College Application Mistakes and the Design of Information Policies at Scale

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February 29, 2024

Abstract

In this paper, we present the results of a multi-year collaboration with policy-makers to design and evaluate whether information policies implemented at scale can effectively improve students' outcomes. Using a series of nationwide surveys, we find that 40% of students do not apply to their preferred college and major, and 10% of these students would have strictly benefited by including these programs. Upon these results, we implemented with the Ministry of Education of Chile a large-scale field experiment for college admissions, which included personalized information about program characteristics, students' admission probabilities, and alternative major recommendations. The intervention significantly reduced application mistakes, increasing the probability of assignment for unmatched students by 20% and the probability of improving the assignment of undermatched students by 38%. After scaling-up the policy, the intervention approximately doubled the matching probability for unmatched and undermatched students and tripled the enrollment likelihood for initially unmatched students.

The authors wish to thank *Ministerio de Educación* (MINEDUC) of the government of Chile and DEMRE for their support and collaboration throughout the research project. This project would not have been possible without the support of JPAL and ConsiliumBots. The authors would like to disclose the following: Tomás Larroucau and Ignacio Rios have previously collaborated with the centralized clearinghouse and the Ministry of Education in designing the assignment process. Christopher Neilson is the founder and CEO of ConsiliumBots, which currently holds active contracts with the Ministry of Education related to the school assignment process.

1 Introduction

The choice of college and major has been shown to be a significant contributor to earnings inequality over the life-cycle (Altonji et al., 2014). As a result, alleviating any frictions that limit students from getting access to their best choice of college and major is a top priority for policymakers worldwide. A growing body of empirical work shows that students are limited not only by credit constraints and application costs but also by an array of broadly defined information frictions that can lead to costly application mistakes that have long-lasting effects. This evidence, primarily based on small-scale interventions, suggests that efforts to guide students by providing targeted and personalized information represent a viable policy strategy to mitigate these barriers. Nonetheless, the effectiveness of such policies when applied at a large scale by the government remains unclear.

There are several reasons why the evidence from prior small-scale research studies may not carry over when the interventions are implemented at scale as a policy. First, the effectiveness of information policies may be dampened by equilibrium effects, such as congestion, that can arise when students update their application strategies in response to new information. Second, students may overreact or misinterpret the information provided when government authorities give it as part of the official process. Finally, if disadvantaged students turn out to be less likely to act on the information provided, the effectiveness of the policy may be limited and can even exacerbate inequality.

In this paper, we present the results of a multi-year collaboration with policy-makers to design and evaluate whether information policies implemented at scale can effectively improve students' outcomes. We draw upon the research on information frictions and human capital investment to broadly categorize four types of frictions that potentially lead to application mistakes and that information policies could tackle and alleviate at scale. These include (i) the lack of awareness about the existence of options, which may lead to *mistakes on awareness*; (ii) being wrong about the attributes of options that are known to exist, which may cause *mistakes on valuations*; (iii) having biased beliefs regarding the chances of admissions, potentially leading to *mistakes on admission probabilities* whereby students get unmatched (due to *over-confidence mistakes*) or undermatched (due to *under-confidence* or *ordering mistakes*); and (iv) not understanding the rules of the system (e.g., admission requirements, constraints on the length of the list, etc.) and the need to be strategic when deciding where to apply, which may lead to *mistakes by truth-tellers*. Throughout our five-year collaboration with the Ministry of Education of Chile (summarized in Figure 1), we designed and implemented large-scale surveys meant to progressively enhance our understanding of the incidence and drivers of application mistakes; field experiments to measure the causal effects of different information interventions; and a scale-up policy that integrated the insights gained throughout the process to assess the effectiveness of information policies at scale.

Based on our initial surveys, we show that a significant fraction of students misreport their true preferences and make payoff-relevant application mistakes. Specifically, we find that close 40% of students do not list their true top preference as their top preference. Moreover, close to 10% of these students face a strictly positive admission probability at this program and would have unambiguously increased the expected value of their application lists by reporting it as their top preference. Similarly, we find that close to 25% of students do not apply to programs they strictly prefer over being unassigned. From this group, between 5% and 10% of students would have strictly benefited from listing such programs in their application. By analyzing the elicited beliefs, we find that these mistakes are driven mainly by biases in beliefs about their admission chances, with optimistic students skipping safety programs and pessimistic students omitting their top true preference from their reported preferences. Consistent with previous literature, we find that these biases are more prominent among students from public schools and with lower scores. Thus, correcting mistakes can reduce inequalities in access.

To assess whether information policies can help mitigate these mistakes, we implemented a field experiment (in collaboration with MINEDUC) where we used students' initial reported preferences to create

Figure 1: Timeline of Interventions

personalized websites that students could access during the application process to obtain more information and potentially improve their application. These websites contained relevant information, ranging from general information about programs included in the student's application list, personalized information on admission probabilities and risk, and customized recommendations about other majors of potential interest. By randomizing the information shown to students, we use this intervention to evaluate the effects of reducing information frictions on different margins. We find that showing personalized information about admission probabilities and risk has a causal effect on improving students' outcomes. Indeed, compared to the control group, students who were initially unassigned were 38% more likely to enter the assignment and avoid being unmatched after receiving the information. On the other hand, students who were initially assigned were 20% more likely to improve in their preference of assignment after receiving the information and avoid being undermatched. Moreover, the effects persist in the long run, as students who were initially unassigned and entered due to the intervention double their probability to remain enrolled in the same program for at least two years. These effects suggest that our intervention effectively decreased the incidence of application mistakes, particularly *over-confidence* mistakes, improving students' outcomes. Finally, to disentangle how these results are affected by congestion and how they would scale when implemented as a policy, we exploit the properties of the centralized assignment mechanism and simulate different counterfactuals that allow us to isolate the average treatment effects. Our results show that intervention scales up effectively despite some degree of congestion.

In light of these findings, MINEDUC implemented our information policy nationwide in 2023. Using an encouragement design through *WhatsApp* messages, we find that providing real-time personalized information about students' admission probabilities, alongside warning messages and cutoff scores for all programs in the centralized system—–resembling sequential implementations of the Deferred Acceptance algorithm—–causally improves students' outcomes, in line with the results of our field experiment. Specifically, by reducing primarily *over-confidence* and *under-confidence* mistakes, we estimate that students who were affected by the policy roughly double the probability of entering the centralized system or improving relative to their initial assignment, which translates to a substantial decrease in unmatched and undermatched students and a significant increase in their monetary returns (1.2% and 0.5%, respectively). Indeed, if the policy were implemented to all applicants with full compliance, it would benefit close to 7,500 students. Moreover, the effects on assignments persist at the enrollment stage, with students who entered due to the policy being three times more likely to enroll in their assignments. Furthermore, by evaluating students' preferences and beliefs before and after the policy implementation, we observe that the improvements in students' outcomes are primarily driven by changes in beliefs concerning admission probabilities at the bottom of their preferences rather than at the top, reducing the incidence of students' biases on their application decisions.

Overall, we find that information frictions and application mistakes are significant and highly relevant, even in high-stakes environments such as college admissions. However, information policies that provide students with personalized information about their admission chances and current cutoff scores can effectively reduce them, reducing the fraction of unmatched and undermatched students and, as a result, reducing inequalities in access. Finally, our results suggest that sequential mechanisms such as *iterative DA* may be more robust to the presence of application mistakes and, thus, market design can also play a role in reducing information frictions and application mistakes, improving students' outcomes.

The remainder of this paper is organized as follows. In Section 2, we discuss the most closely related literature. In Section 3, we describe the Chilean college admissions system, the data, and the taxonomy of mistakes considered throughout the paper. In Section 4, we study the prevalence and relevance of application mistakes and shed light on their potential drivers. In Section 5, we describe our field experiment and discuss its results. In Section 6, we report the results of the scale-up policy implementation. In Section 7, we discuss several lessons and some implications of our findings. Finally, in Section 8, we conclude.

2 Literature

Our paper is related to three strands of the literature. First, our paper contributes to the literature on information policies to address information frictions and bolster efficiency and equity in education. Most of the literature focuses on frictions on options' characteristics and, specifically, on information regarding quality, expected outcomes, financial aid, and costs, and how these frictions differently affect students based on their socioeconomic backgrounds (Hoxby and Avery, 2012; Hoxby and Turner, 2015; Dynarski and Scott-Clayton, 2006). For instance, several papers show that students fail to apply to better options simply because they are unaware of their higher effectiveness at improving students' outcomes(Walters, 2017; Barr and Castleman, 2021; Corradini, 2023; Campos, 2023) or because they lack important information about expected earnings and returns (Jensen, 2010; Wiswall and Zafar, 2015; Bleemer and Zafar, 2018; Ainsworth et al., 2023; Hastings et al., 2015, 2016). Other papers in this literature focus on frictions that result in a lack of awareness about options and biases on beliefs in admission chances. Arteaga et al. (2022) show that many families that participate in the Chilean school choice system (which uses a variant of DA (Correa et al., 2022)) include fewer schools in their lists due to a lack of awareness of all schools, search costs, and also due to over-confidence about their admission chances. The authors show that providing information about different options and alerts to prevent risky applications can help families improve their outcomes. Bobba and Frisancho (2022) also show that students' beliefs about their admission chances are upwardly biased and that providing information (in their case, about test scores) decreases the influence of priors and increases the probability of having accurate beliefs. Our paper adds to this literature in several dimensions. First, we study the effects of providing personalized information targeting different sources of frictions, including information about program characteristics, admissions probabilities, awareness of programs via major recommendations, and providing feedback on their application. By eliciting preferences and beliefs through nationwide surveys, we find that each of these dimensions can contribute to different types of mistakes, and that both *under-confidence* and *over-confidence* play a significant role in explaining them. Second, through our field experiments and scale-up policy implementation, we show that providing information about different sources of frictions can significantly reduce mistakes and improve students' outcomes, reducing significantly the fraction of both unmatched and undermatched students, even when the policy is implemented at scale.

Second, our paper is related to the emerging literature on behavioral economics in education market design. We refer the reader to Rees-Jones and Shorrer (2023) for an excellent review. This literature has focused on several questions, including what is the effect of different mechanisms on strategic behavior, to which extent a mechanism can be manipulated (Pathak and Sönmez, 2013), why students misreport their preferences even in strategy-proof environments (Chen and Sönmez, 2006; Larroucau and Ríos, 2018;

Pais and Pintér, 2008; Calsamiglia et al., 2010; Featherstone and Niederle, 2016; Guillen and Veszteg, 2020), among others. Within this literature, the closest related to our paper is that on application mistakes in settings that involve variants of the Deferred Acceptance (DA) algorithm. Several papers focus on *obvious mistakes*, whereby students apply to programs with both full-fee and reduced-fee options but only include the former in their preference list (Artemov et al., 2017; Shorrer and Sóvágó, 2021; Hassidim et al., 2020). A common finding among these papers is that (i) most misrepresentations are payoff irrelevant, and (ii) they result from misunderstanding of the mechanism's rules due to lower cognitive ability and beliefs that assign low admission probabilities. We make several contributions to the literature. First, we provide a new taxonomy of *application mistakes* that expands the *obvious misrepresentations* previously studied in the literature. Second, we compute bounds on their payoff relevance and show how they can affect different outcomes, even in a seemingly strategy-proof environment. Third, we study the drivers of these mistakes and identify that strategic behavior coupled with information frictions and biases in beliefs as the main drivers. Finally, our scale-up policy implementation, which includes current cutoffs for all programs in the centralized system, emulates a sequential implementation of DA. Hence, the fact that our policy significantly decreases biases on admission chances expands previous findings on when sequential variants of DA can enhance the performance of matching markets (Grenet et al., 2022; Luflade, 2017).

Finally, our paper is related to the emerging literature that studies the effects of scaling up interventions.[1] Al-Ubaydli et al. (2017) propose four key categories to understand scale-up effects: (i) errors in statistical inference, (ii) properties of the population, (iii) properties of the situation, and (iv) spillover and general equilibrium effects. Within this literature, the closest paper to ours is Allende et al. (2019), which combines an RCT with a structural model to analyze how their information policy (applied in the Chilean school choice system) can be affected by capacity constraints, supply-side responses, and other general equilibrium effects when implemented at scale. Our work differs from Allende et al. (2019) since our counterfactual experiments measuring congestion and scale-up effects are model-free, which we achieve by leveraging the rules of the centralized assignment process and precisely knowing how to compute the distribution of assignment outcomes (Karnani, 2023). More broadly, our paper contributes to the scaling-up literature in several ways. First, we document a five-year scaling process, using each stage findings to inform the design of subsequent ones and culminate in the implementation of our information intervention as an actual policy in 2023. Second, we leverage our surveys, the implementation of a pilot intervention in 2021,[2] and a large-scale field experiment in 2022 to reduce the effect of false positives (errors in statistical inference). Third, we discuss how last-mile problems can affect the viability of this type of intervention due to coordination problems between stakeholders and a lack of alignment in the incentives of the different actors (properties of the situation). Finally, we show that our intervention is not significantly affected by interference and congestion effects (general equilibrium effects) and show its effectiveness when implemented as a policy at scale in 2023.

3 Background

In this section, we provide some background to better understand the analyses and interventions in the coming sections. In Section 3.1, we discuss some institutional details about the Chilean college admissions system. In Section 3.2, we describe the different data sources. Finally, in Section 3.3, we provide the classification of the mistakes we will analyze later.

[1] We refer the reader to Al-Ubaydli et al. (2019) for an overview of the implementation science literature, and List (2022) for an in-depth analysis of scale-up problems.

[2] The results for the 2021 pilot are available upon request.

3.1 Institutional Details

We focus on the centralized part of the Chilean tertiary education system, which includes the most selective universities in the country.[3] From now on, we refer to this as the admission system.

To participate in the admissions process, students must undergo a series of standardized tests (*Prueba de Selección Universitaria* (PSU) until 2020, *Prueba de Transición* (PDT) between 2021 and 2022, and *Prueba de Acceso a la Educación Superior* (PAES) startng in 2023). These tests include Math, Language, and a choice between Science or History, providing a score for each of them.[4] The performance of students during high school gives them two additional scores, one obtained from the average grade during high school (*Notas de Enseñanza Media* (NEM)), and a second that depends on the relative position of the student among their cohort (*Ranking de Notas* (Rank)).

