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# The Potential of Smart Matching Platforms in Teacher Assignment: The Case of Ecuador

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This paper studies the potential of personalized "smart" information interventions to improve teacher assignment results in the context of a centralized choice and assignment system (CCAS) in Ecuador. Specifically, we focus on the impact that a personalized non-assignment risk warning, coupled with a list of "achievable" teaching position recommendations, had on teacher applications in the "I Want to Become a Teacher" selection process. We study the causal effect of the intervention on teachers' school choices, assessing its impact on the equilibrium probability of being assigned and on the overall results of the selection process, both in terms of the percentage of filled vacancies and the selection scores of assigned teachers. We find that treated teachers, in equilibrium, are much more likely to modify their application and obtain an assignment. This result highlights the potential of similar information interventions in other contexts. We furthermore present evidence that the intervention led to increased overall assignment rates and selection scores.

# 1 Introduction

Making a "good" or optimal choice is a difficult task, particularly when faced with information frictions. Providing agents with personalized information can facilitate the decision-making process. Such informational interventions are potentially beneficial not only at the individual level (by bettering people's outcomes) but also at the system level (by improving efficiency). The effects of informational interventions have been studied in the context of school selection (Arteaga et al., 2021; Cohodes et al., 2022; Weixler et al., 2020; Andrabi et al., 2017), financial choices (Saez, 2009; Duflo and Saez, 2003), health care (Kling et al., 2012), and consumer behavior (Allcott and Rogers, 2014; Jin and Leslie, 2003), with researchers widely concluding that they can have a low-cost, positive impact on the decision-making process. Moreover, it has been shown that the contact method and intervention design details are of considerable import.

This paper explores the role of information in the context of teacher job markets. Teachers may prefer to work close to where they grew up or live, in urban areas, or in schools with specific characteristics such as higher enrollment, better infrastructure, and more socioeconomically advantaged students (Bertoni et al., 2019; Boyd et al., 2005; Reininger, 2012). These common preferences can lead to inefficiencies in the job market: many candidates cannot secure a vacancy in more attractive schools, which are in high demand, while slots in other schools, often vulnerable and remote, go unfilled. Indeed, such positions may remain vacant despite the existence of candidates who might have been willing to apply had they known this would increase their chances of obtaining a job. We test a low-cost intervention that both provides teachers with information aimed at increasing their chances of securing a position, and seeks to improve system-level assignment outcomes (i.e., better the scores of assigned teachers and the number of filled positions).

The intervention was implemented in Ecuador as part of the "I Want to Be a Teacher" (*Quiero Ser Maestro*; QSM) program, which assigns teachers to schools through a centralized choice and assignment system (CCAS) that uses a deferred acceptance algorithm (Gale and Shapley, 1962).[1] Specifically, to better inform teacher candidates who

[1] Centralized choice and application system (CCAS) refers to algorithmic assignment processes that take applicant preferences and priorities into account in allocating available vacancies (see www.ccas-project.org). Elacqua et al. (2020) identify several advantages of this kind of system for teacher assignment: (i) a potentially sharp reduction in search costs, (ii) increased transparency in assignment criteria, thanks to the use of scoring systems that facilitate the prioritization of teachers with higher potential, and (iii) efficiency improvements relative to teacher preferences, due to assignment algorithms well-suited

participated in the Ecuador's 2021 selection process, the latter received a personalized report via WhatsApp and email containing a summary of their application.[2] For candidates whose estimated risk of not being assigned was "high" (above a defined cutoff level), the report also included a non-assignment risk warning and a list of recommended schools where they had higher chances of securing a position.[3] We evaluate the impact of the intervention on teachers' submitted ranked ordered lists (ROLs), and assess the equilibrium effect on their probability of assignment.

To this end, we use a regression discontinuity design that allows us to estimate the causal effect of providing teachers with information about their non-assignment risk and possible schools to which they could apply. Similar to Arteaga et al. (2021), the running variable is defined as estimated non-assignment risk and the cutoff is set to 30%. Additionally, after the end of the application period but before results were distributed, we conducted a survey aimed at measuring applicants' opinions on different dimensions of the process, as well as assignment beliefs and their knowledge of available alternatives within their area of specialization.

We find that receiving the warning and school vacancy recommendations increased the probability of changing their ROL by 52%.[4] The effect on the equilibrium chances of being assigned to a school is a 37% difference at the discontinuity. As explained in Section 6, this result is an equilibrium effect in that it is impacted by changes in the applications of all the program participants, both close to and far from the discontinuity. Additionally, the descriptive results presented in Section **??** suggest that the overall program results improved after the intervention, at least in part because the relatively high-performing candidates who received the personalized report and changed their application displaced lower-score applicants, or because additional positions were filled.[5]

to bettering horizontal matches between schools and teachers in a given context, potentially impacting teacher satisfaction and retention rates. The authors report that CCAS have been successfully implemented in recent years in a number of countries including France, Germany, Turkey, Peru, Portugal, and Ecuador. For additional evidence on the benefits of CCAS for teacher assignment systems see, for example, Pereyra (2013); Terrier (2014); Cechlárová et al. (2015); Dur and Kesten (2019); Combe et al. (2021).

[2]The personalized report was prepared using a personalized url and a responsive front-end design that was adapted to mobile devices.

[3]In other studies, such as Arteaga et al. (2021), the non-assignment risk intervention focuses on helping applicants to find and add more alternatives (and potentially re-order their portfolio). In this system, teacher candidates can only apply to a maximum of 5 schools. As a result, some needed to change their original application to improve their chances of obtaining a position.

[4]More precisely, these are the estimated effects of the RDD described in Section 5 and presented in Section 6; namely, the local average treatment effect at the 30% non-assignment risk threshold.

[5]Because the algorithm is based on candidates' selection assessment score and preference, these

Our study adds to the literature on information frictions by showing the positive impact of a low-cost informational intervention on teacher preference and assignment. The intervention also seems to have generated system-level efficiency gains. This is of particular importance, given that teachers are the most expensive schooling input and the greatest influential educational factor for student outcomes. Several papers have shown the effects of providing agents with information. For instance, in a similarly configured intervention, Arteaga et al. (2021) use "real-time" feedback on applicants' admissions probabilities in the context of student school choice in the Chilean CCAS to study the effect of non-assignment risk warning pop-ups and SMS/WhatsApp messages on submitted ROLs and assignment probability. The authors find that this real-time feedback led families to add more schools and increased their likelihood of assignment to a more preferred school,[6] both on the order of a 20-25% increase relative to applicants with a similar non-assignment risk that did not receive the warning.

In a similar vein, other work has shown that information about the characteristics of available choices can guide individuals to make better decisions. For example, Hastings and Weinstein (2008) and Allende et al. (2019) demonstrate that when lower-income families have access to information about school quality, they are more likely to select higher-performing schools.

Finally, our findings have important policy implications when it comes to reducing inefficiencies in teacher assignment and improving educational effectiveness. Indeed, teacher recruitment and assignment processes can be lengthy and costly (Allen, 2005). Yet, given that teachers have a strong and long-lasting impact on student outcomes (Rivkin et al., 2005; Kane and Staiger, 2008; Chetty et al., 2014a,b), school vacancies should ideally be filled in a timely manner and with the best possible candidates. These vacancies otherwise risk being assigned to less-qualified teachers through temporary contracts (Bertoni et al., 2020), which can have a negative impact on student achievement (Marotta, 2019). Reducing inefficiencies in teacher assignment can ultimately improve education quality if high-performing teacher candidates who are unable to obtain a position due to congestion in their preferred schools are encouraged to apply to less in-demand establishments with unfilled vacancies or with slots that are instead filled by candidates with lower scores.

To improve equity and efficiency in teacher assignment, some policies use mone-

high-performing treated candidates were ranked higher by schools.

[6] Specifically, to a school included in their ROL in round 1 of the process. The paper, moreover, shows that treated applicants ended up assigned to better-quality schools at the end of the assignment process, with an increase of around 0.2 value-added standard deviations.

tary incentives to influence teacher preferences, though this has been found to have a small or non-significant effect (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). More recently, scholars have examined the impact of low-cost non-monetary interventions on teacher preferences. For instance, Ajzenman et al. (2020) evaluate an intervention aimed at attracting teacher candidates to rural and more vulnerable schools in Peru using behavioral nudges that cultivated their extrinsic and intrinsic motives for pursuing these alternatives. The nudges led to a 3.4% increase in the probability that a candidate included a vulnerable school in their choice set, and a 6% increase in the probability that the applicant would be assigned to one of these schools. Similarly, Ajzenman et al. (2021) assess an intervention in Ecuador that highlighted teaching vacancies in vulnerable schools within an application platform by ranking these vacancies first. The intervention increased the share of applicants that included these schools in their portfolio by almost 9% and raised their probability of assignment by 4%. We build on this literature by testing the effectiveness of a low-cost intervention that provides non-assignment risk information and direct recommendations to teacher candidates. Our results suggest that information that reduces search frictions can have a significant effect on teacher preferences and thus may complement other policies aimed at providing extrinsic or intrinsic incentives.

The paper is organized as follows. Section 2 describes the institutional context of the Ecuadorian teacher assignment system. Section 3 presents the design and implementation of the intervention. Section 4 provides descriptive statistics and Section 5 introduces the empirical strategy employed in the analysis. Section 6 discusses our results and Section 7 concludes.

## 2 The Ecuadorian Teacher Assignment System

Since 2013, the Ecuadorian Ministry of Education has implemented a centralized teacher selection and assignment program known as *Quiero Ser Maestro* (I Want to Be a Teacher; QSM). Here, we focus on the seventh annual intake to the QSM program (QSM7), which took place in 2021.

The QSM includes 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 and Westh (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.). To participate in the second phase, candidates are required to have passed the psychometric test and have scored a minimum of 70 percent on the knowledge exam.

In the "merits and public examination" phase, candidates are evaluated according to their academic and professional credentials (the merits portion). In the "public examination" portion, candidates are scored based on their performance in a mock class.[7] Candidates are required to obtain a minimum grade of 70 percent for this mock class in order to apply to job postings.

The total score of the merits and public examination phase is weighted at 35% for the merits portion and 65% for the public examination portion, as described in Table 5 in Appendix A. Additionally, candidates can also receive up to ten "bonus" points for meeting certain criteria, such as living in an indigenous community, having a disability, or residing in the same "educational circuit" where their preferred school is located.[8]

In the last phase, eligible candidates are given 10 days to apply to up to 5 open positions in schools located in any region of the country by submitting a ranked ordered list (ROL) on an online platform. Once candidates submit their application, they cannot change it during an initial 10-day period. However, after this application period is closed, candidates are allowed to go back to the platform and modify their preferences during a two-day validation period. In this validation phase, they have a single opportunity to add, delete, and change the order of their submitted choices.

ROLs and school rankings based on the candidates' final score are then processed using a deferred acceptance algorithm (Gale and Shapley, 1962). Candidates' final scores take into account the results obtained for each component listed in Table 5 in Appendix A and the computed bonus points based their choices.[9]

[7] This consists of a 40-minute teaching assessment in which the teacher demonstrates his/her teaching ability on a topic in his/her specialty.

[8] Appendix C provides details on the bonus score.

[9] It is important to note that the rationale behind the bonus scores is not made clear to applicants when they are applying, since they are assigned after the application period. Applicants also have little insight into those awarded to other candidates. This has two implications. First, as applicants do not know their exact final score, it is harder for them to assess their assignment probabilities for each vacancy and, therefore, to act upon these probabilities and change their choices. Moreover, not knowing their bonus scores makes it harder to obtain personalized feedback on assignment probabilities, which this intervention aims to address. The fact that candidates can only apply to up to 5 schools (as opposed to

Ecuador's teacher selection and assignment process has, over time, significantly improved. In 2019, for example, the country's Ministry of Education changed the QSM to allow candidates to apply directly to schools rather than school districts, reducing the margin of discretion (Drouet and Westh, 2020). There remain, however, inefficiencies in the selection process–some vacancies are congested while others (vulnerable and remote) have fewer applicants (Bertoni et al., 2020). In fact, Elacqua et al. (2021) show that 27% of vacancies went unfilled in the 2019 QSM program, and these were primarily in schools of low socioeconomic status. Our intervention aims to further diminish these inefficiencies and improve market outcomes via reducing information frictions.

