3%) more likely to rank a hard-to-staff school as their top or 2nd best choices, respectively. Importantly, the gender interaction becomes significant and negative in both cases, resulting in a null effect for female candidates. Considering that 51.7% were assigned to one of their top-2 choices, these outcomes are particularly relevant, as they refer to a high-stakes, consequential decision.

Consistent with these results, columns (7) and (8) show that the probability of being assigned to a hard-to-staff school is 0.8 pp. (male and female candidates pooled) and 1.5 pp. for male candidates (although the gender interaction is not significant), respectively. The effects in both cases are significant at the 10% level (mean in the control group: 45.97% and 52.5% respectively).

The primary objective of the intervention was to improve both the quantity and quality of teachers working in hard-to-staff schools. Since these schools are inherently difficult to staff and every teacher participating in this program passed a rigorous qualifying examination, the goal would have been accomplished even if the treatment impacted only the relatively lower-performing teachers in the sample. Ideally, an even more equitable outcome would have been if not only the lowest-performing teachers were influenced by the treatment.

In Table 4 we analyze the heterogeneous treatment effects by candidate performance (using the qualifying test score as a continuous measure of future performance). To simplify the interpretation, we present separate results for female and male candidates. Our results suggest that the effect was not driven by low performers. Instead, the effect is larger for higher-performing teachers (Column 1).[6] While our quality measure is a proxy and may not be a strong predictor of teachers' value-added, other papers in the same context show

[6]Even if the Deferred Acceptance (DA) mechanism were not strategy-proof, this table shows that the treatment does not reinforce strategic behavior in the expected way. Under a manipulable mechanism, the optimal response would likely involve high-scoring candidates avoiding HTS schools to secure positions in more desirable schools, knowing they would be prioritized for more competitive vacancies, while lower-scoring candidates would have stronger incentives to apply to HTS schools, anticipating lower competition. If the treatment simply made this strategic behavior more salient, we would expect the interaction between *Treatment* $\times$ *PUN Score* to be negative, as lower-scoring candidates should respond more strongly. However, the positive (or null) coefficient contradicts this expectation.

a strong correlation between PUN and value-added (Bertoni et al., 2023a).

Finally, given that the primary motivation for the intervention was to improve access to high-quality teachers for students enrolled in hard-to-staff institutions, we examined whether, as a result of the treatment, teachers selected and were offered positions in schools where students had relatively lower scores. Table 5 shows the results for the two students' test scores: Math and Language. Each outcome reflects the average test score for each school in the corresponding subject. Specifically, the outcome related to top-1 is the average students' test score in Math of the school chosen as the first priority in teachers' choice set. Although the results are noisy, they show that teachers in the treatment group were offered positions in schools where, on average, students have significantly lower performance in Math and Language (and thus, students likely to be most in need).

Table 3: Treatment effects on choices and assignment

	% of listed HTS schools		Selects HTS school in				Teacher is assigned to a HTS school	
			1st position	1st position	2nd position	2nd position		
Panel A: Sample with local supply restriction								
Treatment (I)	0.004*	0.007*	0.004	0.021**	0.006	0.022***	0.008*	0.016*
	(0.002)	(0.004)	(0.004)	(0.008)	(0.004)	(0.008)	(0.004)	(0.009)
Treatment × Woman (II)		-0.004		-0.022**		-0.021**		-0.010
		(0.005)		(0.010)		(0.010)		(0.010)
(I) + (II)		0.003		-0.001		0.001		0.005
		(0.002)		(0.005)		(0.005)		(0.005)
Mean Control (Men)	0.703	0.703	0.646	0.646	0.653	0.653	0.525	0.525
Mean Control (Women)	0.601	0.601	0.553	0.553	0.552	0.552	0.440	0.440
Observations	45,578	45,578	45,578	45,578	45,578	45,578	45,578	45,578
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Sample without local supply restriction								
Treatment (I)	-0.000	0.003	0.000	0.012**	0.000	0.009*	0.002	0.007
	(0.002)	(0.003)	(0.003)	(0.005)	(0.003)	(0.005)	(0.003)	(0.006)
Treatment × Woman (II)		-0.005		-0.016**		-0.012*		-0.008
		(0.003)		(0.007)		(0.006)		(0.007)
(I) + (II)		-0.002		-0.004		-0.003		-0.001
		(0.002)		(0.004)		(0.004)		(0.004)
Mean Control (Men)	0.739	0.739	0.704	0.704	0.708	0.708	0.539	0.539
Mean Control (Women)	0.635	0.635	0.595	0.595	0.592	0.592	0.459	0.459
Observations	74,692	74,692	74,692	74,692	74,692	74,692	74,692	74,692
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTES: The outcome **% of listed HTS schools** is the percentage of hard to staff schools in the choice set. The outcome **Selects HTS school in** is a dummy that equals 1 if the applicants selected a hard-to-staff school in their 1st or 2nd position among their listed schools, respectively. **Teacher is assigned to a HTS school** is a dummy that equals 1 if the applicant is assigned to a HTS school. In all regressions, we include controls for gender, as well as scores from both the centralized (PUN) and decentralized stages. The description of the sample with local supply restriction is in Section 4. Robust standard errors are displayed in parentheses. *** p<0.01, ** p<0.05, * p<0.1