After scores are published, students can access an online platform to submit their applications—which we refer to as Rank-Ordered List (ROL)—, where they can list up to ten programs in decreasing order of preference.[5] DEMRE (the Chilean equivalent of the college board) collects all these applications, checks students' eligibility in each of their listed programs, and, if eligible, computes their application scores and sorts them in decreasing order. Then, considering the preferences of students and the preferences and vacancies of programs, DEMRE runs an algorithm that relies on a variant of the DA algorithm whereby ties on students' scores are not broken.[6] As a result, the algorithm assigns each student to at most one program, and programs may exceed their capacities only if there are ties for their last seat. We refer to the score of the last admitted student as the *cutoff* of each program.

3.2 Data

We combine a panel of administrative data on the admissions process with survey data that we collected to analyze students' mistakes. The administrative data includes information about students (socioeconomic characteristics, scores, and applications), programs (weights, seats available, and admission requirements), and also the results of the admissions process (i.e., for each student and each program they applied to, whether the application was valid, and whether the student was assigned to that program or wait-listed) for the admission processes of 2020 to 2023.

The survey data was collected through three nationwide surveys that we designed and conducted in 2020, 2022, and 2023 in collaboration with MINEDUC and DEMRE. The primary goal of these surveys was to learn about students' preferences, their beliefs, and to characterize the drivers of application mistakes. These surveys included three main modules: (i) preferences, (ii) beliefs, and (iii) understanding of the admission process. We describe each module in Section 4 and include the most relevant questions in Appendix 8.1.1.[7]

To conduct these surveys, MINEDUC/DEMRE directly emailed students including a survey link after the application deadline but before publishing the assignment results. Hence, when completing the survey, students knew their scores and reported preferences but did not know their assignments. Additionally, for the 2023 admissions process, we designed and implemented a baseline survey conducted before the release of the results of the national exams. Since students had not yet applied to the centralized system when responding to this baseline survey, we asked them about their preferences and beliefs

[3] See Kapor et al. (2020) and Larroucau and Rios (2023) for a more general description of tertiary education in Chile and more institutional details. In 2023, 45 out of 58 universities in total were part of the centralized admission system.

[4] In 2023, an additional and more advanced Math test was added to the admissions process. See Appendix 8.4.1 for more details on the changes implemented to the system in 2023.

[5] Students apply directly to programs, i.e., pairs of university-major. In addition, in 2023, MINEDUC increased the number of programs students can list to 20.

[6] See Rios et al. (2021) for a detailed description of the mechanism used and its properties.

[7] The complete translations of the surveys are available upon request.

concerning hypothetical programs.

3.3 Application Behavior and Taxonomy of Mistakes

In this section, we introduce a framework of application behavior under incomplete information and define the different types of mistakes that we will study throughout the paper.

As it is common in the literature (Agarwal and Somaini, 2018; Calsamiglia et al., 2020; Larroucau and Rios, 2023), we assume that students' application behavior is drawn from a mixture of weak truth-tellers and strategic students. On the one hand, we assume that weak truth-tellers are students who do not strategize and list all the programs they prefer over the outside option in decreasing order if the constraint on the length of their list is not binding; otherwise, we assume they truncate their reports at the bottom of their lists, neglecting their admission probabilities. This application strategy is indeed a dominant strategy under student-proposing DA when the constraint in the length of the list is not binding, and it is the suggested strategy by DEMRE, regardless of whether the length of the application list is binding or not. On the other hand, we assume that strategic students decide where to apply by solving an optimal portfolio problem (as in Chade and Smith (2006)) to maximize their expected utility considering (i) the utility they would obtain from each program and (ii) their admission chances.

Formally, consider a set of programs M and a specific student whose indirect utility in each program is captured by $u(\mathbf{X}) = \{u(X_j)\}_{j\in M}$, where $\mathbf{X} = \{X_j\}_{j\in M}$ is the matrix of programs' observable (non-random) characteristics and their interaction with the student's characteristcs. When clear from the context, we remove the dependency on programs' characteristics and simply write $\mathbf{u} = \{u_j\}_{j\in M}$, where $u_j = u(X_j)$ for each $j \in M$. In addition, let $\mathbf{p} = \{p_j\}_{j\in M}$ be the vector of admission probabilities that the student faces in each program, i.e., p_j captures the probability of assignment in program j if the student only applies to that program, and let $\rho_k(R) := \prod_{l=1}^{k-1}(1-p_{r_l})$ be the probability of not being assigned to one of the top $k-1$ preferences reported in the rank order list $R = \{r_1,\ldots,r_{|R|}\} \subseteq M$. We assume that these probabilities are independent across programs.[8] Finally, let $c(R)$ be the cost of applying to the subset of programs R. Note that the function $c(R)$ captures the potential monetary (e.g., application fees) and non-monetary (e.g., the time needed due to search frictions (Arteaga et al., 2022)) costs of reporting a given ROL R.

Given these elements, the expected utility of reporting a ROL R can be computed as:

$$\mathbb{E}\left[U(R) \mid \mathbf{X},\mathbf{p}\right] := u_{r_1}\cdot p_{r_1} + u_{r_2}\cdot p_{r_2}\cdot \rho_1(R) + \ldots + u_{r_{|R|}}\cdot p_{r_{|R|-1}}\cdot \rho_{|R|}(R) - c(R). \tag{1}$$

Then, we say that a student makes a payoff-relevant *application* mistake by reporting ROL $\tilde{R}$ if there exists a ROL $R' \neq \tilde{R}$ such that reporting R' strictly increases the student's expected utility given correct beliefs over programs' characteristics $\mathbf{X}$ and admission probabilities $\mathbf{p}$, i.e.,

$$\mathbb{E}\left[U(\tilde{R}) \mid \mathbf{X},\mathbf{p}\right] < \mathbb{E}\left[U(R') \mid \mathbf{X},\mathbf{p}\right].$$

Based on this definition, application mistakes can arise for four reasons. First, students may choose the subset of programs R to apply given their indirect utilities and admission probabilities by optimizing a function $\pi(R \mid \mathbf{u},\mathbf{p})$ that is different from that in (1). Among these, we will focus on students who apply to their most desired programs without considering their admission chances, potentially making a *mistake by truth-telling*. Second, even if students behave strategically and maximize their expected utility as in (1), they may have biased beliefs about their admission chances, leading them to *mistakes on admission probabilities*. Finally, truth-tellers and strategic students may have (i) biased beliefs about program characteristics that affect their assessment of their utilities, potentially inducing *mistakes on program valuations*; or

[8] Even though these probabilities might be correlated, the literature has shown that students tend fail to account for the correlation between their admission chances (Rees-Jones et al., 2023).

(ii) may be unaware of programs that they may prefer over others and could potentially add in their ROL, potentially causing *mistakes on awareness*. Although application mistakes can result from a combination of these,[9] we now discuss each separately.

3.3.1 Mistakes by Truth-tellers

As mentioned above, being a weak truth-teller is a dominant strategy for students if the constraint on the length of the list is not binding. However, if students prefer more programs than they can list, weak truth-tellers are forced to select a subset of the programs they prefer over being unassigned. When doing so, the lack of strategic behavior might induce application mistakes. For instance, a student who lists their most preferred programs truthfully, i.e., their reported ROL R satisfies

$$R = \arg \max_{R \subseteq M, |R| \leq K} \left\{ \pi(R \mid \mathbf{u}, \mathbf{p}) := \sum_{r \in R} u_r \right\}, \tag{2}$$

where K is the maximum number of programs that students can include in their ROL, might be better off by replacing one of these programs (especially reach programs with low admission chances) with a less preferred safety program with higher admission chances. We refer to this type of mistake as *mistakes by truth-tellers*.

3.3.2 Mistakes by Strategic Students

As formalized by Chade and Smith (2006), the problem faced by a strategic student who aims to choose the subset of programs to include in their application so as to maximize their expected utility can be formulated as:

$$\max_{R \subseteq M : |R| \leq K} \left\{ \pi(R \mid \mathbf{u}, \mathbf{p}) := \mathbb{E}\left[U(R) \mid \mathbf{X}, \mathbf{p} \right] \right\}. \tag{3}$$

Chade and Smith (2006) assume that each student has complete information about the programs' observable characteristics, the corresponding indirect utilities, and their admission probabilities. However, as discussed in Section 2, we know that students may have biased beliefs about their admission chances. Hence, we extend the framework in Chade and Smith (2006) to allow for biases in admission probabilities. Specifically, let $\tilde{\mathbf{p}}$ be their potentially biased beliefs about their admission probabilities. Then, if each student decides where to apply by solving the optimal portfolio problem in (3) considering their beliefs about their admission chances ($\tilde{\mathbf{p}}$), then their observed ROL $\tilde{R}$ satisfies

$$\tilde{R} = \underset{R \subseteq M : |R| \leq K}{\operatorname{argmax}} \left\{ \mathbb{E}\left[U(R) \mid \mathbf{X}, \tilde{\mathbf{p}} \right] \right\}. \tag{4}$$

From (4), we observe that biased beliefs about admission chances, coupled with application costs or strategic incentives induced by the assignment mechanism, may lead to a suboptimal application $\tilde{R}$. We refer to this as a *mistakes on assignment probabilities*. Note that students might incorrectly assess their chances of admission by either being too optimistic or pessimistic.[10] Thus, we consider different variants of this family of mistakes. On the one hand, we say that a student makes an *(over-) under-confidence mistake* if they skip a (reach) safety program where they have a positive chance of admission. On the other hand, we say that students make an *ordering mistake* if they list programs not decreasingly according to their utilities. Notice that *over-confidence* mistakes can lead to unmatched students, while *under-confidence* and *ordering* mistakes can lead to under-matched students.

[9] For instance, a student may report their true preferences and have biased beliefs about program characteristics.

[10] Students might have biased beliefs on admission probabilities, even if students face a non-binding constraint in their application lists, small psychic costs might lead to *mistakes on assignment probabilities*.

3.3.3 Mistakes on Valuations

Students might have incomplete information and biased beliefs about programs' characteristics such as their costs, expected earnings, and employment rates, among others (Wiswall and Zafar, 2015; Bleemer and Zafar, 2018) due to information frictions (e.g., lack of information about programs, no access to counseling, etc.). As a result, students may incorrectly estimate the indirect utility they would get from being assigned to each program, which may lead them to a sub-optimal application.

Formally, we say that a student makes a *mistake on valuations* if they have biased beliefs about programs characteristics, $\tilde{\mathbf{X}} = \left\{\tilde{X}_j\right\}_{j \in M}$, that result in biased indirect utilities $\tilde{\mathbf{u}} = u\left(\tilde{\mathbf{X}}\right)$ leading to ROL

$$\tilde{R} := \arg \max_{R \subseteq M : |R| \leq K} \pi(R \mid \tilde{\mathbf{u}}, \mathbf{p})$$

that is strictly dominated by the ROL $R' := \arg\max_{R \subseteq M:|R| \leq K} \pi(R \mid \mathbf{u}, \mathbf{p})$ that results from having correct beliefs about programs characteristics and their corresponding utilities, i.e.,[11]

$$\mathbb{E}\left[U(\tilde{R}) \mid \mathbf{X}, \mathbf{p}\right] < \mathbb{E}\left[U(R') \mid \mathbf{X}, \mathbf{p}\right].$$

3.3.4 Mistakes on Awareness

All the types of mistakes described above assume that students are fully aware of all the programs that are part of the centralized system and, thus, select the subset of programs to apply from the entire set of programs (i.e., $R \subseteq M$). However, students may not be aware of some programs that they may prefer over others that they included in their lists and, thus, by adding them in their consideration set and consequently in their ROL, they may get a strict improvement in their objective.

Formally, we say that a student makes a *mistake on awareness* if their consideration set $\tilde{M}$ is a subset of all programs (i.e., $\tilde{M} \subset M$) and leads to a reported ROL

$$\tilde{R} = \arg \max_{R \subseteq \tilde{M} : |R| \leq K} \pi\left(R \mid \mathbf{u}, \mathbf{p}\right)$$

that is strictly dominated by the alternative ROL $R' := \arg\max_{R \subseteq M:|R| \leq K} \pi\left(R \mid \mathbf{u}, \mathbf{p}\right)$ that would be obtained if considering all programs, i.e.,

$$\mathbb{E}\left[U(\tilde{R}) \mid \mathbf{X}, \mathbf{p}\right] < \mathbb{E}\left[U(R') \mid \mathbf{X}, \mathbf{p}\right].$$

3.3.5 Other Types of Mistakes

In this paper, we focus on payoff-relevant application mistakes, i.e., mistakes in which students submit a list that leads to a strictly lower expected payoff. This family of mistakes includes the *obvious* mistakes previously identified in the literature (Artemov et al., 2017; Shorrer and Sóvágó, 2021). However, there are other types of mistakes that we do not analyze. First, Larroucau et al. (2023) analyze *admissibility mistakes*, whereby students apply to programs where they do not satisfy the admission requirements and, thus, have zero chance of admission. Note that admissibility mistakes may not be payoff-relevant, as students may list other programs where they satisfy the admission requirements, or there may be no other programs they prefer over those they listed. Since most of the admissibility mistakes we identified in our sample are not payoff-relevant, we decided to omit them. Second, we do not focus on mistakes that students may potentially make if they do not know their preferences (i.e., the utility function $u(\cdot)$) or

[11] Note that we use $\pi(\cdot)$ because *mistakes on valuations* can be made both by truth-tellers and strategic students.

have non-standard preferences (i.e., $\pi(R \mid \mathbf{u}, \mathbf{p})$ other than (1) or (2)) such as report-dependence (Meisner, 2023),—i.e., students are averse to rejection and enjoy being accepted, so they apply where they are most likely to be admitted—and expectation-based reference dependence (Dreyfuss et al., 2022b)—i.e., students may feel endowment effect for schools that offer high chance of admission and avoid creating an expectation of matching with high-value/low probability schools to avoid sense of loss. Finally, we assume that admission probabilities are independent. Hence, we do not consider mistakes that may be due to students neglecting potential correlations in the admissions chances across different programs(Rees-Jones et al., 2023).[12]

3.3.6 Discussion

Note that having biased beliefs over programs' characteristics and admission probabilities are necessary but not sufficient conditions for having payoff-relevant *application* mistakes, since biases need to be (i) large enough, (ii) in the relevant programs (programs the student prefers to her outside option), and (iii) over characteristics with a high enough preference weights, such that correcting beliefs could lead to strict improvements in their expected utility. For instance, if students' care mostly about non-pecuniary elements, information policies might be ineffective at changing students' choices (Wiswall and Zafar (2015)). In addition, notice that we define mistakes from a static perspective given students' current preferences and beliefs. Although there is evidence that students' valuations over programs and their beliefs about their admission chances might change over time due to learning,[13] we consider interventions that are implemented at the moment of the application process, making it unlikely that dynamic effects such as learning through experience are relevant for our analysis.