# 3 Intervention

As discussed in the previous section, there were two stages to the QSM7 application process: the application stage, in which candidates could submit a single ranked list of their choices in a 10-day period, and a two-day validation stage during which they were able to modify their application. We implemented our intervention between these two stages. Specifically, a day before the application period ended, we processed the applications of teachers that had participated up to that point (or the "pre-validation applicants"), and used this information to provide them with a personalized report that included a summary of their application. Before the end of the validation period, 20.3% of contacted teachers opened the report. The applicants at risk of non-assignment also received a warning and a list of recommended schools. A template of the personalized report can be found in Figure 13 in Appendix D. Applicants with no risk of non-assignment received an introductory message, an invitation to visit the application interface (panels 13(a) and 14(e) respectively), and a summary of their application including the following information about each of the selected schools: location, distance from the candidate's home, type of financing, number of students, and number of vacancies (panel 13(b)). Applicants at risk of non-assignment received the same and, additionally, a warning along with a list of recommended schools with characteristics similar to those in the summary section (panels 14(c) and 14(d)).

an unlimited number of positions) makes this exercise of selecting preferences even more strategically challenging. This restriction clearly influences teacher behavior, as shown by the share of applicants applying to 5 schools (Figure 2 in Appendix B), and by the survey result presented in Figure 15 of Appendix E, which shows that 92% of the teachers that answered the survey would have liked to apply to more schools. We nevertheless leave the analysis of the effect of this 5-school restriction to future studies.

The intervention's two main groups thus consist of the candidates who received the sections with the warning and recommendations (treatment) and those who only got a summary of their application despite being close to the cutoff for having a risky application (control). We now turn to how we defined non-assignment risk and constructed the recommendation lists.

### 3.1 Non-Assignment Risk

To determine which applicants were at risk of non-assignment, we first estimated the empirical risk of non-assignment by simulating the partial assignment using the following procedure:

1. With one day remaining in the main application period and prior to the start of the validation period, we generated 200 assignment simulations[10] with 19,190 pre-validation applicants.[11] We also sampled 40%[12] of the 2,527 potential applicants who had not participated in the process at the time of the calculation. The main information used was their score and location.[13]

2. In the case of applicants who had already submitted an application, we considered the same choices in each simulation.

   Since we had no information on the choices of sampled applicants, we instead followed Arteaga et al. (2021) to match each sampled applicant with an existing applicant to impute choices. To match sampled applicants with pre-validation applicants, we drew a registered applicant that had not applied and found the

[10]Given the number of pre-validation applicants and the fact that, in nearly all iterations, a considerable fraction would either be assigned or remain unassigned based on their application score, the goal was to generate enough dispersion in the estimated risk to be able to implement a regression discontinuity design. To that end, as shown in Figure 3 in Appendix B, we ended up with around 5 applicants in each 0.5% risk bin around the discontinuity (200 simulations implies that we estimated risks in 0.5% intervals).

[11]The total program involved 22,015 eligible teachers, however, 2,527 candidates had not yet submitted their applications by August 4, when we processed the partial pre-validation data. Candidates whose applications were submitted after our cutoff were not considered as partial applicants. Additionally, we considered only candidates in specialties with at least 80 registered and 20 pre-validation applicants, leaving us with a total of 19,190 pre-validation applicants.

[12]This percentage was defined based on the guidance of policy-makers who, at the time, estimated that approximately 93% of all applicants would participate based on previous QSM programs. The participation rate ended up being significantly higher, implying that our risk estimation was somewhat conservative.

[13]Location has been extensively documented in literature as a key determinant of teachers' school preferences (Bertoni et al., 2019; Boyd et al., 2005; Reininger, 2012; Rosa, 2017).

"closest" pre-validation applicant with a similar score in the same specialty as follows:

   (a) All applicants within the same geographic unit of the applicant (and specialty) were considered. The geographical scales, in increasing order of size, were the circuit ("*circuito*"), canton, and province.[14]

   (b) Among the applicants drawn from the same geographic unit, we selected those of the same specialization and tercile score. If there were no applicants within the same tercile, we used the closest one(s).

   (c) Where there was more than one applicant in the same geographic unit and tercile (or the closest one), we selected the match randomly.

3. After generating preferences for all applicants in each simulation, we computed the final score of each applicant, equal to the sum of the merits and public examination score and the bonus. Though we had data for the first component, unfortunately neither we nor the applicants knew what bonus score each applicant would receive at each school. It was, however, possible to anticipate some of the bonus criteria described in Appendix C. In cases where bonus criteria could be identified in the registration data (e.g., when the applicant resides in the province where the school is located), we assigned bonus points. We also generated a random uniform bonus of between 0 and 10 points to represent bonuses that we could not identify in the available data, truncating final bonus scores to 10 in line with the rules of the process.[15]

4. Finally, following Gale and Shapley (1962), we ran a DA assignment algorithm for each simulation to compute the proportion of non-assignment of each prevalidation applicant.

   To summarize, that which varies between simulations are the sampled (nonpartial) applicants, the random bonus, and the preference imputing matches when more than one applicant met the matching criteria.

[14]The Ecuadorian system is split into two regimes, one for the interior (Sierra) and another for the coast (Costa). In addition to these regimes, the territory is divided into nine administrative zones, which are further divided into educational districts and educational circuits.

[15]We implemented this approximation as we did not have the data to more precisely simulate potential bonuses using previous program data. This implies that our risk calculations were less accurate than we would have ideally liked. That said, we could still compare applicants with this imperfect measure and identify participants who were more likely to be at risk of non-assignment.

For each pre-validation applicant, we used the 200 simulated assignments to compute the proportion of simulations in which they were not assigned to a position, generating a running variable for non-assignment risk. This allowed us to implement a regression discontinuity approach to study the impact of the information intervention, following Arteaga et al. (2021). We defined risky pre-validation applicants as those for whom 30% or more of the simulations resulted in non-assignment, using the same cutoff value as in Arteaga et al. (2021). In this sense, we have a sharp discontinuity scenario given that compliance with treatment is divided precisely at the 30% cutoff. Figure 3 in Appendix B shows the density of estimated risk for applicants who opened the personalized report, excluding sizable groups of applicants whose risk was evaluated as 0 and 100%, the inclusion of which would make the visualization of the distribution difficult for the 30% cutoff of interest. The figure shows that the density of the running variable is similar on both sides of the cutoff.

## 3.2 Recommendations

The objective of the recommendations was to use data from pre-validation applications to assist applicants with a low estimated probability of assignment with their search for alternatives where they would have a greater chance of obtaining a position. Specifically, risky applicants were pointed towards vacancies where the score of other applicants likely to be assigned (if any) was below their own.[16]

When generating the list of recommended schools for treated applicants, we did not consider the general equilibrium concern that some schools might end up very congested if recommended to many applicants. However, to reduce the concern of generating excessive congestion, and to learn about applicant preferences – particularly, whether they would be willing to apply to schools in other provinces – we did create recommendation lists that varied in the number of recommended schools in provinces other than that where the applicant resided. Additionally, we also selected some of the recommendations randomly within the group that met the criteria described below, and implemented a random recommendation list for some applicants, thus providing additional random variation for our recommendations.

[16]The rationale for not including recommendations for applicants below the 30% risk cutoff was that, given the 5 choice maximum imposed by the system, this could mistakenly lead to a higher non-assignment risk if they eliminated a lower-risk option from their portfolio. To include recommendations for the latter group, it would be necessary to implement a strategy that precisely communicates the non-assignment risk of each choice, which, as explained above, was not possible to precisely estimate.

The process was as follows:

1. We first selected all vacancies in which the cutoff score was below the applicant's score from a partial assignment, including all pre-validation applicants with identifiable bonus points (without sampled applicants).[17]

2. If there were more than 10 options, we selected 10 using the following criteria[18]:

   (a) For a quarter of the applicants, we included up to 4 recommendations within their province and the remainder were selected randomly among the feasible options for the applicant.

   (b) For the rest of the applicants, we included recommendations in their province of residence as well as elsewhere, stratifying recommendations using the following criteria:

      i. **Geographic criteria**
         - Options in the same province
         - Options in provinces included in the application
         - Options in other provinces

      ii. **Rural**
         - Rural schools
         - Urban schools

      These alternatives were sampled using the following procedure:

      i. 2 rural and urban alternatives in the applicant's province of residence (maximum of 4)

      ii. 2 rural and urban alternatives in other provinces included in the application (maximum of 8)

      iii. One rural and urban alternative in provinces other than those included in the application or the applicant's province of residence

      If fewer than 10 alternatives met the criteria, the remainder were sampled randomly from the options that were feasible for the applicant. Some applicants had fewer than 10 feasible alternatives. [19]

[17] The cutoff score of each program was calculated as the score of the last applicant admitted to the program in an assignment considering only the applications of teachers that had applied before the validation period, and before we implemented this procedure.

[18] 1,000 applicants (10.7%) were mistakenly processed twice in the recommendation algorithm, and some of them received more than 10 recommendations (but no duplicates). Specifically, 832 applicants (8.9%) received more than 10 recommendations.

[19] Specifically, 33% of the treated applicants were recommended fewer than 10 schools.

### 3.3 Treatment and Control groups

The personalized reports were sent via WhatsApp to applicants that provided a valid phone number during the process.[20] Additionally, some applicants received their personalized report via email. Applicants without phone numbers were prioritized for email delivery, while other applicants were selected randomly.[21]

The treatment and control groups were selected from among the 19,428 teachers who had passed the merits and public examination phase and submitted an application by the August 4 cutoff. From this group, we omitted teachers in specialties with fewer than 80 registered or 20 pre-validation applicants, which left us with 19,190 teachers. For our information treatment, we selected all the teachers from the above group who met two conditions: (i) those who, conditional on their partial application, had a high non-assignment risk (above 30%) and (ii) those with a high enough score to obtain an assignment in at least one school within their specialty as detailed in Section 3.2. Dropping applicants who did not meet the second condition left us with 14,810 teachers, which we label as the analysis group. In the analysis group, 9,334 had a high non-assignment risk and were thus assigned to the treatment group, with the remaining 5,476 assigned to the control group. A total of 3,653 (24.7%) teachers in the analysis group opened their personalized report. We term this group the "compliers." Of the compliers, 65.5% are from the treated group (which we call "treated compliers") and 35.5% are from the control group (termed "control compliers"). This means that 25.6% of teachers in the treatment group and 23.7% of those in the control group opened the personalized report.

To estimate the effect of our information treatment, we focus exclusively on compliers because we want to study the impact of the information intervention among comparable teachers who received or did not receive the treatment. Finally, by the end of the validation period, which coincides with the end of the QSM, 3 of the pre-validation applicants dropped their application and 2,388 new eligible teachers submitted an application. Hence, we define post-validation applicants as the group consisting of pre-validation applicants who continued until the end of the process, plus the new applicants.

[20]Some of these numbers were validated through complementary communications inviting registered teachers to apply.

[21]The government had a restriction on the number of emails that could be sent each day and emails had to be sent from the official government account.

# 4 Data

We use administrative data from the registration and application process of the 2021 QSM7 program collected by the Ecuadorian Ministry of Education. The data include individual records of teachers' registrations and choices as well as school-level data with information on vacancies.

## 4.1 Individual-Level Data

The dataset contains information on candidates' socio-demographic characteristics (gender, marital status, date and place of birth, ethnicity), residential address, area of specialization, score on the merits and public examination phase by category, and ranked school preferences.

Table 1 presents descriptive statistics of the registered applicants. Column (1) shows the statistics for all eligible applicants (i.e., all the teachers who passed the examination phase): 72% of applicants are female, 9% belong to an ethnic minority (non-mestizo), 55% are married, 7% hold a master's degree, and 43% have more than 5 years of work experience. The most common specialization to which candidates applied was basic general education from second to seventh grade; this accounts for 22% of all eligible applicants. The province of Guayas, where the large city of Guayaquil is located, is the most common region and comprises 14% of total applicants. On average, teachers are 39 years old and scored about 65 points in the merits and public examination phase. We see in column (2) that the statistics are similar for pre-validation applicants.

Columns (3) to (5) present the same statistics for compliers (see Section 3.2) who opened the personalized report. The distributions are similar to columns (1) and (2) except that the shares described in the previous paragraph are slightly higher. Specifically, 52% of compliers have more than 5 years of experience, 29% applied to the most common specialization, and their average score in the merits and public examination phase was 67 points. Finally, columns (4) and (5) compare the characteristics of compliers in the treatment and control groups. Control compliers are, on average, more educated and more experienced, and have higher scores (which correlates with lower risk) than treated compliers. Additionally, the share of females is higher in the treated group. The differences between both groups are not surprising, and, as explained below, these differences are lower around the discontinuity threshold between the treated and control groups, which is the relevant test for our identification strategy.