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Table 4: Effects mediated by PUN scores

	% of listed HTS schools		Selects HTS school in				Teacher is assigned to a HTS school	
	Woman==0	Woman==1	1st position Woman==0	1st position Woman==1	2nd position Woman==0	2nd position Woman==1		
Panel A: Sample with local supply restriction								
Treatment	0.009**	0.003	0.021**	-0.001	0.024***	0.001	0.017**	0.005
	(0.004)	(0.002)	(0.008)	(0.005)	(0.008)	(0.005)	(0.009)	(0.005)
Treatment × PUN score	0.004*	0.000	0.001	-0.001	0.007	0.000	0.002	0.000
	(0.002)	(0.001)	(0.005)	(0.003)	(0.005)	(0.003)	(0.005)	(0.003)
Mean Control	0.703	0.601	0.646	0.553	0.653	0.552	0.525	0.440
Observations	10,380	35,198	10,380	35,198	10,380	35,198	10,380	35,198
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Sample without local supply restriction								
Treatment	0.004	-0.002	0.012**	-0.005	0.009*	-0.003	0.008	-0.001
	(0.003)	(0.002)	(0.005)	(0.004)	(0.005)	(0.004)	(0.006)	(0.004)
Treatment × PUN score	0.003*	-0.001	0.001	-0.002	0.003	-0.001	0.002	-0.000
	(0.002)	(0.001)	(0.003)	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)
Mean Control	0.739	0.635	0.704	0.595	0.708	0.592	0.539	0.459
Observations	22,010	52,682	22,010	52,682	22,010	52,682	22,010	52,682
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTES: The outcome **% of listed HTS schools** is the percentage of hard to staff schools in the choice set. The outcome **Selects HTS school in** is a dummy that equals 1 if the applicants selected a hard-to-staff school in their 1st or 2nd position among their listed schools, respectively. **Teacher is assigned to a HTS school** is a dummy that equals 1 if the applicant is assigned to a HTS school. In all regressions, we include controls for gender, as well as scores from both the centralized (PUN) and decentralized stages. Robust standard errors are displayed in parentheses. The description of the sample with local supply restriction is in Section 4. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Effect on students scores in Maths and Language

	Average ECE test results of selected schools		ECE results in school selected at				Average ECE scores in assigned school	
			1st position	1st position	First 2 choices	First 2 choices		
Panel A: Sample with local supply restriction								
Treatment (I)	-0.248	-0.660**	0.443	-0.045	0.355	-0.178	-0.362	-1.148***
	(0.198)	(0.328)	(0.497)	(0.806)	(0.412)	(0.680)	(0.253)	(0.398)
Treatment × Woman (II)		0.643		0.758		0.831		1.301**
		(0.411)		(1.024)		(0.855)		(0.515)
(I) + (II)		-0.017		0.713		0.653		0.154
		(0.247)		(0.631)		(0.519)		(0.327)
Mean Control (Men)	-0.843	-0.843	-0.724	-0.724	-0.754	-0.754	-0.874	-0.874
Mean Control (Women)	-0.674	-0.674	-0.538	-0.538	-0.562	-0.562	-0.720	-0.720
Observations	28,040	28,040	26,306	26,306	27,531	27,531	18,414	18,414
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Sample without local supply restriction								
Treatment (I)	0.035	-0.073	0.429	-0.081	0.352	-0.085	-0.047	-0.413
	(0.144)	(0.218)	(0.353)	(0.532)	(0.295)	(0.449)	(0.190)	(0.272)
Treatment × Woman (II)		0.185		0.868		0.746		0.666*
		(0.291)		(0.711)		(0.595)		(0.380)
(I) + (II)		0.111		0.787*		0.661*		0.252
		(0.192)		(0.471)		(0.390)		(0.264)
Mean Control (Men)	-0.824	-0.824	-0.725	-0.725	-0.750	-0.750	-0.881	-0.881
Mean Control (Women)	-0.677	-0.677	-0.538	-0.538	-0.561	-0.561	-0.740	-0.740
Observations	51,659	51,659	48,389	48,389	50,694	50,694	32,669	32,669
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTES: **Average ECE test results of selected schools**: Standardized student test scores in Mathematics and Language, averaged across the selected schools. **ECE results in schools selected at**: Standardized student test scores of schools in Mathematics and Language chosen in the 1st or 2nd position among the listed school preferences, respectively. **Average ECE scores in assigned school**: Standardized student test scores in Mathematics and Language for the school to which the teacher was assigned. All regressions include controls for gender, as well as test scores from both the centralized (PUN) and decentralized stages. The description of the sample with local supply restriction is in Section 4. Robust standard errors are displayed in parentheses. *** p<0.01, ** p<0.05, * p<0.1