4 Evidence of Mistakes

A key challenge to characterize the mistakes described above is that they are not directly observable from the administrative data. To tackle this, we leverage several large-scale nationwide surveys designed and implemented to elicit students' true preferences, their beliefs on programs' characteristics and assignment probabilities, and their understanding of the admission rules. This section describes the survey modules we use to characterize the different types of mistakes. Then, we present the empirical evidence on the prevalence and drivers of the different types of mistakes introduced in Section 3.3.

4.1 Surveys

As exposed in Section 3, we designed and conducted several nationwide surveys between 2020 and 2023 in collaboration with MINEDUC and DEMRE. These surveys included three main modules: (i) preferences, (ii) beliefs, and (iii) understanding of the admission process.[14] In this section, we describe each of these modules and the information they provide to characterize the different types of mistakes.

(i) *Preferences module.* This module aimed to elicit students' true preferences. Towards this end, we asked students about their true top preference, i.e., the program they prefer the most among all the programs in the system, assuming that their score was high enough to guarantee admission. In addition, we asked students about their true bottom preference, i.e., any program they did not list in their ROL and that they would prefer compared to being unassigned, assuming their score was

[12] Although our theoretical framework makes this assumption, we can easily adapt our information policies to account for potential correlation patterns across admission probabilities.

[13] See for instance Narita (2018) for school-choice settings and Arcidiacono et al. (2016), Fu (2014), and Larroucau and Rios (2023) for college admissions.

[14] In Appendix 8.1.2, we report summary statistics for all surveys.

high enough to guarantee admission. These questions allow us to know whether students misreport their true top and bottom preferences, excluding them from their ROL.

(ii) *Beliefs module.* This modules aimed to elicit students' beliefs about several relevant factors affecting their application, including admission probabilities, expected earnings, chances of retention and graduation, expected cutoffs, etc. We elicited this information for programs included in the students' preference list and others outside their ROL, including their true top and bottom preference and some random programs. These questions allow us to understand the role of biased beliefs on application mistakes.

(iii) *Knowledge about the mechanism*. This module aimed to measure students' knowledge and understanding of the system's rules, the requirements of the programs they applied to, their awareness of potential mistakes, and also to learn the reasons behind some of their decisions such as excluding their true top or true bottom programs. These questions allow us to further understand the drivers of application mistakes.

4.2 Mistakes on Admission Probabilities

As discussed in Section 3.3, there are three types of *mistakes on admission probabilities*:

1. *Under-confidence*: A student makes an under-confidence mistake if they skip a program they prefer more than other programs in their ROL and for which they have positive admission probability. As there may be many programs that satisfy this condition, we focus on students that skip their true top preference (elicited through our surveys), and we restrict the analysis to students for whom the constraint on the length of their list is not binding. We do the latter because skipping their true top preference may be optimal for students constrained by the length of the list, so we cannot directly label those as mistakes.

2. *Ordering*: A student makes an ordering mistake if they do not rank programs with a positive admission probability in decreasing order of utility. As a result, the student would benefit from submitting a ranked ordered list with the same subset of programs but in a different order. Since this could hold in any part of the preference list, we focus for simplicity on ordering mistakes involving the true top preference (elicited through the survey), i.e., we focus on students who apply to their true top preference but do not include it as their top reported preference.

3. *Over-confidence*: A student makes an over-confidence mistake if (i) they skip a program that they prefer more compared to being unassigned and for which they have a positive admission probability, and (ii) the constraint on the length of their list is not binding and they have a positive risk of being unassigned. As before, since there may be multiple programs that satisfy these conditions, we focus on the true bottom preference (elicited through our surveys).

Note that *mistakes on admission probabilities* can be measured (i) ex-ante, i.e., students have a positive admission probability in their top (bottom) true preference; or (ii) ex-post, i.e., students have an application score greater than or equal to the realized cutoff of their top (bottom) true preference and, thus, they could have been admitted to those programs if they had listed them in their preferences. Importantly, notice that students who make ex-post *under-confidence* or *ordering* mistakes result undermatched, while students who make ex-post *over-confidence* mistakes end up being unmatched.

In the remainder of this section, we analyze each type of these mistakes using our survey data. Specifically, we rely on the 2020 survey and its questions about students' true top preference to study *under-confidence* and *ordering* mistakes, while we use the 2022 survey and its questions on students' true bottom preference to analyze *over-confidence* mistakes.

4.2.1 Under-confidence and Ordering Mistakes

A challenge to measure *mistakes in admission probabilities* using students' true top preference (elicited in our survey) is that students may incorrectly interpret the hypothetical scenario posited by our survey question and declare they are skipping their true top preference due to characteristics of the program they do not like. For instance, a student may skip it because they believe their chances of graduation are too small or because of the program's cost. These reasons are inconsistent with the definition of true top preference because these characteristics should enter the indirect utility and be captured in the students' preference order. Thus, we take a conservative approach and consider only students who listed "low chances of admission" as the reason for skipping their true top preference when analyzing underconfidence mistakes.

In Table 1, we report summary statistics to characterize underconfidence and ordering mistakes among students who provided consistent answers to the survey questions (23,596 students, which represents 67% of survey respondents), separating by whether the student is short-list or full-list (i.e., whether the constraint on the length of the application list is binding). Given that full-list students may optimally misreport their true top preference to satisfy the constraint on the length of their application list, we focus on short-list students. Note that this is without major loss of generality, as short-list students represent 88.87% of the survey respondents. Moreover, recall that these students could strictly improve their expected utility by adding their true top preference at the top of their preference list if their admission probability is positive. Thus, any misreport regarding the true top preference among short-list students with positive admission probability would lead to a mistake on assignment probability.

Table 1: Summary statistics for underconfidence and ordering mistakes

						Underconfidence mistake				Ordering mistake			
			Misreport			Ex-ante		Ex-post		Ex-ante		Ex-post	
	N	Truth	Total	Exclude	Order	N	%	N	%	N	%	N	%
Full-list	1337	673	664	480	184	20	1.496	17	1.272	20	1.496	18	1.346
Short-list	10390	6235	4155	3264	891	266	2.560	221	2.127	197	1.896	162	1.559

Note: Sample includes all survey respondents who completed the survey, provided consistent answers regarding their true top preference, and are not PACE. The column *Truth* reports the number of respondents whose true top preferences matches their top reported preference. The columns below *Misreport* include the total number of students who misreport their preferences (*Total*), the number of respondents who exclude their true top preference (*Exclude*), and the number of respondents who include their true top preference in their list but not as their top preference. The columns below *Underconfidence mistakes* (*Ordering mistakes*) report the number and fraction of survey respondents who make ex-ante and ex-post underconfidence (ordering) mistakes.

First, we observe that 39.99% (4,155) of short-list students misreport their true top preference. Second, we observe that 78.56% (3,264) of short-list students who misreport their true top preference exclude it from their preference list, while the remainder includes it but not as their top preference. Third, we observe that 2.56% (266) short-list survey respondents had a positive admission probability and 2.13% (221) had an application score above the cutoff of their true top preference and, thus, made *ex-ante* and *ex-post under-confidence* mistake, respectively. Finally, we find that 1.89% (1.56%) of short-list respondents listed their true top preference in a preference below the top, had a positive admission probability (application score above the cutoff), got assigned to another program and, thus, made an *ex-ante* (*ex-post*) *ordering* mistake.

Overall, these results suggest that a significant fraction of students make *underconfidence* and *ordering* mistakes. Indeed, we estimate that 7.43% of short-list students who misreport their true top preference make a payoff-relevant mistake. Moreover, note that these estimates provide only a lower bound for the incidence of these mistakes, as we do not account for students who, although they have no chance of admission in their true top preference, may have alternative programs they prefer less but more than their reported top preference, where they could potentially be admitted (ex-ante or ex-post). Thus, we conclude that *underconfidence* and *ordering* mistakes may be prevalent and sizable in our setting.

Drivers. Understanding the drivers of mistakes is essential to design information policies and provide students with tools to improve their applications. To this end, in this section, we focus on analyzing (i) the extent to which biases in beliefs on admission probabilities explain *mistakes on assignment probabilities* involving students' true top preference and (ii) understanding the key drivers of the biases on beliefs.

Biases in Beliefs. To test whether biases on belief can explain mistakes, we use the elicited beliefs about their admission probability in the 2020 survey. Specifically, we asked students to report their belief about their admission probability for some programs in their ROL, for their true top preference, and also the overall probability of getting assigned. Then, by comparing these beliefs with the estimated rational-expectation probabilities (computed as described in Appendix 8.3.3), we can quantify biases in beliefs and assess whether these explain mistakes on assignment probabilities. Formally, we will denote by $\tilde{p}_{ij}$ and p_{ij} the elicited belief and the rational-expectation admission probability of student i in program j, respectively, and we will denote the bias as $\eta_{ij} = \tilde{p}_{ij} - p_{ij}$. Similarly, let $\tilde{\rho}_i = 1 - \prod_{r \in R}(1 - \tilde{p}_r)$ and $\rho_i = 1 - \prod_{r \in R}(1 - p_r)$ the elicited and rational-expectation overall probability of student i getting assigned in any preference of their ROL R, and let $\bar{\eta}_i = \tilde{\rho}_i - \rho_i$ the bias on the overall admission probability. When clear from the context, we may drop the indexes to facilitate exposition. Moreover, we asked students about their knowledge regarding previous year cutoffs and other elements of the assignment mechanism. Hence, we can use these responses to study the biases' drivers and to evaluate the impact of information frictions on these mistakes.

In Table 2, we report the results of linear probability models in which the dependent variables are whether the student made any kind of ex-post mistake (i.e., No mistake, Underconfidence, Ordering).[15] The main variable of interest is the bias η at the true top preference, i.e., the difference between the student's belief and the ex-ante rational-expectations probability, and we control for demographics including gender, score, and region stratas. To rule out misreports that may not constitute a mistake, we exclude from the analysis students whose true top preference is not valid, who have an application score below the cutoff, or students for whom the constraint on the length of their list is binding.

Table 2: Effect of Bias on Underconfidence and Ordering Mistakes

	No mistake	Underconfidence	Ordering
Bias - Top True	0.362^{***}	-0.271^{***}	-0.091^{***}
	(0.025)	(0.022)	(0.017)
Constant	0.750^{***}	0.146^{***}	0.105^{***}
	(0.030)	(0.026)	(0.020)
Demographics	Yes	Yes	Yes
Observations	4,496	4,496	4,496

Note: Sample includes all students who are not PACE, completed the survey, are short-list, and reported a top true preference for which they satisfy the application requirements and they have an ex-ante admission probability over 1%. Significance reported: $^{*}p < 0.1;^{**} p < 0.05;^{***} p < 0.01$.

First, we observe that the bias has a positive and significant effect on making no mistake, suggesting that students who are more optimistic are more prone to report their preferences truthfully and make

[15] Note that all students who made a mistake in this context ended up undermatched.

no mistake. Second, we observe that the bias has a negative and significant effect on *underconfidence* and *ordering* mistakes. This result suggests that these mistakes increase with students' pessimism, as larger bias values imply that students' beliefs are lower than their admission probability, suggesting that biases on beliefs are relevant drivers of *underconfidence* and *ordering* mistakes. Finally, we find that the magnitude of the effect of bias is larger for underconfidence mistakes and quite sizable. Indeed, the estimates suggest that being completely pessimistic (i.e., a bias equal to 1) increases the probability of making an underconfidence mistake by 27.1%, while it increases the probability of an ordering mistake by 9.1%.

Understanding Biases. In Table 3, we report the results of linear regressions examining the norm of bias related to admission probabilities and expected cutoff scores,[16] focusing on students' knowledge and various socioeconomic and demographic factors. We observe that greater bias in admission probabilities is positively correlated with increased biases in cutoff scores (*Bias Norm Cutoff*) and negatively correlated with students' awareness of admission requirements (*Requirements Knowledge Share*) and their connections within the program (*Knows someone in the program*). Furthermore, this analysis reveals that students from more affluent backgrounds, with higher application scores, and those attending private schools exhibit lower bias in admission probabilities and cutoff scores. These results suggest a disparity in information accessibility, implying that students from less advantaged socioeconomic backgrounds may possess less accurate beliefs about their admission probabilities, potentially leading to application mistakes.

Another potential explanation for biases in admission probabilities is that students may not fully understand how the assignment mechanism works and how changes in the system may affect their admission chances. To illustrate this, in Figure 11 in Appendix 8.2, we show the distribution of bias relative to rational expectations and the distribution of bias relative to adaptive beliefs (i.e., rational expectations estimated using data from the previous year). Panel (a) shows these distributions for all programs. Similarly, Panels (b)and (c) show the distribution for programs that increased and decreased their vacancies by at least 25% compared to the previous year, respectively. Overall, we observe that the distributions of bias are close to each other considering all programs and centered around zero. However, when programs significantly increase their vacancies relative to the previous year, students seem to not correctly anticipate changes in cutoffs. This behavior might be explained by a combination of factors, including (i) anchoring effects, (ii) lack of knowledge about changes in vacancies and requirements, and (iii) misunderstanding of the mechanism's rules, among others.

The previous results suggest that having information about previous year cutoff scores could help students to reduce their biases over admission probabilities and, thus, reduce the prevalence of *mistakes on assignment probabilities*. However, since students might anchor their beliefs to the previous year's information and misunderstand the meaning of cutoff scores, it is essential also to provide information about the current admission process and educate them on the mechanism's rules.