Table 1: Summary statistics

| | (1) All eligible applicants | (2) Partial applicants | (3) Compliers | (4) Treated compliers | (5) Control compliers |
|---|---|---|---|---|---|
| Total | 22015 | 19190 | 3653 | 2392 | 1261 |
| Share female | 0.72 | 0.73 | 0.74 | 0.77 | 0.69 |
| Share non-mestizo | 0.09 | 0.09 | 0.10 | 0.09 | 0.11 |
| Share married | 0.55 | 0.55 | 0.57 | 0.58 | 0.55 |
| Share with master degree | 0.07 | 0.07 | 0.09 | 0.07 | 0.12 |
| Share with more than 5 years of experience | 0.43 | 0.44 | 0.52 | 0.48 | 0.60 |
| Share in the most common specialty | 0.22 | 0.22 | 0.29 | 0.29 | 0.29 |
| Share in the most common province | 0.14 | 0.15 | 0.16 | 0.14 | 0.19 |
| Mean age | 38.57 | 38.69 | 38.70 | 38.69 | 38.70 |
| Mean score | 64.62 | 64.69 | 67.15 | 64.64 | 71.91 |

**Note:** Eligible applicants are the teachers who passed the merits and public examination phase. Partial applicants are the teachers who had a personalized report available, that is the ones who applied before the validation period and applied to specialties with at least 80 registered and 20 partial applicants. Compliers are the ones who opened the personalized report. The most common specialty is basic general education from second to seventh grade. The most common province is Guayas.

## 4.2 School-Level Data

The school data provide information on the locations, specializations offered, and available vacancies for each school. The dataset contains a total of 3,345 schools, 33 specialties, and 8,057 vacancies. To generate recommendations, we only consider specializations with at least 80 registered and 20 pre-validation applicants, leaving us with 19,190 pre-validation applicants, 24 specializations, and 8,009 vacancies in our main sample. Table 6 in Appendix A shows the shares of pre-validation applicants by specialty.

## 4.3 Outcomes

We are mainly interested in estimating the effect of the information intervention described in Section 3 on candidates' choices during the validation period (probability of changing the application and/or adding new schools). Additionally, we assess how the intervention affected the actual equilibrium assignment at the end of the QSM.

Table 2 shows that treated compliers change their application twice as often as those in the control group. Specifically, 65% of treated compliers changed their original application and 35% added vacancies from the list of recommendations. In contrast, 29% of control compliers added a vacancy to their application and only 6% would have added a vacancy from the list of recommendations had they received one.[22]

The final assignment of treated compliers increased from 14% before the validation period to 20% after, while that of control compliers went from 99.6% to 91%. Note that these shifts are due both to changes in the compliers' applications as well as the entry of new applicants who applied to vacancies after the cutoff date.[23] Interestingly, among the treated compliers who received an assignment at the end of the process, 61% were assigned to one of the recommended alternatives. This may indicate the importance of such recommendations, a topic we return to in Section 6.

Table 2: Summary statistics of outcomes within the analysis group

| | **(1)** **Treated compliers** | **(2)** **Control compliers** | **(3)** **Mean difference** |
|---|---|---|---|
| Total | 2392 | 1261 | |
| Any modification (%) | 68.02 | 35.37 | 32.65*** |
| | | | (0.02) |
| Add any (%) | 65.34 | 28.71 | 36.64*** |
| | | | (0.02) |
| Add any from recommendations (%) | 35.58 | 6.03 | 29.55*** |
| | | | (0.01) |
| Partially assigned (%) | 13.92 | 99.68 | -85.76*** |
| | | | (0.01) |
| Finally assigned (%) | 20.32 | 90.96 | -70.64*** |
| | | | (0.01) |
| Assigned in recommendation (%) | 12.46 | 0.79 | 11.67*** |
| | | | (0.01) |

**Note:** *** p <0.01, ** p <0.05, * p <0.1. Standard errors in parentheses. Compliers are the ones who opened the personalized report. Treated refers to teachers that received the warning and the list of recommended schools. Control refers to teachers that received only the summary of the applications. Column (3) shows a mean difference test.

[22]Using the procedure described in section 3.2, we can also identify the list of recommendations that would have been included if candidates with non-risky applications had received them. It is important to note that this is possible because we did not include general equilibrium effects into the recommendation-generating procedure, meaning that including these applicants would not have affected recommendations for treated applicants (and vice versa).

[23]As explained above, candidates initially had 10 days to apply for vacancies (the application period). We generated the simulations and produced personalized reports for candidates who sent their applications no later than one day before the end of the 10-day period (so-called pre-validation applicants). The new applicants are those who completed their applications at the very end of the application period and, therefore, did not receive a personalized report.

# 5 Empirical Strategy

To explore the impact of providing teachers at risk of non-assignment with information, we rely on a regression discontinuity strategy. The underlying assumption is that observations that lie close to either side of the threshold are, on average, similar in all their characteristics except for treatment status.

Formally, treatment is assigned as shown in equation 1, where $z_i$ represents the risk of applicant $i$ and $c$ represents the 30% predicted non-assignment risk cutoff.

$$T_i = \mathbb{1}\{z_i \geq c\} \tag{1}$$

Figure 4 in Appendix B confirms that the probability of treatment rises sharply at the discontinuity. Consequently, as shown by Imbens and Lemieux (2008), for a given outcome of interest $Y_i$, the estimated impact of the treatment at the discontinuity point is given by:

$$\tau = lim_{z \downarrow c}\mathbb{E}[Y|z = c] - lim_{z \uparrow c}\mathbb{E}[Y|z = c] \tag{2}$$

In this setting, an appropriate econometric model to estimate the impact of the intervention is

$$Y_i = \beta_0 + \beta_1 T_i + h(z_i) + \varepsilon_i \tag{3}$$

Where $Y_i$ represents the choice of an applicant, $\beta_1$ is the estimator of the treatment effect of the information intervention on that choice, and $h$ is a continuous function of $z_i$. We specify $h$ as linear and quadratic following Gelman and Imbens (2019).

The main shortcoming of the regression discontinuity design is that we are only able to identify the treatment effect at the discontinuity, which implies that we cannot simply extrapolate estimates to the complete population of interest.

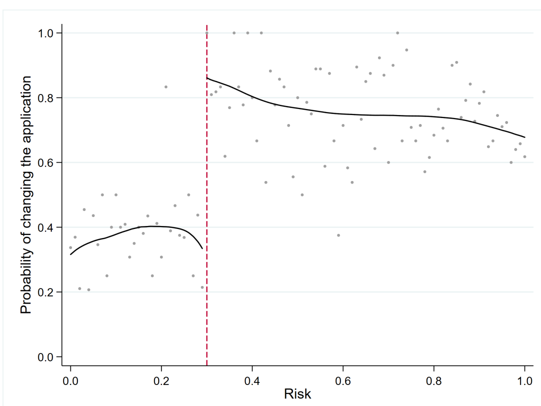
# 6 Results

## 6.1 Baseline Results

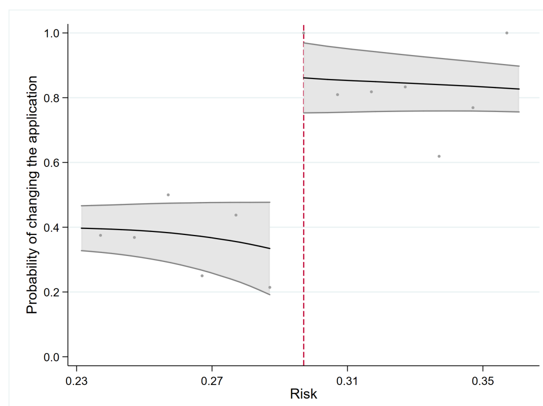
We are primarily interested in assessing the extent to which the treatment induces changes in reported preferences during the validation period. While we also explore the effects on assignment, these depend on the equilibrium (see Section 3). The latter, in turn, depends on applicants both close to and far from the discontinuity, and even on those who did not open their personalized reports but did alter their ROL. Figure 1(a) plots the probability of changing the application at different risk levels. We observe a clear discontinuity at the 30% non-assignment risk, with a large and statistically significant upwards shift in the probability of changing the application at the threshold. Figure 1(b) confirms the statistical significance of the jump at the threshold by showing the same plot with confidence intervals within the optimal non-assignment risk bandwidth (using the one common MSE-optimal bandwidth selector following Imbens and Kalyanaraman (2012), which minimizes the mean squared error).

Figure 1: RDD results on application changes during the validation period

((a)) Probability of changing application




((b)) Probability of changing application with CI




**Note:** Figure (a) plots the probability of an applicant changing their application using linear polynomials. Figure (b) plots the same but within the optimal BW and with confidence intervals. Total observations: 3,653. The bin that contains 0 consists of 727 observations. The bin that contains 1 consists of 1,078 observations. The remaining bins consist, on average, of 18.7 observations. The size of the bins is 0.01.

Figures 5(a) and 5(b) in Appendix B show the same plots for the probability of obtaining an assignment. We observe that treated applicants close to the discontinuity had a large and statistically significant increase in their probability of obtaining an assignment relative to the control group. Figure 7 in Appendix B compares pre- and post-validation period applications, graphically demonstrating that this difference is an equilibrium result. Specifically, we see that the difference in assignment probability at the discontinuity is a result of a drop in assignment probability for applicants on the left of the discontinuity (both close and far), and an increase among applicants on the right of the discontinuity that is concentrated among the applicants with a higher estimated non-assignment risk. This suggests that treatment, on average, induced changes in candidates' applications that helped them to obtain an assignment at equilibrium.

Next, we confirm the graphical evidence by formally estimating the effects of the information intervention using alternative RDD specifications. Table 3 shows the main results for the outcomes of interest. We report 5 models with different specifications and optimal bandwidths. Model (1) is estimated using a parametric approach with a linear interaction and the bandwidth is calculated using the one common MSE-optimal method following Imbens and Kalyanaraman (2012), which minimizes the mean squared error. Model (2) is the same as model (1) except that it calculates two different bandwidths above and below the cutoff instead of one common bandwidth. Model (3) also relies on a parametric regression with linear interaction, but the bandwidth selection is calculated using the one common CE-optimal method following Calonico et al. (2020), which minimizes the coverage error of the interval estimator. Model (4) is the same as model (3), but estimates two different bandwidths on either side of the threshold. Finally, model (5) is estimated using a parametric approach with quadratic interaction and the one common MSE-optimal selector bandwidth. All models use robust standard errors and the total observations in the optimal bandwidth are reported. Though the results are relatively consistent across different bandwidths, we will focus on estimates from model (1) as it is the most standard in the literature.

We observe that choice behavior changed due to the warning and recommendations. Conditional on opening the personalized report, receiving the warning increased the likelihood that applicants would change their application by 52%.Specifically, the probability of adding a preference to the application increased by about 59%. Treated teachers added, on average, 0.4 schools after the validation period, while control teachers added an average of 0.1 schools. Moreover, conditional on having added a preference, the probability of adding any of the schools recommended in the personalized report increased by 43% (when compared to what the recommendations would have been for control applicants using the same process to generate recommendations). This aligns with the fact that most of the teachers were not entirely sure about all the schools they wanted to apply to, as shown in Figure 14 in Appendix E on the survey results.[24] That is, recommendations seemed to help treated teachers to learn about new schools they had not considered before.

[24]As mentioned in the introduction, the survey was implemented after the application period, but before the results of the QSM were published. See Appendix E for more information about the survey.