6.1 What teachers drive the results?

To validate our heterogeneous results, we use causal forest techniques to identify teachers' characteristics that maximize the treatment through honest splitting (Athey and Imbens, 2017). Although we pre-registered the heterogeneous analysis by gender, based on our knowledge of the context and previous evidence from Peru (Ajzenman et al., 2024a), this approach reduces researcher discretion in selecting relevant dimensions of heterogeneity, allowing the data to speak for themselves. Following the "honest" method developed by Athey and Wager (2019), we estimate Conditional Average Treatment Effects (CATE) for each individual in our sample using a generalized random forest (grf R package). We estimate CATE based on all teacher characteristics included in our data. We present the results for our main outcomes for the full and likely treated samples.

Figures A7 to A10 display the predicted treatment effects distribution for our main outcomes. To analyze the characteristics that maximize the effects, we estimate the treatment effects for the 20% of teachers where the treatment was least effective (Q1) and most effective (Q5). Table A1 presents the mean of each covariate for Q1 and Q5. We report the sample mean, the mean for the individuals in each group (least or most affected by the treatment), and the standardized difference between each group.

Interesting patterns emerge from this analysis.[7] First, patterns are relatively similar across outcomes, as expected. Second, the differences between the top and bottom quintiles of treatment effect are small for most characteristics with the exception of gender. Specifically, the probability of being a male in the lowest impact quintile is 16.3%, versus 25.6% in the highest impact quintile: a large difference of almost 10 percentage points. Other covariates also show differences, but they do not seem to be economically meaningful in terms of their magnitude. These heterogeneous results confirm our findings from

[7]Performing inference of the difference between the means of the two groups is not straightforward in this context. We thus follow Britto et al. (2022) and Ajzenman et al. (2023) and analyze the magnitude of the standardized difference in each case qualitatively.

tables 3 and 4.

6.2 Unpacking the giving advice effect

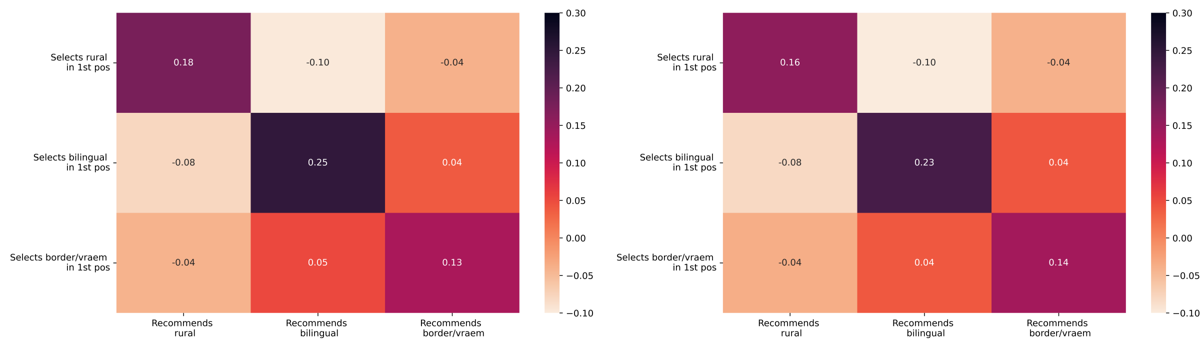
The giving advice effect could operate through several channels. Cognitive dissonance and priming are two main candidates. Although we cannot fully disentangle these two factors, we provide evidence that suggests that cognitive dissonance could be a relevant driver of the effect.

The specificity of the treatment question allows us to examine this. When responding to the treatment prompt, teachers in the treatment group can recommend different types of schools: rural schools; intercultural bilingual schools (which incorporate Indigenous languages); schools in the VRAEM region; or schools in frontier zones. If cognitive dissonance drives the treatment effect, we would expect teachers to recommend the same type of school they personally selected for their service. This alignment between teachers' own choices and their recommendations would provide evidence supporting (the avoidance of) cognitive dissonance as a key mechanism.

Figure 2 displays the correlation between binary indicators for school selection and school recommendation among teachers in the treatment group. The diagonal elements represent concordance between teachers' personal school choices (a binary variable indicating if teachers ranked a specific type of school as their first choice) and their subsequent recommendations. Given that school classifications are not mutually exclusive (for instance, intercultural bilingual schools and VRAEM schools frequently overlap with rural designations), we constructed mutually exclusive categories based on each school's primary characteristic. The diagonal elements of this correlation matrix provide insight into whether teachers tend to recommend the same type of school they themselves ranked as their first choice.

