Order Effects and teachers' labor supply: a Nation-wide RCT in Ecuador.*

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Abstract

Inequality in access to high-quality teachers is an important driver of student socioeconomic achievement gaps. We experimentally evaluate a nation-wide zero-cost government program in Ecuador to reduce teacher sorting (that is, lower-income students are more likely to attend understaffed schools with less qualified teachers) based on an insight from behavioral economics: order effects. In the treatment arm, teachers' job application platform showed hard-to-staff schools first, while in the control group teaching vacancies were displayed in alphabetical order. In both arms, hard-to-staff schools were labeled with the same icon. Teachers in the treatment arm were more likely to apply to hard-to-staff schools, rank them as their highest priority, and be assigned to a job vacancy in one of them. The effects were not driven by inattentive, altruistic, or less-qualified teachers. Instead, choice overload and fatigue seem to have played a role. The intervention seemed to reduce the unequal distribution of qualified teachers across schools of different socioeconomic backgrounds. ***JEL classification*: I24, D91, I25**

Keywords: Order Effects, Teacher sorting, Satisficing

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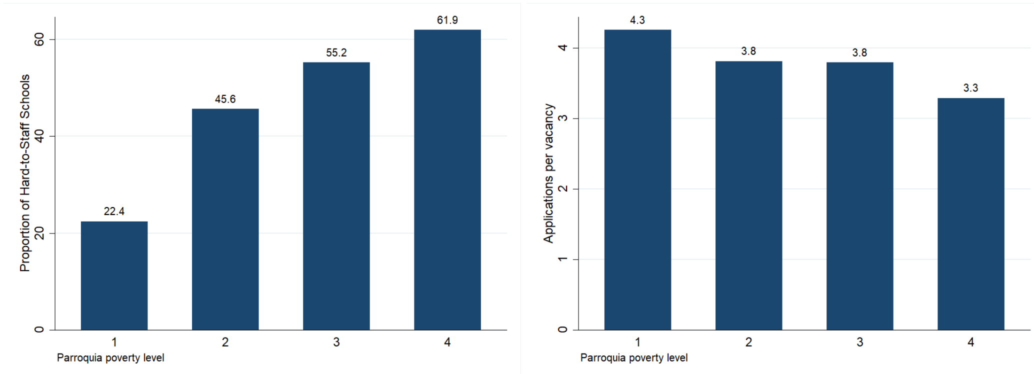
1 Introduction

Public education is crucial to providing equality of opportunity for students of different socioeconomic backgrounds. This goal is many times jeopardized by a widespread problem of many low and middle income countries: teacher sorting (Jackson 2009, Lankford et al. 2002, Boyd et al. 2013, Pop-Eleches and Urquiola 2013), which arises when low-income students are more likely to attend schools with less qualified teachers and understaffed schools. Teachers are a crucial input in the education production function (Rivkin et al. (2005)) and their impact is typically larger among lower-income students (Araujo et al. (2016)). The lack of high-quality instructors in more vulnerable areas has thus serious implications in terms of equity: teacher sorting exacerbates potential achievement gaps (Sass et al. 2012, Thiemann 2018).

In Ecuador (the context of this paper), for instance, slightly more than 22% of the schools located in districts in the top quartile of income in 2019 were categorized by the government as hard-to-staff, while almost 62% of the schools located in districts in the bottom quartile of income in 2019 were categorized by the government as hard-to-staff. Consistent with those numbers, in 2019 the number of applications per vacancy in a school located in the richest quartile of income is more than 30% higher than a school located in the poorest quartile of income (see Figure 1). Not surprisingly, hard-to-staff schools tend to have worse-performing and poorer students and higher teacher turnover.

Teacher sorting could also be detrimental from the teacher's point of view. The sorting of candidates increases the congestion of the assignment systems and, ultimately, risk their chances of securing a job vacancy. For instance, in the 2016 teacher selection process in Ecuador, 26 percent of teaching vacancies remained unfilled, while 56 percent of candidates were not assigned to a position. This is also a sub-optimal outcome for the government since many of the unassigned teachers will need to reapply in a future selection process.

Figure 1: School demand by District Poverty Level



(a) Proportion of Hard-to-staff Schools, by District Poverty Level

(b) Number of applications per vacancy, by District Poverty Level

Notes: Quartiles are based on average household income by *parroquia*, which is the smallest administrative unit in Ecuador. **a)** A school is categorized as hard-to-staff by the government based on three criteria: a) they had a high proportion of unfilled vacancies in prior teacher selection processes; b) they had a high share of teachers with temporary contracts; and c) they had poor infrastructure as well as low-performing teachers and students. **b)** The number of applicants per vacancy was calculated using data from the 2019 selection process, but keeping only the teachers that were not exposed to the treatment described in this paper.

With a few exceptions (Ajzenman et al. (2021)), policy responses to reduce teacher sorting typically focus on monetary incentives to teach in hard-to-staff positions. Not only in most cases the effects of these type of policies are null or mild (Glazerman et al. 2012; Rosa 2017; Elacqua et al. 2019; with the exception of Neilson et al. (2021)), they are also costly. In Peru, for instance, monetary incentives can represent up to a 40% increase of the base salary. (Bertoni et al. (2021)). In Chile, a teacher can earn up to 16% more if she works in a disadvantaged school, which is of a similar magnitude to wage bonuses in many U.S. school districts (Elacqua et al. (2022)).

In this paper, we evaluate the effects of a novel nationwide zero-cost behavioral intervention designed by the government of Ecuador to attract teacher candidates to permanent job vacancies in schools categorized by the government as hard-to-staff. The intervention was based on an insight from the psychology and behavioral economics literature, that so far has mostly been tested in relatively low-stakes decisions (Krosnick and Alwin (1987), Levav et al. (2010), Miller and Krosnick (1998); Koppell and Steen (2004); Augenblick and Nicholson (2016)): order effects.

The job application system randomly listed hard-to-staff schools first for teacher candidates in the treatment group. In the control group, schools were listed in alphabetical order. In both groups, hard-to-staff schools were labeled with an icon highlighting the potential for teachers to have a greater impact in these schools.[1] We find strong order effects. In our preferred specification, candidates in the treatment group were 5.2 percentage points (pp) more likely to rank a hard-to-staff school as their first choice (the mean of the control group was 39.8%). The proportion of hard-to-staff schools was 1.43 pp higher in the choice

[1] As explained below, schools labeled with an icon on the application platform suffer from greater teacher turnover and a shortage of certified teachers. In light of prior research (Aaronson et al. 2007; Araujo et al. 2016), these schools were identified as "higher social impact" institutions where teachers could have a greater effect on student learning.

sets of the treatment group (the mean of the control group was 42.7%), while the probability of being assigned by an algorithm (see Elacqua et al. (2020) for a description) to a teaching position in a hard-to-staff school was 3.4 pp higher in the treatment group (the mean of the control group was 26.9%). The results were not driven by lower-performing teachers. On the contrary, we find suggestive evidence of higher-performing teachers likely driving the effect. Importantly, teachers in the treatment group were significantly more likely to list schools with poorer and lower-performing students higher in their rank order. As a result, the number of teachers that were assigned to low-performing schools (bottom 25% of student performance) was 5.5 pp larger in the treatment group (mean of the control group: 34.6%).

Order effects could be driven or magnified by different factors (see, for example, Meredith and Salant (2013); Kim et al. (2015)). We explore and provide suggestive evidence related to several hypotheses. For instance, a factor that might explain our results is limited attention, as inattentive individuals may be more likely to rely on heuristics (Lacetera et al. (2012)). To test this hypothesis we conducted a Stroop-type test (MacLeod (1992)) to measure attention as an individual-specific trait. We found no heterogeneous effects by candidates' levels of inattention.

As Kim et al. (2015) suggests, lower cognitive ability may also make individuals more susceptible to being affected by the order of alternatives. The ability to interpret information, commit it to memory, retrieve it when necessary, and use it to form a judgement may be a plausible moderator of order effects. Using candidates' test scores on a qualifying exam as a proxy of cognitive ability, we show that this mechanism does not play an important role in our context. If any, results seemed to be driven by high-performing teachers.

Hard-to-staff schools in both arms (treatment and control) were labeled with an icon signaling that they were schools where teachers could have a higher social impact on students. A plausible hypothesis could be that prosocial/altruistic aspects of the candidates' identities were primed by the icons (Ajzenman et al. (2021)) in the treatment group, where schools with the icons were placed first. Using a measure of self-reported altruism inspired by the Global Preferences Survey (Falk et al. (2016)), we show that, although the unconditional probability of choosing a hard-to-staff school does indeed increase with teachers' level of altruism, the treatment effect was not higher among altruistic candidates.

We finally analyze how the complexity of the task (choosing schools from many options) could have played a role. The act of making a decision can be exhausting and effort consuming and thus individuals may rely on simpler heuristics to choose. In this context, making a decision when faced with many options may trigger what the literature on behavioral economics and psychology has termed a *choice overload* (Iyengar and Lepper (2000); Augenblick and Nicholson (2016)). If so, the larger the number of alternatives, the larger the order effects would be. This prediction would be also consistent with optimal stopping models (McCall (1965)). Whether it is because of whether because or less rational reasons, task complexity could have important consequences in decision making. Indeed, we find and document that the order effects are indeed larger when teachers have a larger set of vacancies to choose from.

Related to this last point, we also explore the possibility of teachers using simpler choice rules rather than strict rules of optimization (Salant (2011)) when deciding which schools they will include in their choice sets. Especially when the choosing task is relatively complex, teachers might not compare each alternative vacancy with every other one in every single dimension. Thus, they might end up not choosing the optimal set of schools (the ones that maximize their utility, considering every observable characteristic of each school), but a set of schools that are good enough in terms of some important characteristics.

To investigate this, we analyze if the treatment affected school choices in terms of the school characteristics typically identified by the literature as the most important determinants of teachers' preferences (Boyd et al. 2005; Reininger 2012; Rosa 2017; Bertoni et al. 2021): location (regularly considered the most important dimension of teachers' choices), student performance and school poverty level. Given that hard-to-staff schools are, on average, farther away (nearly 1.5 times more distant from candidates' place of residence than the other schools), we would expect a mechanical effect of the treatment on the average distance or commuting time to schools in teachers' choice set. Likewise, we would expect to find a mechanical effect on average students' performance and the poverty level of the areas where selected schools are located, considering that hard-to-staff schools are typically located in more disadvantaged communities. We find no effect of the treatment on the average distance or commuting times but, on the contrary, we show that teachers in the treatment group were more likely to list poorer and lower-performing schools first in their rank order.

We interpret the effect on these outcomes in light of Simon (1955)'s satisficing decision-making strategy. Teachers selected schools that may have not been the *optimal* choice in every single dimension, but that were satisfactory in terms of one particularly important variable: commuting time, a variable typically recognized as one of the most or even *the most* important dimension for teachers' preferences.

An important question from a policy perspective is if the intervention was successful in terms of reducing the number of unfilled vacancies. While our design does not allow us to directly answer this question, we present suggestive evidence that it did. We show that teachers in the treatment group substituted the most over-subscribed schools (top 25% in terms of applicants per vacancy) with the most under-subscribed schools (bottom 25% in terms of applicants per vacancy) in their choice sets. These results show that the distribution of applications to vacancies in the treatment group were more uniform which, in turn, should help to reduce congestion and the number of unfilled vacancies.

Finally, we present evidence suggesting that teachers in the treatment group were equally satisfied with their choices and allocations (the opposite could be detrimental for teachers and students). First, treated teachers were not more likely to change their original selection in a "validation phase" (a stage in which, up to four days after the application, teachers could change their choices at no cost). Second, teachers in the treatment group were equally prone to accept or decline the offer they received by the end of the process. Third, in 2022 (three years after the process), teachers in every arm were equally likely to remain in the position/school they were initially assigned.

Our paper relates to several strands of the literature in education and its intersection with behavioral economics. First, we contribute to the literature on policies that reduce teacher sorting and educational inequalities. An extensive body of work shows that low-income and low-performing students are more likely to attend hard-to-staff 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). Moreover, it is well documented that limited access to high-performing teachers has a negative impact on educational outcomes (Aaronson et al. 2007, Sass et al. 2012, Thiemann 2018). Meanwhile, the literature on strategies to mitigate teacher sorting is more scarce and, in most cases, focuses on monetary incentives, which have been found to have a small or non-significant impact on teachers' choices of disadvantaged schools (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). An exception is a recent paper by Ajzenman et al. (2021), which shows the results of an effective low-cost behavioral intervention to reduce teacher sorting in Peru. We add to these studies by showing how a novel behavioral intervention exploiting order effects can contribute to reducing teacher sorting at zero cost.