Table 3: RDD Results

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Panel A. Any modification** | | | | | |
| RDD estimate | 0.519*** | 0.521*** | 0.751*** | 0.648*** | 0.860*** |
| | (0.130) | (0.124) | (0.168) | (0.136) | (0.180) |
| Left BW | 0.069 | 0.065 | 0.046 | 0.043 | 0.069 |
| Right BW | 0.069 | 0.236 | 0.046 | 0.157 | 0.069 |
| Total observations in BW | 170 | 357 | 111 | 229 | 170 |
| **Panel B. Add any** | | | | | |
| RDD estimate | 0.591*** | 0.515*** | 0.796*** | 0.602*** | 0.780*** |
| | (0.114) | (0.106) | (0.140) | (0.113) | (0.159) |
| Left BW | 0.090 | 0.084 | 0.060 | 0.056 | 0.090 |
| Right BW | 0.090 | 0.285 | 0.060 | 0.189 | 0.090 |
| Total observations in BW | 216 | 440 | 137 | 283 | 216 |
| **Panel C. Add any from recommendations** | | | | | |
| RDD estimate | 0.427* | 0.562* | 0.697*** | -0.017 | 0.754 |
| | 0.248 | 0.315 | 0.261 | 0.459 | 0.581 |
| Left BW | 0.064 | 0.051 | 0.044 | 0.035 | 0.064 |
| Right BW | 0.064 | 0.234 | 0.044 | 0.160 | 0.064 |
| Total observations in BW | 75 | 216 | 36 | 149 | 75 |
| **Panel D. Assigned** | | | | | |
| RDD estimate | 0.371*** | 0.352*** | 0.365** | 0.470*** | 0.324* |
| | (0.124) | (0.119) | (0.157) | (0.153) | (0.188) |
| Left BW | 0.101 | 0.089 | 0.067 | 0.059 | 0.101 |
| Right BW | 0.101 | 0.263 | 0.067 | 0.174 | 0.101 |
| Total observations in BW | 248 | 416 | 170 | 264 | 248 |
| **Panel E. Assigned in recommendation** | | | | | |
| RDD estimate | 0.347*** | 0.343*** | 0.356*** | 0.331** | 0.310** |
| | (0.084) | (0.109) | (0.119) | (0.146) | (0.127) |
| Left BW | 0.148 | 0.091 | 0.102 | 0.063 | 0.148 |
| Right BW | 0.148 | 0.144 | 0.102 | 0.099 | 0.148 |
| Total observations in BW | 241 | 173 | 162 | 125 | 241 |

**Note:** *** p <0.01, ** p <0.05, * p <0.1. Robust standard errors in parentheses. This table reports parametric estimates using different strategies to calculate the optimal bandwidth and different types of polynomials. (1) is estimated using a linear polynomial and the BW is calculated using the "one common MSE-optimal". (2) is estimated using a linear polynomial and the BW is calculated using the "two different MSE-optimal" that calculates two different BW below and above the cutoff. (3) is estimated using a linear polynomial and the BW is calculated using the "one common CER-optimal" bandwidth selector. (4) is estimated using a linear polynomial and the BW is calculated using the "two different CER-optimal" that calculates two different BW below and above the cutoff. (5) is estimated using a quadratic polynomial and the BW is calculated using the "one common MSE-optimal" method. All estimates control for specialty, sex, marital status, and region. Panel C is conditional on having added something to the application. Panel E is conditional on having been assigned.

The equilibrium outcome of treatment on final assignment at the discontinuity shows that treatment was helpful even after other applicants edited their application during the validation period. Teachers who received the treatment ended up being 37% more likely to obtain an assignment than those that did not. Additionally, those who obtained a position were 35% more likely to be assigned to one of the recommended schools. The survey results are consistent with people evaluating the information received in the personalized reports and acting upon it. Specifically, 82% of respondents said they wanted more information about their assignment chances. In addition to this, teachers rated the information received in the personalized report at 8.22 on average, on a scale of 1 to 10 (see Table 13 of Appendix E).

In general, the estimates are robust to the different specifications we use in Table 3. As an additional robustness check, we test the sensitivity of our main specification (model 1) to different arbitrary bandwidths. Table 7 in Appendix A shows that if we vary the bandwidth between 0.1 and 0.3, our estimates lead to the same conclusions. The probability of modifying preferences (and ultimately obtaining an assignment) is significantly larger for treated applicants close to the discontinuity.

To assess the validity of the regression discontinuity design, we test the balance on covariates on either side of the threshold. Table 8 in Appendix A shows the same estimates as Table 3 using the covariates as outcomes. They are consistently not significant with the exception of the marital status variable in model 5. This implies, in general, that observable characteristics are similar in the neighborhood of the cutoff, suggesting that the identifying assumptions are met. Graphic evidence is reported in Figure 8 in Appendix B.

Additionally, we further assess the validity of the estimates by introducing a placebo test. To check whether there is any significant effect when we know that there should not be, we use arbitrary fake cutoffs at the 0.5 and 0.2 non-assignment risk levels. Figure 9 in Appendix B shows that there are no unexpected discontinuities at these cutoffs. These results, combined with the covariates test, suggest that the positive effects we find are caused by the information intervention.

We do not study the content of the recommendations in this paper, leaving such analysis for future research. Our main goal in introducing variation in recommended alternatives was to study teacher preferences, and more specifically to be able to identify variation in consideration sets (i.e., schools known and of interest to a specific applicant). Though we do not extensively analyze these results here, Table 9 in appendix A nonetheless presents evidence suggesting that, as expected, recommendations were more likely to be included when the institution was either in the province where the candidate resided, or in the same province as one of the preferences given in the pre-validation ranking. Additionally, rural institutions are marginally less preferred and male teachers are marginally more likely to add a recommendation (both coefficients show a 1% change in the respective probability).

## 6.2 Heterogeneous Effects

In this section, we explore whether certain factors related to applicant characteristics can explain or amplify our results. To this end, we estimate our RDD model allowing for heterogeneous effects of teachers' gender, marital status, skill level, and experience. We then estimate a specification based on equation (3) in which treatment is interacted with these characteristics. Panel A of Table 4 shows the results of the same 5 models described in Section 6.1 for the probability of changing the application. Similarly, Panel B of Table 4 presents the findings for the difference in equilibrium assignment at the discontinuity, where we observe significant differences only for skilled and experienced teachers.

Specifically, our results suggest that males were more affected by the treatment in terms of their likelihood of modifying their application, but that the difference in equilibrium assignment is smaller (with a non-statistically significant coefficient). This suggests that the assignment results depend on the overall behavior of applicants and not only on application changes.

As expected, married people seem to be less affected by the intervention, likely because they are more restricted by location (e.g., places where their spouse can find better work opportunities) and may therefore be less willing to change their original choices.[25] However, the coefficients of the interaction between treatment and marital status are not significant.

To explore the potential role of skills, we interact the treatment variable with a dummy variable that identifies whether a teacher has a score above the median in the public examination portion of the total score (which evaluates specific skills). The results suggest that skilled teachers are no more likely than others to change their original application after treatment. However, the equilibrium assignment difference for highly skilled teachers close to the discontinuity is larger, an unsurprising outcome given that these teachers had more potential recommendations thanks to their higher

[25] The direction of the interactions change across bandwidth specifications, so we focus again on our preferred model from column (1).

scores. Nevertheless, it is important to note that the non-interacted effect on the difference in equilibrium assignment remains economically large (around 20% or more), and that it is also statistically significant in specifications (2) and (4), implying that treated "unskilled" teachers also had better assignment chances than unskilled teachers in the control group.

Similarly, we look at the role of experience by interacting the treatment variable with a dummy variable that indicates whether an applicant has worked for more than 5 years as a teacher. As shown in Table 8 and Panel D of Figure 8, there is no change in experience at the discontinuity and, as we can see in Panel A of Table 4, also no significant effect on the probability of changing the application. However, we do find that the difference in equilibrium assignment is smaller for experienced teachers, as shown in Panel B of table 4, arguably explained by the large negative correlation between experience and public examination scores (-0.29).

Table 4: Heterogeneous effects

| **Panel A. Heterogeneous effects: Any modification** | | | | | |
|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** |
| **Male** | | | | | |
| Treated | 0.496*** | 0.456*** | 1.014*** | 0.650*** | 0.824*** |
| | (0.144) | (0.137) | (0.191) | (0.148) | (0.193) |
| Male | -0.149 | -0.149 | -0.037 | -0.142 | -0.116 |
| | (0.142) | (0.120) | (0.155) | (0.148) | (0.146) |
| Treated*Male | 0.032 | 0.046 | -0.151 | -0.054 | 0.019 |
| | (0.175) | (0.137) | (0.180) | (0.167) | (0.173) |
| Left BW | 0.068 | 0.065 | 0.045 | 0.043 | 0.068 |
| Right BW | 0.068 | 0.241 | 0.045 | 0.160 | 0.068 |
| Total observations in BW | 170 | 353 | 94 | 232 | 170 |
| **Married** | | | | | |
| Treated | 0.534*** | 0.453*** | 0.898*** | 0.543*** | 0.828*** |
| | (0.181) | (0.159) | (0.241) | (0.180) | (0.217) |
| Married | 0.032 | -0.038 | 0.056 | -0.137 | 0.030 |
| | (0.141) | (0.129) | (0.158) | (0.159) | (0.140) |
| Treated*Married | -0.043 | -0.013 | 0.094 | 0.139 | 0.006 |
| | (0.175) | (0.137) | (0.197) | (0.171) | (0.176) |
| Left BW | 0.067 | 0.065 | 0.044 | 0.043 | 0.067 |
| Right BW | 0.067 | 0.254 | 0.044 | 0.169 | 0.067 |
| Total observations in BW | 170 | 371 | 94 | 239 | 170 |

**Table 4 continued from previous page**

| | | | | | |
|---|---|---|---|---|---|
| **High skilled** | | | | | |
| Treated | 0.566*** | 0.459*** | 0.923*** | 0.577*** | 0.903*** |
| | (0.170) | (0.156) | (0.212) | (0.170) | (0.224) |
| High skilled | 0.038 | 0.018 | 0.157 | -0.056 | 0.054 |
| | (0.151) | (0.128) | (0.186) | (0.162) | (0.158) |
| Treated*High skilled | -0.095 | 0.002 | -0.245 | 0.088 | -0.101 |
| | (0.163) | (0.130) | (0.193) | (0.157) | (0.166) |
| Left BW | 0.068 | 0.064 | 0.045 | 0.043 | 0.068 |
| Right BW | 0.068 | 0.247 | 0.045 | 0.164 | 0.068 |
| Total observations in BW | 170 | 358 | 111 | 232 | 170 |
| **More than 5 years of experience** | | | | | |
| Treated | 0.371** | 0.441*** | 0.567*** | 0.645*** | 0.707*** |
| | (0.167) | (0.145) | (0.207) | (0.154) | (0.202) |
| Experienced | 0.014 | -0.143 | 0.087 | -0.062 | -0.005 |
| | (0.133) | (0.126) | (0.149) | (0.153) | (0.132) |
| Treated*Experienced | 0.214 | 0.080 | 0.236 | 0.009 | 0.236 |
| | (0.179) | (0.134) | (0.201) | (0.165) | (0.178) |
| Left BW | 0.068 | 0.064 | 0.045 | 0.043 | 0.068 |
| Right BW | 0.068 | 0.247 | 0.045 | 0.164 | 0.068 |
| Total observations in BW | 170 | 358 | 111 | 232 | 170 |
| **Panel B. Heterogeneous effects: Assignment** | | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| **Male** | | | | | |
| Treated | 0.412*** | 0.418*** | 0.413** | 0.508*** | 0.383** |
| | (0.130) | (0.116) | (0.163) | (0.149) | (0.191) |
| Male | 0.016 | -0.007 | 0.094 | 0.095 | 0.017 |
| | (0.120) | (0.119) | (0.158) | (0.167) | (0.123) |
| Treated*Male | -0.114 | -0.037 | -0.107 | -0.139 | -0.115 |
| | (0.147) | (0.130) | (0.187) | (0.188) | (0.148) |
| Left BW | 0.102 | 0.090 | 0.068 | 0.060 | 0.102 |
| Right BW | 0.102 | 0.263 | 0.068 | 0.175 | 0.102 |
| Total observations in BW | 248 | 422 | 170 | 264 | 248 |
| **Married** | | | | | |
| Treated | 0.421*** | 0.387*** | 0.408* | 0.473*** | 0.391* |
| | (0.154) | (0.146) | (0.211) | (0.182) | (0.212) |
| Married | 0.066 | 0.072 | 0.055 | 0.054 | 0.066 |
| | (0.099) | (0.106) | (0.141) | (0.158) | (0.100) |
| Treated*Married | -0.054 | -0.039 | -0.033 | 0.004 | -0.056 |

**Table 4 continued from previous page**

| | | | | | |
|---|---|---|---|---|---|
| | (0.134) | (0.119) | (0.184) | (0.175) | (0.134) |
| Left BW | 0.101 | 0.089 | 0.067 | 0.059 | 0.101 |
| Right BW | 0.101 | 0.263 | 0.067 | 0.174 | 0.101 |
| Total observations in BW | 248 | 416 | 170 | 264 | 248 |
| **High skilled** | | | | | |
| Treated | 0.196 | 0.254* | 0.254 | 0.390** | 0.179 |
| | (0.157) | (0.138) | (0.204) | (0.184) | (0.211) |
| High skilled | -0.065 | -0.022 | -0.111 | 0.026 | -0.069 |
| | (0.115) | (0.113) | (0.149) | (0.147) | (0.116) |
| Treated*High skilled | 0.281** | 0.179 | 0.203 | 0.153 | 0.283** |
| | (0.133) | (0.117) | (0.172) | (0.151) | (0.133) |
| Left BW | 0.101 | 0.089 | 0.067 | 0.059 | 0.101 |
| Right BW | 0.101 | 0.263 | 0.067 | 0.174 | 0.101 |
| Total observations in BW | 248 | 416 | 170 | 264 | 248 |
| **More than 5 years of experience** | | | | | |
| Treated | 0.563*** | 0.506*** | 0.398** | 0.588*** | 0.479** |
| | (0.157) | (0.129) | (0.181) | (0.162) | (0.201) |
| Experienced | 0.182* | 0.248** | 0.070 | 0.151 | 0.194* |
| | (0.098) | (0.101) | (0.131) | (0.140) | (0.104) |
| Treated*Experienced | -0.281** | -0.294** | -0.044 | -0.262* | -0.295** |
| | (0.130) | (0.115) | (0.165) | (0.156) | (0.135) |
| Left BW | 0.101 | 0.089 | 0.067 | 0.059 | 0.101 |
| Right BW | 0.101 | 0.263 | 0.067 | 0.174 | 0.101 |
| Total observations in BW | 248 | 416 | 170 | 264 | 248 |

**Note:** *** p <0.01, ** p <0.05, * p <0.1. Robust standard errors in parentheses. This table reports parametric estimates using different strategies to calculate the optimal bandwidth and different types of polynomials. (1) is estimated using a linear polynomial and the BW is calculated using the "one common MSE-optimal". (2) is estimated using a linear polynomial and the BW is calculated using the "two different MSE-optimal" that calculates two different BW below and above the cutoff. (3) is estimated using a linear polynomial and the BW is calculated using the "one common CER-optimal" bandwidth selector. (4) is estimated using a linear polynomial and the BW is calculated using the "two different CER-optimal" that calculates two different BW below and above the cutoff. (5) is estimated using a quadratic polynomial and the BW is calculated using the "one common MSE-optimal" method. All estimates control for specialty, sex, marital status, and region. Panel C is conditional on having added something to the application. Panel E is conditional on having been assigned.