The recategorization follows a hierarchical classification based on institutional complex-

Figure 2: Correlation Matrix: school type recommendation and own choice



(a) Sample without local restriction **(b)** Sample with local restriction

NOTES: The figure shows the correlation between the recommendation and the vacancy chosen. **Selects rural in 1st pos**: Is a dummy equal to one if the vacancy selected at first position is a rural school. **Selects bilingual in 1st pos**: Is a dummy equal to one if the vacancy selected at first position is a bilingual school. **Selects border/VRAEM in 1st pos**: Is a dummy equal to one if the vacancy selected at first position is a VRAEM or frontier school. **Recommends rural**: Is a dummy equal to one if the teacher advise is to choose rural. **Recommends bilingual**: Is a dummy equal to one if the teacher advise is to choose bilingual. **Recommends border/VRAEM**: Is a dummy equal to one if the teacher advise is to choose border/VRAEM. Categories were constructed to be mutually exclusive considering the importance of each feature for the teacher's decision with bilingual schools being prioritized first, followed by VRAEM or frontier schools, and then rural schools. If a school exhibits a combination of these characteristics, the dummy for the most important characteristic is set to one and the others to zero.

ity. Intercultural bilingual schools received highest priority given their unique pedagogical demands and distinct instructional challenges, including specialized language requirements and cultural adaptations. VRAEM and frontier schools were assigned second-order priority, reflecting their operation in higher-risk zones characterized by socioeconomic vulnerability. Rural designation served as the residual category, encompassing schools that, while located in rural areas, did not present the specialized features of either bilingual or VRAEM/frontier institutions. This hierarchical classification ensures mutual exclusivity while preserving the most salient characteristics of each school type.

Our classification approach rests on the assumption that teachers' school selection reflects a hierarchical preference structure based on schools' most distinctive attributes. When choosing intercultural bilingual schools, teachers likely prioritize the linguistic and cultural dimensions of instruction over geographic location. Similarly, the selection of VRAEM or frontier schools suggests that teachers weigh institutional vulnerability and challenging conditions more heavily than rural status per se. This assumption aligns with a decision-making framework where teachers evaluate schools based on their most distinguishing characteristics rather than their more general features. Such hierarchical preferences provide theoretical justification for our mutually exclusive categorization.

7 Discussion

This paper underscores the critical problem of global teacher sorting in education systems and presents an experimental assessment to mitigate this issue in the Peruvian teacher selection process. By leveraging the behavioral principle of the *giving advice* effect, the study prompts teachers to offer recommendations about schools they should prioritize to impact students' learning. The results contribute to the understanding of teacher sorting, educational inequality, and the efficacy of behavioral interventions in reducing such disparities at no additional cost.

Addressing teacher sorting through these interventions holds promise in promoting equity in education by attracting qualified teachers to underprivileged schools. This is particularly vital considering the substantial influence of effective educators on student outcomes, especially among those from disadvantaged backgrounds. Additionally, our findings indicate that scalable, cost-effective behavioral strategies can play a pivotal role in diminishing educational inequities by alleviating teacher shortages in less privileged schools.

8 Data Availability Statement

The data supporting this study's findings belong to the MINEDU and were used under specific license for this work; therefore, they are not publicly available. The data are, however, available from the authors upon reasonable request and with permission from MINEDU.

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Appendix

A1 Application Platform

Following Ajzenman et al. (2024a), schools were labeled with icons highlighting their associated incentive schemes. Specifically, these consisted of a money bag icon, referencing the monetary incentives, a ladder, indicating the opportunity for faster career progression, and a school with a heart icon, symbolizing places where teachers could have a greater social impact.

In both the treatment and control groups, right after teachers entered the platform, they saw a brief paragraph describing the next steps and pictures of the three icons that identified schools as hard-to-staff, as shown in Figure A1. These icons were shown to all teachers, regardless of the treatment condition, as shown in Figure A2.