Second, our results align with a large body of literature on the contextual factors affecting decision making (Kamenica (2012)). Seemingly irrelevant factors that disproportionately influence an individual's choice may be interpreted as a signal of preference instability due to behavioral anomalies (Slovic (1995); Tversky and Kahneman (1974); Ariely et al. (2003)), or such factors may be rationalized by theories of contextual inference (Kamenica (2008)), or explained as the actions of expected utility maximizers who optimally decide to economize on the procedural costs associated with complex choices (Salant (2011)). Regardless of the interpretation, an extensive literature shows that contextual factors can affect a diverse range of outcomes, from voting choices (Berger et al. (2008); Ajzenman and Durante (2020)) to financial decisions (Barber and Odean (2008)) and consumers' product evaluations (Pope (2009)), among many others (see DellaVigna (2009) or Kamenica (2008)).

In particular, several papers have shown the existence of order effects, a specific type of contextual factor, in different situations. For instance, Miller and Krosnick (1998) were among the first to use real-world election data to document ballot order effects, where the order of candidate names on a ballot affects election results. This result has been at least partially confirmed in other settings (Koppell and Steen, 2004; Ho and Imai, 2006; Meredith and Salant, 2013; Marcinkiewicz, 2014). Order effects have also been documented in other contexts. Feenberg et al. (2017), for instance, find that the order in which NBER working papers are included in an email announcement influences the number of downloads and citations. Research in marketing and management sciences has shown that screen location (in online marketplaces or search engines, for example) is an important determinant of the number of clicks that the firm, product or ad will receive (see, for instance Agarwal et al. (2011); Ghose et al. (2014)). Likewise, Levav et al. (2010) show that order effects also influence customer purchases. While these findings are significant, most of the effects were tested in relatively low-stakes contexts.[2] Our paper contributes to this literature by showing the influence of order effects in a real-world, high-stakes decision-making context. Indeed, teachers' choices at the stage where the program operates are very high-stakes. This is because their decisions typically imply a long-term commitment to a specific job post. Although teachers can, in theory, move to a

[2]Although voting decisions are relevant, as they can have crucial socioeconomic consequences, the decision is arguably not very high-stakes from an individual point of view, since the impact of a vote is normally negligible.

different school in the subsequent years, this is uncommon because teachers seeking reassignment must make a special request (which is only available after a minimum of two years in their assigned school) and go through an additional application process. In our sample, for instance, about 8% of the teachers changed their jobs three years after the original assignment.

Our results have substantial policy implications. Addressing teacher sorting is an important aspect of promoting the equality of opportunity for students of different socioeconomic backgrounds. Teachers are a crucial input in the education production function as they have a significant effect on students' test scores (Rivkin et al. 2005; Kane and Staiger 2008), non-cognitive outcomes such as absenteeism and school suspension (Ladd and Sorensen 2017; Jackson 2018), as well as long-term outcomes, including college attendance, earnings, and teenage pregnancy (Chetty et al. 2014). Importantly, teachers' impact has been found to be greater among low-performing and low-income students (Aaronson et al. 2007; Araujo et al. 2016; Marotta 2019; Elacqua and Marotta 2020). Due to teacher sorting, disadvantaged schools tend to experience more severe shortages of teachers and often fail to attract higher-quality professionals (Sutcher et al. 2016; Dee and Goldhaber 2017; Bertoni et al. 2020). The concentration of teacher shortages and the lack of high-quality instructors in more disadvantaged schools thus has serious implications for educational inequality. Our paper shows that a zero-cost intervention can have a sizeable effect in terms of reducing teacher sorting (potentially reducing the number of unfilled vacancies), increasing the number of highly skilled teachers that apply to hard-to-staff schools, increasing the number of teachers that apply to schools with poorer and lower-performing students. In sum, not only treated teachers were more likely to apply to poorer and low-performing schools but also to vacancies that would have been typically less demanded (and thus, less likely to be filled). This low-cost policy could thus have an important long-term effect in terms of closing the student learning gaps and thus reducing inequality.

Section 2 provides background information on the teacher selection process in the Ecuadorian public school system. Section 3 presents the experiment, while Section 4 introduces the data and the empirical strategy. Section 5 presents the main results and interpretation. Finally, Section 6 concludes.

2 Institutional Context

2.1 Teacher selection in Ecuador

Since 2013, the Ministry of Education of Ecuador has selected teacher candidates and assigned them to school vacancies through a centralized teacher selection process known as *Quiero Ser Maestro* (QSM). This paper focuses on the sixth edition of the QSM program (QSM6), which was conducted throughout 2019 and included three phases: i) the eligibility phase, ii) the "merits and public examination" phase, and iii) the application phase. A more in-depth description of the QSM selection process is provided by Drouet Arias and Westh Olsen (2020).

In the eligibility phase, teacher candidates must pass a psychological test, comprised of personality and reasoning questions, and a knowledge test that is specific to the specialty area for which candidates are applying (e.g. general primary education). The tests in the eligibility phase are the same for all candidates across the country, and are designed and administered by Ecuador's National Institute of Educational Evaluation. To be eligible to participate in the second phase, candidates must have passed the psychological test scoring a minimum of 70 percent on the knowledge exam.

In the second phase ("merits and public examination"), candidates receive a final score based on their academic and professional credentials, their score on the knowledge test from phase one, and their performance on a mock class–candidates must have a minimum of 70 percent on the mock class to proceed with their application to job vacancies.

In the last phase, eligible candidates apply for school vacancies within their field on an online platform. The application phase for the QSM6 lasted one week. Candidates were able to apply to no more than five vacancies in any region of the country, which they ranked according to their preferences.[3] Finally, candidates were assigned to a vacancy by an algorithm with properties similar to a deferred acceptance algorithm (Elacqua et al. 2020), which takes into account candidates' scores in the second phase as well as their ranked choices for vacancies. After submitting their preferences in the application phase, candidates had the opportunity to revisit their original application during a four-day "validation phase."

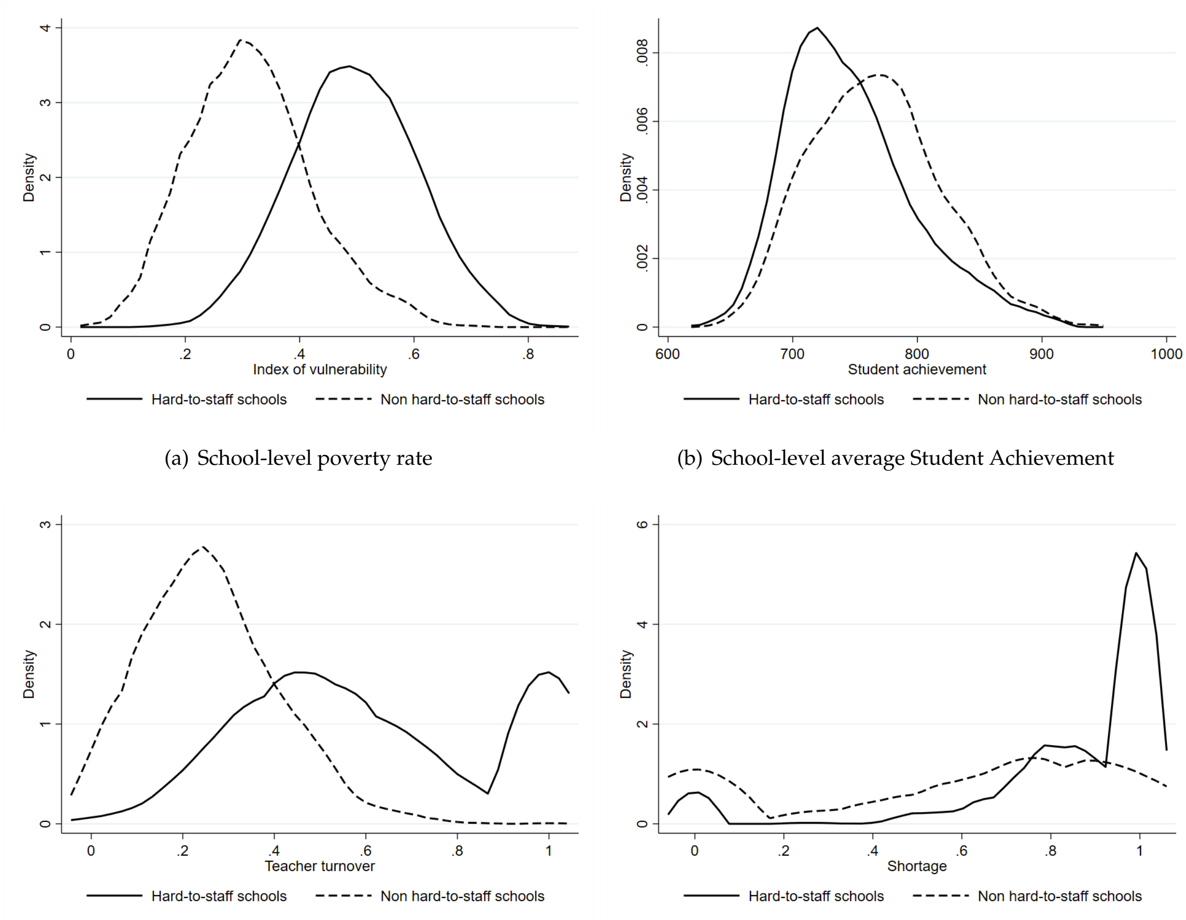
Teachers' choices are very high-stakes, because they typically imply a long-term commitment to a specific job post. Although they can, in theory, move to a different school in the subsequent years, this, in practice, is uncommon. Teachers seeking reassignment must make a special request (which is only available after a minimum of two years in their assigned school) and go through an additional application process. Moreover, as we show in the following sub-section, hard-to-staff-schools are significantly and systematically different to the rest of the schools in many important dimensions.

2.2 Hard-to-Staff Schools: how are they different from the rest?

As Figure 1 shows, poorer areas tend to have a much larger proportion of hard-to-staff schools in comparison to richer areas. Moreover, schools categorized as hard-to-staff perform typically worse in every observable dimension. As Figure 2 shows, hard-to-staff schools have typically poorer students, lower-performing students, higher rates of teacher turnover, and a higher level of teacher shortage.

[3]For details of each step of the application process on the online platform see https://educacion.gob.ec/quiero-ser-maestro-6/.

Figure 2: Hard-to-Staff versus Non-Hard-to-Staff schools



(a) School-level poverty rate

(b) School-level average Student Achievement

(c) School-level teacher turnover

(d) School-level Teacher Shortages

Notes: All graphs shows a kernel estimation of the distribution of different outcomes for hard-to-staff and non-hard-to-staff schools. Graph (a) Shows the distributions of the average student poverty rate by school. Graph (b) shows the distribution of the average student achievement levels. Graph (c) shows the distribution of the average teacher turnover. Graph (d) shows the distribution of the average teacher shortages by school.

2.3 Government efforts to improve the teacher selection process

Although *Quiero Ser Maestro* has improved transparency, Ecuador's teacher selection process still generates some inefficiencies and inequities. While some schools receive more applications than available vacancies, others struggle to attract applicants. As a result, a large proportion of teaching positions remain unfilled at the end of the process, and a number of candidates are unable to secure a job offer. These schools are typically in lower-income areas (see Figure 1).

Due to budget tightening, the government recently introduced low-cost interventions in its teacher selection process in an effort to reduce market congestion and attract candidates to hard-to-staff schools. For instance, they changed the algorithm so the entry test score is now weighted higher than teacher choices. Elacqua et al. (2020) show that the changes in the application rules and the adoption of a deferred acceptance algorithm led to a reduction in the number of vacant positions.

In addition to changing the algorithm, the government also made changes to the application platform of the QSM6 in order to encourage more teacher candidates to consider applying for job vacancies in hard-to-staff schools. The intervention we evaluate in this paper consisted of listing vacancies in hard-to-staff schools first on the application platform. Importantly, the information or incentives provided to teacher candidates remained the same. Moreover, after applications closed, the government allowed candidates to return to the platform for a four-day period to change their job choices, in case they regretted their original selection. Candidates in the treatment and control groups were equally likely to resubmit new choices during the validation phase, which suggests that teachers in the treatment group were not more likely to have regrets during the application process –these data are not shown but are available upon request.

3 Experimental Design

The experiment was implemented during the 2019 Ecuadorian national teacher selection process. The evaluation involved 18,133 candidates who successfully completed the "merits and public examination" phase of the teacher selection process. These candidates were high performers, given that the system is highly selective: in 2019, only 27% of the 129,114 candidates who registered for the teacher selection process passed the eligibility phase.

