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Order Effects and Employment Decisions: Experimental Evidence from a Nationwide Program.*

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Abstract

In this paper, we show that order effects operate in the context of high-stakes, real-world decisions: employment choices. We experimentally evaluate a nationwide program in Ecuador that changed the order of teaching vacancies on a job application platform in order to reduce teacher sorting (that is, lower-income students are more likely to attend schools with less qualified teachers). In the treatment arm, the platform showed hard-to-staff schools (institutions typically located in more vulnerable areas that normally have greater difficulty attracting teachers) first, while in the control group teaching vacancies were displayed in alphabetical order. In both arms, hard-to-staff schools were labeled with an icon and identical information was given to teachers. We find that a teacher in the treatment arm was more likely to apply to hard-to-staff schools, to rank them as their highest priority, and to be assigned to a job vacancy in one of these schools. The effects were not driven by inattentive, altruistic, or less-qualified teachers. The program has thus helped 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

Choosing the "*best*" available option can be cognitively demanding, as it involves comparing each alternative to every other one. Especially when decisions are relatively complex, decision makers may use heuristics (Simon (1955)) or procedurally simpler choice rules rather than a strict rule of optimization (Salant (2011)). In this context, certain environmental cues, such as the order of the options, can play an important role.

The order in which alternatives are presented to decision makers has been found to impact individuals' choices in relatively low-stakes decisions, such as answering surveys (Krosnick and Alwin (1987)) and consumer purchases (Levav et al. (2010)). Likewise, studies also show that the order of candidate names on ballots has an influence on voting behavior, with candidates listed first enjoying an advantage (Miller and Krosnick (1998); Koppell and Steen (2004); Augenblick and Nicholson (2016)). However, less attention has been paid to order effects on high-stakes decisions. In this paper, we address this gap by providing experimental evidence of order effects in a real-world context of high-stakes decision making using the case of employment applications.

We present the results of an experimental evaluation of a zero-cost nationwide intervention designed by the government of Ecuador to attract applicants (teacher candidates) to hard-to-staff schools (institutions typically located in more remote and vulnerable areas that normally have greater difficulty attracting teachers) by altering the order in which teaching vacancies were presented on a job application platform (without altering the information or incentives provided to teachers).

The program involved changes to the official application procedure for teachers aspiring to work in a permanent, stable posting in the public education system. Although teachers can eventually move to a different school, mobility tends to be low because there is a high fixed cost for applications in subsequent years. Specifically, 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. The changes to the application process were made in the context of a larger effort to reduce a long-standing problem of teacher sorting and market congestion in which candidates disproportionately apply to a group of in-demand and more socioeconomically advantaged schools. This problem results in suboptimal outcomes for prospective teachers due to congestion and reduced probabilities of obtaining a job offer: in the 2016 teacher selection process in Ecuador, 26 percent of teaching positions remained unfilled and 56 percent of candidates were not assigned to a position. Furthermore, it is detrimental in terms of equity, since disadvantaged schools are more likely to be hard to staff (see Jackson (2009)). With the goal of filling all vacant positions, the application system listed hard-to-staff schools first for teacher candidates in the treatment group. In the placebo 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. 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 40%). The proportion of hard-to-staff schools was 1.4 pp higher in the choice sets of the treatment group (the mean of the control group was 43%), 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 27%). Candidates in the treatment group were 3.1 pp more likely to accept a teaching position in a hard-to-staff school (the mean of the control group was 27%).

Order effects can be driven or magnified by different factors (see, for example, Meredith and Salant (2013); Kim et al. (2015)). They may, for instance, be triggered by choice fatigue or cognitive overload (Augenblick and Nicholson (2016)). The act of making a decision can be exhausting and effort consuming. In this context, having a large number of alternatives may trigger what the literature on behavioral economics and psychology has termed a *choice overload* (Iyengar and Lepper (2000)). We test this hypothesis and find consistent evidence: the effects are significantly larger when teachers have a larger set of alternatives (vacancies) to choose from.

Another 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)) and, using different measures (correct answers, indexes of response time), we found no heterogeneous effects for any measure of inattention.

[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.

The results are thus unlikely to be driven by inattentive individuals.

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 the result of candidates' test scores on a qualifying exam as a proxy of cognitive ability, we show that this does not appear to play a role in our context.

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, as expected, the unconditional probability of choosing a hard-to-staff school does indeed increase with teachers' level of altruism, the treatment effect was not more pronounced among altruistic candidates.

Finally, Simon (1955) maintains that the complexity of a task or choice might prevent individuals from making the decision that maximizes their utility. Instead, they make a choice that results in a sufficient utility (i.e., the option that "satisfies"). In this case, rather than choosing the *best* vacancy at a school out of every available alternative and weighing all tradeoffs, candidates may choose the first positions that seem to be sufficiently good. Indeed, we do find evidence consistent with teacher candidates selecting schools that are "good enough" rather than ideal. 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. Instead, we find no effect on the average distance or commuting times in any of the outcomes (choice set, school ranked as first preference or school assigned to the teacher at the end of the process). It has been extensively documented in the literature that the main determinant of teachers' school preferences is distance or commuting time to work (Boyd et al. 2005; Reininger 2012; Rosa 2017; Bertoni et al. 2021). We thus interpret the null 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, but which were satisfactory in terms of their most important characteristic: distance.