4.2.2 Over-confidence Mistakes

As previously discussed, students make an *over-confidence* mistake if they risk being unassigned and skip programs with a positive admission chance that they prefer over being unassigned. Then, we can use the true bottom preference elicited in the 2022 survey to measure this type of mistake. Specifically, we asked

[16] As part of our survey, we also elicit students beliefs on the cutoffs of some relevant programs, including their true top preference, their top reported one, among others. Moreover, we ask students whether they know the previous year's cutoff. Indeed, we find that only 58% declare to know the previous year's cutoffs for all their listed programs, and 9% declare to ignore all of them. Although DEMRE does not provide any information about programs' cutoffs during the application process, this information can be typically found on universities' websites. One reason behind the lack of centralized information about cutoff scores is the concern that some students might not understand what a cutoff score exactly means. For instance, they might believe that programs predetermine cutoffs and may not understand that these vary from year to year.

Table 3: OLS regression for Bias Norms over Adm. Probabilities and Cutoffs

	Bias Norm Adm. Prob.		Bias Norm Cutoff	
	(1)	(2)	(3)	(4)
Avg. Math-Verbal normalized	-7.162^{***}	-11.889^{***}	-2.623^{***}	-2.820^{***}
	(0.121)	(0.118)	(0.031)	(0.035)
Female	1.571^{***}	1.342^{***}	-0.078^{**}	-0.306^{***}
	(0.138)	(0.145)	(0.037)	(0.044)
Family income below median	0.592^{***}	0.851^{***}	-0.037	-0.124^{**}
	(0.154)	(0.163)	(0.041)	(0.049)
Public	1.447^{***}	1.155^{***}	-0.067	0.587^{***}
	(0.237)	(0.249)	(0.063)	(0.075)
Voucher	1.181^{***}	0.593^{***}	-0.070	0.648^{***}
	(0.200)	(0.210)	(0.053)	(0.063)
Requirements Knowledge Share	-1.379^{***}	-	-0.404^{***}	-
	(0.212)	-	(0.056)	-
Knows the cutoffs for every program	-6.271^{***}	-	-2.979^{***}	-
	(0.273)	-	(0.072)	-
Knows the cutoffs for some programs	-3.389^{***}	-	-2.154^{***}	-
	(0.275)	-	(0.073)	-
Knows someone in the program	-1.055^{***}	-	-0.456^{***}	-
	(0.172)	-	(0.046)	-
Bias Norm Cutoff	0.982^{***}	-		-
	(0.010)	-		-
Constant	39.365^{***}	37.156^{***}	7.029^{***}	9.042^{***}
	(0.388)	(0.245)	(0.102)	(0.073)
Observations	145,647	148,539	145,647	145,647

Note: Each observation is a pair student-program where the student applies and give a response in the 2020 survey for which there is a well-defined cutoff score. *Requirement Knowledge Share* is the share of requirements that the student declares to know for the program she applied to. *Knows the cutoffs for every program* (*Knows the cutoffs for some programs*) is a dummy variable that takes value 1 if the student declares to know the cutoffs for every (some) program. *Knows someone in the program* is a dummy variable that takes value 1 if the student declares to know someone in the program. *Bias Norm Adm. Prob.* (*Bias Norm Cutoff*) is the bias norm for the admission probability (realized cutoff in 2020) of the program the student applied to. *Avg. Math-Verbal normalized* is the standardized average math-verbal score. Sample includes all students who are not PACE and completed the survey. Models (1) and (3) include as additional controls: the distance between student's application score to the realized 2020 cutoff score, whether the program is the true top, and the preference rank. Significance reported: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

students to report any program in the centralized system that they did not include in their list and would prefer over being unassigned, assuming they would not get assigned to any of the programs in their list.[17] Then, we use the responses to this question to estimate bounds on the incidence of *over-confidence* mistakes. Specifically, we compute the ex-ante lower bound by counting the number of students who faced a positive risk of not being assigned to the system ($> 1\%$) and have a strictly positive probability of being assigned to their true bottom preference. Similarly, we compute the ex-post lower bound by counting the number of unassigned students who would have been assigned to their true bottom preference if they had applied to it. We compute the ex-ante and ex-post upper bounds by counting all students who face positive risk (ex-ante) or result unassigned (ex-post) and reported a true bottom preference. Contrary to the lower bounds, we do not restrict the probability (ex-ante or ex-post) of being assigned to the true bottom preference because students may have other programs not listed that they prefer over being unassigned and for which they have a positive probability of being assigned (ex-ante or ex-post).

In Table 4, we report bounds on the number of students making ex-ante and ex-post overconfidence mistakes among short-listed students who completed the survey. Since students may be affected by the field experiment we discuss in Section 5, we exclude students who potentially received one of our information treatments from this analysis.

Table 4: Over-confidence mistakes' bounds

Bounds	N	Ex-ante	Ex-post
Lower	5349	1.18%	0.64%
		(0.15)	(0.11)
Upper	5349	2.60%	1.85%
		(0.22)	(0.18)

Note: The sample is restricted to students who applied to less than 10 programs (short-list students), responded to the survey with 100% progress and, (i) who were not in the RCT sample, or (ii) who were in the RCT sample but in the control group. These two subsamples are pooled together to compute the bounds and the sample size is given in column *N*. Standard errors (in parenthesis) are multiplied by 100.

We find that close to 25% of survey respondents report a *true bottom* preference that they did not include in their list. In addition, we observe that at least 1.1% and at most a 2.5% of applicants make an *over-confidence* mistake, which represents between 5% and 10% of students who skip their true bottom program. Note that *over-confidence* are highly consequential since they translate into students being unmatched to the centralized system and, thus, students may not benefit from the high monetary return of enrolling in the university system (Rodriguez et al., 2016; Bucarey et al., 2024). Indeed, in Section 6, we show how a reduction in *over-confidence* mistakes translates into an increase in students' predicted log-monthly earnings.

Drivers. As we did for *under-confidence* and *ordering* mistakes, we now analyze the effect of biases in admission probabilities on *over-confidence* mistakes. In Table 5, we report the results of linear probability models that consider as dependent variables whether the student made an ex-post overconfidence mistake regarding their bottom true preference. The main variable of interest is the bias on the overall admission probability and the bias in the bottom true preference, and we control for demographics including gender, region and score stratas. The sample considered in this analysis includes all students that are short-list, not PACE, and have an average between Math and Verbal greater than or equal to 450.[18]

[17] In Appendix 8.1.1, we provide the exact translation for the question.

[18] We include this filter to remove all students for whom there are no valid programs.

Table 5: Effect of Bias on Overconfidence Mistakes

	Overconfidence mistake	
	(1)	(2)
Bias - Overall	0.068^{***}	-
	(0.004)	-
Bias - Bottom True	-	-0.078^{***}
	-	(0.010)
Constant	0.021^{***}	0.072^{***}
	(0.004)	(0.015)
Demographics	Yes	Yes
Observations	7,152	1,722

Note: Sample includes all students who are not PACE, completed the survey, are short-list, and reported a top true preference for which they satisfy the application requirements. Significance reported: $^{*}p < 0.1;^{**} p < 0.05;^{***} p < 0.01$.

We observe that the bias in the overall admission probability has a positive and significant effect. In contrast, the effect of the bias on the admission probability at the bottom true is negative and significant. Recalling that we compute bias as the difference between beliefs and rational expectations probabilities, the former result suggests that overconfidence mistakes increase with students' level of optimism, as smaller values of the bias imply that students are less confident about their admission chances. On the other hand, the latter result suggests that students who are pessimistic about their admission chances at their bottom true are more likely to make an overconfidence mistake.[19] Altogether, these results indicate that overconfidence mistakes are due to two factors: (i) an overconfidence about admission chances among the programs included in the ROL, and (ii) an underconfidence in their admission chances at their bottom true preference.

4.3 Mistakes by Truth-tellers

As discussed in Section 3.3, a truthful student only makes an application mistake if the constraint on the length of their list is binding and there is a program they prefer over their assignment that they did not list.

As previously discussed, a challenge to characterize *mistakes on truth-tellers* (and the other types of mistakes) is that we need information about students' true preferences. Since our surveys only elicited students' top and bottom true preferences, we cannot fully characterize students who skip a program they prefer over some of those they listed due to a binding constraint on the length of the list. However, we can obtain a lower bound on the number of constrained truth-tellers that made a mistake by analyzing those who applied to the maximum number of programs allowed (10) and that included either (i) their top true preference or (ii) their bottom true preference in their list. The idea behind this is that truthful students may report their preferences in decreasing or increasing order of preferences and, once they hit the constraint on the length of their lists, stop listing programs. These heuristics would result in students omitting their bottom and top true preferences and, thus, we can quantify the number of mistakers by computing the fraction of them who made an overconfidence and an underconfidence mistake, respectively.

In Table 6, we analyze the sample of students who responded the survey of 2022 and included either their top true (columns below Overconfidence) or their bottom true preference in their list (columns below

[19]In Table 19 in Appendix 8.2, we show that this result is robust to alternative measures of bias that are not bounded to be positive, such as bias over expected cutoff scores. We observe that students whose application scores are below the realized cutoff tend to be optimistic about the expected cutoffs for the programs they applied to.

Underconfidence). Within this sample, we report the number of students for whom the constraint on the length of the list was binding, and the fraction of students who made ex-ante and ex-post over and underconfidence mistakes, respectively.

Table 6: Mistakes on Truth-Telling

	Overconfidence			Underconfidence		
	N	Ex-ante [%]	Ex-Post [%]	N	Ex-ante [%]	Ex-Post [%]
Non-binding	1556	0.093	0.029	1380	0.244	0.053
Binding	347	0.000	0.000	287	0.000	0.000

Note: Sample includes students who completed the survey of 2022 and included either their top or their bottom true preference in their list.

From this table, we observe that none of the students who were candidates to make a *mistake on truth-tellers* (i.e., students for whom the constraint is binding) made ex-ante or ex-post application mistakes. Hence, these results suggest that truthful students do not make application mistakes in our setting.

4.4 Mistakes on Valuations and Awareness

In Section 3.3.2, we introduced *mistakes on valuations*, whereby students have biased beliefs about programs' characteristics that play a relevant role in assessing the utilities they would get if they attend a specific program. We also defined *mistakes on awareness*, whereby students may not be aware of some programs they may prefer over others they included in their lists. Since fully understanding the drivers of students' utilities and their consideration sets' formation process is beyond the scope of the paper, we exploit information about students' beliefs and awareness of programs' characteristics to illustrate how these two types of mistakes can be at play. Specifically, we show these using the elicited awareness and beliefs about the average income in the fourth year of graduation for different programs (including their top and bottom reported and true preferences, and also for a random program not included in their list) in the 2022 survey.[20] As Hastings et al. (2015) show, expected earnings upon graduation are one of the primary drivers of college choices, so lack of awareness or biases may introduce large distortions in the estimated utilities, potentially leading to mistakes.

In Figure 2, we report the distribution of levels of knowledge about this characteristic for the different programs.

We observe that a significant fraction of students (between 40% and 50%) report that they have poor knowledge (medium or less) about the average income for the top and bottom programs (both true and reported), and this result is even larger for the random program. Indeed, close to 45% of students declare not to know this key program characteristic for the random program. This result suggests that a significant fraction of students might not be aware of these programs, potentially leading them to *mistakes on awareness*.

To quantify the magnitude of this lack of knowledge and understand how it correlates with students' characteristics, in Table 7, we report the results of linear regressions on the absolute percentage of bias on the average income,[21] controlling by application scores, gender, income, and type of high school. We only consider students who did not open the intervention to avoid potential effects driven by the intervention we discuss in Section 5. As shown in Table 21 in Appendix 8.2, the results are similar for students who received the intervention.

[20] For each student, we draw the random program from a distribution of second reported programs conditional on student's top-reported programs. We exclude from the distribution all reported programs in the ROL.

[21] This metric is computed as the absolute value of the difference between students' beliefs and the actual value of the average income, divided by the latter.

Figure 2: Knowledge about Average Income

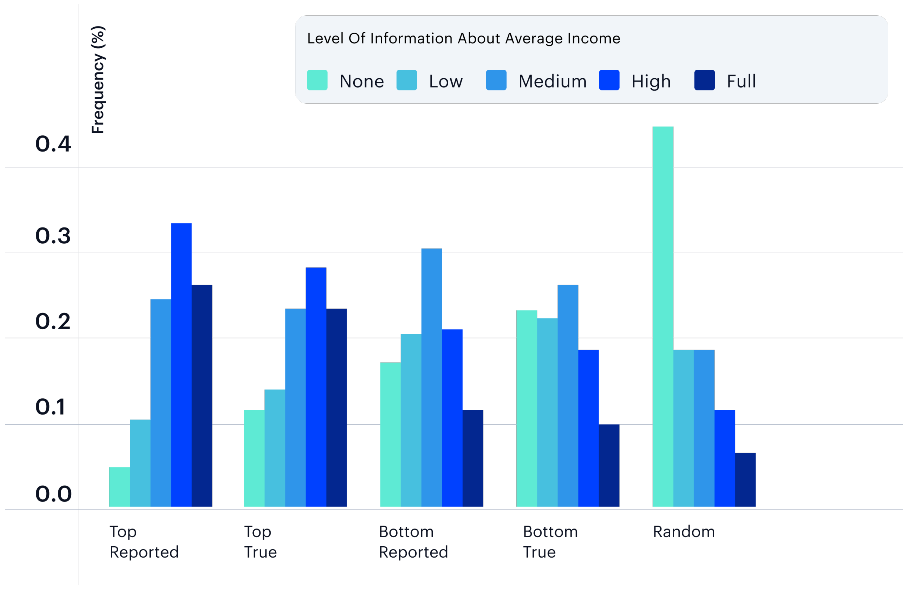


Table 7: Regression Results for Absolute Bias on Average Income

	Percentage of Absolute Bias in Average Income				
	Top Reported (1)	Top True (2)	Bottom Reported (3)	Bottom True (4)	Random (5)
Avg. Math-Verbal normalized	-21.317^{***}	-20.285^{***}	-20.441^{***}	-20.358^{***}	-20.648^{***}
	(0.888)	(1.273)	(0.959)	(1.815)	(0.922)
Female	7.329^{***}	4.373^{**}	6.441^{***}	8.444^{**}	6.898^{***}
	(1.551)	(2.119)	(1.674)	(3.340)	(1.626)
Family income below median	-4.338^{***}	-4.167^{**}	-4.256^{***}	-4.785	-5.831^{***}
	(1.509)	(2.009)	(1.629)	(3.280)	(1.584)
Public	1.943	-0.395	4.323^{*}	4.391	0.919
	(2.223)	(2.862)	(2.422)	(4.495)	(2.303)
Voucher	-1.717	-2.914	-0.174	1.402	-0.815
	(2.769)	(3.675)	(2.986)	(5.643)	(2.881)
Constant	58.016^{***}	58.704^{***}	58.966^{***}	59.890^{***}	62.983^{***}
	(2.429)	(3.215)	(2.627)	(4.959)	(2.506)
Observations	6,854	3,365	6,144	1,606	3,943

Note: Sample includes all students who completed the survey and did not receive the intervention. Significance reported: $^{*}p < 0.1;^{**}p < 0.05;^{***}p < 0.01$.