A1.1 Hard-to-staff schools in the platform

A1.2 Intervention

Before selecting vacancies on the platform, candidates answered two questions: one multiple-choice and one open-ended. The multiple-choice question for the treatment group was as follows: "Many teachers committed our country's education are participating in the 2022 CPM Entry Contest. If you had the opportunity to speak to one of them who expressed indecision about which educational institution to choose but knew they wanted to go to one where they could have the greatest impact on student learning, what type of educational institution would you recommend?" The options were: Vulnerable urban. Border or VRAEM, Intercultural bilingual, Rural, and Non-vulnerable urban. For the control group, the multiple-choice question was: "Through which communication channel did you receive

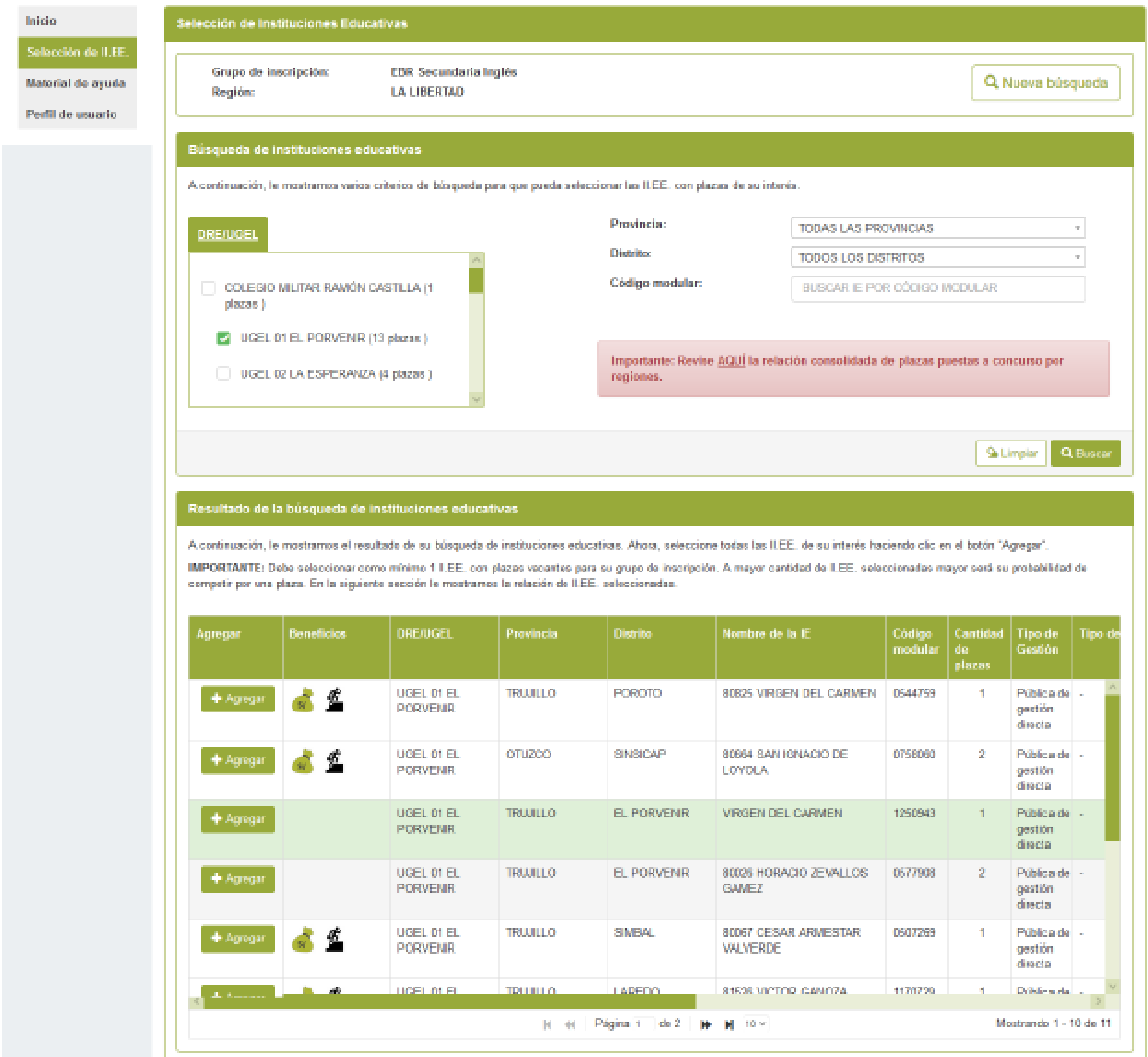
Figure A1: Screenshot: Icons that define hard-to-staff schools

Source: Ministry of Education, Perú.

the most information about the 2022 CPM Entry Contest?" The options were: Teacher Evaluation Website (Perú Educa), text messages. email, helpline, and UGEL/DRE. Candidates also had the option to choose "I do not wish to respond" and proceed directly to the vacancy selection screen.

The second question was open-ended. For the treatment group, it was: "Why would you advise the teacher to apply to this type of educational institution?" For the control group, the question was: "Which communication channel do you think is the best for receiving information about the contest?". Figure A3 to Figure A6 display the screens for the treatment and control groups.

Figure A2: Screenshot: List of vacancies in the platform



Source: Ministry of Education, Perú.