Our paper relates to several strands of the literature in behavioral economics and education. First, 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 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)).

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. Our paper contributes to this literature by showing the influence of order effects in a real-world, high-stakes context.

Our paper also contributes 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' preferences for 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.

This experiment also has critical policy implications. The sorting of candidates in teacher selection processes leads to inefficiency due to congestion and, ultimately, does not optimize teachers' well-being, in terms of their chances of finding a job. The teacher selection process in Ecuador, known as "Quiero Ser Maestro" ("I Want to Become a Teacher", abbreviated as QSM), saw 26 percent of teaching positions go unfilled in 2016. Moreover, 56% of candidates were not assigned to a position despite passing the QSM selection criteria. Finding solutions to minimize the problem of teacher sorting in selection processes can decrease market congestion while also helping teachers to secure a stable job.

Addressing teacher sorting is also 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.

We proceed as follows. 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* (I Want to Become a Teacher, abbreviated QSM). This paper focuses on the sixth year of the QSM program (QSM6), which was conducted throughout 2019 and included three phases: i) the eligibility phase, ii) the "merits and public examination" (*méritos y oposición*) 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 psychometric 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, secondary school math, etc.). Candidates must pass the personality and reasoning assessments to be eligible to take the knowledge test. 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 (INEVAL). To be eligible to participate in the second phase, candidates must have passed the psychometric test and have scored a minimum of 70 percent on the knowledge exam.

In the second phase, known as "merits and opposition phase", candidates are first evaluated on their academic and professional credentials (known as the "merits" portion, which accounts for 35% of their second phase score).[2] In the "opposition" portion, weighted at 65%, candidates' scores are based on their grade on the knowledge test from phase one combined with their performance giving a mock class[3]. The mock class is carried out on a topic assigned

[2] Each credential is weighted differently: 20% of the score is based on education degrees, 10% on teaching experience, 3% on publications, and 2% on specialization in the education field.

[3] Of the 65% allocated to this portion, the candidate's performance on the knowledge test in phase one accounts for 40%, while 25% is allocated based on the mock class performance

by the Ministry of Education according to the candidate's specialty area and is a centralized process conducted at randomly selected schools around the country. The candidate's mock class is evaluated by a panel composed of a school principal, a teacher in the same field, a representative of the local parent association, and a pupil on the local student council. On the "merits and opposition phase", candidates are required to have a minimum of 70 percent on the knowledge test and 70 percent on the mock class to proceed with their application to job vacancies. Candidates can also receive up to five "bonus" points for meeting certain criteria, such as living in an indigenous community or having a disability that does not affect their performance in the classroom.

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 during the month of November 2019. Candidates were able to apply to no more than five vacancies in any region of the country, which they ranked according to their preferences.[4] 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 merits and opposition phase as well as their ranked preference for vacancies. In the event of a tie, the following criteria are used as tiebreakers, in order of priority: 1) performance in the mock lesson, 2) points awarded for education degrees, and 3) points awarded for teaching experience. After submitting their preferences in the application phase, candidates had the opportunity, in the event they were not satisfied with their original selection, to revisit their application during a four-day "validation phase."

Although teachers can, in theory, eventually move to a different school in the subsequent years, this happens only rarely in practice because there is a significant fixed cost to doing so. They must make a special request, which can only been done after working in the assigned school for at least two years, and pass through an additional application process.

2.2 Government efforts to improve the teacher selection process

Although *Quiero Ser Maestro* has improved transparency and reduced the costs of applying to vacancies in different parts of the country, Ecuador's teacher selection process still generates

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

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.

Due to budget tightening following a lengthy economic downturn, 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. One of these policies included changing the teacher application rules and assignment algorithm in the QSM6 from one resembling immediate acceptance to a deferred acceptance version. First, teachers now apply directly to up to five schools, as opposed to three local school districts. Second, the entry test score is now weighted higher than teacher preferences. Previously, the latter took precedence over scores, e.g., a teacher with a lower score could be assigned to a vacancy over a higher scoring teacher because the former had ranked that specific district higher in her reported preferences (immediate acceptance). Elacqua et al. (2020) examine the impact of this intervention and find that the changes in the application rules and the adoption of the deferred acceptance algorithm led to a reduction in the number of vacant positions upon conclusion of the QSM6 and improved the overall quality of the pool of teacher candidates who were offered a position.

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. This intervention 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 validate their job preferences. During this validation phase, teacher candidates could review their application and freely modify their list of chosen vacancies. We find that candidates in the treatment and control groups were equally likely to resubmit new preferences during the validation phase–this data is not shown but is available upon request. In the next section, we explain in detail how we experimentally evaluated this low-cost government intervention to avoid congestion and attract teacher candidates to hard-to-staff schools.