We do not have enough statistical power to analyze heterogeneous effects related to the characteristics of the added recommendations, or the probability of adding a recommendation conditional on the heterogeneity in recommendation lists.[26]

[26] As shown in Table 3, the number of observations in the regressions of Panel C and Panel E are less than the others because the first is conditional on having added something to the application and the second is conditional on having been assigned. This reduces the statistical power of both regressions and increases the standard deviation.

## 6.3 System-Level Outcomes

We now descriptively explore the effects of the intervention on system-level outcomes such as the number of filled vacancies and the general quality of assigned teachers. Importantly, it should be noted that due to the large number of applicants relative to offered vacancies, most positions were filled even when only pre-validation applications were considered. Thus, with regard to the overall effect of the intervention, we should expect a relatively small impact in equilibrium.

As mentioned in Section 3.2, when generating recommendations we did not consider the general equilibrium concern that some schools might end up very congested if they were recommended to many applicants. That said, we did design the intervention to include many diverse recommended alternatives, so as to reduce the risk of generating excessive congestion at highly demanded vacancies. Our aim was also, as explained above, to better understand consideration sets and preferences. The negative spillover effects from the recommendations could have, in theory, increased the number of unassigned applicants, as well as potentially reducing the scores of assigned teachers.[27] However, descriptive evidence suggests that, although there was some congestion and a few teachers remained unassigned despite having added recommended schools to their applications, a much larger percentage of teachers who followed the recommendations were assigned.

We interpret this as a positive result despite the fact that the intervention was not designed with the general equilibrium consideration in mind. The total number of vacancies filled increased with the intervention, as shown in Table 10 of Appendix A. Column (1) considers only the initial applications submitted by pre-validation applicants before changes during the validation period plus the actual final application of teachers who applied only after the validation period. This column therefore represents how the assignment would have ended up if no one had made any changes in the validation period. Column (2) shows the actual scenario in which pre-validation applicants changed their preferences and some teachers only applied during the validation period.[28]

To explore the overall quality of the assigned teachers, we first analyze the scores of

[27]Because if high scoring teachers compete for the same vacancies, we may end up with a bi-modal distribution of assigned scores for vacancies in high and low demand.

[28]We would ideally want to study a counterfactual without treatment to ascertain the causal effect on the general equilibrium. However, we do not have an identification strategy to estimate who would have changed their application in the counterfactual scenario and how.

those who changed their application. Figure 10 of Appendix B presents the distribution of scores for teachers moving from partially non-assigned to assigned, as well as those moving from partially assigned to non-assigned. The mean scores of teachers that were assigned to a vacancy is 68.02, which is 1.77 points above those who did not receive an assignment. This provides preliminary evidence that the intervention may have helped increase overall assignment scores and, thus, the general quality of assigned teachers.[29]

Focusing on the assigned vacancies before and after the validation period, we observe in Figure 11 in Appendix B that the distribution shifts to the right. This shift becomes more pronounced (Panel B) when looking solely at vacancies that were assigned to different applicants before and after the validation period. Together with the overall increase in assigned vacancies, we interpret these results as positive, although not causal, evidence that the intervention had a positive impact on the equilibrium results of the QSM7.

# 7 Conclusion

This paper evaluates a low-cost information intervention in the context of Ecuador's centralized teacher assignment system. We show that teachers in the treatment group, who received and opened a non-assignment risk warning and a list of recommended schools, were much more likely to change their choices and add new schools to their applications. Ultimately, this translated into a significant difference in the equilibrium assignment of teachers close to the treatment cutoff, as also illustrated in Figure 7 of Appendix B comparing pre- and post-validation period equilibrium assignments.

Our results are robust to different specifications. Moreover, the findings point to a positive general equilibrium effect by improving both the average scores of teachers who obtained an assignment and total assignments,[30] even though we did not design

[29]Note that 1.77 points is a significant difference, representing an increase of 0.23 standard deviations in the evaluation scores of assigned teachers, and this from an extremely low-cost information intervention.

[30]Total assignments changed slightly when comparing the number of assignments with and without the changes in the applications of teachers who participated before the intervention, as shown in Table 10 of Appendix A. However, this does not necessarily mean that the intervention's potential to affect the total number of assignments is small. The magnitude of the effect, rather, depends on the overall congestion of the available alternatives. Congestion in the context of the QSM is considerable, with an average of over three applicants for each available vacancy and over 86% of positions filled. Moreover, some vacancies in specific specialties and schools may be structurally unappealing, making reaching a

the intervention to maximize spillover effects. Similar interventions that incorporate general equilibrium effects in their design might form the subject of future work.

It is important to note that our strategy identifies a local average treatment effect (LATE) on compliers (teachers who opened the personalized feedback report). This implies that our estimates do not extend directly to the whole population, or to compliers with a non-assignment risk level far from the 30% non-assignment risk cutoff. More research is needed to understand how these results would have changed if we had increased the compliance rate (e.g., with a more salient intervention), or if we had implemented a different cutoff (the impact of which could be explored, for example, with an RCT design).

The low-cost intervention studied in this paper has important policy implications, in that teachers are the most expensive and valuable educational input and significantly impact short- and long-term student outcomes (Chetty et al., 2014a,b). Improving the efficiency of teacher assignments will likely positively effect resource allocation and learner success. Moreover, centralized choice and assignment systems have been gaining popularity around the world as a tool for organizing student and teacher application and assignment processes. Information interventions will potentially play an important role in optimizing the results obtained through these systems. In this paper, we demonstrate the capacity of such interventions to affect teacher behavior, while other work shows their impact on student behavior in the context of school choice (see, for example, Arteaga et al. (2021).

Future studies might consider changes to the mechanism's rules. In the particular case of the QSM program, this could include assessing the effect of expanding or eliminating the restriction on portfolio sizes. These restrictions are often implemented to force applicants to limit their applications to a small number of relevant alternatives (i.e., schools where they would actually be willing to work). However, this limitation comes at the expense of introducing strategic considerations in the submitted preferences, particularly in contexts with high congestion such as the QSM7. In other settings, it has been shown that applicants face significant difficulties in formulating optimal application strategies (see, for example, Kapor et al. (2020)), which can also make information interventions more challenging. When the mechanism eliminates such strategic considerations, communication efforts (and information interventions)

goal of 100% unrealistic. The effect of the intervention on the aggregate also depends on the uptake of the intervention, which in this case was around 20%. This rate could be improved by introducing similar interventions directly within the application interface.

can focus on expanding searches and recommending that applicants apply to all the schools in which they would be willing to work, in the true order of their preference.

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# A Tables

Table 5: Pre-Bonus Scoring System

| Merits | | Oposition | |
|---|---|---|---|
| Criteria | Maximum score | Criteria | Maximum score |
| Academic Background | 20 | Specific knowledge test | 40 |
| Work experience | 10 | Mock lecture | 25 |
| Publications | 3 | | |
| Continuous training | 2 | | |
| **Total weight** | **35** | **Total weight** | **65** |

Table 6: Shares by specialties

| **Specialty** | **Share** |
|---|---|
| Basic General Education (Egb) From 2nd to 7th grade | 22.33 |
| Initial education | 17.54 |
| Mathematics Basic General Education (Egb From 8th To 10th grade) | 9.15 |
| Social Studies Basic General Education (Egb From 8th To 10th grade) | 6.96 |
| Entrepreneurship and Management General Unified High School (Bgu) | 6.72 |
| Natural Sciences Basic General Education (Egb From 8th To 10th grade) | 6.58 |
| Fip: Accounting | 4.62 |
| English | 3.59 |
| Education for Citizenship General Unified High School (Bgu) | 3.54 |
| Language and literature General Unified High School (Bgu) | 3.23 |
| Physical Education 2nd grade Egb to Bgu | 2.99 |
| Biology General Unified High School (Bgu) | 2.10 |
| Artistic and Aesthetic Education 2º grade Egb to Bgu | 1.93 |
| Fip: Computing | 1.83 |
| Special education | 1.36 |
| Chemistry General Unified High School (Bgu) | 1.01 |
| Fip: Agricultural production | 0.88 |
| History General Unified High School (Bgu) | 0.71 |
| Physics General Unified High School (Bgu) | 0.67 |
| Fip: Sales and Tourist Information | 0.54 |
| Fip: Electromechanics | 0.47 |
| Philosophy General Unified High School (Bgu) | 0.43 |
| Fip: Consumer electronics | 0.43 |
| Fip: Music | 0.40 |

Table 7: Sensitivity test

| | (1) BW 0.3 | (2) BW 0.25 | (3) BW 0.2 | (4) BW 0.15 | (5) BW 0.1 |
|---|---|---|---|---|---|
| **Panel A. Any modification** | | | | | |
| RDD estimate | 0.387*** | 0.388*** | 0.407*** | 0.429*** | 0.425*** |
| | (0.065) | (0.074) | (0.086) | (0.096) | (0.115) |
| Total observations in BW | 941 | 650 | 490 | 384 | 244 |
| **Panel B. Add any** | | | | | |
| RDD estimate | 0.437*** | 0.469*** | 0.515*** | 0.518*** | 0.538*** |
| | (0.064) | (0.072) | (0.084) | (0.092) | (0.109) |
| Total observations in BW | 941 | 650 | 490 | 384 | 244 |
| **Panel C. Add any from recommendations** | | | | | |
| RDD estimate | 0.661*** | 0.634*** | 0.604*** | 0.549*** | 0.449** |
| | (0.087) | (0.104) | (0.126) | (0.135) | (0.210) |
| Total observations in BW | 441 | 334 | 256 | 201 | 127 |
| **Panel D. Assigned** | | | | | |
| RDD estimate | 0.215*** | 0.286*** | 0.271*** | 0.317*** | 0.359*** |
| | (0.063) | (0.072) | (0.083) | (0.095) | (0.123) |
| Total observations in BW | 941 | 650 | 490 | 384 | 244 |
| **Panel E. Assigned in recommendation** | | | | | |
| RDD estimate | 0.491*** | 0.461*** | 0.422*** | 0.354*** | 0.361*** |
| | (0.063) | (0.071) | (0.079) | (0.085) | (0.119) |
| Total observations in BW | 680 | 442 | 330 | 256 | 161 |

**Note:** *** p <0.01, ** p <0.05, * p <0.1. Robust standard errors in parentheses. All columns only consider model (1) from table 3 using different arbitrary BW. All estimates control for specialty, sex, marital status, and region. Panel C is conditional on having added something to the application. Panel E is conditional on having been assigned.