The original pre-registered experiment was designed to include all of the 27,207 candidates who passed the merits and public examination phase, and had two treatment and one control arm. Unfortunately, due to an implementation error, the platform was not properly programmed for one treatment arm. We thus excluded this arm and focus on the two arms that were properly implemented, which are described in detail below.

The remaining 18,133 teacher candidates were randomly assigned to two groups, stratified by district of residence: 9,074 were assigned to a control group, and 9,059 were assigned to the treatment group. The experiment was designed to ensure that teachers in both groups received exactly the same information–both groups had access to the same list of vacancies and relevant information about schools with job openings. The only difference between the treatment and control groups was that the system listed hard-to-staff schools first for candidates in the treatment group. In the control group, schools were displayed in alphabetical order.

After passing the qualifying exam, teachers had seven days to apply for a vacancy on the online platform. Once teachers entered the platform, they first had to select the area where they wished to search for vacancies (a province, city, and county). The system would then show all job vacancies available for the candidate's area of specialization. Candidates could select up to five vacancies of their choice in any geographic area of the country, including vacancies in different provinces, cities and counties (see Figure

A1 in the Appendix). The list of options showed basic information about each school: its location, the number of vacancies offered by the school, and the number of applicants for each vacancy at the time of the candidate signed in to the platform. Once teachers finished their selection, the system listed all the selected vacancies on a final screen (Figure A2 in the Appendix), allowing teachers to rank the vacancies in their preferred order. After completing the application, teachers were given an opportunity to re-enter the system and change their original selection within a four-day validation period.

As Figure A1 in the Appendix shows, schools labeled as "hard-to-staff" had an icon highlighting their potential for higher teacher impact, which was visible to candidates in the control and treatment arms. The Ministry of Education classified schools as "hard-to-staff" when i) they had a high proportion of unfilled vacancies in prior teacher selection processes; ii) they had a high share of teachers with temporary contracts; and iii) they had poor infrastructure as well as low-performing teachers and students. In Section 2.2 we analyze how hard-to-staff-schools differ from the rest of the schools). On the platform, these schools were indicated with an icon and were described with the following label: "Educational institutions where you [teacher candidate] can have a high social impact." (see the box located on top of Figure A1 in the Appendix). In light of prior research (Araujo et al. 2016; Marotta 2019), the Ministry of Education wanted to make candidates aware that students in these hard-to-staff schools could benefit more from having certified and higher-achieving teachers. In Figure A3 (Appendix), we provide an example of the screens shown to teachers in the treatment and control groups. The only difference between the two screens is the order in which the options are displayed.

Although the experiment was successfully implemented for the two arms described above, a large number of teachers were exposed to no variation in school type, making the treatment innocuous. Given that teachers first select location and specialty areas and then schools are displayed, it could happen (and it did), that a candidate had either a list of schools exclusively composed of hard-to-staff schools or exclusively composed of non-hard-to-staff schools.

For instance, in more than half of the combinations of county/area of specialization have no option of hard-to-staff schools (this typically happens in urban areas), and in more than 20% of the combinations of county/area of specialization have no option of non hard-to-staff-school (this typically happens in very remote rural areas). For teachers that selected vacancies in one of those combinations, the treatment was, in practice, not implemented. Regardless of being in a control o treatment group, if there are only hard-to-staff or only non-hard-to-staff options, the order would be the same and thus teachers would be exposed exactly to the same screen. It is important to emphasize that, given that county and area of specialization are selected before treatment, teachers could not avoid being or not being exposed to the treatment.

We thus run the main regressions using two samples. First, a restricted sample in which we exclude the observations for which the treatment could not be implemented due to lack of variability in the type of vacancy (because there were all hard-to-staff or all non-hard-to-staff options in a specific county/area of specialization). Second, the full sample, including teachers that were effectively not exposed to our treatment. The results of the full sample could be interpreted as an "Intention to Treat" effect. Although teachers could in principle not choose to be compliers or non-compliers (the random or non-random order was only visible after they selected the location and area of specialization), their location and area of specialization would make them belong to one of those groups. The results using the restricted sample could thus be interpreted as a "Local Average Treatment Effect", as it only operates with those that were effectively compliers. We believe the most interesting interpretation comes from the restricted sample, as it is focused on the decisions of individuals that had the option to choose between hard-to-staff and non-hard-to-staff schools and thus we refer to this as our preferred specification. Throughout the paper we call these samples "compliers" and "full" samples. In the main text we present the results using both samples for the main outcomes in the same table (teachers' choices and assignment). For the rest of the results, we show the "compliers" tables in the main document, while presenting their "full sample" equivalents in Appendix C.

The results show similar patterns using any of the samples. Moreover, in Table B1 (Appendix), we show that the probability of being included in the restricted sample does not correlate with the treatment, meaning that there is no selection induced by the sample restriction. Furthermore, in Section 4 we show that the final sample is balanced in all observable characteristics.

To better understand the mechanisms behind the order effects, we also administered an online survey to all teacher candidates in the evaluation sample. The online survey was sent by the Ministry of Education to candidates' email address throughout the month of September 2020. A total of 56% of all candidates in the study sample responded to the survey. In Section 4, we show that this sub-sample is balanced in all observable characteristics and is fairly representative of the full sample.

The survey included some questions that were relevant for our analysis, namely a measure of attention and a measure of altruism. Many other questions were related to the Ministry's evaluation of teachers' perception of the process, which did not provide any insights into our study. In Appendix E we present all the questions included in the survey in Spanish and English.

4 Empirical strategy, data and balance test

This paper uses administrative data from the 2019 public school teacher selection process in Ecuador. The data include candidates' socio-demographic characteristics (gender, marital status and ethnicity), years of teaching experience, total score on the merits and public examination phase, address of residence, area of specialization, ranked school choices, and, finally, the school where they were appointed to a position. For each school with a vacancy, the platform also provided the the school's address and whether it was classified as “hard to staff” by the Ministry of Education. Table B2 presents a descriptive summary of the 5,760 candidates in our final compliers sample, which excludes candidates whose options had no variability in type of vacancy. In the compliers sample, Hard-to-staff schools accounted for 36% of the vacancies in the QSM6 and 28% of candidates ended up being assigned to a hard-to-staff school. Candidates chose schools that are, on average, 34 km away from their home, with a commuting time of 64 minutes.[4] As these last variables are right-skewed, we use a logarithmic transformation of the measures of school distance and commuting time.

Table B3 compares candidates' characteristics in the compliers sample across treatment groups. As expected, because of the initial randomization, there are no significant differences between candidates in the treatment and control groups. The table also shows that these observable characteristics are balanced between the treatment and control arms in the survey sample as well. However, Table B4 indicates that candidates who completed the survey had lower test scores, were less likely to be single, and had fewer years of teaching experience. Although some of the differences between the people that answered the survey and those who opted out are significant, the magnitudes are quite small.

4.1 Empirical strategy

To measure the impact of changing the order in which teaching vacancies are listed on candidates' choices of certain schools, we run regressions of the following form:

$$y_i = \alpha T_i + \beta X_i + \delta_i + \varepsilon_i \tag{1}$$

where y_i is a "choice" or "assignment" outcome for teacher candidate i. T_i is a dummy in which "1" refers to candidates in the treatment group and "0" to candidates in the control group. X_i is a vector that includes a constant and candidate-level covariates (gender, marital status, ethnicity, years of experience and test scores). The model also includes fixed effects δ_i for candidates' district of residence, which is the level at which the randomization was stratified.

4.2 Main Measures

The first analysis includes three (pre-registered) outcomes relating to teacher choices: (i) **Whether their first choice was a hard-to-staff school**; (ii) **Whether their first two choices included a hard-to-staff school** (we focus on the top two choices because candidates are more likely to be assigned to one of their two most preferred schools (out of the teachers that are assigned to a school, 57% get their first preference and more than 76% their top two preferences); and (iii) **The percentage of hard-to-staff schools in candidates' choice sets**. Our **"assignment"** outcome captures whether candidate i was offered a teaching job at a hard-to-staff school. The outcome **Assigned to hard-to-staff school** takes a 1 if the candidate was assigned to work in a school categorized as "hard to staff" after the market cleared. In the pre-registered analysis plan, we also included an outcome defined as the absolute number (instead of the percentage) of hard-to-staff

[4] The average travel distance and commuting time varies by the candidates' ranked school choices: while their preferred choice of school is located, on average, 31 km away from their home, the distance to their least preferred school is about 56 km. This is not surprising considering that teachers usually prefer to teach close to their homes (Boyd et al. 2005; Reininger 2012; Rosa 2017; Bertoni et al. 2021).

schools in the choice set. This outcome was highly correlated with the percentage since most teachers applied to the maximum number of vacancies (5) permitted by the system. We therefore report the latter outcome in Table B5 in the Appendix.[5]

We also analyze the *average performance* of candidates who selected and were assigned to hard-to-staff schools. We use two indicators pre-registered in our plan, namely "average test score of candidates assigned to a hard-to-staff school" and "average test score of candidates who included a hard-to-staff school in their choice set." We also test whether the effect of the treatment on candidates' choices and assignments varies between **high-performing** (above the median test score) and low-performing teachers. That said, it should be noted that the definition of these groups is relative in that all teachers in the sample are qualified, having passed the highly competitive eligibility phase.

We also analyze four outcomes to explore potential teachers' regrets after submitting their list of preferences. First, after submitting their choices in the application phase, teachers had the opportunity to revisit their original application during a four-day "validation phase." We first define a dummy **"Changed in Validation"** that takes a one if teachers used the validation phase to make any change. Second, we analyze the outcome **"Declined an offer"**, which takes a 1 if a teacher *declined* an offer that was extended to them. Third, using administrative data from 2022 we create and analyze a dummy **"Moved in 2022"** that takes a one if, between 2019 and 2022, a teacher changed the school where she/he accepted an offer in 2019.

To explain the order effects, we estimate heterogeneous effects on a series of potential mediators. First, to examine whether order effects are potentially being driven by choice fatigue or cognitive **overload**, we investigate whether the treatment effect is larger when candidates have a wider range of vacancies to choose from. We estimate the number of options seen by a candidate on the platform based on the number of available vacancies in the counties in which her preferred schools were located. We only consider vacancies within candidates' area of specialization. If the candidate selected schools in more than one county, we average the number of available vacancies across all preferred counties.

We also examine whether the treatment effect was stronger among candidates with more limited attention. We conduct two different tests. First, we measured candidates' attention in the survey using a Stroop-type Color and Word test (MacLeod (1992)) with three questions. In each question, candidates were presented with around 11 names of colors, some of which had a mismatch between the name of the color and the font color used (e.g., the word "blue" printed in green). Candidates were asked to indicate the number of correct matches between the name of the color and the font color.[6] We estimate candidates' final **attention score** by assessing the number of correct matches and the time taken to answer each question. For each of the three questions, we have two variables: response accuracy ("1" if they solved the number of matches correctly and "0" otherwise) and time (number of seconds a respondent took to answer the question).

[5]In the same table we also report an outcome that was not pre-registered but that could be of interest: the number of choices included in teachers' choice sets (which, by design, is capped at five)

[6]We acknowledge that this test would have been better implemented in person with a larger number of questions. As a result, findings should be interpreted with caution. For logistical reasons and time constraints, we were only able to administer the survey online.

We calculated candidates' final score based on the main factor produced by a factor analysis of all six variables. On average, the response accuracy variables and the estimated factor had a positive correlation of 0.6, while the response time variables and the factor had a negative correlation of 0.5.

We also test whether order effects were larger among candidates with stronger altruistic preferences. Partially drawing on the measure of altruism experimentally validated by the Global Preferences Survey (Falk et al. (2016)), we asked candidates "Imagine the following hypothetical situation: Suppose that today, you unexpectedly receive $100. How much of this amount would you donate to a good cause? (Enter a quantity between 0 and 100)." Our indicator of **altruism** is the amount candidates were willing to donate.

To analyze treatment effects on the composition of schools included in teachers' choice sets we used three measures we constructed from administrative data. First, using data of candidates we calculated and analyzed the **Euclidean distance** and **travel time** from candidates' homes to each school in their choice set. Second, we used school-level data on average **students' scores** per school. Third, we used school-level data to calculate a measure of **poverty rate**.

Finally, we analyze several outcomes to explore the type of schools teachers' included in their choice sets in terms of vacancy-level demand. For this, we used teachers' choices from the control group (unaffected by the treatment). We first create an **index of demand** at the vacancy level, defined as the number of teachers (in the control group) that included that particular vacancy in their choice sets (that is, a measure of applications per vacancy). We then use this index to create several outcomes that we explain in detail in the corresponding section.