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 therefore 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 arms and one control group. One of the treatment arms aimed to test the impact of reducing cognitive dissonance during the application process on candidates' probability of selecting a vacancy in hard-to-staff schools. Candidates in the treatment arm would be prompted to answer whether or not they were interested in working in schools where they could have a greater impact on students' learning before seeing the application page. Unfortunately, due to an implementation error, the platform was not properly changed for this treatment arm. The platform was programmed in such a way that only candidates who answered the question in the affirmative were able to see and select hard-to-staff schools. 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 (50%) were assigned to a control group, and 9,059 (50%) were assigned to the treatment group. The experiment was designed to ensure that teachers in both groups received exactly the same information–that is, 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 (from November 18-24, 2019) 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 (up to 10 vacancies per screen). 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 1 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's entry on the platform. Once teachers finished their selection, the system listed all the selected vacancies on a final screen (Figure 2 in the Appendix), allowing teachers to list the vacancies in their preferred order. Before submitting their application, teachers could change their preferences at any time. As mentioned, 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 1 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. 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 1 in the Appendix). In light of prior research (Aaronson et al. 2007; 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 3 (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 not, in practice, exposed to the treatment. This is because the number of vacancies was extremely limited in certain counties (for example, in remote areas). Indeed, many candidates could only apply to a single vacancy. Other teachers, meanwhile, found their options comprised of exclusively hard-to-staff schools or had no hard-to-staff schools among their options, making the exercise irrelevant. We therefore exclude observations in which the treatment could not be implemented due to only a single vacancy being shown, or there being no variability in the type of vacancy (all or none of the vacancies were in hard-to-staff schools).

Since that we did not anticipate this issue, it was not originally considered in the pre-registration plan. Thus, we also present the main results using the unrestricted sample, which yields very similar patterns. Moreover, in Table 1, we show that the probability of being included in our final 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, with weekly reminders to those who had not yet responded. To avoid asking for individuals' identification information and thus depressing survey participation, the Ministry sent each candidate a personalized survey link which could only be answered once. 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 11 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 preferences, 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 2 presents a descriptive summary of the 5,760 candidates in our final sample, which excludes candidates whose options had no variability in terms of the type of vacancy as well as candidates who were only provided with one available vacancy. In this final sample, 77% of the candidates are female, 54% are single, and 11% belong to an ethnic minority group (neither white nor mestizo). On average, candidates have just under four years of teaching experience and scored about 68 points on the merits and public examination phase, with scores ranging from 44 to 95. Hard-to-staff schools comprised 43% of all schools in candidates' choice sets. Some 42% of candidates ranked a hard-to-staff school as their first choice, while 63% ranked at least one hard-to-staff school among their first two choices and 75% ranked at least one hard-to-staff school among their first three choices. 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.[5] Note that as these last variables are right-skewed, we use a logarithmic transformation of the measures of school distance and commuting time.

Table 3 compares candidates' characteristics in the final 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 4 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. For example, survey participants have 3.67 years of experience compared to 3.81 for non-participants; the average knowledge test score for participants was 66.9 points while non-participants' average was 68.5 points.

[5]It is important to note that the average travel distance and commuting time varies by the candidates' ranked school preferences: 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).

4.1 Empirical strategy

To measure the impact of changing the order in which teaching vacancies are listed on candidates' preferences for 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 "preference" 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 has been stratified.

4.2 Measures

The first analysis includes four outcomes relating to teacher preferences: (i) **the percentage of hard-to-staff schools in candidates' choice set**; (ii) **whether their first choice was a hard-to-staff school**; (iii) **whether their first two choices included a hard-to-staff school**; and (iv) **whether their first three choices included a hard-to-staff school**. We focus on the top three choices because candidates are more likely to be assigned to one of their three most preferred schools. 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. Finally, we include an outcome **"Accepted offer in hard-to-staff school"**, which takes a 1 if a teacher *accepted* a position at a hard-to-staff school. In the pre-registered analysis plan (which we follow closely), we also included an outcome defined as the absolute number (instead of the percentage) of hard-to-staff schools in the choice set. This outcome was highly correlated with the percentage since most teachers applied to the maximum number of vacancies (5) allowed by the system. We therefore only report the latter outcome in Table A3 in the Appendix.

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 test whether the effect of the treatment on candidates' preferences 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 quite qualified, having passed the highly competitive eligibility phase.

To explain the order effects, we estimate heterogeneous effects on a series of potential mediators. First, to examine whether order effects are 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 [6].

We also examine whether the treatment effect was stronger among candidates with more limited attention. 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 instead of blue). Candidates were asked to indicate the number of correct matches between the name of the color and the font color.[7] We estimate candidates' final 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). We calculated candidates' final score based on the main factor produced by a factor analysis

[6] The application platform shows teaching vacancies by county. That is, if a candidate's preferred vacancies were located in two different counties, she may have seen a different list of available schools on the platform when selecting each county from the drop down menu. Considering that candidates may analyze the available options for each county separately, we opted for averaging the number of vacancies in each county rather than adding them up. For example, if a candidate selected vacancies from 5 different counties and each one of them had 2 schools with job openings, we believe that an average of 2 vacancies per county (rather than a total of 10 vacancies) is a better representation of the cognitive overload experienced by the candidate. In any case, we also estimated heterogeneous effects using the total number of vacancies and the results are similar.

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

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. Our final attention measure is dichotomous, where "1" refers to the top 50% respondents of the score distribution and "0" to the bottom 50%.[8]

We 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, therefore, the amount candidates were willing to donate.