First, we observe that the constant is large and significant for all the models considered, suggesting that students have incorrect beliefs about their expected income upon graduation. Second, we observe that biases are negatively correlated with application scores, meaning that students with lower scores have less accurate beliefs. Third, we observe that the variable Female is positive and significant, i.e., women are more biased about average expected earnings than men. Finally, in Tables 20 and 22 Appendix 8.2, we report the results considering the percentage of bias (i.e., without absolute value), and we find that students over-estimate the average expected earnings upon graduation. Overall, these results suggest that students have large biases in their assessment of the average future earnings, potentially leading them to *mistakes on valuations*.

In summary, the results reported in this section suggest that application mistakes are prevalent and relevant and that their effects are particularly severe for students from low-SES backgrounds. In addition, these mistakes correlate with biased beliefs about programs' cutoffs, information frictions about requirements, biased beliefs about programs' characteristics, and potential misunderstanding of the assignment process, which can impact their beliefs about admission probabilities, their level of awareness and valuations and, consequentially, their applications. To assess whether information policies can address these frictions and the resulting application mistakes, we designed and implemented two nationwide interventions providing students with key information at the time they were submitting their applications. We will discuss these in the remainder of this paper.

5 Randomized Information Intervention

In collaboration with MINEDUC, we designed and implemented an intervention to provide information and recommendations to students during the application process in 2022. Specifically, we created a personalized website for each student who submitted their application within the first two days of the five-day application window using their initial reported preferences,[22] and MINEDUC sent emails inviting students to open their personalized websites at the beginning of the third day of applications.

Our intervention exploits the fact that students are allowed to modify their list as many times as they want within this time window, so we can measure the effectiveness of the intervention by comparing the preferences reported before and after it and their corresponding outcomes. Moreover, as we discuss next, we randomized the information provided to each student, allowing us to study the effect of each part of the intervention.

5.1 Experimental Design.

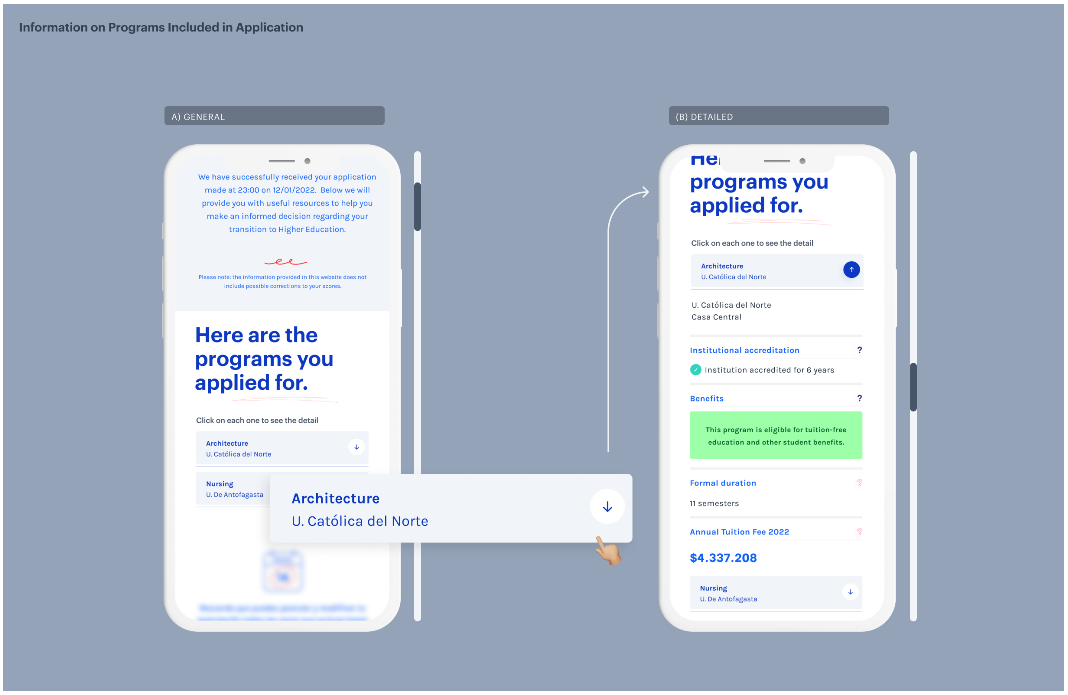
The information included in the personalized websites was carefully tailored to address the information frictions and causes of mistakes outlined in the above sections. Specifically, the intervention had four main modules:

M1 *General information about programs included in the applicant's list.* This module displays the application list of the student and, upon clinking on a particular program, the student can access detailed information including the program's address, the number of years that the institution is accredited for[23], benefits and types of financial aid for which the student is eligible to when enrolling in that program, its formal duration, measured in semesters, as well as yearly tuition fees in pesos. Figure 3 shows the design of module M1. This module aims to reduce *mistakes on valuations.*

[22] Students could submit their application list from January 11 to January 15, 2022, and we use January 12 at 8 pm as the cutoff to collect all applications and create the personalized websites.

[23] The years of accreditation is a signal of the quality of the institution. If the institution is not accredited, enrolled students cannot receive public student aid. See details in `https://www.cnachile.cl/`.

Figure 3: Information on Programs' Characteristics Included in Application



M2 *Personalized information about scores for programs included in the applicant's list.* This module extends M1 to include the application scores of the first and last student admitted in the processes of 2020 and 2021 among the admission tracks in which the student is participating, and a graphical comparison of these with their application score. Moreover, this module includes alerts at the program level in case that the student does not fulfill the requirements of the program. Figure 4 shows the design of module M2. Overall, this module aims to reduce *mistakes on admission probabilities.*

M3 *Personalized alerts depending on the admission probabilities.* This module extends M1 and M2 by including alerts at the program level and also overall depending on the students' admission probabilities. Specifically, M3 incorporates two new sources of information:

(a) Program-level alert: when the estimated admission probabilities are below 1%, we display the red alert in Figure 5 and include a message that stresses the low admission chances and invites the student to include more programs in their list. See Figure 13 in Appendix 8.3 for a detailed zoom.

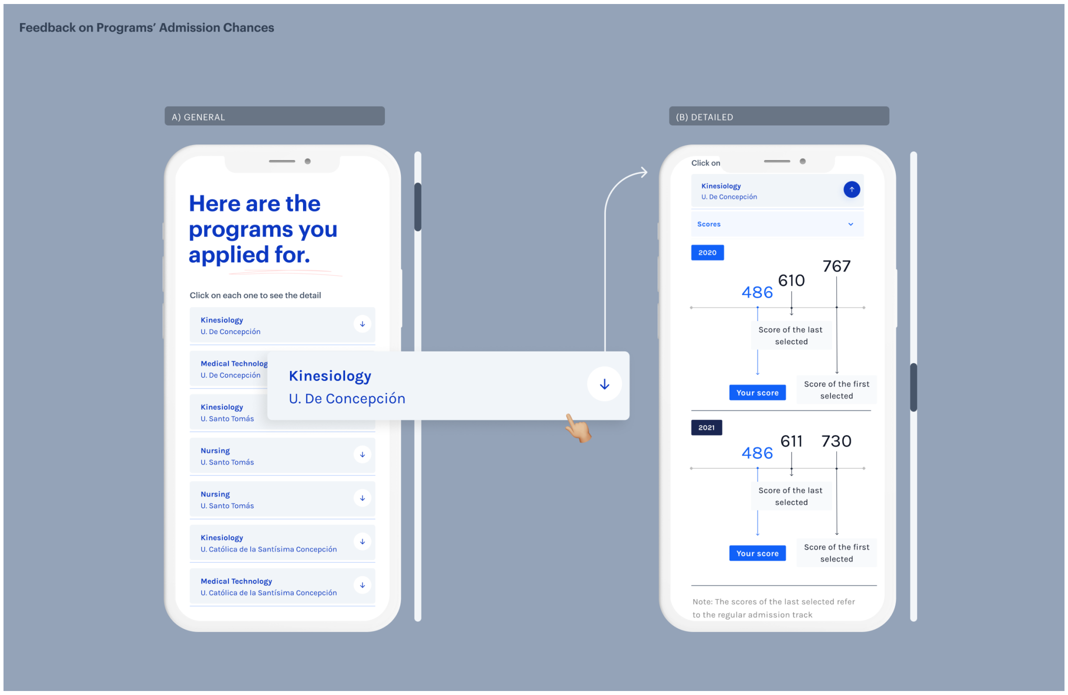
(b) Overall alert: depending on the admission probability at the top preference and also the overall probability of being assigned to any program, we display an alert nudging students to consider additional programs in their application list. We consider three different messages:

   i. If the overall probability of being assigned is below 99%, we recommend students to add *safety* programs, i.e., programs for which the student faces a positive admission probability. The idea of this message is to prevent potential *over-confidence* mistakes.

   ii. If the probability of being assigned to the top preference is above 99%, we recommend students to add *reach* programs, i.e., programs that are generally more preferred, that the student may be interested in,[24] and for which the student faces positive admission probability. The idea of this message is to prevent potential *under-confidence* mistakes.

[24]To determine potential *reach* programs, we use the information on students' top-true preferences in the survey of 2021. We

Figure 4: Feedback on Programs' Cutoffs



iii. Otherwise, we display a message inviting students to *explore* and get information about other programs. Since these students are almost certainly getting assigned to some preference in their list (but not necessarily their top preference), this message also aims to prevent potential *under-confidence* mistakes.

Figure 6 shows the different message types.

compute transition matrices for programs that are typically declared to be top-true preferences conditional on the top-reported preference submitted by the student.

Figure 5: Feedback on Programs' Admission Chances

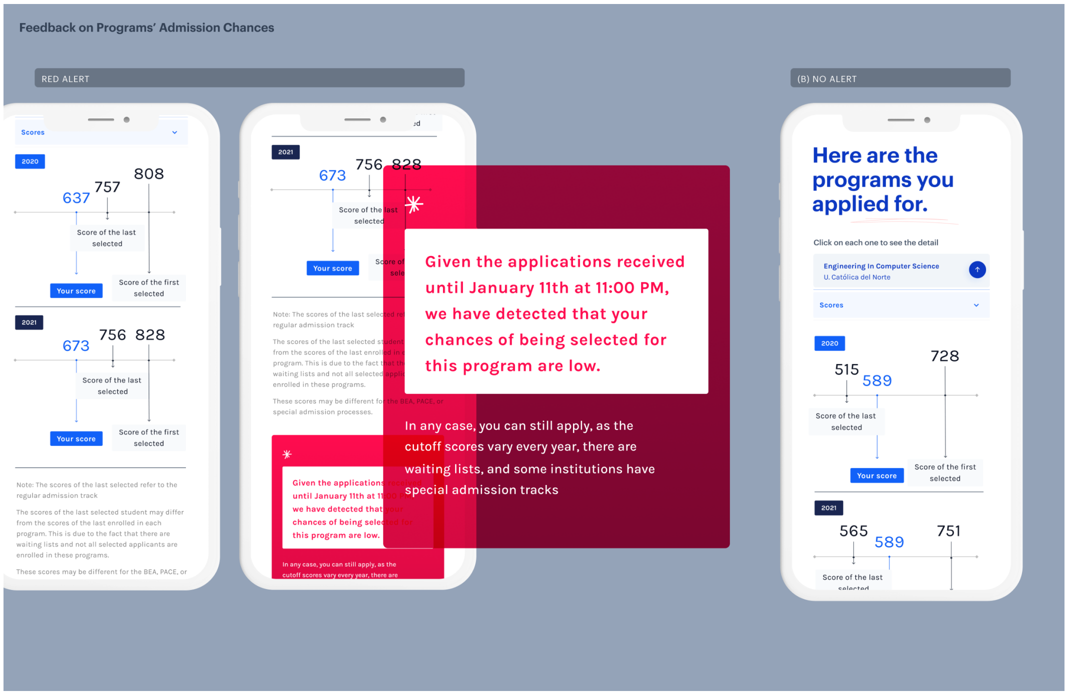
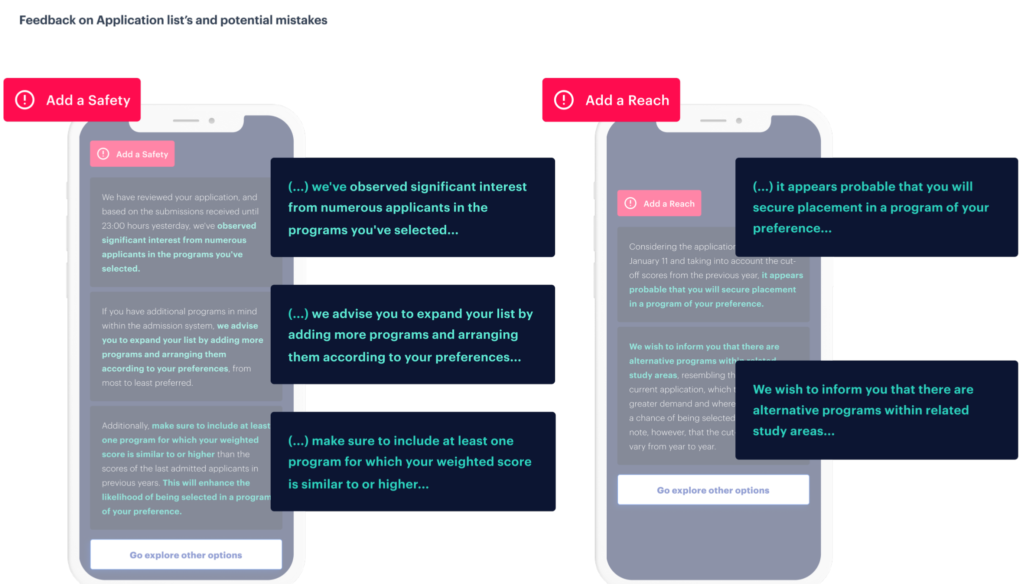