4.3 Multiple Hypothesis

We test if the main hypotheses survive the Holm (1979)'s correction for multiple comparisons in the main results of the paper. Holm (1979)'s method works as follows. Within a family of hypotheses, we want to test a set of K hypotheses. We first order the p-values from the smallest to the largest so that $\hat{p}_1 \leq \hat{p}_2 \leq \ldots \leq \hat{p}_K$. Then, we find the smallest p-value that satisfies $p_k > \frac{\alpha}{K+1-k}$, where α is the maximum accepted proportion of false positives (in our case 5%) and k is the p-value's index. This and all larger p-values are insignificant; all smaller p-values are significant. We indicate which p-values remain significant after this correction in each table. A family in our setting is defined as the mian four hypothesis pre-registered, including preferences and assignment: "Understaffed school as 1st choice", "At least one understaffed school among first 2 choices", "Percentage of understaffed schools in choice set", "Assigned to understaffed school" "Assigned to understaffed school".

5 Results and Interpretation

5.1 Teachers' choices and assignment

We first analyze the main outcomes related to choices and final allocation. Table 1 shows the main results with our preferred sample (compliers) and the full sample. All outcomes include the socio-demographic controls described in Section 4 as controls. The order effects are significant, robust, and large.

Focusing on the compliers sample, teachers in the treatment group were 5.2 pp more likely to rank a hard-to-staff school as their first choice (mean of the control group: 39.8%). They were 2.7 pp more likely to include at least one hard-to-staff school among their first two choices (mean of the control group: 61%) and the proportion of hard-to-staff schools included in the choice set of teachers in the treatment group was 1.3 percentage points (pp) higher (mean of the control group: 42.7%). The probability that they were assigned by the algorithm to a teaching position in a hard-to-staff school was 3.4 pp higher (mean of the control group: 26.9%). The effects are sizeable: given that approximately 27% of each cohort is assigned to a hard-to-staff-school (in the compliers sample), the intervention implies that the number of incoming teachers in the public system in a given year allocated to a hard-to-staff school could increase as much as 12.7% at zero cost.

5.2 Teachers' Quality

The final goal of the intervention was to improve the quantity but also the quality of teachers that work in hard-to-staff schools. Given that most of these schools are, by definition, under-staffed and that every teacher that participated in this program passed a selective qualifying exam, the goal would have been fulfilled even if the treatment affected only the relatively lower-performing teachers in the sample. Naturally, an even better result in terms of equity would be that the highest-performing teachers were affected by the treatment. In Table 2 we explore the teachers' quality dimension.

In the first two columns of the table, we show that treated candidates who applied to at least one hard-to-staff school and who were assigned to a hard-to-staff school tend to be higher performing. Although, these estimations are not precise enough to identify a significant outcome in the interactions, these findings suggest that the treatment probably did not induce only relatively low-performing candidates to apply to hard-to-staff schools.[7] Moreover, when analyzing heterogeneous order effects of our main outcomes by candidates' performance (columns 4 to 6), results suggest that the effect is hardly driven by low performers. Although the differences are not significant, the interaction with the "high-performing" dummy is always positive and, moreover, most of the results are only significant for high-performers.[8]

[7]These results focus on teachers that selected hard-to-staff schools and therefore should be interpreted cautiously, given that selection into these group was affected by the treatment.

[8]In Table B6 we run a similar regression but using "teacher experience" as the variable defining high and low performance. We find very similar results.

5.3 What explains order effects? Hypotheses and suggestive evidence

Since various factors may help explain or amplify the main results, we analyze potential mediators mentioned in the literature (e.g., Meredith and Salant 2013; Kim et al. 2015).

First, we study the potential mediator role of limited attention. If inattentive individuals rely more on heuristics (Lacetera et al. (2012)), we would expect them to be more affected by the treatment. In order to do this, we build an index of attention defined as the main factor produced by a factor analysis of all three "response accuracy" and "response time" variables of a Stroop-type test obtained from an on-line survey.[9] The higher the index, the higher the attentiveness of the candidate–that is, the greater their likelihood of answering the questions correctly in a shorter amount of time. In Table 3, we interact our attention index with the treatment and show that attention levels do not appear to be explaining our results. We found that the effects are not mediated by this variable.[10]

Second, we explore the possibility of individuals with lower cognitive ability driving the order effects, as suggested by Kim et al. (2015). As previously shown in Table 2, the treatment effect is not larger among candidates with lower test scores, which makes this possibility unlikely. [11]

Another plausible hypothesis is that candidates' altruistic identity was primed by the icons displayed alongside hard-to-staff schools (Ajzenman et al. (2021)), especially in the treatment group, where these schools appeared first. Using an indicator of altruism based on the measure proposed by the Global Preferences Survey (Falk et al. (2016)), we show that this does not seem to be the case. In Table 4, we show that the interaction between the treatment and a continuous measure of altruism (amount of money individuals are willing to donate to charity) is insignificant for all outcomes.

We finally explore the potential role of the complexity of the task (choosing schools from many options) on teachers choice. Making a decision when faced with many options and dimensions to consider might induce individuals to use heuristics. In particular, in this context it might trigger what the literature on behavioral economics and psychology has termed a *choice overload* (Iyengar and Lepper (2000); Augenblick and Nicholson (2016)). In our context, the effect should be increasing on the number of alternatives, a prediction would be also consistent with optimal stopping models (McCall (1965)).

To test this hypothesis, we estimate a model that interacts the treatment with the number of school vacancies seen by each candidate on the platform. In Table 5, we show that the interaction is positive and significant for most of the outcomes: facing more choices amplifies the order effects.

Also related to the effect of task complexity, Salant (2011) show that in many situations individuals use simpler choice rule rather than a strict rule of optimization even in high-stakes situations. In such cases,

[9] To confirm that the sample composed by individuals that answered the survey is fairly representative, in Table B7 we show our main results using the survey sample.

[10] We also tested an alternative measure of attention in which candidates were considered "attentive" if they answered the three questions correctly with a completion time above the median. Heterogeneous effects using this alternative measure of attention are provided in Table B8.

[11] While in Table 2 we use a dichotomous indicator of performance, we find similar results if we interact the treatment with a continuous measure of candidates' test scores, available upon request.

individuals may not compare each alternative with every other on multiple dimensions when deciding their preferred choices. In a context such as ours, for instance, teachers may not choose the *optimal* set of schools (the ones that maximize their utility, considering every observable characteristic of each schools), but a set of schools that are good enough in terms of some important dimensions.

To explore this possibility, we analyze the effect of the treatment on a set of school characteristics that the literature has identified as the most important determinants of teachers' preferences (Boyd et al. (2013)). We focus on three school characteristics often mentioned in the literature ((Boyd et al. 2005; Reininger 2012; Rosa 2017; Bertoni et al. 2021): location/commuting time (extensively documented in the literature as probably the most important determinant of teachers' preferences, see Bertoni et al. (2021)), students' performance and how vulnerable/poor is the area where the schools are located. Considering that hard-to-staff schools are, on average, farther away (and thus imply longer commuting times), they are poorer and they have worse-performing students, we would expect a mechanical effect of the treatment on each of these dimensions.

In Table 6 we estimate the treatment effects on commuting time, distance, student achievement and poverty level of the schools ranked in the first position of teachers' choice set. We show that the intervention did not impact the Euclidean distance between candidates' homes and their preferred schools or their estimated commuting time (first two columns). On the contrary, we show that the treatment motivated teachers to apply to lower-performing schools (third column) and poorer schools (fourth column). A plausible interpretation of this result is that teachers maximized only in terms of distance/commuting times (arguably the most important dimension for them). We interpret the effect on these outcomes in light of Simon (1955)'s satisficing decision-making strategy. Teachers selected schools that may have not been the *optimal* choice in every single dimension (for instance, students' performance and school area's poverty rate), but that were satisfactory in terms of one particularly important characteristic: commuting time.[12]

These results are also important from a policy point of view: although teachers were not willing to significantly increase their commuting times, on average, teachers' in the treatment group selected schools with relatively poorer and lower-performing students. In other words: the treatment persuaded teachers to apply to schools that were most in need. Indeed, a similar pattern is observed when analyzing allocation outcomes. In Table 7 we show that the proportion of teachers that were assigned to work in the lowest-performing schools (bottom 25% of students achievement) was significantly higher among treated teachers. In the same table we show the results for the poorest schools (top 25% of poverty rate), where we do not find any significant effect.[13]

In sum, our results suggest that, on the one hand, inattention, cognitive ability, and altruism priming are not relevant drivers of the order effects. On the other hand, factors associated with the complexity of choice see to mater more. In particular, choice overload does seem to have played a significant role.

[12]Many schools do not have data on student test scores or poverty rates. For that reason, those outcomes may have a missing value. That explains why the sample size is smaller.

[13]Many schools do not have data on student test scores or poverty rates. For that reason, those outcomes may have a missing value. That explains why the sample size is smaller. This could also explain why the effect on poverty is not significant.

These results are consistent with teachers using a procedurally simpler choice rule rather than a strict rule of optimization (Salant 2011), which would involve comparing each alternative with every other on multiple dimensions.

5.4 Should we expect a decrease in unfilled vacancies?

An important question from a policy point of view is if the intervention was successful in terms of reducing the number of unfilled vacancies. Unfortunately the design of our experiment does not allow us to identify a causal effect on the total number of unfilled vacancies for the entire system. However, we present a set of additional results that suggest that, indeed, the number of unfilled vacancies was likely reduced due to the intervention. First, we formally confirm that the treatment persuaded teachers to apply to the most (normally) under-subscribed/low demand schools: institutions where no teachers or very few candidates typically apply.

Using teachers' choices from the control group (unaffected by the treatment), we first create an index of demand at the vacancy level, defined as the number of teachers (in the control group) that included that particular vacancy in their choice sets. A vacancy in the control group has, on average 3.21 applicants (approximately 20% of the vacancies in this group received zero applications). We then use this index to create four different outcomes based on our main outcomes related to teachers' choices and allocation patterns. **Applicants per vacancy in first choice** takes the value of the applicants per vacancy indicator considering only the first-ranked vacancy in teachers' choice set; **Applicants per vacancy in first two choices** is calculated as the simple average of the applicants per vacancy indicator considering the vacancies ranked in the first and second positions of teachers' choice set; **Average applicants per vacancy in choice set** is calculated as the simple average of the applicants per vacancy indicator considering all the schools included in teachers' choice set; **Applicants per vacancy in assigned school** takes the value of the applicants per vacancy indicator considering only the school where teachers were assigned to work.[14]

In Table 8 we show that the average number of applicants per vacancy of teachers' first choice was 0.75 lower in the treatment group, while the average of teachers' first two preferred vacancies was 0.76 lower among treated teachers. On average, the vacancies included in teachers' choice set in the treatment group have significantly less demand than those of teachers in the control group: 0.72 less applicants per vacancy. Finally, the average number of applicants per vacancy was 0.70 lower in schools where teachers in the treatment group were assigned versus those where teachers in the control group were assigned. All of these point estimates are sizeable, considering a mean of 4.9 applicants per vacancy.

A subsequent relevant question is which are the schools that were substituted in the choice set of treated teachers. A problem could arise if, for instance, teachers substituted "almost" hard-to-staff schools (schools close to the margin of being hard-to-staff) with hard-to-staff schools. If that was the case, the treatment would probably trigger a replacement of old hard to staff with new hard to staff schools. On

[14] Naturally, the last indicator (assignment) should be interpreted cautiously because the probability of being assigned a school could be affected by the treatment.

the contrary, if teachers in the treatment group replaced the most over-subscribed schools with the most under-subscribed schools, we would expect to see a reduction in unfilled vacancies (as the distribution of applications would probably become more uniform).

To explore substitution patterns, in Table 9 we run the main regressions using different quartiles of the index of demand at the vacancy level. For instance, Q4 dummy takes a one if the school ranked as teachers' first option is in the top 25% of the applications per vacancy indicator conditional to a county and area of specialization, whereas the dummy Q1 takes a one if teachers most preferred option (ranked first) is in the bottom 25% of the applications per vacancy indicator conditional to a county and area of specialization. Teachers in the treatment group included on average an additional 12.9% of vacancies classified as Q1 ("bottom 25%") as their first option. They were vastly substituting vacancies in the "top 25%" (Q4): on average, vacancies in this quartile were reduced by almost 11.2%. There was some substitution in the mid-quartiles (between 25% and 75%. Q2 and Q3) but the effect there was notably smaller (close to 1% or 2%). The fact that the substitution in terms of teachers' choices happened from the typically most over-subscribed to the typically most under-subscribed schools suggests that the treatment, if implemented at scale, would help to reduce the number of unfilled vacancies.