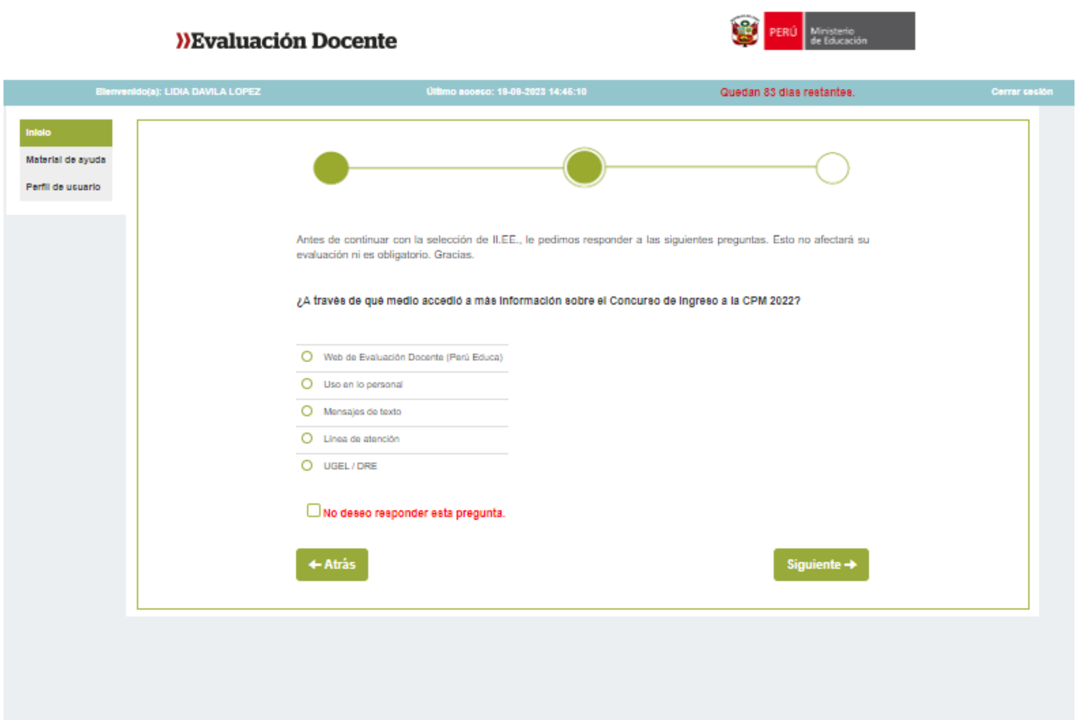
Figure A3

Source: Ministry of Education, Perú.

Figure A4

Source: Ministry of Education, Perú.

Figure A5



Source: Ministry of Education, Perú.

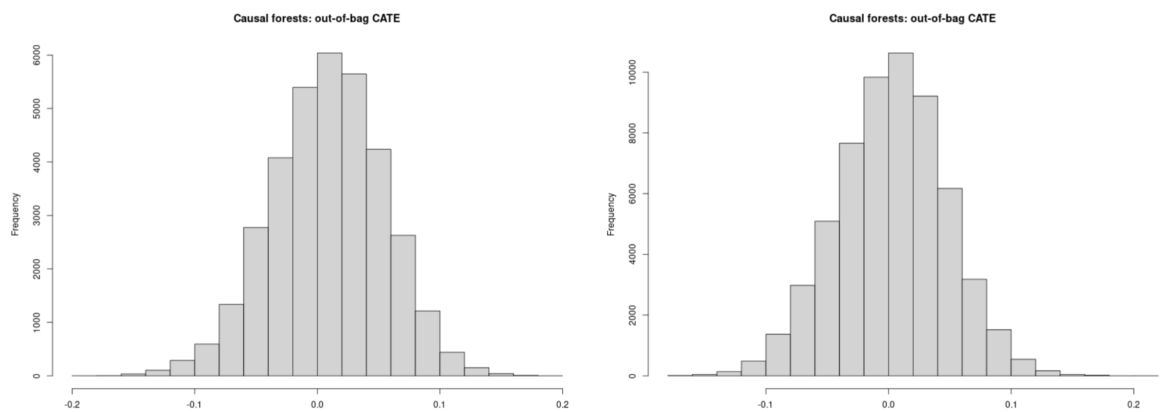
Figure A6

Source: Ministry of Education, Perú.