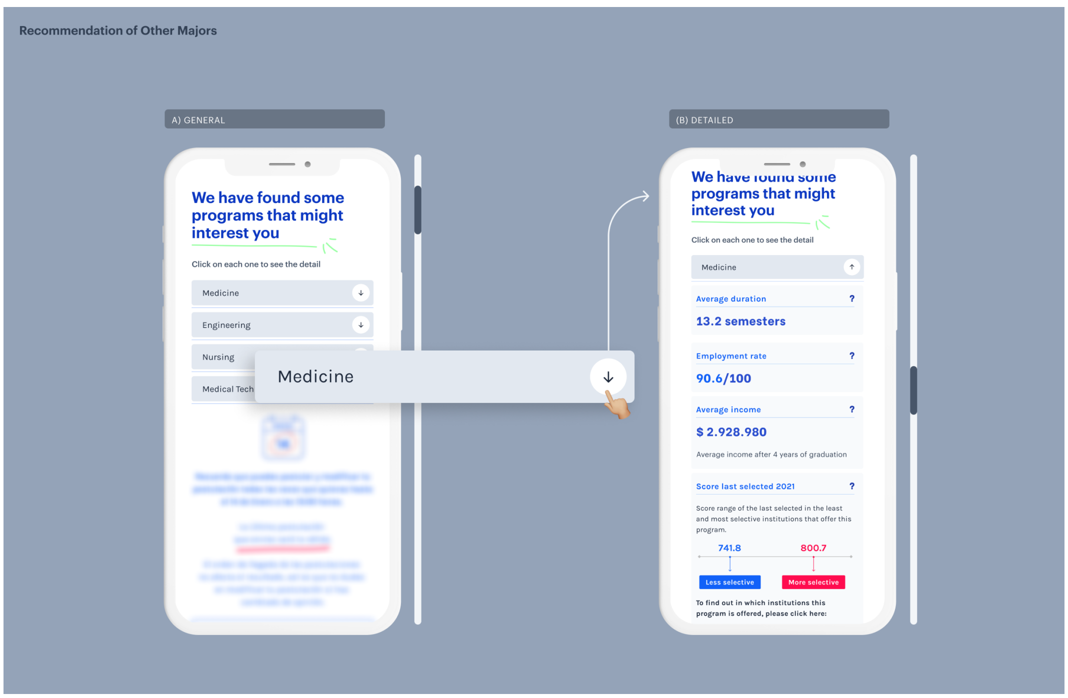
Figure 6: Feedback on Application and Potential Mistakes



In Appendix 8.3.3, we describe in detail how we compute the admission probabilities used in this module and also some additional details.

M4 *Personalized recommendations about other majors of potential interest.* Based on students' scores and their reported preferences, this module displays four personalized major recommendations to encourage students to consider other options.[25] Specifically, we recommend the two most preferred majors predicted based on the student's list, and the two majors with the highest expected wage among majors that may be of interest for the students and where they have a positive admission chance (one of these majors is from IPs/CFTs and the other from universities).[26] For each recommended major, we include relevant information including the average duration of the programs belonging to that major, the minimum/maximum application score of the last admitted student to any of the programs belonging to that major, and labor market outcomes such as average employment rate and average wages four years after graduation among programs belonging to that major. Figure 7 shows an example of this module. The idea of this module is to (i) reduce potential information frictions about non-listed programs' characteristics, potentially reducing *mistakes on valuations*; (ii) prompt search behavior to reduce potential *mistakes on awareness*, and (iii) affect students' beliefs on admission probabilities for programs that are not in their consideration sets, potentially mitigating *under-confidence* mistakes.

Figure 7: Major Recommendations



To properly evaluate the impact of each module, we consider four treatment groups that differ in the selection of modules displayed on the applicants' personalized websites.

T1 General information: only M1 is displayed.

T2 General information + scores: M1 and M2 are displayed.

T3 General information + scores + alerts: M1, M2 and M3 are displayed.

T4 General information + recommendations: M1 and M4 are displayed.

[25] MINEDUC did not allow us to make program-specific recommendations to avoid favoring some schools/universities.
[26] In Appendix 8.3.4, we describe in detail how we compute these recommendations.

To ensure balance on observable characteristics across groups, we randomly assigned each student who applied before January 12 at 8 pm CT to one of these groups in a stratified way. Moreover, we randomly selected 30,000 students and sent them an SMS encouraging them to open their personalized websites using the same stratification.[27] Finally, note that T1 serves as a control group since students in this group only received module M1, also included in the other treatments.[28]

5.2 Effect on Outcomes

In Table 8, we report summary statistics for the results of the intervention across different outcomes, separated by treatment group. The column *Opened* reports the fraction of students who opened the intervention. The columns *Modified* and *Increased* (*Decreased*, resp.) relate to the application and represent the fraction of students who modified their application and who increased (decreased, resp.) the number of valid applications included in their preference list. The columns *Inc. Prob* and *Assigned* capture the fraction of students that increased their overall admission probability and got assigned to some program at the end of the admissions process, respectively. The columns *Entered* (*Left*, resp.) and *Improved* report the fraction of students who entered (left, resp.) the assignment (i.e., who were initially unassigned (assigned) and later result (un)assigned at the end of the admissions process and, thus prevented (resulted) being unmatched) and improved their assignment (preventing being undermatched), respectively. Finally, the column *Persisted* present the fraction of students who entered the assignment and enrolled in their preference of assignment at the end of the process in the two years after the intervention.

Table 8: Summary Statistics by Group

			Application			Assignment					
Treatment	Total	Opened [%]	Modified [%]	Increased [%]	Decreased [%]	Assigned [%]	Inc. Prob. [%]	Entered [%]	Left [%]	Improved [%]	Persisted [%]
T1	26403	28.19	11.044	9.635	1.583	78.321	1.401	3.049	0.161	3.219	1.346
		(0.277)	(0.193)	(0.182)	(0.077)	(0.254)	(0.072)	(0.224)	(0.028)	(0.123)	(0.15)
T2	26546	28.2	11.636	10.077	1.695	78.144	1.405	3.374	0.223	3.06	1.511
		(0.276)	(0.197)	(0.185)	(0.079)	(0.254)	(0.072)	(0.234)	(0.033)	(0.12)	(0.158)
T3	26512	28.134	11.855	10.222	1.777	78.308	1.562	3.741	0.185	3.407	1.769
		(0.276)	(0.199)	(0.186)	(0.081)	(0.253)	(0.076)	(0.246)	(0.03)	(0.126)	(0.171)
T4	26467	28.398	11.448	9.801	1.787	78.135	1.462	3.251	0.209	2.971	1.482
		(0.277)	(0.196)	(0.183)	(0.081)	(0.254)	(0.074)	(0.23)	(0.032)	(0.119)	(0.157)

Note: Opened is a binary variable equal to 1 if the student opened the personalized website, 0 otherwise. Modified is a binary variable equal to 1 if the student modified its application after the personalized websites were sent, 0 otherwise. Increased (decreased) is a binary variable equal to 1 if the student increased (decreased) the number of valid applications in their list, 0 otherwise. Assigned is a binary variable equal to 1 if the student resulted assigned at the end of the process, 0 otherwise. Inc. Prob. is a binary variable equal to 1 if the student increased her admission probability at the end of the process, 0 otherwise. Entered (left) is a binary variable equal to 1 if the student resulted unassigned (assigned) given their list of preferences before the intervention and (un)assigned given their preferences after the intervention, 0 otherwise. Improved is a binary variable equal to 1 if the student improved on their preference of assignment, 0 otherwise. Persisted is a binary variable equal to 1 if the student entered the assignment and persisted for at least two years in that program, 0 otherwise. Standard errors reported in parentheses.

First, we observe that approximately 28% of the recipients of the intervention opened it, and we observe no significant differences across treatments. Second, we observe that students in treatment T2 and T3 were significantly more likely to modify their application and increase the number of valid preferences reported, while we observe no significant differences in decreasing the number of valid applications. This

[27] In Appendix 8.3.2, we describe the variables used for stratification and report the results of several balance checks.

[28] MINEDUC did not want to randomize the general information displayed for listed programs. However, information displayed in M1 is publicly available in the government website `https://mifuturo.cl/`.

result suggests that the intervention was effective at inducing students to add more programs to their lists. Third, we observe no significant differences in increasing the overall admission probability nor in the fraction of students assigned at the end of the process. However, we find significant differences in the number of students who enter and improve their assignment if we compare T3 with T1 (our control). Finally, we observe that students in T3 are more likely to persist in their program of assignment for at least two years after the intervention.

To formally analyze the effect of our intervention, in Table 9, we report the results of linear probability models on the outcomes described above among students who opened the intervention.[29]

Table 9: Regression Results among Openers OLS

	Applications			Assignment				
	Modified (1)	Increased (2)	Decreased (3)	Inc. Prob. (4)	Entered (5)	Left (6)	Improved (7)	Persisted (8)
Treatment 2	0.016***	0.007	0.002	0.001	0.004	0.002*	0.001	0.004
	(0.006)	(0.005)	(0.002)	(0.002)	(0.007)	(0.001)	(0.003)	(0.005)
Treatment 3	0.018***	0.009*	0.0002	0.005**	0.014**	0.001	0.007**	0.014***
	(0.006)	(0.005)	(0.002)	(0.002)	(0.007)	(0.001)	(0.003)	(0.005)
Treatment 4	0.006	−0.002	0.0002	−0.001	−0.004	0.002*	0.001	0.0003
	(0.006)	(0.005)	(0.002)	(0.002)	(0.007)	(0.001)	(0.003)	(0.005)
Constant	0.132***	0.113***	0.020***	0.016***	0.037***	0.001**	0.035***	0.014***
	(0.004)	(0.004)	(0.002)	(0.001)	(0.005)	(0.001)	(0.002)	(0.003)
Observations	29,904	29,904	29,904	29,904	6,328	23,576	23,576	6,328

Note: Modified is a binary variable equal to 1 if the student modified its application after the personalized websites were sent, 0 otherwise. Increased (decreased) is a binary variable equal to 1 if the student increased (decreased) the number of valid applications in their list, 0 otherwise. Inc. Prob. is a binary variable equal to 1 if the student increased her admission probability at the end of the process, 0 otherwise. Entered (left) is a binary variable equal to 1 if the student resulted unassigned (assigned) given their list of preferences before the intervention and (un)assigned given their preferences after the intervention, 0 otherwise. Improved is a binary variable equal to 1 if the student improved on their preference of assignment, 0 otherwise. Persisted is a binary variable equal to 1 if the student entered the assignment and persisted for at least two years in that program, 0 otherwise. Standard errors reported in parentheses. Significance reported: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

We observe that treatments T2 and T3 had a positive and significant effect on inducing students to modify their preferences, while T4 had no effect. However, we find that only T3 had a positive and significant effect on inducing students to increase the number of valid preferences and to improve their assignment outcomes. Indeed, students in T3, compared to the control group, are 38% more likely to enter the assignment (thus reducing the number of students who are unmatched), 20% more likely to improve in their preference of assignment (thus reducing the number of students who are undermatched), and 100% more likely to persist in their program of assignment for at least two years after the intervention when they entered. These results suggest that combining information about previous cutoffs with alerts at the program and overall level can effectively induce changes in students' application behavior, resulting in better outcomes.

[29]In Table 24 in Appendix 8.3.5, we show summary statistics by treatment group and reception status. The analysis on this conditional sample can still be interpreted as causal because the treatments have no impact on students prior to opening their emails, and the sample is balanced across our stratification variables.

5.3 Effect on Mistakes on Assignment Probabilities

We now analyze the effects of our intervention on mistakes in assignment probabilities. First, recall that a student makes an *over-confidence* mistake if they risk being unassigned and skip a program that they prefer over being unassigned and for which they have a positive admission probability. Note that a student who *entered* necessarily had a high risk of being unassigned with their initial preferences, and they necessarily modified them to add a program where they had a positive admission chance, allowing them to get assigned. Hence, whenever students receive information about previous years cutoff scores (T2 and T3) or *safety* messages (T3), we interpret the effect of our intervention on the outcome *enter* as a reduction in *over-confidence mistakes*. Second, note that a student makes an *under-confidence* mistake if they skip a program they prefer over other programs in their ROL and have a positive admission probability. By definition, students who *improved by adding* a program necessarily included one they preferred over those in their initial ROL and for which they had a positive admission probability. Hence, whenever students receive *Explore* and *Reach* messages, we interpret the effect of our intervention on the outcome *improve by adding* as a reduction in *under-confidence mistakes*. Finally, recall that a student makes an *ordering* mistake if they report their preferences in incorrect order (i.e., not decreasingly according to their utilities). Hence, whenever students receive *Explore* and *Reach* messages, we interpret the effect of our intervention on the outcome *improve by re-ordering* as a reduction in *ordering mistakes*.

To test these conjectures, in Table 10, we report the results of linear probability models for the outcomes *enter*, *improve by adding* and *improve by ordering* among students who opened the intervention, separating by message type.