Table 8: Balance test

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Panel A. Male** | | | | | |
| RDD estimate | -0.099 | -0.062 | -0.069 | -0.035 | -0.085 |
| | (0.098) | (0.091) | (0.129) | (0.119) | (0.150) |
| Left BW | 0.113 | 0.096 | 0.075 | 0.063 | 0.113 |
| Right BW | 0.113 | 0.221 | 0.075 | 0.147 | 0.113 |
| Total observations in BW | 267 | 384 | 180 | 245 | 267 |
| **Panel B. Non-mestizo** | | | | | |
| RDD estimate | -0.062 | -0.058 | -0.100 | -0.046 | -0.039 |
| | (0.066) | (0.070) | (0.073) | (0.076) | (0.098) |
| Left BW | 0.097 | 0.095 | 0.064 | 0.063 | 0.097 |
| Right BW | 0.097 | 0.219 | 0.064 | 0.145 | 0.097 |
| Total observations in BW | 238 | 366 | 152 | 245 | 238 |
| **Panel C. Married** | | | | | |
| RDD estimate | -0.151 | -0.153 | -0.245 | -0.156 | -0.430** |
| | (0.137) | (0.113) | (0.170) | (0.150) | (0.194) |
| Left BW | 0.103 | 0.099 | 0.068 | 0.066 | 0.103 |
| Right BW | 0.103 | 0.223 | 0.068 | 0.148 | 0.103 |
| Total observations in BW | 248 | 384 | 170 | 255 | 248 |
| **Panel D. Experienced** | | | | | |
| RDD estimate | 0.170 | 0.300* | 0.407 | -0.082 | 0.396 |
| | (0.180) | (0.155) | (0.260) | (0.215) | (0.281) |
| Left BW | 0.063 | 0.048 | 0.042 | 0.032 | 0.063 |
| Right BW | 0.063 | 0.190 | 0.042 | 0.126 | 0.063 |
| Total observations in BW | 152 | 276 | 94 | 180 | 152 |

**Note:** *** p <0.01, ** p <0.05, * p <0.1. Robust standard errors in parentheses. This table reports parametric estimates using different strategies to calculate the optimal bandwidth and different types of polynomials. (1) is estimated using a linear polynomial and the BW is calculated using the "one common MSE-optimal". (2) is estimated using a linear polynomial and the BW is calculated using the "two different MSE-optimal" that calculates two different BW below and above the cutoff. (3) is estimated using a linear polynomial and the BW is calculated using the "one common CER-optimal" bandwidth selector. (4) is estimated using a linear polynomial and the BW is calculated using the "two different CER-optimal" that calculates two different BW below and above the cutoff. (5) is estimated using a quadratic polynomial and the BW is calculated using the "one common MSE-optimal" method. All estimates control for specialty, sex, marital status, and region. Panel C is conditional on having added something to the application. Panel E is conditional on having been assigned.

Table 9: Determinants of the probability of adding a recommendation

| | (1) | (2) | (3) |
|---|---|---|---|
| **Coefficients of interest** | | | |
| Recommendation in teacher's province | 0.230*** | 1.320*** | 2.516*** |
| | (0.008) | (0.039) | (0.076) |
| Another province in pre-validation applications | 0.184*** | 0.962*** | 1.819*** |
| | (0.012) | (0.046) | (0.085) |
| Rural institution | -0.011*** | -0.065** | -0.120** |
| | (0.004) | (0.028) | (0.056) |
| Original application size | 0.003* | 0.018* | 0.036* |
| | (0.001) | (0.011) | (0.021) |
| $\frac{Score - Mean\ score\ specialty}{Sd.specialty}$ | -0.025*** | -0.210*** | -0.423*** |
| | (0.003) | (0.025) | (0.050) |
| Male | 0.014*** | 0.101*** | 0.167** |
| | (0.005) | (0.036) | (0.072) |
| **Number of observations** | 19,783 | 19,694 | 19,694 |
| **Controls** | | | |
| Number of recommendations | Yes | Yes | Yes |
| Specialty | Yes | Yes | Yes |
| Teacher's Province | Yes | Yes | Yes |

**Note:** *** p <0.01, ** p <0.05, * p <0.1. Robust standard errors in parentheses. This table reports estimates of the probability of adding one of the recommendations included in the personalized report, during the validation period. We include only teachers in the treatment group that opened the personalized report and include different controls to try to isolate the coefficient on recommendations in the applicant's province or other provinces included in the pre-validation applications, as well as on rural alternatives, in line with the process used to create the recommendations in the first place.
(1) is estimated using a linear probability model, while (2) is estimated using a probit model, and (3) is estimated using a logit model.

Table 10: Percentage of assignment

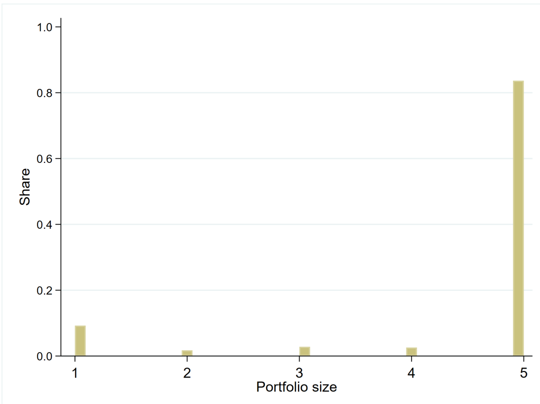
| | (1)<br>Partial assignment + new applicants | (2)<br>Final assignment |
|---|---|---|
| **Total assignment** | 6839 | 6904 |
| **Unfilled vacancies** | 1170 | 1105 |
| **Treatment compliers (% assignment)** | 9.07 | 20.32 |
| **Control compliers (% assignment)** | 96.83 | 90.95 |
| **New applicants (% assignment)** | 30.78 | 28.02 |

**Note:** Column (1) considers partial applicants and the new applicants that appeared after the validation period. Column (2) considers new applicants and partial applicants with modifications after the validation period.

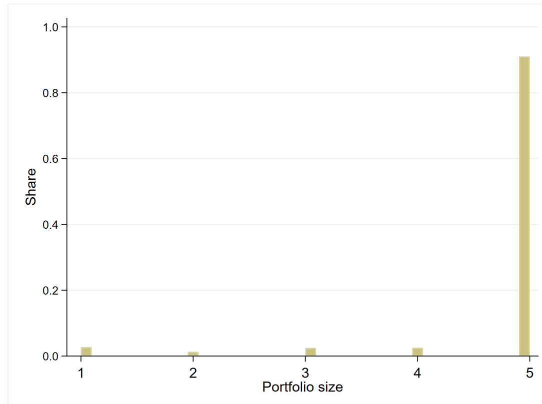
## B Figures

Figure 2: Portfolio size

((a)) Pre-validation period

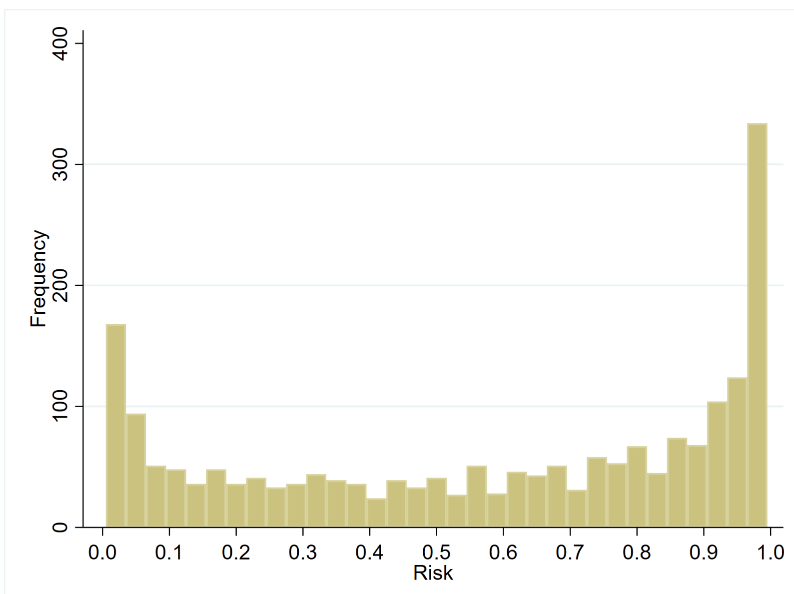



((b)) Post-validation period



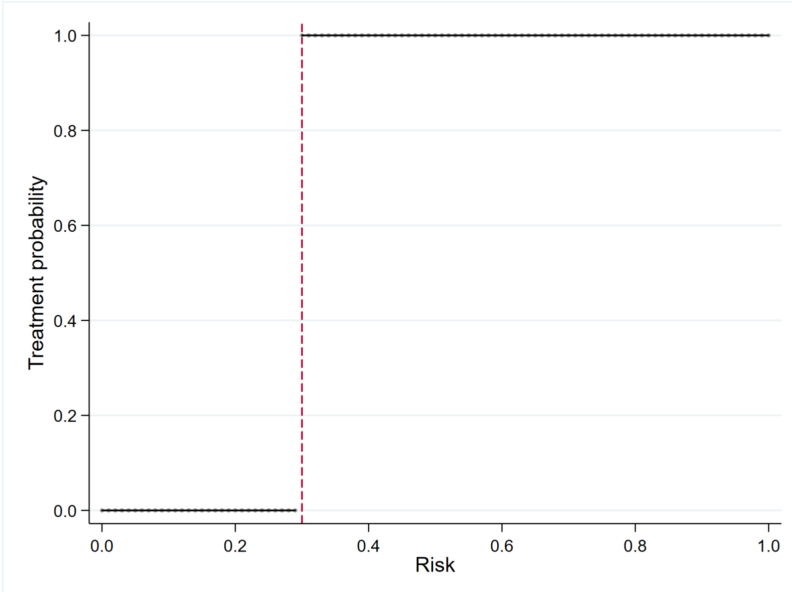

**Note:** Distribution of portfolio size pre- and post-validation period. The sample is limited to the applicants who received the personalized report.

Figure 3: Risk distribution for compliers




**Note:** N=2051. The extremes are omitted.

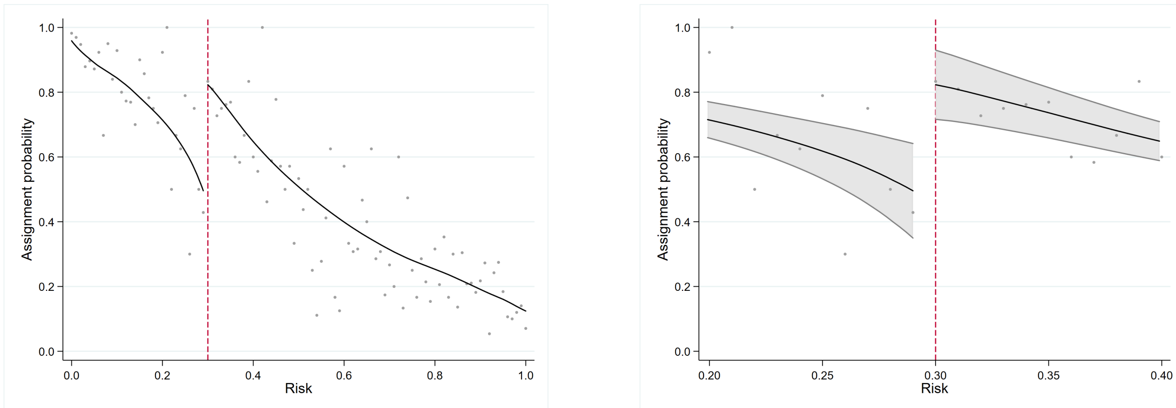
## Figure 4: Treatment Probability




**Note:** Total observations: 3653. The bin that contains 0 consists of 727 observations. The bin that contains 1 consists of 1078 observations. The rest of the bins consist, on average, of 18.7 observations. The size of the bins is 0.01.

Figure 5: RDD results on assignment probability

((a)) Assignment probability ((b)) Assignment probability with CI




**Note:** Figure (a) plots the probability of obtaining an assignment using linear polynomials. Figure (b) plots the same but within the optimal BW and with confidence intervals. Total observations are 3,653. The bin that contains 0 consists of 727 observations. The bin that contains 1 consists of 1,078 observations. The rest of the bins consist, on average, of 18.7 observations. The size of the bins is 0.01.