Finally, the administrative data of candidates contain their Código Único Eléctrico, which is an identification number tied to the electric bill of their residence. The data also include geo-referenced information on the location of schools. This information allowed us to calculate the Euclidean distance and travel time from candidates' homes to each school in their choice set.

5 Results and Interpretation

We first analyze the main outcomes related to preferences, final allocation, and job offer acceptance. Table 5 shows the main results. For each outcome, we present one column in which we include all the socio-demographic controls described in Section 4 and one column with no controls. The order effects are significant, robust, and large in every outcome.

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 (the mean of the control group was 43%). Teachers in the treatment group were also 5.2 pp more likely to rank an understaffed school as their first choice (the mean of the control group was 40%). They were 2.7 pp more likely to include at least one hard-to-staff school among their first two choices (the mean of the control group was 61%) and 2.9 pp more likely to include at least one hard-to-staff school among their first

[8] 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 A4 in the Appendix. We found that results are similar for both measures of attention.

three choices (the mean of the control group was 73%). The probability that they were assigned by the algorithm to a teaching position in a hard-to-staff school was 3.4 pp higher (the mean of the control group was 27%). Ultimately, teachers in the treatment group were 3.1 pp more likely to accept a position at a hard-to-staff school (the mean of the control group was 26.7%).[9] In Table A3 in the Appendix, we show the results for the outcome "Number of hard-to-staff schools in the choice set," which shows a pattern consistent with the rest of the outcomes. This outcome is highly correlated with "Percentage of hard-to-staff schools in choice set," since most teachers applied to the maximum number of vacancies allowed by the system (five).

Table 6 displays the outcomes related to candidates' performance during the qualifying stage and their propensity to **choose and be assigned to a hard-to-staff school**. Although we find that candidates who apply to at least one hard-to-staff school and candidates who are assigned to a hard-to-staff school tend to be higher performing, these estimations are not precise enough to identify a significant effect. Our findings suggest that, at the very least, the treatment did not induce only low-performing candidates to apply to hard-to-staff schools. Moreover, when analyzing heterogeneous order effects with respect to candidates' performance, it would seem that the effect is driven by high performers. It is, in any case, important to remember that every teacher candidate who passed the qualification exam and was allowed to submit an application is indeed among the most qualified candidates in the country.

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). We do this by studying different heterogeneous effects. Since some of the observable characteristics may be correlated with unobserved ones (for instance, attention may be related to motivation), most of the results in Table 7 should be interpreted as suggestive.

We begin by investigating the potential role of choice fatigue or cognitive overload. It is well documented that making decisions can be exhausting and require intensive effort (Iyengar and Lepper (2000), Augenblick and Nicholson (2016)). In this context, decisions between a small number of alternatives may be easier than deciding between many options. To test

[9] As explained in Section 3, we excluded a sub-set of observations from our main dataset in which the treatment was not implemented in practice, either because there was only one option (school) to choose from, or because all the schools in the county fell into the hard to staff category (or, alternatively, none of them did). In Table A1 in the Appendix, we report the main results using the full sample, including these observations. As expected, the order effects were slightly smaller for the full sample because it includes candidates who were not affected by the treatment. Even so, the effects on candidates' preferences are positive and significant.

this hypothesis, we estimate a model that includes an interaction with the number of school vacancies seen by each individual on the platform. In Panel A of Table 7, we show that the interaction is positive and significant for most of the outcomes: facing more choices amplifies the order effects.

We then 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. As explained above, our measure of attention is based on a Stroop test that combines the number of correct answers and response time for each candidate. Our index of attention represents the main factor produced by a factor analysis of all three "response accuracy" variables and all three "response time" variables. 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 Panel B of Table 7, we interact our attention index with the treatment and show that attention levels do not appear to be relevant to our results.

Kim et al. (2015) suggest that order effects may be driven by individuals with lower cognitive ability–that is, order effects may be moderated by skills such as interpreting information, storing it in one's memory, retrieving it when necessary, and being able to use it to form a thoughtful judgement. We use candidates' test scores from the qualifying stage as a proxy of cognitive ability. We previously interacted the treatment with a dichotomous measure of candidates' test scores (table 6). The interactions are insignificant, but the coefficients suggest that high-performing individuals may have been *more* affected by the treatment. We find similar results if we interact the treatment with a continuous measure of candidates' test scores.

Another plausible hypothesis is that candidates' prosocial/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.[10] In Panel B of Table 7, 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.

Finally, we analyze the effect of the treatment in a crucial outcome for candidates' decision:

[10] Given the limitations in terms of cost and time, we were only able to partially implement the altruism module of the GPS. We are aware of the limitations of our proxy of altruism and thus interpret these results as suggestive.

distance to school. Distance and commuting times are key determinants of teachers' school preferences, a fact which has been extensively documented in the literature (Boyd et al. 2005; Reininger 2012; Rosa 2017; Bertoni et al. 2021). Moreover, as many hard-to-staff schools are significantly farther away than others, we would expect a mechanical effect of the treatment on the average distance/commuting time from home to school of teachers' choice set. Instead, we find no effect on the average distance/commuting times in all of the outcomes. In Table 8, we show that the intervention did not impact the Euclidean distance between candidates' homes and their preferred schools or their estimated commuting time. We interpret the null effect on these outcomes in light of the satisficing decision-making strategy proposed by Simon (1955). That is, teachers selected schools that may have not been the *optimal* choice, but which were satisfactory in terms of their most important characteristic: distance.