Table 10: Regression Results among Openers OLS by Message Type

	Entered		Improved by Adding			Improved by Re-Ordering		
	Safety (1)	Explore (2)	Safety (3)	Explore (4)	Reach (5)	Safety (6)	Explore (7)	Reach (8)
Treatment 2	0.011^{*}	-0.067^{**}	-0.000	0.002	-0.002	-0.011	0.000	-0.003
	(0.007)	(0.031)	(0.015)	(0.002)	(0.004)	(0.007)	(0.003)	(0.008)
Treatment 3	0.016^{**}	-0.016	0.010	0.001	-0.000	-0.011	0.007^{**}	0.003
	(0.007)	(0.031)	(0.015)	(0.002)	(0.004)	(0.007)	(0.003)	(0.008)
Treatment 4	-0.001	-0.035	0.030^{**}	-0.001	-0.002	-0.011	0.002	-0.003
	(0.007)	(0.032)	(0.015)	(0.002)	(0.004)	(0.007)	(0.003)	(0.008)
Constant	0.029^{***}	0.115^{***}	-0.000	0.012^{***}	0.007^{**}	0.011^{**}	0.024^{***}	0.021^{***}
	(0.005)	(0.023)	(0.011)	(0.002)	(0.003)	(0.005)	(0.002)	(0.006)
Observations	5,677	651	385	20,628	2,563	385	20,628	2,563

Note: Significance reported: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

First, we observe that T3 had a positive and significant effect on *enter* among students eligible to receive the safety message. Moreover, the magnitude of the effect is sizable, as it implies an increment of over 50% in the probability of entering the assignment. Second, we observe no effect of T3 on *enter* for students eligible for the *explore* message, while we find a negative and significant effect of T2 on this group.[30] These two results suggest that alerts are key to effectively guide students on improving their application to increase their chances of admission and avoid *over-confidence* mistakes. Third, we observe that T4 had a positive and significant effect on *improve by adding* among students in the safety group message, while no other treatment had an effect on this outcome for any group. This result suggests that, despite its limitations, the recommendation module of our intervention induced students to add programs (more

[30]Note that the outcome *enter* is not defined for students who qualified for the *reach* message, as these students had an admission probability equal to one in their top preference of their initial preference list.

preferred than those in their initial ROL) for which they had a positive admission probability and where they ended up being assigned, reducing the incidence of *under-confidence* mistakes. Finally, we observe that T3 is the only treatment with a positive and significant effect on *improve by re-ordering*. Moreover, this effect holds only for students eligible to receive the *explore* message. This result suggests that T3 can also reduce *ordering* mistakes, specially among students who are likely to get assigned to a preference below their top reported one. However, the effects are smaller compared to the reduction in *over-confidence* mistakes. Overall, these results suggest that combining previous cutoffs with personalized alerts and recommendations at the major level can effectively reduce application mistakes.

5.3.1 Drivers

As discussed in Section 4.2, the primary driver of mistakes on admission probabilities are biased beliefs on admission chances. To test whether the reduction in mistakes discussed above results from correcting students' biases about their admission chances, we use the elicited beliefs from our survey, compute the absolute value of the difference between beliefs and rational expectation probabilities for the top-reported, bottom-reported, true top, and true bottom programs, and we compare these biases across treatments.

In Table 11, we report the results of linear regressions of the absolute bias in beliefs on admission probabilities over each program, controlling for our stratification variables. As neither the Control group nor Treatment 4 provided any information regarding cutoffs or admission probabilities, we pooled the data from these groups.[31] Our results suggest that students in Treatment 3, who received warning messages, show lower bias in beliefs on admission probabilities over their bottom-reported program. Additionally, students in Treatment 2 and Treatment 3 show lower bias in beliefs over their true bottom program. However, we observe no significant differences in the bias in beliefs over the top-reported and true top programs.

These results support our hypothesis that the intervention influences students' application behavior by altering their beliefs about their admission probabilities. Notably, we observe these effects in programs not at the top of students' preferences—programs for which students have higher baseline biases. Our findings indicate that the treatment primarily impacts students initially at a high risk of not being assigned to the system, potentially due to *over-confidence* mistakes.

Table 11: Treatment effects on absolute bias on admission probabilities

	Top-reported (1)	Top-true (2)	Bottom-reported (3)	Bottom-true (4)	Random-program (5)
Treatment 2	-0.003	-0.006	-0.005	-0.064^{***}	-0.004
	(0.010)	(0.010)	(0.011)	(0.020)	(0.011)
Treatment 3	-0.002	-0.006	-0.020^{*}	-0.046^{**}	-0.013
	(0.010)	(0.010)	(0.011)	(0.020)	(0.011)
Constant	0.215^{***}	0.316^{***}	0.302^{***}	0.353^{***}	0.337^{***}
	(0.027)	(0.027)	(0.028)	(0.054)	(0.029)
Observations	5,151	5,320	4,356	1,205	4,496

Note: The analysis employs OLS regression models to examine the absolute value of each student's subjective bias towards admission probabilities for a given program. The sample is limited to students who responded to the survey and opened the intervention, with exclusions for misfits. Programs with well-defined cutoff scores are included in the sample. Gender, scores, region, and general message (risk) are used as controls. Standard errors are reported in parentheses. Significance reported: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

[31] We perform Welch Two Sample t-tests for each outcome variable and reject the null hypothesis that T1 and T4 have different means.

Spillover effects between channels. Note that the information provided in treatments T2 and T3 may reduce students' biases on their admission chances, incentivizing them to modify (either adding or replacing) programs to their ROL. As a result, these treatments may induce students to search for new programs to add or review other programs' characteristics, potentially reducing mistakes on valuations and awareness. If this were the case, we would expect students in these treatments to also update their beliefs about average wages. However, we do not find evidence of this, as shown in Table 12.

5.4 Effect on Mistakes on Valuations and Awareness

To assess whether the information policy can alleviate biases on beliefs about program characteristics that could translate into *mistakes on valuations* or prompt search behavior to expand students' consideration sets and reduce *mistakes on awareness*, we exploit the fact that students in Treatment 4 received information for programs outside their initial ROL (aggregated at the major level), while students in the other treatments only received information for their listed programs.

As discussed in Section 5.1, module M4 provided students with information on average wages post-graduation. Additionally, in the 2022 survey, we elicited students' beliefs about average wages conditional on enrollment and graduation. Then, using these two sources of data (RCT and survey), we compute measures of absolute bias for average wages over the top-reported, bottom-reported, true top, true bottom, and a random program.

In Table 12, we report the results of linear regressions on the percentage of absolute bias in beliefs about average wages on the treatment group.[32]

Table 12: Regression Results for Absolute Bias on Average Income

	Top-Reported (1)	Bottom-Reported (2)	Top-True (3)	Bottom-True (4)	Random Program (5)
Treatment 2	3.351	-1.098	-5.045	8.733	-0.414
	(2.890)	(3.037)	(4.196)	(6.784)	(3.171)
Treatment 3	-1.583	-1.707	-3.510	7.191	-1.195
	(2.902)	(3.018)	(4.256)	(6.647)	(3.146)
Treatment 4	-2.661	-3.512	-7.376^{*}	-6.657	-6.150^{**}
	(2.897)	(3.009)	(4.270)	(6.784)	(3.131)
Constant	47.521^{***}	51.612^{***}	53.513^{***}	48.461^{***}	55.372^{***}
	(2.070)	(2.145)	(3.069)	(4.803)	(2.233)
Observations	3,940	3,567	1,799	890	3,699

Note: The analysis employs OLS regression models to examine the proportional change in absolute value of each student's subjective bias towards the average earnings of graduates of a given program by the fourth year of graduation. The sample is limited to students who responded to the survey and opened the intervention, with exclusions for misfits. Programs with well-defined cutoff scores are included in the sample. Standard errors are reported in parentheses. Significance reported: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

We observe that students in Treatment 4, who received the recommendation module, exhibit lower biases in beliefs over average wages for random programs outside their ROL. However, we observe no significant differences for the other treatments relative to the control group. Since Treatment 4 was designed to potentially have an effect on *mistakes on valuations* and *mistakes on awareness*, the reduction on biases on program characteristics may explain the significant effect of Treatment 4 on students who improved by adding programs to their ROL.

[32] In Table 28 in Appendix 8.3.5, we report the results over bias for this characteristic.

5.5 Congestion Effects and Scale-up

To evaluate whether information policies like the one described above may scale up effectively, it is crucial to isolate its effects from the potential interference generated by congestion. Indeed, because the intervention affects a significant fraction of applicants, changes in the applications of students who received a given treatment may affect programs' cutoffs, impacting the assignment of students in the other groups. Thus, our measured effects load both the treatment and congestion effects produced by the policy. For this reason, in this section, we focus on studying how congestion and interference affect our results.

To accomplish this, we follow the bootstrap procedure described in Karnani (2023) and perform three counterfactual simulations: (i) *RCT*, which aims to replicate the results from our intervention and provide a benchmark for comparison; (ii) *Control*, which aims to simulate what would happen in the absence of the information intervention; and (iii) *Scale-up*, which aims to simulate what would happen in the counterfactual where every student that applies in the first half of the process receives the full information policy (i.e., T3), mimicking the scale-up of the policy that we later discuss in Section 6. In each case, and for each bootstrap simulation, we remove all students who participated in our intervention (i.e., T1, T2, T3, or T4) and replace them with students from a specific group that depends on the counterfactual. Specifically, for *RCT*, we replace these students with others sampled from any treatment group, all with equal probability. In contrast, for *Control* (*Scale up*, resp.), we replace them with students sampled from the T1 (T3, resp.).

The previous procedure generates, for each bootstrap simulation, a market with the same number of students (equal to the total number of students who applied to the system) that were either part of the intervention sample or were not eligible to receive the intervention because they applied in the second half of the application time window. Since we randomly assigned students to the control and the different treatment groups, replacing treated students with others in the control or treatment arms (instead of using students who applied later and were not eligible to receive the intervention) prevents selection issues from arising from the time of application. Then, for each bootstrap simulation, we solve for the assignment, compute the cutoff of each program, and use these cutoffs to evaluate what would have been each student's assignment in the bootstrapped market considering both their interim and final applications. For the latter, we assign each student to the most preferred program in their list for which their application score is above the corresponding cutoff in the bootstrap simulation.[33] Finally, using these assignments, we can compute all the outcomes described in Section 5.2 in the simulated counterfactual scenarios.

In Table 13, we report the mean value across students and bootstrap simulations for each outcome of interest in each counterfactual. The first four rows report the results for *RCT*, separating by treatment group, while the next two rows report the results for *Control* and *Scale-up*, respectively.

Table 13: Counterfactual outcomes varying congestion levels

Counterfactual	Treatment	Assigned [%]	Entered [%]	Left [%]	Improved [%]	Worsened [%]
RCT	T1	78.131	3.646	0.256	3.857	0.349
	T2	78.042	4.049	0.307	3.696	0.345
	T3	78.168	4.271	0.272	4.079	0.368
	T4	78.014	3.802	0.316	3.675	0.356
Control	T1	77.824	3.815	0.351	4.116	0.597
Scale-up	T3	77.800	4.482	0.332	4.282	0.599

[33]Note that this approach implicitly makes a large market assumption, as students are "cutoff takers" and do not affect them. As a result of this procedure, we can simulate the assignment that each student (regardless of the information they received) would get in the absence of the effects of the intervention and, thus, we can use these assignments to estimate the treatment effects of each of our interventions.

First, comparing the results from *RCT* and *Control*, we observe that there exists some degree of congestion. Specifically, by comparing the results for T1 among these counterfactuals, we observe that there is *spillover effect on the control group* (as defined in Karnani (2023)) for several outcomes, including entered (-0.169% = 3.646% - 3.815%) and improve (-0.259% = 3.857% - 4.116%). Second, since the level of congestion is not negligible, the results reported in Section 5.2 represent the causal effect of receiving our information policy and should not be interpreted as average treatment effect on the treated (ATT). However, following Karnani (2023), we can estimate the ATT by taking the difference between the outcomes in *RCT* and those in *Control*. For instance, the ATT for enter among students who were part of T3 is 0.456% (=4.271%-3.815%). Third, comparing the results of *RCT* and *Scale-up* for T3, we observe that congestion does not prevent the policy to scale-up effectively. In particular, we observe that students are more likely to enter the assignment under the scale-up counterfactual (4.482% vs. 4.271% in the RCT), and the results for improve are relatively similar. One potential reason behind these effects is that the centralized system is not at full capacity, as close to a third of programs have vacancies at the end of the process. Hence, improvements in assignment outcomes, especially at the extensive margin (such as *Entered*), are not necessarily a zero-sum game.

6 Policy Implementation

Given the positive results discussed in Section 5, MINEDUC decided to implement the information policy nationwide in 2023, addressing the main limitations of the 2022 field experiment and adapting the policy design to accommodate important changes in the application process. Overall, the policy implementation was similar to that described in the previous section, i.e., we generated personalized websites including all the modules listed above (i.e., no randomization) for students who applied in the first half of the application period and provided them with personalized alerts to improve their application.

As we discuss in detail in Appendix 8.4.1, the admissions process of 2023 was one of the most challenging and uncertain for students since the inception of the centralized system, as the entrance exam changed by both including one additional exam and, more critically, by changing the scale of scores of all the admissions factors (moving from a range $[210, 850]$ to $[100, 1000]$]). As a result, previous year cutoffs were no longer informative of programs' selectivity and, thus, students had no point of comparison nor information to assess their chances of admission.

Given this challenge, the most relevant change we implemented in 2023 was to replace previous years' cutoffs with the score of the last student that would be admitted to each program considering the applications received so far and modified M3 to condition the personalized alerts only on the interim probabilities.[34] Figures 8 and 9 show an example of the personalized information that students received regarding their listed programs.

[34]Since the range of possible scores in the entrance exams changed in 2023, previous year cutoffs were no longer informative.