A2 Causal forest

Figure A7: Histogram of CATE: Selects HTS in first position

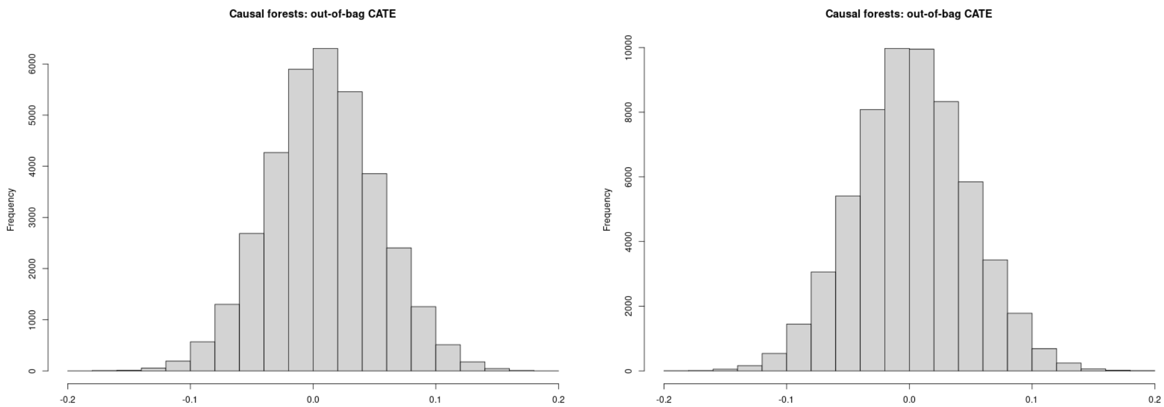
(a) Likely treated sample **(b)** Full sample



NOTES: This figure shows the treatment effect heterogeneity obtained by the causal forest estimation in the outcome **Selects HTS in first position**. **Selects HTS school in first position** is a dummy that equals 1 if the applicants selected a hard-to-staff school in their 1st position among their listed schools. The description of the sample with local supply restriction is in Section 4.

Figure A8: Histogram of CATE: Selects HTS in second position

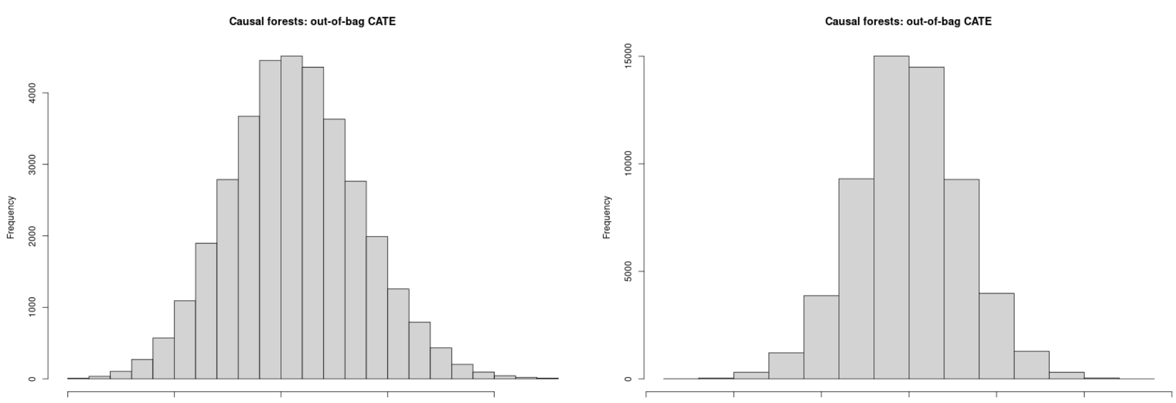
(a) Likely treated sample **(b)** Full sample



Notes: This figure shows the treatment effect heterogeneity obtained by the causal forest estimation in the outcome **Selects HTS in second position**. **Selects HTS school in second position** is a dummy that equals 1 if the applicants selected a hard-to-staff school in their 2nd position among their listed schools. The description of the sample with local supply restriction is in Section 4.

Figure A9: Histogram of CATE: % of HTS

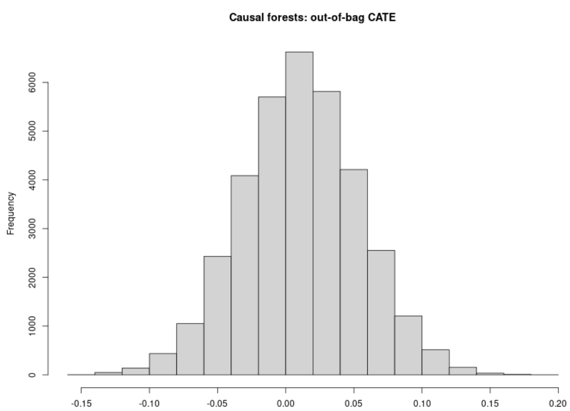
(a) Likely treated sample **(b)** Full sample