Overall, our results suggest that, on the one hand, inattention, cognitive ability and altruism priming are not relevant drivers of the effects. On the other hand, choice overload and fatigue do seem to have played a significant role. 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 every single dimension.

6 Conclusions and Policy Implications

This paper finds evidence of order effects impacting choices in a real-world, high-stakes environment, namely employment decisions. We study this in the context of Ecuador's nationwide teacher recruitment process for entering the public education system. We use data from an experimental setting implemented throughout the country and show that teachers in the treatment group, where the application platform listed hard-to-staff schools first, were more likely to include the latter type of school in their choice sets, as well as to be assigned and ultimately to accept a position in such a school.

We also conducted a post-experiment survey to explore mechanisms and present suggestive evidence that the order effects were not mediated by cognitive skills, inattention or a different level of altruism. Instead, we show that choice overload may have played a relevant role. Moreover, we find no effect on the average distance/commuting times in any of the outcomes. 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. We interpret the null effect on these outcomes 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.

Beyond its academic contribution, 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.

A policy that has often been put forward to decrease market congestion and reduce sorting is monetary incentives for teachers willing to work in hard-to-staff schools. However, the evidence on the effectiveness of these types of incentives on teacher preferences is mixed or inconclusive (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). Salary increases can also be 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. Budget constraints are of particular concern in Latin America, where government revenues have declined substantially over the past years. Governments of the region allocate a non-trivial amount of the educational budgets to teachers' incentives to work in hard-to-staff schools. In Colombia and some cities in Brazil (such as Rio de Janeiro), 13% of teachers' salaries are composed by a bonus to work in remote areas. In Chile, this number could reach more than 30% and, in Peru, almost 37% (see Bertoni et al. (2018)). The zero-cost intervention evaluated in this paper therefore provides a timely contribution to mitigating teacher sorting and reducing market congestion in application processes.

7 Tables and figures

Table 1: Random selection into final sample

	Probability of being included in the restricted 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 restricted 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 2: Summary statistics

	Obs	Mean	Std. Dev.	Min	Max
Candidate's attributes					
Female	5,760	0.77	0.42	0	1
Single	5,760	0.54	0.50	0	1
Ethnic minority	5,754	0.11	0.31	0	1
Years of experience	5,757	3.74	3.27	0	10
Test score	5,760	67.70	9.42	44.26	94.60
Outcomes					
Percentage of understaffed schools in choice set	5,760	0.43	0.27	0	1
Ranked an understaffed school in their first choice	5,760	0.42	0.49	0	1
At least one understaffed school among first two choices	5,760	0.63	0.48	0	1
At least one understaffed school among first three choices	5,760	0.75	0.44	0	1
Assigned to an understaffed school	5,760	0.29	0.45	0	1
Accepted offer in understaffed school	5,760	0.28	0.45	0	1
Average commuting time to schools in the choice set	5,755	64.19	106.55	0.57	1889.45
Average distance to schools in the choice set	5,758	34.26	59.44	0.18	998.92

Table 3: Balance tests

Variable	Final sample			Survey sample		
	Control	Treatment	Difference	Control	Treatment	Difference
Female	0.767	0.765	-0.002	0.757	0.762	0.006
Test scores	67.725	67.681	-0.044	66.844	66.994	0.150
Single	0.533	0.548	0.016	0.521	0.531	0.010
Years of experience	3.739	3.742	0.003	3.676	3.666	-0.010
Ethnic minority	0.111	0.102	-0.009	0.108	0.100	-0.009
Observations	2,903	2,857	5,760	1,471	1,485	2,956

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

Table 4: Representativeness of survey sample

Variable	Survey participants		Difference
	Opted out	Opted in	
Female	0.774	0.759	-0.014
Test scores	68.530	66.919	-1.611***
Single	0.555	0.526	-0.029**
Years of experience	3.814	3.671	-0.143*
Ethnic minority	0.110	0.104	-0.006
Observations	2,804	2,956	5,760

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

Table 5: Treatment effect on preferences and assignment

	Percentage of understaffed schools in choice set		Understaffed school at 1st choice		At least one understaffed school among first 2 choices		At least one understaffed school among first 3 choices		Assigned to understaffed school		Accepted offer in understaffed school	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Treatment	0.014**	0.013**	0.053***	0.052***	0.028**	0.027**	0.030**	0.029**	0.034***	0.034***	0.031**	0.031**
	(0.006)	(0.006)	(0.012)	(0.012)	(0.011)	(0.011)	(0.012)	(0.011)	(0.012)	(0.012)	(0.012)	(0.012)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean (Control group)	42.7	42.7	39.8	39.8	61.1	61.2	73.1	73.1	26.9	26.9	26.7	26.7
N	5760	5760	5760	5760	5760	5760	5760	5760	5760	5760	5760	5760