5.5 Did teachers show regret?

A potential problem with interventions such as the one we evaluate in this paper is that, after teachers make their selections affected by the treatment, they regret their decisions. That would be detrimental from many points of view. From teachers' point of view, it could mean that they made a mistake and thus are less satisfied than they would have been if they had listed different schools in their rank order. From the system's point of view, teacher turnover could increase, with potential consequences in terms of student learning (Hanushek et al. (2016)). We use several measures to show this was not the case. There are several opportunities for teachers to change their mind and preferences, which we analyze in this section.

First, by design, teachers could change their original choices in the "validation phase" at no cost: after submitting their choices in the application phase, candidates had the opportunity to revisit their original application during a four-day period. In the first column of table 10 we show that teachers in the treatment group were not more likely to revisit their choices.

After the allocation process finished, teachers were offered a vacancy, which they could accept or decline. In the second column of table 10 we show that teachers in the treatment group were equally likely to decline or accept an offer in each arm.[15]

We followed teachers three years later and obtained data on their labor status in 2022. In the third column of Table 10 we show that the probability that a teacher is in a different job post in 2022 as in 2019 is not

[15] A natural caveat when analyzing this outcome is that it suffers from selection (since the probability of receiving an offer could be affected by the treatment.

significantly different in any arm. The same caveat as in the previous outcome is valid here: we are only observing teachers that got a job in 2019 (which could be partially affected by the treatment).

Overall, were were not able to identify, at any instance, signals of teachers regretting or being particularly dissatisfied with their jobs because of the treatment.

6 Discussion and Policy Implications

We show evidence of order effects impacting choices in a real-world, high-stakes environment, namely teachers' employment decisions. We also explore mechanisms and present suggestive evidence that the order effects were not mediated by cognitive skills, inattention, or altruism priming. Instead, we show that choice overload may have played a relevant role in explaining the order effects. Moreover, we find no effect of the treatment on the average distance/commuting times. This is intriguing, since the importance of commuting times for teachers' decision to seek employment in a given school is well documented (Boyd et al. 2005; Reininger 2012; Rosa 2017; Bertoni et al. 2021) and hard-to-staff schools are, on average, farther away. On the contrary, we find that teachers in the treatment group are more likely to apply to schools with lower-performing and poorer students. We interpret these results in light of Simon (1955)'s satisficing choice strategy. That is, teachers selected schools that may have not been the *optimal* choice, but which were satisfactory in terms of commuting time, a very important characteristic.

The intervention analyzed in this paper has important policy implications. Teacher sorting is a major concern for policymakers. Given that teachers have short- and long-term effects on students' educational outcomes, especially among the most vulnerable students (Aaronson et al. 2007; Araujo et al. 2016), teacher shortages and a preponderance of temporary and non-certified teachers in more disadvantaged schools can exacerbate socioeconomic inequalities in education. Moreover, the fact that most applications for teaching positions are concentrated among more advantaged schools is inefficient and reduces the chances of teacher candidates securing a job. Hard-to-staff schools are typically concentrated in areas where poorer students attend, they have lower-performing students, higher turnover and, by definition, higher levels of shortages.

Thus, the intervention was successful in dimensions that are crucial in terms of equity and the narrowing of student socioeconomic achievement gaps. Not only teachers in the treated group applied and were assigned to work in schools that are typically hard-to-staff, they also applied to schools with poorer and lower-performing students. Moreover, the fact that teachers in the treatment group substituted the most popular (over-subscribed) vacancies with the less popular (under-subscribed) vacancies suggests that the intervention, if applied at scale (that is, without treatment and control groups) would likely reduce the number of unfilled vacancies. Equally important, our results show that the treatment did not induce low-performing teachers to apply to hard-to-staff schools. Rather, our findings suggest that higher-quality teachers were those most impacted by the treatment. Finally, the fact that teachers did not seem to show any tangible sign of regret (in terms of their actions and self-reported satisfaction) is encouraging, not only in terms of teachers' welfare but also in relation to the long term effects of the intervention.

Besides its benefits, the intervention presented in this paper has an obvious advantage: it is free. A policy that has often been put forward to reduce sorting is monetary incentives for teachers willing to work in hard-to-staff schools. However, salary increases are typically very costly for governments. For example, one of the most successful cases in the literature, the Governor's Teaching Fellowship (GTF) in California, raised teachers' salaries in low-performing schools by 15%. However, despite increasing the likelihood that talented novice teachers would work in low-income schools by 28%, the program had to be discontinued due to high overhead costs. In Peru, a case probably more familiar to Ecuador, teachers' incentives could reach up to 40% of baseline salaries and the effects in terms of final allocation seem to be modest (Neilson et al. (2021)). In Chile, the bonus for a high-performing teacher to work in a disadvantaged school was equivalent to 16% of an average salary and evidence finds no effect in terms of attracting better teachers to these schools (Elacqua et al. (2022)). [16]. Budget constraints are of particular concern in developing countries and especially in Latin America, where government revenues have declined substantially over the past years. The zero-cost intervention evaluated in this paper therefore provides a timely contribution to mitigating teacher sorting and reducing market congestion in application processes.

[16]The incentive was effective at retaining high-performing teachers already working in disadvantaged schools, but did not accomplish the goal of attracting better teachers from more advantaged schools.

7 Tables and figures

Table 1: Treatment effects on choices and assignment

	Understaffed school at 1st choice	At least one understaffed school among first 2 choices	Percentage of understaffed schools in choice set	Assigned to understaffed school
Panel A: Compliers Sample				
Treatment	0.052***†	0.027**†	0.013**†	0.034***†
	(0.012)	(0.011)	(0.006)	(0.012)
Mean (Control group)	39.8	73.1	42.7	26.9
N	5760	5760	5760	5760
Panel B: Full Sample				
Treatment	0.026***†	0.023***†	0.006*	0.005
	(0.007)	(0.006)	(0.003)	(0.005)
Mean (Control group)	27.7	43.2	29.8	14.3
N	18133	18133	18133	18133

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Understaffed school at 1st choice"**: takes a 1 if the first choice in a teacher's choice set is an understaffed school. **"At least one understaffed school among first 2 choices"**: takes a 1 if the first and/or second choices in a teacher's choice set are understaffed schools. **"Percentage of understaffed schools in choice set"**: number of understaffed choices divided by the total number of choices of a given teacher. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. The description of the compliers sample is in Section 3. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.
Multiple-hypothesis testing: we compute Holm (1979)'s FWER correction at the 5% significance level. † indicates that coefficient remains significant after the correction. See 4.3 for details on the method.

Table 2: Treatment effects on teacher quality

	Test scores (selected at least one understaffed school)	Test scores (assigned to understaffed schools)	Understaffed school at 1st choice	At least one understaffed school among first 2 choices	Percentage of understaffed schools in choice set	Assigned to understaffed school
Compliers Sample						
Treatment (I)	0.023	0.251	0.036*	0.019	0.008	0.022*
	(0.279)	(0.349)	(0.019)	(0.008)	(0.008)	(0.012)
High-performing (II)			-0.041**	-0.033*	-0.018**	0.144***
			(0.017)	(0.017)	(0.009)	(0.019)
Treatment*High-performing (III)			0.032	0.017	0.011	0.024
			(0.026)	(0.027)	(0.012)	(0.024)
(I) + (III)			0.068***	0.036**	0.018**	0.046**
			(0.018)	(0.018)	(0.009)	(0.021)
Mean (Control group)	67.7	70.8	39.8	61.2	42.7	26.9
N	5005	1637	5760	5760	5760	5760

Notes: Robust standard errors in parentheses; * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. **"Test scores (selected at least one understaffed school)"**: teachers' average test score on the qualifying exam, considering only teachers that included at least one understaffed school in their choice set. **"Test scores (assigned to understaffed school)"**: teachers' average test score on the qualifying exam, considering only teachers that were assigned to an understaffed school in their choice set. **"Understaffed school at 1st choice"**: takes a 1 if the first choice in a teacher's choice set is an understaffed school. **"At least one understaffed school among first 2 choices"**: takes a one if the first and/or second choices in a teacher's choice set are understaffed schools. **"Percentage of understaffed schools in choice set"**: number of understaffed schools in a teacher's choice set divided by the total number of schools in the choice set. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. The description of the compliers sample is in Section 3. **Low performing"**: takes a 1 if a teacher's test score on the qualifying exam is below the median. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table 3: Heterogeneous effects - Attention

	Understaffed school at 1st choice	At least one understaffed school among first 2 choices	Percentage of understaffed schools in choice set	Assigned to understaffed school
Compliers Sample				
Treatment	0.045**	0.025	0.005	0.034*
	(0.020)	(0.018)	(0.011)	(0.019)
Attentiveness	-0.015	-0.003	-0.006	0.009
	(0.011)	(0.013)	(0.006)	(0.011)
Treatment*Attentiveness	0.001	0.017	0.000	0.003
	(0.018)	(0.019)	(0.009)	(0.015)
Mean (Control group)	40.1	60.7	42.3	23.2
N	2617	2617	2617	2617

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Understaffed school at 1st choice"**: takes a 1 if the first choice in a teacher's choice set is an understaffed school. **"At least one understaffed school among first 2 choices"**: takes a 1 if the first and/or second choices in a teacher's choice set are understaffed schools. **"Percentage of understaffed schools in choice set"**: number of understaffed schools as a percentage of a teacher's total number of choices. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. The description of the compliers sample is in Section 3. **"Attentiveness"**: a continuous measure based on the main factor produced by a factor analysis of all six variables associated with the Stroop-type Color and Word test. The higher the index, the greater the number of correct responses given by the teacher on a shorter amount of time. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table 4: Heterogeneous effects - Altruism

	Understaffed school at 1st choice	At least one understaffed school among first 2 choices	Percentage of understaffed schools in choice set	Assigned to understaffed school
Compliers Sample				
Treatment	0.053***	0.032*	0.012	0.040**
	(0.019)	(0.017)	(0.010)	(0.017)
Altruist	0.000	-0.001	-0.000*	-0.001
	(0.000)	(0.001)	(0.000)	(0.000)
Treatment*Altruist	0.000	0.000	0.000	-0.000
	(0.001)	(0.001)	(0.000)	(0.001)
Mean (Control group)	40.1	60.7	42.2	22.1
N	2697	2697	2697	2697

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Understaffed school at 1st choice"**: takes a 1 if the first choice in a teacher's choice set is an understaffed school. **"At least one understaffed school among first 2 choices"**: takes a 1 if the first and/or second choices in a teacher's choice set are understaffed schools. **"Percentage of understaffed schools in choice set"**: number of understaffed schools as a percentage of a teacher's total number of choices. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. The description of the compliers sample is in Section 3. **"Altruism"**: an amount between 0 and $100 that the teacher was willing to donate to a good cause. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table 5: Heterogeneous effects - Overload

	Understaffed school at 1st choice	At least one understaffed school among first 2 choices	Percentage of understaffed schools in choice set	Assigned to understaffed school
Compliers Sample				
Treatment	0.053***	0.028**	0.013**	0.033***
	(0.013)	(0.011)	(0.006)	(0.012)
Vacancies	-0.002***	-0.003***	-0.002***	0.001*
	(0.001)	(0.001)	(0.000)	(0.001)
Treatment*Vacancies	0.001**	0.002**	0.001***	0.001
	(0.001)	(0.001)	(0.000)	(0.000)
Mean (Control group)	39.7	61.1	42.7	27.0
N	18133	18133	18133	18133

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Understaffed school at 1st choice"**: takes a 1 if the first choice in a teacher's choice set is an understaffed school. **"At least one understaffed school among first 2 choices"**: takes a 1 if the first and/or second choices in a teacher's choice set are understaffed schools. **"Percentage of understaffed schools in choice set"**: number of understaffed schools as a percentage of a teacher's total number of choices. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. The description of the compliers sample is in Section 3. **"Vacancies"**: number of vacancies seen by the teacher on the job application platform. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table 6: Treatment effect on school characteristics of teachers' first choice

	Log time-traveled to schooled ranked first	Log geodesic distance (km) to school ranked first	Student scores of school ranked first	Poverty rate of school ranked first
Compliers Sample				
Treatment	-0.012	-0.015	-3.17**	0.040*
	(0.026)	(0.030)	(1.46)	(0.006)
Mean (Control group)	7.75	6.90	747	0.27
N	5,755	5,758	2,488	4,794