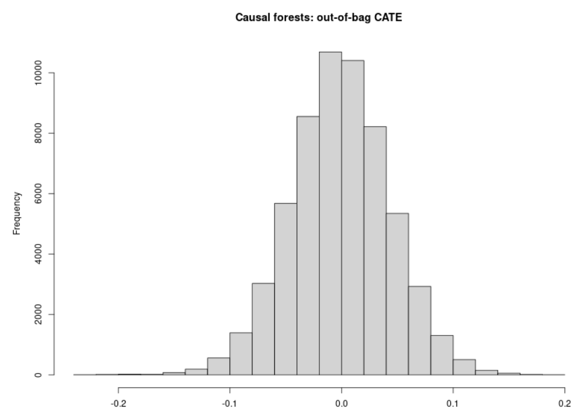
Notes: This figure shows the treatment effect heterogeneity obtained by the causal forest estimation in the outcome **% of listed HTS schools**. **% of listed HTS schools** is the percentage of hard to staff schools in the choice set. The description of the sample with local supply restriction is in Section 4.

Figure A10: Histogram of CATE: Assigned to HTS

(a) Likely treated sample



(b) Full sample



NOTES: This figure shows the treatment effect heterogeneity obtained by the causal forest estimation in the outcome **Assigned to HTS**. **Assigned to HTS** is a dummy that equals 1 if the applicant is assigned to a HTS school. The description of the sample with local supply restriction is in Section 4.

Table A1: Covariates mean for top and bottom quintiles of treatment effect

	Full Sample			Restricted Sample		
% of listed HTS schools	Q1 (I)	Q5 (II)	Difference (II)-(I)	Q1 (I)	Q5 (II)	Difference (II)-(I)
Man	0.262	0.272	0.010	0.196	0.225	0.029
			[0.034]			[0.133]
Age	39.763	40.209	0.446	39.120	39.953	0.833
			[0.011]			[0.021]
Disabled	0.015	0.015	0.000	0.011	0.012	0.000
			[0.011]			[0.031]
Specialty knowledge	101.505	109.386	7.881	101.306	111.263	9.958
			[0.075]			[0.095]
Centralized exam score	137.312	146.828	9.515	136.242	148.959	12.717
			[0.068]			[0.090]
Personal interview score	8.428	8.200	-0.228	8.152	8.294	0.142
			[-0.028]			[0.017]
Selects HTS school in 1st position						
Man	0.224	0.298	0.074	0.166	0.243	0.077
			[0.257]			[0.351]
Age	38.744	40.891	2.147	38.547	40.603	2.057
			[0.054]			[0.052]
Disabled	0.014	0.016	0.002	0.011	0.015	0.003
			[0.126]			[0.241]
Specialty knowledge	103.455	106.641	3.186	102.495	107.254	4.759
			[0.030]			[0.045]
Centralized exam score	140.295	143.075	2.780	138.779	143.199	4.420
			[0.020]			[0.031]
Personal interview score	8.337	8.297	-0.040	8.067	8.416	0.348
			[-0.005]			[0.042]
Selects HTS school in 2nd position						
Man	0.240	0.280	0.040	0.166	0.220	0.054
			[0.139]			[0.244]
Age	39.520	40.289	0.769	39.435	39.464	0.029
			[0.019]			[0.001]
Disabled	0.014	0.016	0.001	0.012	0.014	0.002
			[0.082]			[0.131]
Specialty knowledge	102.353	108.606	6.253	102.311	109.332	7.021
			[0.060]			[0.067]
Centralized exam score	138.811	144.991	6.180	138.282	144.895	6.614
			[0.044]			[0.047]
Personal interview score	8.420	8.190	-0.229	8.325	8.333	0.008
			[-0.028]			[0.001]
Teacher is assigned to a HTS school						
Man	0.254	0.293	0.039	0.181	0.216	0.035
			[0.135]			[0.160]
Age	40.006	38.392	-1.614	39.650	38.869	-0.781
			[-0.040]			[-0.020]
Disabled	0.015	0.014	-0.001	0.013	0.014	0.000
			[-0.093]			[0.031]
Specialty knowledge score	103.140	107.437	4.297	104.954	106.293	1.339
			[0.041]			[0.013]
Centralized exam score	138.886	144.657	5.771	140.816	142.481	1.665
			[0.041]			[0.012]
Personal interview score	8.306	8.359	0.053	8.346	8.326	-0.020
			[0.006]			[-0.002]

NOTES: This table shows the mean value of each covariate for the bottom and top quintiles of the treatment effect. The difference columns display the differences in points and also as differences expressed in standard deviations of the variable. The outcome **% of listed HTS schools** is the percentage of hard to staff schools in the choice set. The outcome **Selects HTS school in** is a dummy that equals 1 if the applicants selected a hard-to-staff school in their 1st or 2nd position among their listed schools, respectively. **Teacher is assigned to a HTS school** is a dummy that equals 1 if the applicant is assigned to a HTS school. The description of the sample with local supply restriction is in Section 4.