Notes: Robust standard errors in parentheses; * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. **"Percentage of understaffed schools in choice set"**: number of understaffed choices divided by the total number of choices of a given teacher. **"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 one or more of the first three choices in a teacher's choice set is an understaffed school. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. **"Accepted offer in understaffed school"**: takes a 1 if the teacher accepted an offer to work at an understaffed school. 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 6: Treatment effect on teacher quality

	Test scores (selected at least one understaffed school)	Test scores (assigned to understaffed schools)	Percentage of understaffed schools in choice set	Understaffed school at 1st choice	At least one understaffed school among first 2 choices	At least one understaffed school among first 3 choices	Assigned to understaffed school	Accepted offer in understaffed school
Treatment (I)	0.023	0.251	0.018**	0.068***	0.036**	0.032*	0.046**	0.040*
	(0.279)	(0.349)	(0.009)	(0.018)	(0.018)	(0.017)	(0.021)	(0.021)
Low-performing (II)			0.018**	0.041**	0.033*	0.002	-0.144***	-0.145***
			(0.009)	(0.017)	(0.017)	(0.015)	(0.019)	(0.019)
Treatment*Low-performing (III)			-0.011	-0.032	-0.017	-0.005	-0.024	-0.018
			(0.012)	(0.026)	(0.027)	(0.023)	(0.024)	(0.024)
(I) + (III)			0.007	0.036	0.019	0.027	0.022	0.022
			(0.008)	(0.019)	(0.017)	(0.016)	(0.012)	(0.012)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean (Control group)	67.7	70.8	42.7	39.8	61.2	73.1	26.9	26.7
N	5005	1637	5760	5760	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. **"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. **"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. **"At least one understaffed school among first 3 choices"**: takes a one if at least one of the first three choices in a teacher's choice set is an understaffed school. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. **"Accepted offer in understaffed school"**: takes a 1 if the teacher accepted an offer to work at an understaffed school. The description of the final 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 7: Heterogeneous effects

	Percentage of understaffed schools in choice set	Understaffed school at 1st choice	At least one understaffed school among first 2 choices	At least one understaffed school among first 3 choices	Assigned to understaffed school	Accepted offer in understaffed school
Panel A: Overload						
Treatment	0.013**	0.053***	0.028**	0.030***	0.033***	0.030**
	(0.006)	(0.013)	(0.011)	(0.011)	(0.012)	(0.012)
Vacancies	-0.002***	-0.002***	-0.003***	-0.003***	0.001*	0.001*
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treatment*Vacancies	0.001***	0.001**	0.002**	0.002***	0.001	0.001
	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean (Control group)	42.7	39.7	61.1	73	27	26.7
N	5760	5760	5760	5760	5760	5760
Panel B: Attention						
Treatment	0.005	0.045**	0.025	0.019	0.034*	0.026
	(0.011)	(0.020)	(0.018)	(0.017)	(0.019)	(0.019)
Attentiveness	-0.006	-0.015	-0.003	0.001	0.009	0.007
	(0.006)	(0.011)	(0.013)	(0.013)	(0.011)	(0.011)
Treatment*Attentiveness	0.000	0.001	0.017	0.006	0.003	0.008
	(0.009)	(0.018)	(0.019)	(0.018)	(0.015)	(0.016)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean (Control group)	42.3	40.1	60.7	73.3	23.2	22.9
N	2617	2617	2617	2617	2617	2617
Panel C: Altruism						
Treatment	0.012	0.053***	0.032*	0.026*	0.040**	0.033*
	(0.010)	(0.019)	(0.017)	(0.016)	(0.017)	(0.017)
Altruist	-0.000*	0.000	-0.001	-0.001*	-0.001	-0.001
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Treatment*Altruist	0.000	0.000	0.000	0.000	-0.000	-0.000
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean (Control group)	42.2	40.1	60.7	73.1	22.1	21.7
N	2697	2697	2697	2697	2697	2697

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Percentage of understaffed schools in choice set"**: number of understaffed schools as a percentage of a teacher's total number of choices. **"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 one if at least one of the first three choices in a teacher's choice set is an understaffed school. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. **"Accepted offer in understaffed school"**: takes a 1 if the teacher accepted an offer to work at an understaffed school. The description of the final sample is in Section 3. **"Vacancies"**: number of vacancies seen by the teacher on the job application platform. **"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. **"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 8: Treatment effect on commuting time

	Average log time traveled to selected schools	Average log geodesic distance (km) to selected schools
Treatment	-0.012	-0.015
	(0.026)	(0.030)
Controls	Yes	Yes
Mean (Control group)	7.75	6.9
N	5755	5758

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Average log time traveled to selected schools"**: log of commuting time by car from a teacher's home to each school in their choice set (calculated by Google Maps). **"Average log geodesic distance (km) to selected schools"**: log of geodesic distance from a teacher's home to each school in their choice set (calculated by Google Maps). All models includes the following controls at the teacher level: gender, marital status, ethnicity, years of experience, test scores and district of residence.