Figure 7: RDD on assignment probability comparing partial and final applicants

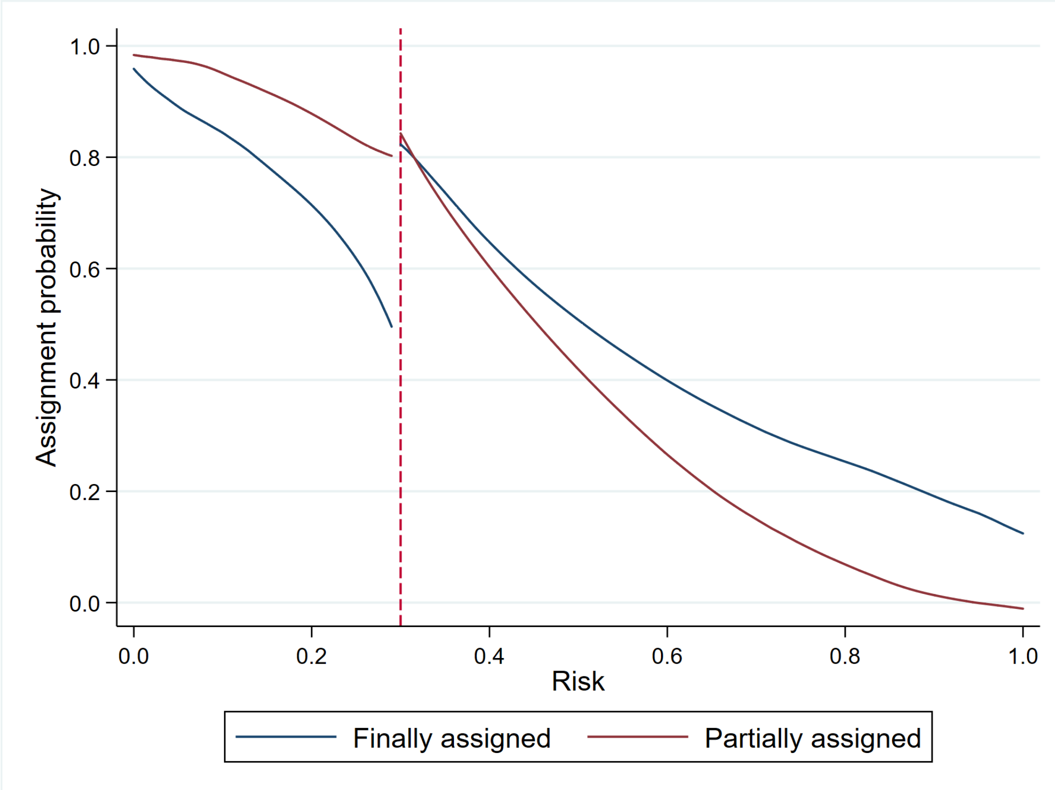
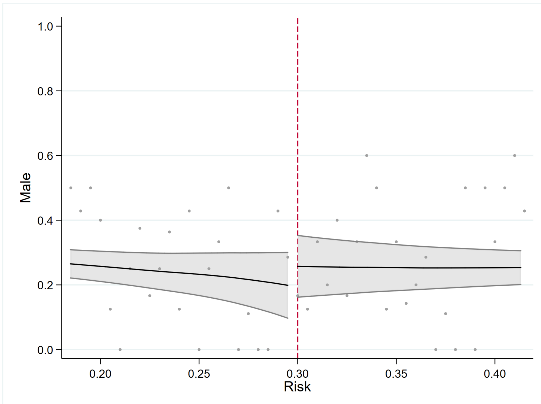
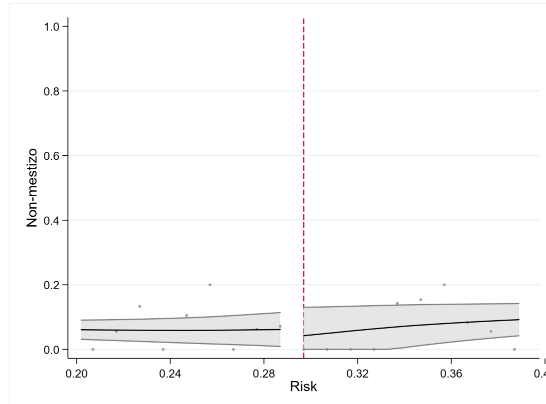


Figure 8: Balance

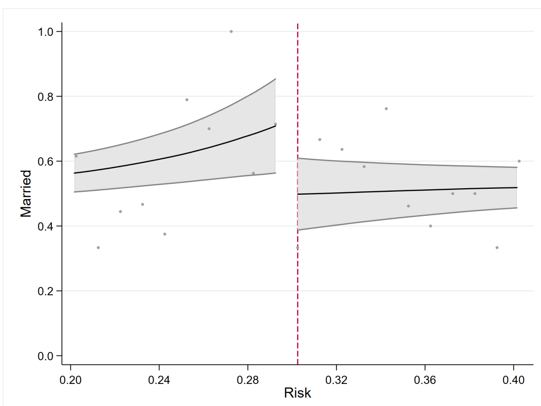
((a)) Male



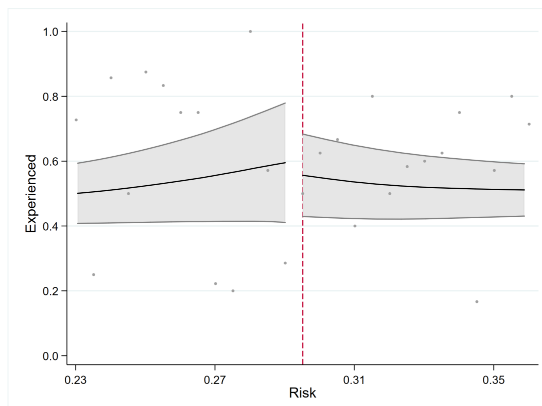

((b)) Married




((c)) Non-mestizo



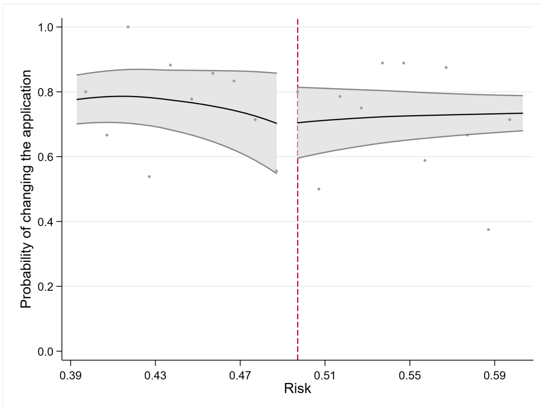

((d)) Experienced



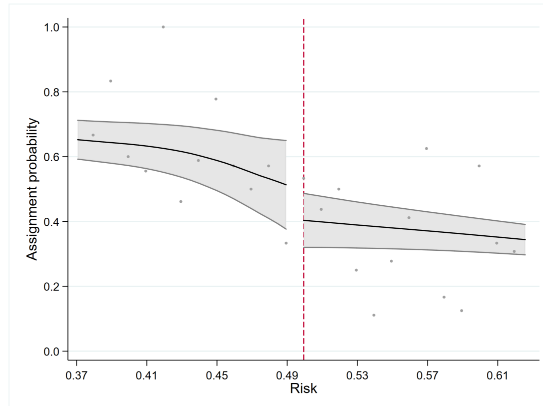

**Note:** Figure (a) plots the probability of being male. Figure (b) plots the probability of being married. Figure (c) plots the probability of being non-mestizo. Figure (d) plots the probability of being experienced.

## Figure 9: Placebo test

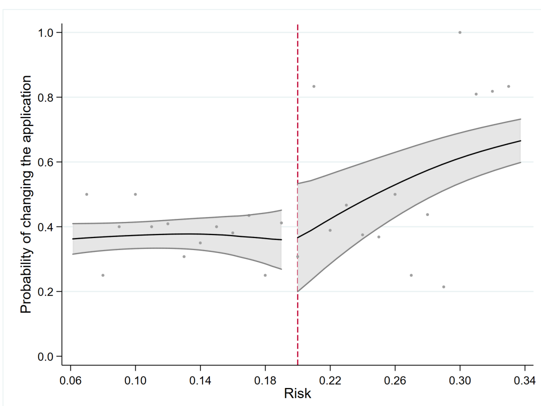
((a)) Probability of changing the application with fake cutoff at risk level 0.5



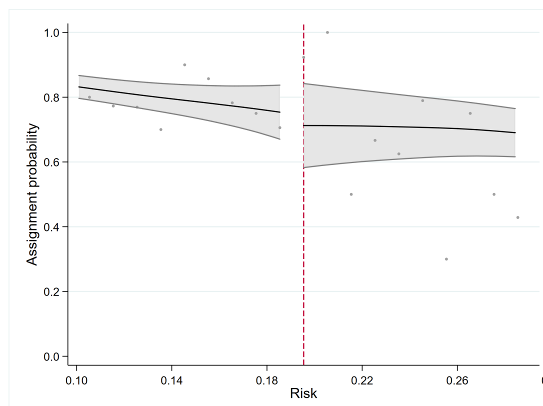

((b)) Assignment probability with fake cutoff at risk level 0.5




((c)) Probability of changing the application with fake cutoff at risk level 0.2




((d)) Assignment probability with fake cutoff at risk level 0.2




**Note:** Figure (a) plots the probability of changing the application with a fake cutoff at risk level 0.5. Figure (b) plots the assignment probability with fake cutoff at risk level 0.5. Figure (c) plots the probability of changing the application with a fake cutoff at risk level 0.2. Figure (d) plots the assignment probability with fake cutoff at risk level 0.2.

Figure 10: Quality of reassigned teachers

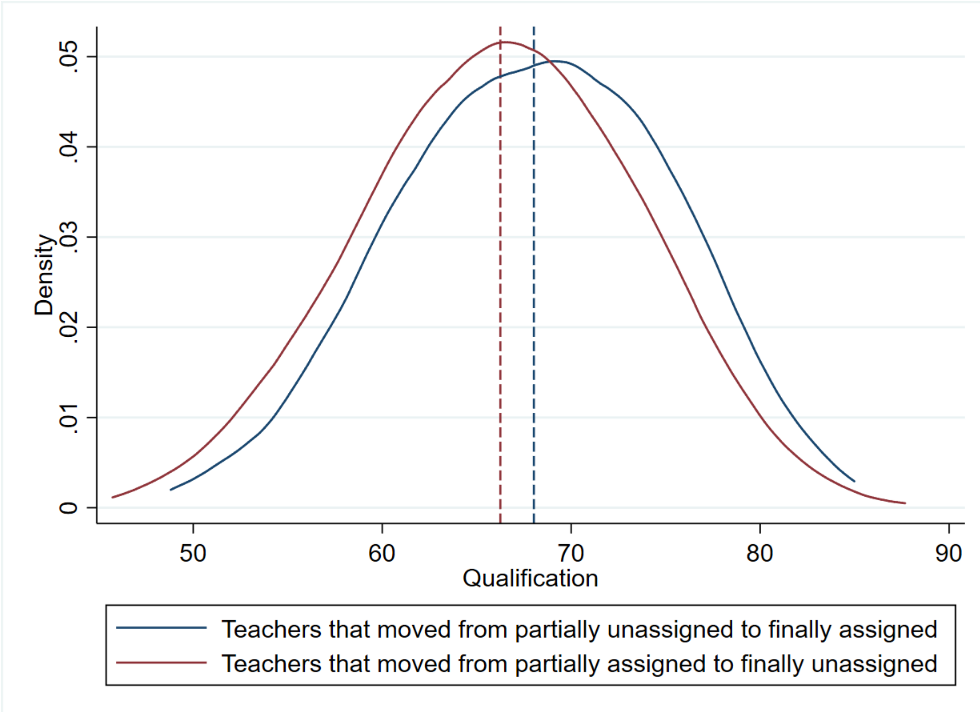
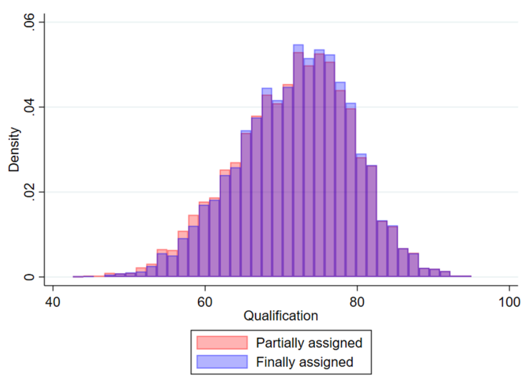
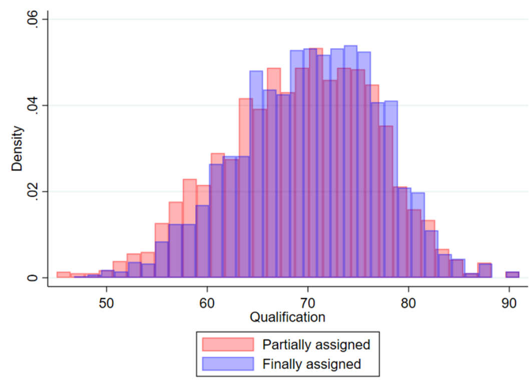



Figure 11: Scores of assigned applicants pre- and post-validation period

((a)) All vacancies assigned pre- and post-validation




((b)) All vacancies assigned to different applicants pre- and post-validation




**Note:** Figure (a) presents the distribution of scores for vacancies that had someone assigned both pre- and post-validation. Figure (b) presents only the vacancies where the assigned teacher is different in the post-validation assignment.