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Log time-traveled to schooled ranked first"**: log of commuting time by car from a teacher's home to the school that was ranked first in their choice set (calculated by Google Maps). **"Log geodesic distance (km) to school ranked first"**: log of geodesic distance from a teacher's home to the school ranked first in their choice set (calculated by Google Maps). **"Student scores of school ranked first"**: Average student performance of the school that was ranked first in teachers' choice set. Schools with missing data are excluded. **"Poverty rate of school ranked first"**: Average poverty rate of the school that was ranked first in teachers' choice set. Schools with missing data are excluded. All models includes the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table 7: Assignment to schools with poorer and lower-performing students

	Lower-Performing	Poorer
Compliers Sample		
Treatment	0.055**	-0.008
	(0.021)	(0.012)
Mean (Control group)	0.35	0.20
N	1,497	2,967

Note: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Lowest performing"**: takes a 1 if the teacher was assigned to a school that belongs to the bottom 25% in terms of average student performance. Schools with missing data are excluded. **"Poorest"**: takes a 1 if the teacher was assigned to a school that belongs to the top 25% in terms of average poverty level. Schools with missing data are excluded. The description of the compliers sample is in Section 3. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table 8: Effect on Applicants per vacancy

	Applicants per vacancy of 1st choice	Applicants per vacancy of first two choices	Applicants per vacancy in choice set	Applicants per vacancy in assigned school
Compliers Sample				
Treatment	-0.751***	-0.762***	-0.724***	-0.706***
	(0.118)	(0.107)	(0.096)	(0.062)
Mean (Control group)	4.9	5.3	5.2	2.9
N	5,760	5,760	5,760	3,616

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Applicants per vacancy of 1st choice"**: applicants per vacancy to the school ranked first in teachers' choice set **"Applicants per vacancy of first two choices"**: average applicants per vacancy to the schools ranked first or second in teachers' choice set. **"Applicants per vacancy in choice set"**: average applicants per vacancy to the schools included in teachers' choice set. **"Applicants per vacancy in assigned school"**: applicants per vacancy to the school where teachers were assigned. The description of the compliers sample is in Section 3. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table 9: Effect on Applicants per vacancy: quartiles

	Bottom 25%	25-50%	50-75%	Top 25%
Compliers Sample				
Treatment	0.129***	-0.003	-0.014*	-0.112***
	(0.013)	(0.015)	(0.008)	(0.012)
Mean (Control group)	19.7	26.5	10.9	42.9
N	5,760	5,760	5,760	5,760

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Bottom 25%"**: applicants per vacancy of the school ranked first in teachers' choice set belongs to the bottom 25% of applicants per vacancy in its county and area of specialty.**"25-50%"**: applicants per vacancy of the school ranked first in teachers' choice set belongs to the percentiles 25 to 50 applicants per vacancy in its county and area of specialty. **"50-75%"**: applicants per vacancy of the school ranked first in teachers' choice set belongs to the percentiles 50 to 75 applicants per vacancy in its county and area of specialty. **"top 50%"**: applicants per vacancy of the school ranked first in teachers' choice set belongs to the top 25% of applicants per vacancy in its county and area of specialty. The description of the compliers sample is in Section 3. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table 10: Measures of regret

	Changed in validation	Declined an offer	Moved in 2022
Compliers Sample			
Treatment	0	-0.004	0.01
	(0.012)	(0.004)	(0.009)
Mean (Control group)	0.3	0.016	0.082
N	5,760	3,616	3,616

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Changed in validation"**: takes a 1 if teacher changed her preferences in the validation phase.**"Declined an offer"**: takes a one if a teacher declined an offered (considers only teachers that were offered a position). **"Moved in 2022"**: takes a 1 if a teacher is not working in 2022 in the same school that was assigned to him/her in 2019 (considers only teachers that were offered a position in 2019). The description of the compliers sample is in Section 3. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

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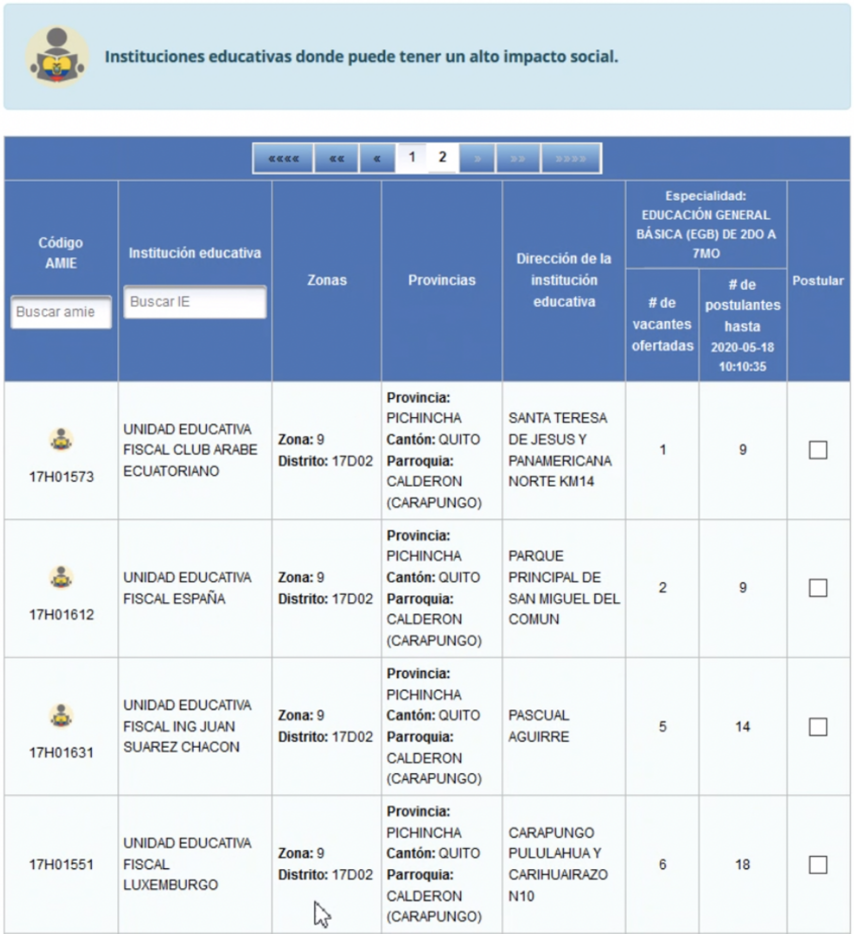
A Appendix A (Figures)

Figure A1: Platform Screenshot - School list



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17H01573	UNIDAD EDUCATIVA FISCAL CLUB ARABE ECUATORIANO	**Zona:** 9 **Distrito:** 17D02	**Provincia:** PICHINCHA **Cantón:** QUITO **Parroquia:** CALDERON (CARAPUNGO)	SANTA TERESA DE JESUS Y PANAMERICANA NORTE KM14	1	9	☐
17H01612	UNIDAD EDUCATIVA FISCAL ESPAÑA	**Zona:** 9 **Distrito:** 17D02	**Provincia:** PICHINCHA **Cantón:** QUITO **Parroquia:** CALDERON (CARAPUNGO)	PARQUE PRINCIPAL DE SAN MIGUEL DEL COMUN	2	9	☐
17H01631	UNIDAD EDUCATIVA FISCAL ING JUAN SUAREZ CHACON	**Zona:** 9 **Distrito:** 17D02	**Provincia:** PICHINCHA **Cantón:** QUITO **Parroquia:** CALDERON (CARAPUNGO)	PASCUAL AGUIRRE	5	14	☐
17H01551	UNIDAD EDUCATIVA FISCAL LUXEMBURGO	**Zona:** 9 **Distrito:** 17D02	**Provincia:** PICHINCHA **Cantón:** QUITO **Parroquia:** CALDERON (CARAPUNGO)	CARAPUNGO PULULAHUA Y CARIHUAIRAZO N10	6	18	☐

Source: Ministry of Education, Ecuador

Figure A2: Platform Screenshot - Final Screen

Source: Ministry of Education, Ecuador

Figure A3: Platform Screenshot - Control versus Treatment

(a) Control (b) Treatment

B Appendix B (Tables)

Table B1: Random selection into final sample

	Probability of being included in the compliers sample	
	(1)	(2)
Treatment	-0.005	-0.005
	(0.007)	(0.007)
Controls	No	Yes
Mean (Control group)	32	32
N	18133	18133

Note: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01.The outcome "Probability of being included in the compliers sample" takes a 1 if the observation is included in the final sample we use for the main analysis. The description of the final sample is in Section 3. Model (1) does not include controls. Model (2) includes the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table B2: Summary statistics

	Obs		Mean		Std. Dev		Min		Max	
Sample	Full	Compliers	Full	Compliers	Full	Compliers	Full	Compliers	Full	Compliers
Candidate's attributes										
Female	18,133	5760	0.77	0.71	0.42	0.45	0	0	1	1
Single	18,133	5760	0.51	0.54	0.50	0.50	0	0	1	1
Ethnic minority	18,121	5754	0.99	0.11	0.30	0.31	0	0	1	1
Years of experience	18,120	5757	3.36	3.74	3.27	3.27	0	0	10	10
Test score	18,133	5760	66.24	67.70	9.94	9.42	42.51	44.26	94.60	94.60
Outcomes										
Percentage of understaffed schools in choice set	18,133	5760	0.30	0.43	0.28	0.27	0	1		
Ranked an understaffed school in their first choice	18,133	5760	0.29	0.42	0.45	0.49	0	0	1	1
At least one understaffed school among first two choices	18,133	5760	0.44	0.62	0.50	0.48	0	0	1	1
At least one understaffed school among first three choices	18,133	5760	0.54	0.74	0.50	0.44	0	0	1	1
Assigned to an understaffed school	18,133	5760	0.15	0.29	0.35	0.45	0	0	1	1
Accepted offer in understaffed school	18,133	5760	0.14	0.28	0.35	0.45	0	0	1	1
Average commuting time to schools in the choice set	18,110	5755	80.57	64.19	122.68	106.55	0.47	0.57	1889.45	1889.45
Average distance to schools in the choice set	18,120	5758	43.41	34.26	68.26	59.44	0.10	0.18	1167.54	998.92

Table B3: Balance tests

Variable	Final sample						Survey sample					
	Control		Treatment		Difference		Control		Treatment		Difference	
Sample	Full	Compliers	Full	Compliers	Full	Compliers	Full	Compliers	Full	Compliers	Full	Compliers
Female	0.715	0.767	0.718	0.765	0.003	-0.002	0.706	0.757	0.715	0.762	0.009	0.006
Test scores	66.247	67.725	66.238	67.681	-0.009	-0.044	65.744	66.844	65.720	66.994	-0.023	0.150
Single	0.507	0.533	0.517	0.548	0.010	0.016	0.502	0.521	0.507	0.531	0.005	0.010
Years of experience	3.371	3.739	3.346	3.742	-0.025	0.003	3.385	3.676	3.299	3.666	-0.086	-0.010
Ethnic minority	0.096	0.111	0.101	0.102	0.005	-0.009	0.094	0.108	0.097	0.100	0.003	-0.009
Observations	9,074	2,903	9,059	2,857	18,133	5,760	5,093	1,471	5,078	1,485	10,131	2,956

Notes: * p < 0.10 ** p < 0.05 *** p < 0.01.

Table B4: Representativeness of survey sample

Variable	Survey participants					
	Opted out		Opted in		Difference	
Sample	Full	Compliers	Full	Compliers	Full	Compliers
Female	0.723	0.774	0.711	0.759	-0.012*	-0.014
Test scores	66.895	68.530	65.732	66.919	-1.163***	-1.611***
Single	0.523	0.555	0.504	0.526	-0.019**	-0.029**
Years of experience	3.380	3.814	3.342	3.671	-0.038	-0.143*
Ethnic minority	0.103	0.110	0.095	0.104	-0.008*	-0.006
Observations	7,962	2,804	10,171	2,956	18,133	5,760

Notes: * p < 0.10 ** p < 0.05 *** p < 0.01.