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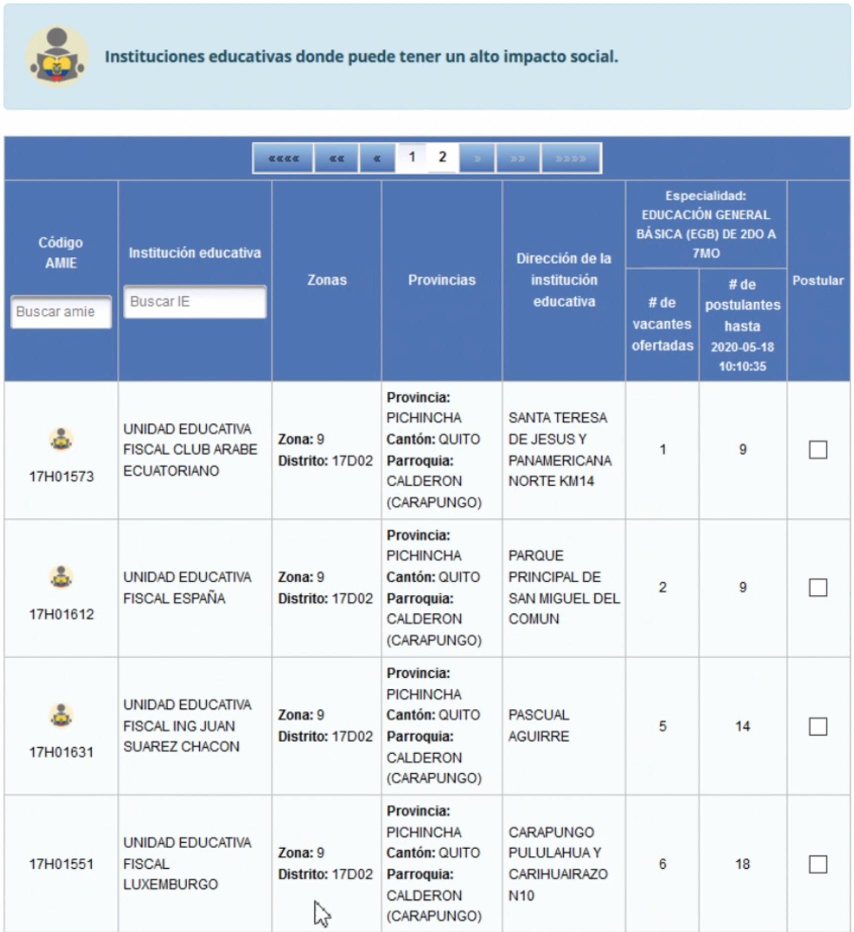
8 Appendix (Figures)

Figure 1: Platform Screenshot - School list



Instituciones educativas donde puede tener un alto impacto social.

«««« «« « 1 2 » »» »»»»

Código AMIE (Buscar amie)	Institución educativa (Buscar IE)	Zonas	Provincias	Dirección de la institución educativa	Especialidad: EDUCACIÓN GENERAL BÁSICA (EGB) DE 2DO A 7MO		Postular
					# de vacantes ofertadas	# de postulantes hasta 2020-05-18 10:10:35	
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 2: Platform Screenshot - Final Screen

Source: Ministry of Education, Ecuador

Figure 3: Platform Screenshot - Control versus Treatment

(a) Control (b) Treatment

9 Appendix (Tables)

Table A1: Treatment effect on preferences and assignment - Full sample

	Percentage of understaffed schools in choice set		Understaffed school at 1st choice		At least one understaffed school among first 2 choices		At least one understaffed school among first 3 choices		Assigned to understaffed school		Accepted offer in understaffed school	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Treatment	0.006**	0.006*	0.026***	0.026***	0.024***	0.023***	0.023***	0.022***	0.005	0.005	0.004	0.004
	(0.003)	(0.003)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean (Control group)	29.8	29.8	27.7	27.7	43.2	43.2	53.3	53.3	14.3	14.3	13.9	13.9
N	18133	18133	18133	18133	18133	18133	18133	18133	18133	18133	18133	18133

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Percentage of understaffed schools in choice set"**: number of understaffed schools as a percentage of the total number of choices of a given teacher. **"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. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. **"Accepted offer in understaffed school"**: takes a 1 if the teacher accepted an offer to work at an understaffed school. 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 A2: Treatment effect on preferences and assignment - Survey sample

	Percentage of understaffed schools in choice set		Understaffed school at 1st choice		At least one understaffed school among first 2 choices		At least one understaffed school among first 3 choices		Assigned to understaffed school		Accepted offer in understaffed school	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Treatment	0.015	0.014	0.048**	0.047**	0.037**	0.036**	0.030**	0.029*	0.042***	0.039**	0.036**	0.033**
	(0.010)	(0.010)	(0.018)	(0.018)	(0.017)	(0.016)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)	(0.016)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean (Control group)	42.2	42.2	40.5	40.5	60.4	60.5	72.8	72.9	22.8	22.9	22.5	22.6
N	2956	2956	2956	2956	2956	2956	2956	2956	2956	2956	2956	2956

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Percentage of understaffed schools in choice set"**: number of understaffed schools as a percentage of the total number of choices of a given teacher. **"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. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. **"Accepted offer in understaffed school"**: takes a 1 if the teacher accepted an offer to work at an understaffed school. 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 A3: Treatment effect on number of understaffed vacancies