# C Description of the bonus score

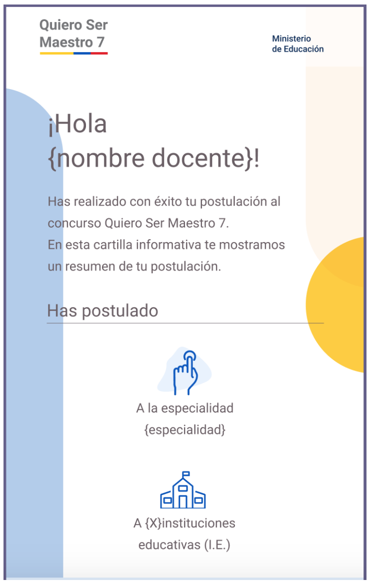
In the QSM7 contest, the bonus score was calculated using the following criteria:

1. 2 points for each of the following:
   - Applicants residing in the "educational circuit" where the institution offering the vacancy is located.
   - Applicants that present proof of a non-limiting disability.
   - Applicants currently residing abroad in "migration" status for at least one year.
   - Applicants choosing "fiscomisional" institutions (which are private institutions receiving government funds to complement public alternatives).
   - Applicants who already served their one year mandatory rural service.
   - Applicants from indigenous, Afro-Ecuadorian or Montubio ethnic groups.
   - Applicants demonstrating status as a "person returned to Ecuador."
   - Applicants residing in rural localities within a 40km radius of the Ecuadorian border.
2. 1 point for each of the following criteria:
   - Applicants currently serving under an occasional, definitive or provisional contract in public schools.
   - Applicants who are a "former community teacher."
3. Additional criteria:
   - 10% score bonus over the pre-bonus score for applicants demonstrating the status of "heroe" (hero) according to the corresponding law.
   - 5% score bonus over the pre-bonus score for applicants demonstrating the status of "former *combatiente*" (former combatant) according to the corresponding law.
   - 6 points to applicants residing in the Galápagos province and applying to a school within that province.

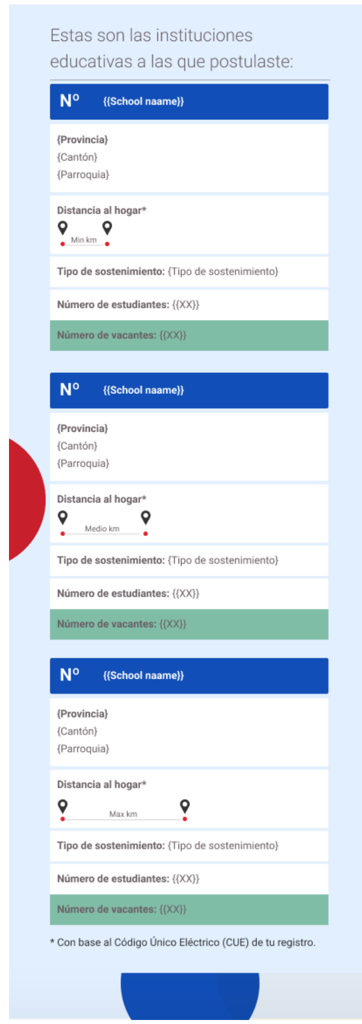
# D Personalized Report Outline

Figure 13: Personalized Report Outline

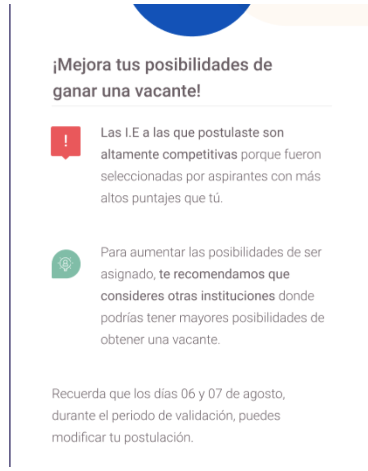
((a)) Section 1: Welcome




((b)) Section 2: Your Portfolio

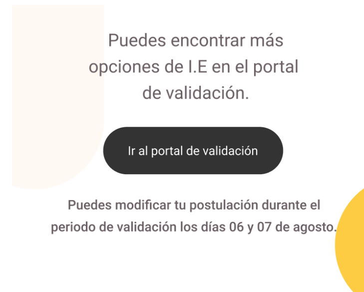


((c)) Section 3: Non-assignment Warning




((d)) Section 4: Recommendations


((e)) Section 5: Link to Application Webpage



# E Survey Results

The survey was implemented after the application period but before the results of the contest were published. It was distributed via email to all teacher candidates and aimed to measure different dimensions of the process, as well as beliefs regarding assignment and awareness of available alternatives within an applicant's specialization.

- 11,948 teachers participated in the survey. On average, they rated the application process at 6.96 on a scale of 1 to 10.

Table 11

| | Mean | Standard deviation | Total |
|---|---|---|---|
| **Vacancy search** | 6.85 | 2.49 | 11609 |
| **Information about educational institutions** | 6.93 | 2.47 | 11609 |
| **On average, which grade would you give to the application process?** | 6.96 | 2.38 | 11609 |

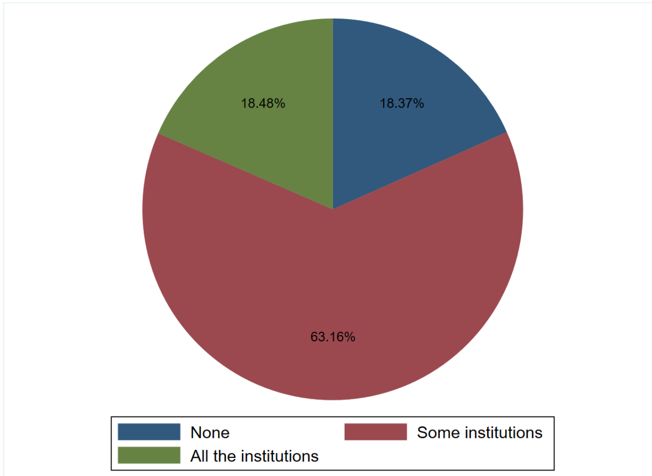
- If only the best teachers are considered (those who are above the 75th percentile of the distribution), the average of the evaluation rises to 7.17.

Table 12

| | Mean | Standard deviation | Total |
|---|---|---|---|
| **Vacancy search** | 7.05 | 2.45 | 2902 |
| **Information about educational institutions** | 7.14 | 2.44 | 2902 |
| **On average, which grade would you give to the application process?** | 7.17 | 2.28 | 2902 |

- Most of the teachers did not have a clear idea about the institutions to which they were going to apply: 18.3% did not have any institution in mind, 63% only had some in mind, and just 18.4% knew all or almost all of them.

Figure 14: Answer to the question: "Did you have in mind which educational institutions you wanted to work at?"



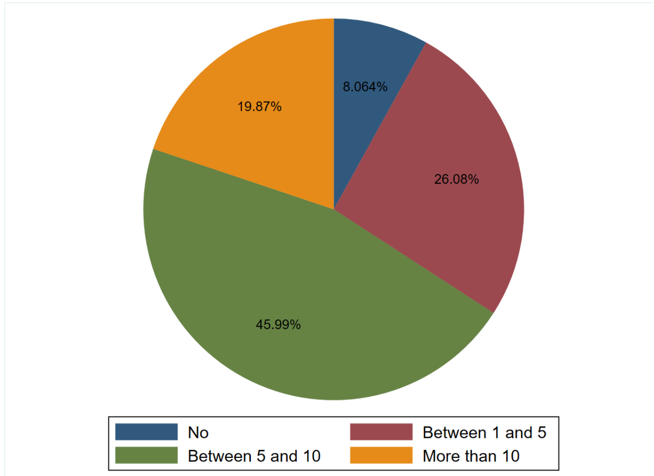

- 69% claim to have received the personalized report and, on average, they rated the report at 8.22 on a scale of 1 to 10.

Table 13

| | Mean | Standard deviation | Total |
|---|---|---|---|
| **Ease of access to the link** | 8.32 | 1.82 | 8067 |
| **Design and clarity of the personalized report** | 8.28 | 1.82 | 8067 |
| **Usefulness of the information presented** | 8.17 | 1.93 | 8067 |
| **Usefulness of the recommendations received** | 7.55 | 2.36 | 3286 |
| **Clarity of the message** | 7.70 | 2.30 | 3400 |
| **On average, which grade would you give to the personalized report?** | 8.22 | 1.86 | 8067 |

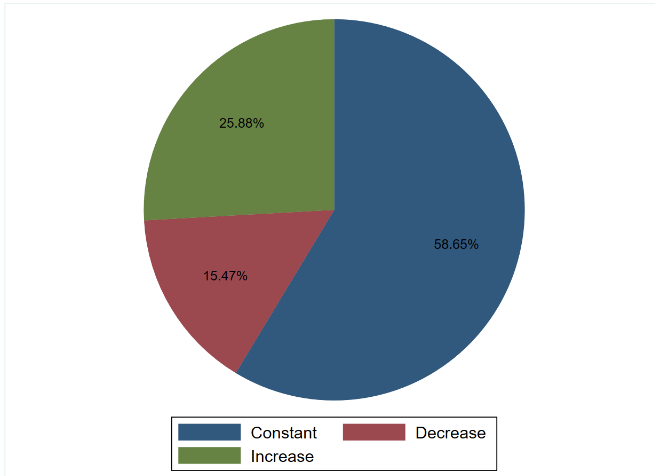
- For those who answered the question about what information they would like to receive in the personalized report, 34.8% stated that they would like to receive more information about the educational institutions to which they applied.
- 82% say they want more information about their chances of getting assigned.
- 15% want more information about the institutions they did not apply to.
- For those not assigned, 55% state they would have wanted more information about their chances of assignment.
- 91.94% of the teachers would have liked to apply to more educational institutions.

Figure 15



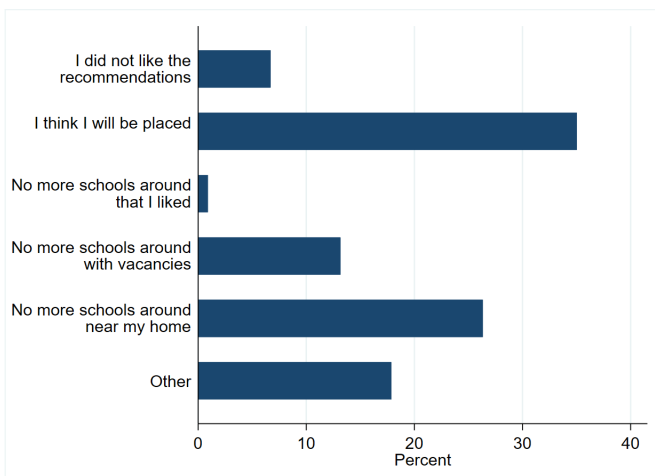

- 33% did not apply to their preferred educational institutions because they thought they would not get assigned. Of those, 58% said that they wanted more information about probabilities of assignment.
- The main reason why applicants applied for fewer than 5 options was because the system did not display more vacancies in their specialty.
- 16% stated that it was difficult to find other institutions to apply to.
- 2% preferred to not be assigned to a position rather than apply to the available alternatives.
- 13.1% were sure that they were going to get assigned; however, only 25% of these applicants were finally assigned.
- Most of the teachers did not change their beliefs after receiving the personalized report.

Figure 16



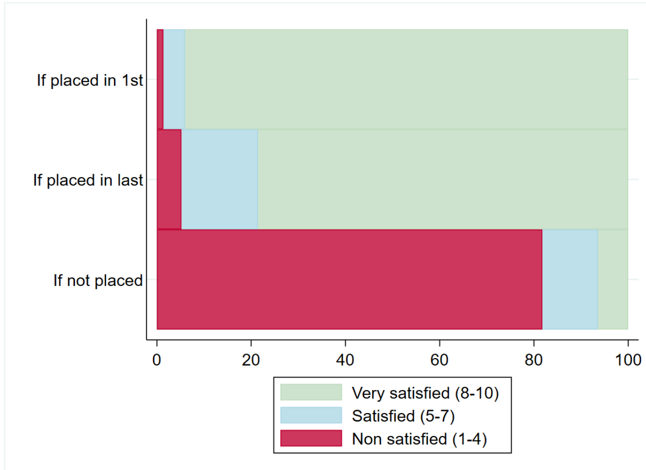

- Most of the teachers that did not change their application despite receiving the personalized report were confident about their assignment probabilities.

Figure 17




- Teachers were asked how satisfied they would feel if they were placed at the first-ranked school on their application, if they were placed at the last-ranked school, or if they were not placed. Most of the teachers stated that they would feel satisfied if placed at any option, while 81% would be unsatisfied with non-placement.

Figure 18


If placed in 1st
If placed in last
If not placed
0
20
40
60
80
100
Very satisfied (8-10)
Satisfied (5-7)
Non satisfied (1-4)