Table B5: Treatment effect on number of understaffed vacancies

	Number of understaffed schools in choice set
Compliers Sample	
Treatment	0.064**
	(0.031)
Mean (Control group)	2.1
N	5760

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Number of understaffed schools in choice"**: number of understaffed schools in a teacher's choice set. The description of the compliers sample is in Section 3. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table B6: Heterogeneous effects - Teacher experience

	Understaffed school at 1st choice	At least one understaffed school among first 2 choices	Percentage of understaffed schools in choice set	Assigned to understaffed school
Compliers Sample				
Treatment (I)	0.044**	0.019	0.012	0.021
	(0.018)	(0.017)	(0.009)	(0.014)
Non-rookie (II)	-0.016	-0.014	-0.015	-0.000
	(0.018)	(0.018)	(0.009)	(0.018)
Treatment*Non-rookie (III)	0.015	0.014	0.002	0.022
	(0.026)	(0.019)	(0.011)	(0.018)
(I) + (III)	0.059**	0.033**	0.014*	0.043**
	(0.017)	(0.013)	(0.007)	(0.016)
Mean (Control group)	39.8	61.2	42.7	26.9
N	5757	5757	5757	5757

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Understaffed school at 1st choice"**: takes a 1 if the first choice in a teacher's choice set is an understaffed school. **"At least one understaffed school among first 2 choices"**: takes a 1 if the first and/or second choices in a teacher's choice set are understaffed schools. **"At least one understaffed school among first 3 choices"**: takes a 1 if at least one of the first three choices in a teacher's choice set is an understaffed school. **"Percentage of understaffed schools in choice set"**: number of understaffed schools as a percentage of the total number of choices of a given teacher. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. The description of the compliers sample is in Section 3. **"Non-Rookie"** takes a 1 if a teacher has les than 3 years of experience. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table B7: Treatment effects on choices and assignment: Survey sample

	Understaffed school at 1st choice	At least one understaffed school among first 2 choices	Percentage of understaffed schools in choice set	Assigned to understaffed school
Compliers Sample				
Treatment	0.047**	0.036**	0.014	0.039**
	(0.018)	(0.016)	(0.010)	(0.016)
Mean (Control group)	40.5	60.5	42.2	22.9
N	2956	2956	2956	2956

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Understaffed school at 1st choice"**: takes a 1 if the first choice in a teacher's choice set is an understaffed school. **"At least one understaffed school among first 2 choices"**: takes a 1 if the first and/or second choices in a teacher's choice set are understaffed schools. **"Percentage of understaffed schools in choice set"**: number of understaffed schools as a percentage of the total number of choices of a given teacher. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. The description of the compliers sample is in Section 3. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table B8: Heterogeneous effects - Alternative measure of attention

	Understaffed school at 1st choice	At least one understaffed school among first 2 choices	Percentage of understaffed schools in choice set	Assigned to understaffed school
Compliers Sample				
Treatment	0.035*	0.018	0.007	0.024
	(0.020)	(0.019)	(0.011)	(0.019)
Attentive	-0.040	-0.033	-0.010	-0.022
	(0.036)	(0.036)	(0.018)	(0.033)
Treatment*Attentive	0.080	0.057	-0.010	0.070
	(0.049)	(0.039)	(0.023)	(0.047)
Mean (Control group)	40	60.7	42.3	23.2
N	2617	2617	2617	2617

Notes: Robust standard errors in parentheses; * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. **"Understaffed school at 1st choice"**: takes a 1 if the first choice in a teacher's choice set is an understaffed school. **"At least one understaffed school among first 2 choices"**: takes a 1 if the first and/or second choices in a teacher's choice set are understaffed schools. **"Percentage of understaffed schools in choice set"**: number of understaffed schools as a percentage of a teacher's total number of choices. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. The description of the compliers sample is in Section 3. **"Attentive"**: takes a 1 if teacher answered the three questions in the Stroop-type Color and Word test correctly with a completion time above the median. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

C Appendix C (Main Tables using the Full Sample)

Table C1: Treatment effects on teacher quality: Full sample

	Test scores (selected at least one understaffed school)	Test scores (assigned to understaffed schools)	Understaffed school at 1st choice	At least one understaffed school among first 2 choices	Percentage of understaffed schools in choice set	Assigned to understaffed school
Full Sample						
Treatment (I)	0.090	0.280	0.019**	0.017**	-0.001	-0.004
	(0.187)	(0.322)	(0.009)	(0.019)	(0.005)	(0.005)
High-performing (II)			-0.025**	-0.024*	-0.017***	0.084***
			(0.010)	(0.010)	(0.006)	(0.010)
Treatment*High-performing (III)			0.013	0.012	0.013*	0.019*
			(0.015)	(0.015)	(0.007)	(0.011)
(I) + (III)			0.033**	0.029**	0.013**	0.015
			(0.011)	(0.011)	(0.005)	(0.009)
Mean (Control group)	66.2	70.7	27.7	43.3	29.8	14.3
N	12261	2601	18133	18133	18133	18133

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Test scores (selected at least one understaffed school)"**: teachers' average test score on the qualifying exam, considering only teachers that included at least one understaffed school in their choice set. **"Test scores (assigned to understaffed school)"**: teachers' average test score on the qualifying exam, considering only teachers that were assigned to an understaffed school in their choice set. **"Understaffed school at 1st choice"**: takes a 1 if the first choice in a teacher's choice set is an understaffed school. **"At least one understaffed school among first 2 choices"**: takes a one if the first and/or second choices in a teacher's choice set are understaffed schools. **"Percentage of understaffed schools in choice set"**: number of understaffed schools in a teacher's choice set divided by the total number of schools in the choice set. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. The description of the compliers sample is in Section 3. **Low performing"**: takes a 1 if a teacher's test score on the qualifying exam is below the median. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table C2: Heterogeneous effects - Attention: Full sample

	Understaffed school at 1st choice	At least one understaffed school among first 2 choices	Percentage of understaffed schools in choice set	Assigned to understaffed school
Full Sample				
Treatment	0.009	0.014*	-0.002	0.006
	(0.009)	(0.008)	(0.005)	(0.007)
Attentiveness	-0.020***	-0.019***	-0.015***	-0.000
	(0.006)	(0.007)	(0.004)	(0.005)
Treatment*Attentiveness	0.004	0.013	0.006	0.006
	(0.008)	(0.008)	(0.004)	(0.006)
Mean (Control group)	27.7	42.4	29.2	11.8
N	9173	9173	9173	9173

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Understaffed school at 1st choice"**: takes a 1 if the first choice in a teacher's choice set is an understaffed school. **"At least one understaffed school among first 2 choices"**: takes a 1 if the first and/or second choices in a teacher's choice set are understaffed schools. **"Percentage of understaffed schools in choice set"**: number of understaffed schools as a percentage of a teacher's total number of choices. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. The description of the compliers sample is in Section 3. **"Attentiveness"**: a continuous measure based on the main factor produced by a factor analysis of all six variables associated with the Stroop-type Color and Word test. The higher the index, the greater the number of correct responses given by the teacher on a shorter amount of time. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table C3: Heterogeneous effects - Altruism: Full sample

	Understaffed school at 1st choice	At least one understaffed school among first 2 choices	Percentage of understaffed schools in choice set	Assigned to understaffed school
Full Sample				
Treatment	0.014	0.016**	-0.000	0.009
	(0.009)	(0.008)	(0.005)	(0.007)
Altruist	0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Treatment*Altruist	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Mean (Control group)	27.6	42.5	29.3	11.3
N	9342	9342	9342	9342

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Understaffed school at 1st choice"**: takes a 1 if the first choice in a teacher's choice set is an understaffed school. **"At least one understaffed school among first 2 choices"**: takes a 1 if the first and/or second choices in a teacher's choice set are understaffed schools. **"Percentage of understaffed schools in choice set"**: number of understaffed schools as a percentage of a teacher's total number of choices. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. The description of the compliers sample is in Section 3. **"Altruism"**: an amount between 0 and $100 that the teacher was willing to donate to a good cause. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table C4: Heterogeneous effects - Overload: Full sample

	Understaffed school at 1st choice	At least one understaffed school among first 2 choices	Percentage of understaffed schools in choice set	Assigned to understaffed school
Full Sample				
Treatment	0.026***	0.023***	0.006**	0.004
	(0.007)	(0.006)	(0.003)	(0.005)
Vacancies	-0.001	-0.001	-0.001	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Treatment*Vacancies	0.002***	0.002**	0.001***	0.001***
	(0.001)	(0.001)	(0.000)	(0.000)
Mean (Control group)	27.7	43.2	29.8	14.3
N	18133	18133	18133	18133

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Understaffed school at 1st choice"**: takes a 1 if the first choice in a teacher's choice set is an understaffed school. **"At least one understaffed school among first 2 choices"**: takes a 1 if the first and/or second choices in a teacher's choice set are understaffed schools. **"Percentage of understaffed schools in choice set"**: number of understaffed schools as a percentage of a teacher's total number of choices. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. The description of the compliers sample is in Section 3. **"Vacancies"**: number of vacancies seen by the teacher on the job application platform. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table C5: Treatment effect on school characteristics of teachers' first choice: Full sample

	Log time-traveled to schooled ranked first	Log geodesic distance (km) to school ranked first	Student scores of school ranked first	Poverty rate of school ranked first
Full Sample				
Treatment	0.018	0.019	-1.3*	0.036**
	(0.013)	(0.016)	(0.074)	(0.017)
Mean (Control group)	8.00	7.19	768	-0.15
N	18,110	18,120	7,904	

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Log time-traveled to schooled ranked first"**: log of commuting time by car from a teacher's home to the school that was ranked first in their choice set (calculated by Google Maps). **"Log geodesic distance (km) to school ranked first"**: log of geodesic distance from a teacher's home to the school ranked first in their choice set (calculated by Google Maps). **"Student scores of school ranked first"**: Average student performance of the school that was ranked first in teachers' choice set. Schools with missing data are excluded. **"Poverty rate of school ranked first"**: Average poverty rate of the school that was ranked first in teachers' choice set. Schools with missing data are excluded. All models includes the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table C6: Assignment to schools with poorer and lower-performing students: Full Sample

	Lower-Performing	Poorer
Full Sample		
Treatment	0.055**	-0.008
	(0.021)	(0.012)
Mean (Control group)	0.35	0.20
N	1,497	2,967

Note: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Lowest performing"**: takes a 1 if the teacher was assigned to a school that belongs to the bottom 25% in terms of average student performance. Schools with missing data are excluded. **"Poorest"**: takes a 1 if the teacher was assigned to a school that belongs to the top 25% in terms of average poverty level. Schools with missing data are excluded. The description of the compliers sample is in Section 3. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table C7: Effect on Applicants per vacancy: Full sample

	Applicants per vacancy of 1st choice	Applicants per vacancy of first two choices	Applicants per vacancy in choice set	Applicants per vacancy in assigned school
Full Sample				
Treatment	-0.634***	-0.745***	-0.654***	-0.654***
	(0.196)	(0.211)	(0.220)	(0.097)
Mean (Control group)	9.9	10.8	10.5	3.9
N	18,133	18,133	18,133	8,004

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Applicants per vacancy of 1st choice"**: applicants per vacancy to the school ranked first in teachers' choice set **"Applicants per vacancy of first two choices"**: average applicants per vacancy to the schools ranked first or second in teachers' choice set. **"Applicants per vacancy in choice set"**: average applicants per vacancy to the schools included in teachers' choice set. **"Applicants per vacancy in assigned school"**: applicants per vacancy to the school where teachers were assigned. The description of the compliers sample is in Section 3. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table C8: Effect on Applicants per vacancy: quartiles, Full Sample

	Bottom 25%	25-50%	50-75%	Top 25%
Full Sample				
Treatment	0.091***	0.011*	-0.001	-0.101***
	(0.008)	(0.006)	(0.005)	(0.007)
Mean (Control group)	12.7	20	11.3	56.1
N	18,133	18,133	18,133	18,133

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Bottom 25%"**: applicants per vacancy of the school ranked first in teachers' choice set belongs to the bottom 25% of applicants per vacancy in its county and area of specialty.**"25-50%"**: applicants per vacancy of the school ranked first in teachers' choice set belongs to the percentiles 25 to 50 applicants per vacancy in its county and area of specialty. **"50-75%"**: applicants per vacancy of the school ranked first in teachers' choice set belongs to the percentiles 50 to 75 applicants per vacancy in its county and area of specialty. **"top 50%"**: applicants per vacancy of the school ranked first in teachers' choice set belongs to the top 25% of applicants per vacancy in its county and area of specialty. The description of the compliers sample is in Section 3. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

Table C9: Measures of regret: Full sample

	Changed in validation	Declined an offer	Working in 2022
Full Sample			
Treatment	-0.004	-0.002	0
	(0.007)	(0.005)	(0.007)
Mean (Control group)	0.3	0.038	0.082
N	18,133	4,388	4,388

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Changed in validation"**: takes a 1 if teacher changed her preferences in the validation phase.**"Declined an offer"**: takes a one if a teacher declined an offered (considers only teachers that were offered a position). **"Moved in 2022"**: takes a 1 if a teacher is not working in 2022 in the same school that was assigned to him/her in 2019 (considers only teachers that were offered a position in 2019). The description of the compliers sample is in Section 3. All models include the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

D Appendix D (Survey)

Dear applicant: In order to better understand how teacher candidates select vacancies in the application phase and to improve future teacher selection processes, the Ministry of Education of Ecuador and the Inter-American Development Bank invite you to answer a short **6-minute survey** about your experience in the "I Want to Be a Teacher 6" (Quiero Ser Maestro-QSM6) contest.