	Number of understaffed schools in choice set	
	(1)	(2)
Treatment	0.068**	0.064**
	(0.032)	(0.031)
Controls	No	Yes
Mean (Control group)	2.1	2.1
N	5760	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 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 A4: Heterogeneous effects - Alternative measure of attention

	Percentage of understaffed schools in choice set	Understaffed school at 1st choice	At least one understaffed school among first 2 choices	At least one understaffed school among first 3 choices	Assigned to understaffed school	Accepted offer in understaffed school
Treatment	0.007	0.035*	0.018	0.019	0.024	0.016
	(0.011)	(0.020)	(0.019)	(0.019)	(0.019)	(0.019)
Attentive	-0.010	-0.040	-0.033	-0.009	-0.022	-0.019
	(0.018)	(0.036)	(0.036)	(0.031)	(0.033)	(0.033)
Treatment*Attentive	-0.010	0.080	0.057	0.003	0.070	0.076
	(0.023)	(0.049)	(0.039)	(0.045)	(0.047)	(0.047)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean (Control group)	42.3	40	60.7	73.3	23.2	22.9
N	2617	2617	2617	2617	2617	2617

Notes: Robust standard errors in parentheses; * p < 0.10 ** p < 0.05 *** p < 0.01. **"Percentage of understaffed schools in choice set"**: number of understaffed schools as a percentage of a teacher's total number of choices. **"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 one if at least one of the first three choices in a teacher's choice set is an understaffed school. **"Assigned to understaffed school"**: takes a 1 if the teacher was assigned to an understaffed school. **"Accepted offer in understaffed school"**: takes a 1 if the teacher accepted an offer to work at an understaffed school. The description of the final 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.

10 Appendix (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?

- 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?

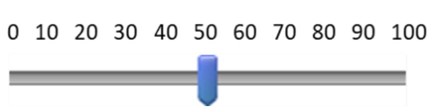
   Schools with this icon  were:

    - 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

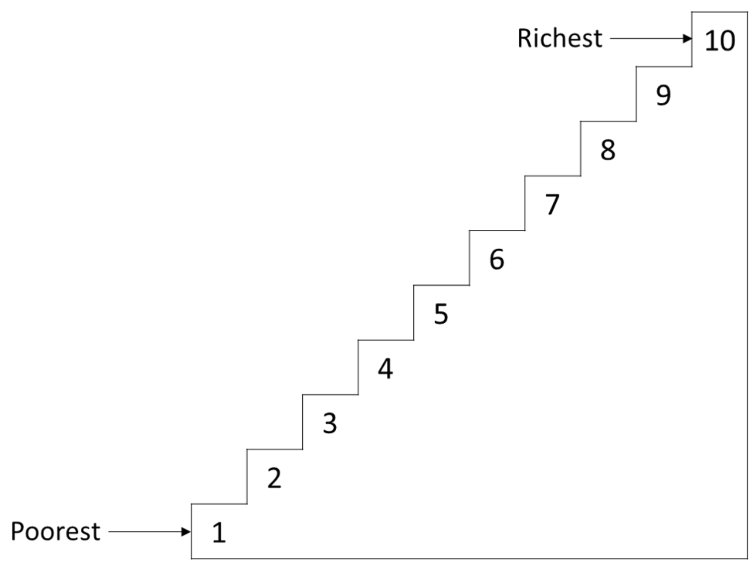
11. We would like you to imagine the following hypothetical situation: Suppose that today you unexpectedly received $100. How much of this amount would you donate to a good cause? Use the that slider to indicate an amount between $0 and $100:

    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)?

11 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?

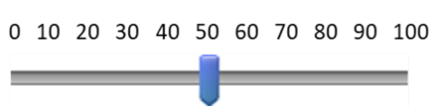
Las Instituciones Educativas con este ícono  eran:

- 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

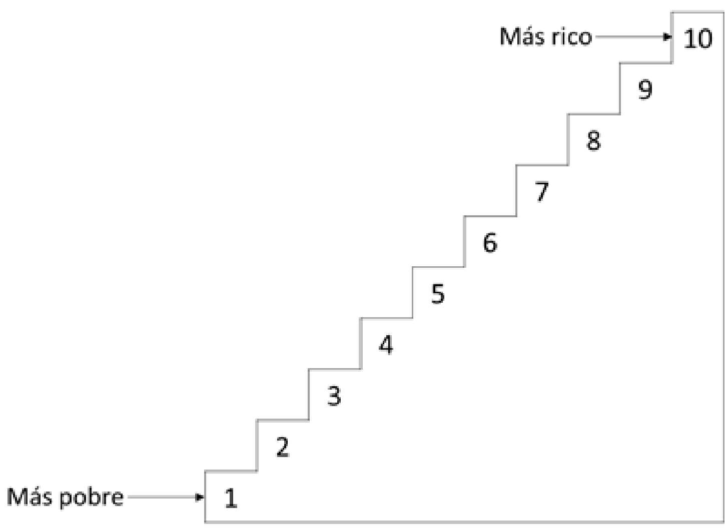
11. Quisiéramos que imagine la siguiente situación hipotética: Suponga que hoy, de forma inesperada, recibe 100 dólares. ¿Que cantidad de este monto donaría a una buena causa? Use el control deslizante que para indicar una cantidad entre 0 y 100 dólares:

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)?