Your answers will serve for research purposes only and will not affect your result in the "I Want to Be a Teacher 6" contest. It should be noted that the information entered is confidential and your participation in this research is not mandatory.

If you agree to participate in our research and answer the questions in this survey, click "Yes":

- Yes, I wish to participate
- No, I do not wish to participate

If you have any questions about this study, you can contact the Ministry of Education by phone: 593-2-396-1300 / 1400/1500. We appreciate your help!

1. Before entering the platform to select vacancies, did you have in mind schools where you would like to work?
    - None
    - Yes, some
    - Yes, all or almost all
2. How difficult was it for you to decide which vacancies to apply for?
    - Not difficult at all
    - Somewhat difficult
    - Moderately difficult
    - Very difficult
    - Extremely difficult
3. How many times did you enter the platform before submitting the final application?
    - 1
    - 2
    - 3
    - 4

- 5 or more

4. During the application process, did you research the schools with vacancies available on the platform? You can select more than one option.
   - I did not research schools
   - I already knew the schools where I wanted to apply
   - I spoke with other teachers and / or principals
   - I spoke with districts and / or zones
   - I used websites
   - I visited schools
   - Other, which one?

In the next section we would like to do a simple “word game”, consisting of three questions.

Your answer is for informational purposes of MINEDUC only. Your answer does not affect at all your results in the "I Want to Be a Teacher 6" Contest.

5. (Question 1 of 3) How many words are shown below whose meaning matches the color in which they are written?

   - 6
   - 7
   - 8
   - 9

6. (Question 2 of 3) How many words are shown below whose meaning matches the color in which they are written?

- 3
- 4
- 5
- 6

7. (Question 3 of 3) How many words are shown below whose meaning matches the color in which they are written?

Pink Black Blue
Green Purple Red
Yellow Red Blue
Brown Black

- 1
- 2
- 3
- 4

8. During the selection of vacancies on the platform, do you remember seeing the following icon?

- Yes
- No

9. Do you remember what this icon meant?

Schools with this icon  were:

- Better to work
- Near my house
- Schools where teachers could have a high social impact
- Schools recommended by MINEDUC for me
- I don't remember

10. In which schools do you think you can generate a greater social impact? Select all the options that you consider correct:

- Schools with high-performing students
- Schools with vulnerable students
- Schools with qualified teachers
- Schools where teachers can generate greater changes in the lives of students
- Schools with more support from principals
- Schools that are emblematic

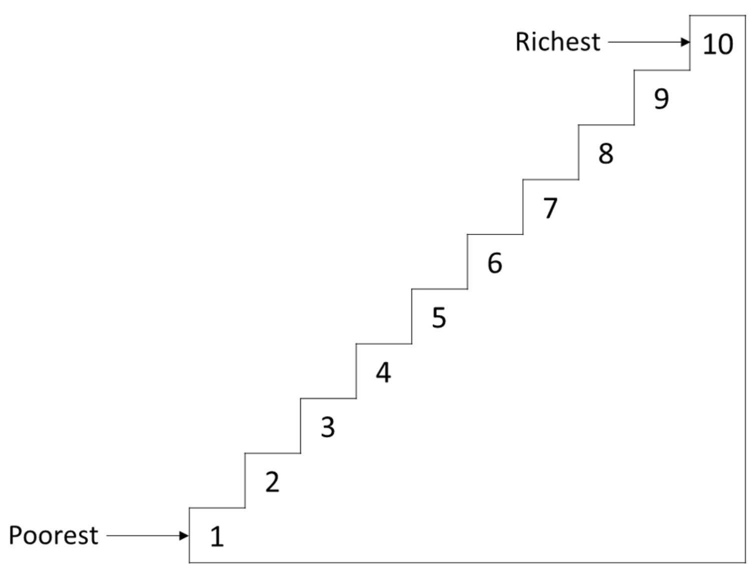
11. We would like you to imagine the following hypothetical situation: Suppose that today you unexpectedly received \$100. How much of this amount would you donate to a good cause? Use the that slider to indicate an amount between \$0 and \$100:

I would donate: 

12. How many people under the age of 18 live in your home?

- None
- 1
- 2
- 3
- 4
- More than 4

13. About your socioeconomic status, imagine a ten-step ladder, where at the bottom (the first step) are the poorest people in Ecuador and at the highest step (the tenth step), the richest people. In what step do you think you are? (Check only one answer)



- 1
- 2
- 3
- 4
- 5
- 6
- 7
- 8
- 9
- 10

14. What is the highest educational level of your mother or primary female caregiver (grandmother, aunt etc.)?

    Note: Primary refers to grades 1 to 7 of Basic Education. Secondary refers to grades 8, 9 and 10 of Basic Education plus 1, 2 and 3 of Baccalaureate.

    - None
    - Incomplete primary education
    - Complete primary education
    - Incomplete secondary education
    - Complete secondary education
    - Incomplete college
    - Complete college
    - Incomplete graduate school
    - Complete graduate school
    - I don't know

15. What is the highest educational level of your father or primary male caregiver (grandfather, uncle etc.)?

    - None
    - Incomplete primary education
    - Complete primary education
    - Incomplete secondary education
    - Complete secondary education
    - Incomplete college
    - Complete college
    - Incomplete graduate school
    - Complete graduate school
    - I don't know

16. What is your date of birth (day / month / year)?

E Appendix (Survey - Original language)

Estimado: Con el fin de comprender mejor cómo los aspirantes a docentes eligen las vacantes en la fase de postulación y de mejorar los futuros concursos de méritos y oposición Quiero Ser Maestro, el Ministerio de Educación de Ecuador y el Banco Interamericano de Desarrollo lo invita a responder **una breve encuesta de 6 minutos** sobre su experiencia en el concurso Quiero Ser Maestro 6 (QSM6).

Sus respuestas servirán únicamente para fines de investigación y no afectarán su resultado en el concurso Quiero Ser Maestro 6. Cabe señalar que la información ingresada es confidencial y su participación en esta investigación no es obligatoria.

Si acepta participar en nuestra investigación y responder las preguntas de esta encuesta, haga clic en "Sí":

- Si, deseo participar
- No, no deseo participar

Si tiene alguna pregunta sobre este estudio, puede comunicarse con el Ministerio de Educación a través del teléfono: 593-2-396-1300 / 1400 / 1500. ¡Agradecemos su colaboración!

1. Antes de ingresar a la plataforma para seleccionar las vacantes, ¿tenía en mente las Instituciones Educativas donde le gustaría trabajar?
   - Ninguna
   - Sí, algunas
   - Sí, todas o casi todas
2. ¿Qué tan difícil fue para usted decidirse sobre cuáles vacantes postular?
   - Nada difícil
   - Algo difícil
   - Medianamente difícil
   - Muy difícil
   - Extremadamente difícil
3. ¿Cuántas veces ingresó a la plataforma antes de enviar la postulación final?
   - 1
   - 2
   - 3
   - 4

- 5 o más

4. Durante el proceso de postulación, ¿investigó sobre las Instituciones Educativas con vacantes disponibles en la plataforma? Puede seleccionar más de una opcion.

   - No hice investigación
   - Ya conocía las Instituciones Educativas donde quería postular
   - Hablé con otros docentes y/o directores
   - Hablé con distritos y/o zonas
   - Usé sitios web
   - Visité Instituciones Educativas
   - Otro, ¿cuál?

En la próxima sección nos gustaría realizar un "juego de palabras" sencillo, que consta de tres preguntas.

Su respuesta es solo para fines informativos del MINEDUC. La respuesta no afecta en lo absoluto los resultados obtenidos en el Concurso de Quiero Ser Maestro 6.

5. Pregunta 1 de 3: ¿Cuántas palabras se muestran debajo cuyo significado coincide con el color en el que están escritas?

   - 6
   - 7
   - 8
   - 9

6. Pregunta 2 de 3: ¿Cuántas palabras se muestran debajo cuyo significado coincide con el color en el que están escritas?

- 3
- 4
- 5
- 6

7. Pregunta 3 de 3: ¿Cuántas palabras se muestran debajo cuyo significado coincide con el color en el que están escritas?

- 1
- 2
- 3
- 4

8. Durante la selección de vacantes en la plataforma, ¿recuerda haber visto el siguiente ícono?

- Sí
- No

9. ¿Recuerda cuál era su significado?

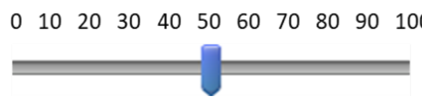
Las Instituciones Educativas con este ícono  eran:

- Mejores para trabajar
- Cercanas de mi domicilio
- Aquellas donde los maestros podían tener un alto impacto social
- Las recomendadas por el Mineduc para mí
- No lo recuerdo

10. ¿En qué Instituciones Educativas (IE) cree que puede generar un mayor impacto social? Selecciones todas las opciones que considere correctas:

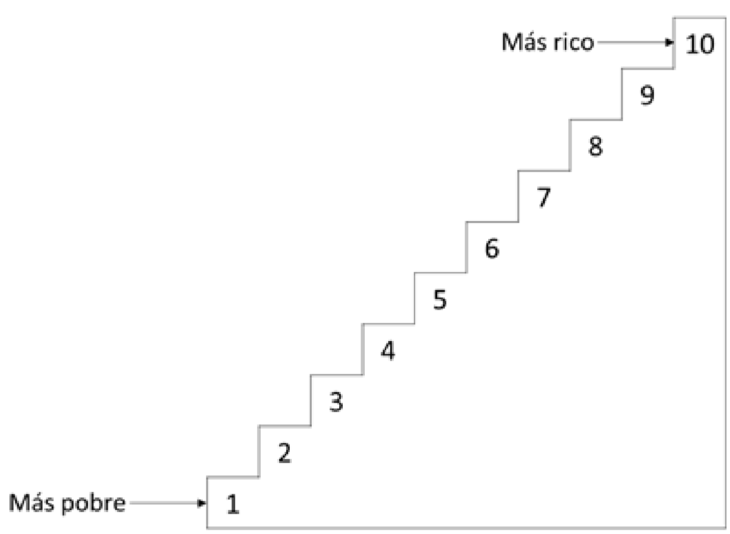
- IE que tienen estudiantes de alto rendimiento
- IE que tienen estudiantes más vulnerables
- IE que tienen docentes más calificados
- IE que son donde un docente puede generar mayores cambios en la vida de los estudiantes
- IE que tienen más apoyo por parte de los directivos
- IE que son emblemáticas

11. Quisiéramos que imagine la siguiente situación hipotética: Suponga que hoy, de forma inesperada, recibe 100 dólares. ¿Que cantidad de este monto donaría a una buena causa? Use el control deslizante que para indicar una cantidad entre 0 y 100 dólares:

Yo donaría: 

12. ¿Cuántas personas menores de 18 años viven en su vivienda?
    - Ninguna
    - 1
    - 2
    - 3
    - 4
    - Más de 4

13. Sobre su situación socio-económica ¿Imagine una escalera de diez peldaños, donde en la parte inferior (el primer peldaño) se encuentran las personas más pobres del Ecuador y en el peldaño más alto (el décimo peldaño), las personas más ricas. ¿En qué escalón considera que se encuentra usted actualmente? (Marque solo una respuesta)



- 1
- 2
- 3
- 4
- 5
- 6
- 7
- 8
- 9
- 10

14. ¿Cuál es el nivel de instrucción más alto de su madre o cuidadora primaria (abuela, tía etc.)?

    Nota: Primaria se refiere a los grados 1ro a 7mo de la Educación Básica. Secundaria se refiere a los grados 8, 9 y 10mo de la Educación Básica más 1, 2 y 3 de Bachillerato.

    - Ninguno
    - Primaria incompleta
    - Primaria completa
    - Secundaria incompleta
    - Secundaria completa
    - Superior incompleto
    - Superior completo
    - Post-grado incompleto
    - Post-grado completo
    - No sé

15. ¿Cuál es el nivel de instrucción más alto de su padre o cuidador primario (abuelo, tío etc.)?

    - Ninguno
    - Primaria incompleta
    - Primaria completa
    - Secundaria incompleta
    - Secundaria completa
    - Superior incompleto
    - Superior completo
    - Post-grado incompleto
    - Post-grado completo
    - No sé

16. ¿Cuál es su fecha de nacimiento (día / mes / año)?