Figure 8: General

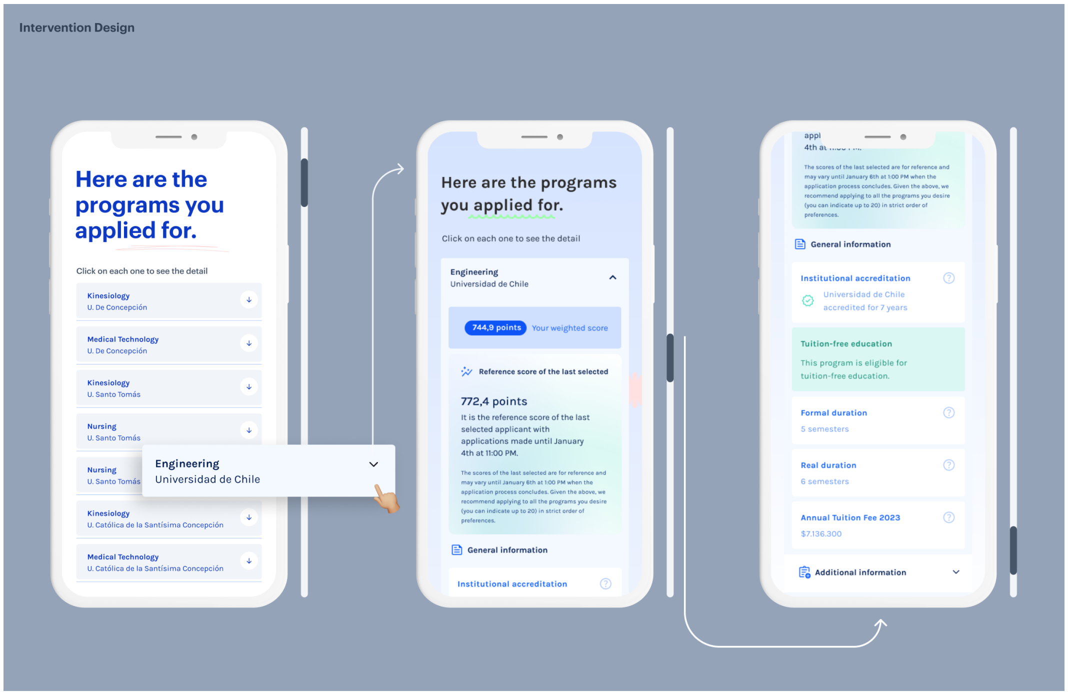
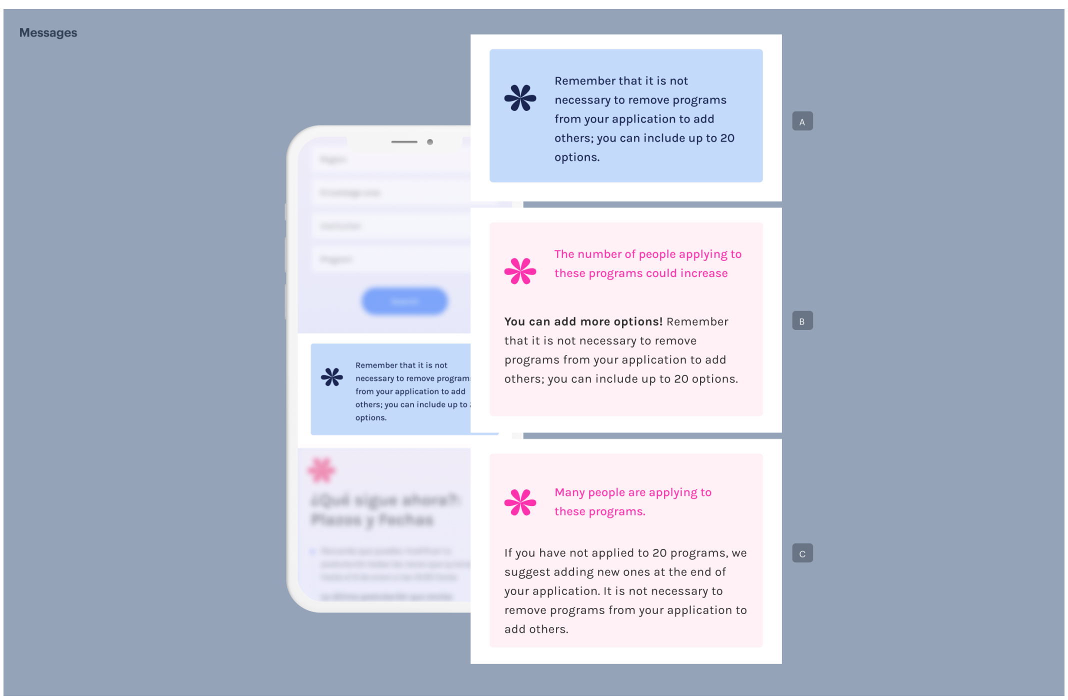
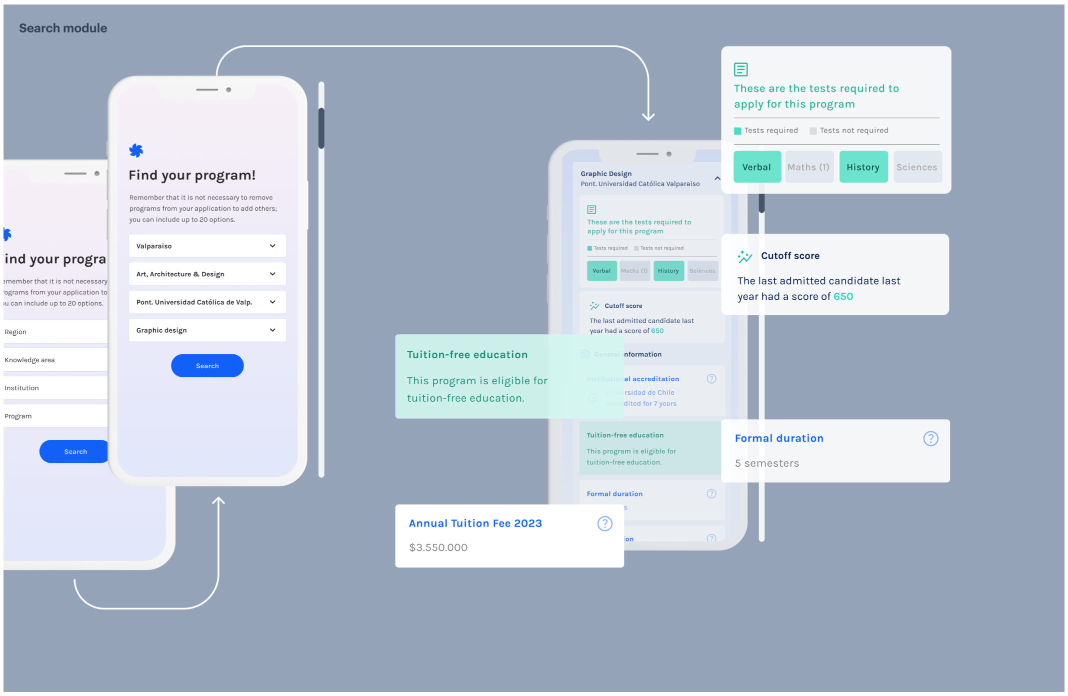


Figure 9: Feedback on Application Strategy



Motivated by the limitations of the recommendation module discussed in Section 5 (i.e., recommendations at the major level as opposed to at the program level), we also updated module M4 to provide students with a search engine to find programs based on different filters (e.g., location, major, university) and provide them information related to their current cutoff given the applications received so far (aiming to reduce *mistakes on admission probabilities*) and some other relevant information, including tuition, duration, and benefits (aiming to reduce *mistakes on valuations* and *mistakes on awareness*) (see Figure 10). Implemented at scale, this new design resembles a one-shot version of the *iterative* Deferred Acceptance algorithm, whereby students receive live information about the current cutoffs and can modify their preferences accordingly.

Figure 10: Search module



Finally, to evaluate the impact of this policy while addressing potential unobserved differences between students who did and did not open their personalized websites, we use an encouragement design whereby we randomly select a group of students and send them a WhatsApp message motivating them to open their personalized website. Then, we use the fact of receiving the Whatsapp message as an instrument to measure the causal effect of opening the intervention.

6.1 Results

In Table 14, we report the second-stage results of our IV estimation strategy considering the same outcomes as in Table 9. Given that we have enrollment only for 2023, we replace the outcome *Persisted* with *Enrolled*, which is equal to one if the student was initially unassigned, enters and enrolls in the program of assignment, and zero otherwise.[35] Moreover, the last two columns consider as outcome the difference in predicted log monthly earnings between the areas of assignment given students' final and initial application, separating by their status based on their initial assignment (unassigned or assigned).[36]

[35] In Table 31 in Appendix 8.4.3, we report summary statistics for several outcomes of interest, separating by whether the student open the email and their risk group.

[36] We estimate these returns using the results in Bucarey et al. (2024), who estimate the log monthly earnings in 2019 for each area of study conditional on graduation, separating by gender. Note that the authors do not consider Agriculture or Art and Architecture programs, so we dropped these observations.

Table 14: Regression results: Instrumental Variables

	Applications			Assignment					Returns	
	Modify	Increased	Decreased	Incr. Prob.	Entered	Left	Improved	Enrolled	Unassigned	Assigned
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Open	0.120***	0.076***	0.009	0.021***	0.039**	−0.002	0.038***	0.033***	0.012**	0.005**
	(0.015)	(0.011)	(0.006)	(0.006)	(0.015)	(0.003)	(0.011)	(0.011)	(0.006)	(0.002)
Constant	0.159***	0.101***	0.010***	0.069***	0.042***	0.006*	0.042***	0.011*	0.015***	−0.004***
	(0.008)	(0.006)	(0.004)	(0.003)	(0.008)	(0.004)	(0.014)	(0.006)	(0.003)	(0.001)
Risk group	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	132,893	132,893	132,893	132,893	33,742	99,151	99,151	33,742	33,586	91,105

Note: Significance reported: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

First, consistent with the results reported in the previous section, we observe a positive and significant effect of opening the intervention in modifying the application, increasing the number of valid applications, and no impact on decreasing them. Although these effects align with those reported in Section 5, their magnitude is significantly larger. For instance, the share of students who modified (increased) their lists increases from 16% (10%) to roughly 28% (17%). Second, we observe that opening the intervention has a positive and significant effect on increasing the overall probability of admission (from 7% to 9%). Third, we find that opening the intervention has a positive and significant effect on improving students' assignments, as its impact is positive and significant for both *enter* and *improve*. Fourth, we observe that opening the intervention had a positive and significant effect on inducing students who entered the assignment to enroll in the corresponding programs, suggesting that our intervention significantly affected long-term outcomes. Remarkably, we observe that the latter three effects (i.e., on *enter*, *improve*, and *enroll*) are pronounced, as they involve duplicating and triplicating the effects relative to students in the compliers group who did not open the intervention. Finally, from the last two columns, we observe that opening the intervention had a positive and significant effect on the difference in expected returns, regardless of the initial assignment status. This result suggests that students who opened the intervention changed their assignment and ended up assigned to programs where they face significantly higher earnings conditional on graduation. Overall, these results suggest that the information policy scaled well and significantly affected several outcomes of interest, including assignment outcomes and relevant long-term ones such as enrollment and returns.

To further characterize the set of students who benefitted the most from the intervention, in Table 15, we report the results on *enter*, *improve by adding* and *improved by re-ordering*, separating by risk. Specifically, we classify students depending on their overall admission probability for their initial ROL in three groups: (i) high-risk, which includes students with an overall admission probability below 1%; (ii) medium-risk, which includes students with an overall admission probability in $[1\%, 99\%]$; and (iii) low-risk, which includes students with overall admission probability above 99%. Note that this classification is not equivalent to that used in Section 5, as students eligible to receive the *safety* message would belong to either the low or medium-risk groups, while the students eligible for the *explore* message would belong to the low-risk group.

First, we observe that the positive effect on *entered* comes from students in the high-risk group. This result is intuitive, as medium and low-risk students get assigned with almost complete certainty, and thus, there is no way to increase their overall probability of assignment. However, the magnitude of the effect is quite substantial, as it implies that students in the high risk group who opened the intervention were twice more likely to enter than students who did not open it, decreasing substantially the number of unmatched students when scaling up. Second, we observe a positive and significant effect on *improved by adding* among students in the low-risk group. Recall that students in this group would most likely get assigned with their initial preferences lists, so this result implies that when they receive the information

Table 15: Regression results: Instrumental Variables (admission outcomes by risk level)

	Entered			Improved by Adding			Improved by Re-Ordering		
	High (1)	Medium (2)	Low (3)	High (4)	Medium (5)	Low (6)	High (7)	Medium (8)	Low (9)
Open	0.040**	0.015	0.458	−0.018	0.012	0.024***	0.383	0.003	0.016
	(0.016)	(0.057)	(0.423)	(0.140)	(0.016)	(0.007)	(0.287)	(0.015)	(0.010)
Constant	0.041***	0.075**	−0.236	0.031	0.011	0.008**	−0.182	0.015*	0.041***
	(0.009)	(0.031)	(0.288)	(0.083)	(0.009)	(0.004)	(0.170)	(0.008)	(0.006)
Observations	29,939	3,642	161	389	10,208	88,554	389	10,208	88,554

Note: Significance reported: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

policy, they update their lists by adding a program they prefer over those already included. Noteworthy, students in the low-risk group who opened the intervention were three times more likely to improve by adding than those who did not, decreasing substantially the number of undermatched students when scaling up.

In Appendix 8.4.2, we provide several robustness checks. First, in Table 29, we report the results of the first-stage of our IV estimator. We find that receiving the WhatsApp had a positive and significant effect on encouraging students to open their personalized websites and receive the intervention, and in Table 30 we show that this encouragement was properly randomized. Second, in Table 31, we show the average outcomes separating by risk group and by whether the students opened the intervention, and we find consistent results to those reported above. Third, in Table 33, we report the results for enrollment and find that the intervention had a positive effect on students from the high-risk group.[37] Fourth, in Table 32, we report the results for other outcomes of potential interest and find that the positive effect of the intervention is consistent across all these outcomes. Fourth, in Table 34, we analyze the use of the search engine and find that it is correlated with increasing the number of applications, valid applications, and also is positively correlated with increasing the overall probability of admission and entering the assignment. Finally, in Appendix 8.5, we analyze the effects of the policy on beliefs over cutoffs and admission probabilities and find a similar but stronger pattern on biases reduction than for the 2022 RCT.

In summary, the intervention positively and significantly affected both application and admission-related outcomes. Moreover, we find that the results of the policy implementation are consistent with those observed for treatment T3 in our randomized control trial. However, for some outcomes (e.g., entered and improved), we observe that the effects of the scaled-up policy were significantly larger than those reported in Section 5.3. One possible reason is that students in the 2023 admissions process faced significantly more uncertainty than those in 2022, given all the changes introduced to the system and, precisely, the change in scale and the corresponding uninformativeness of previous year cutoffs. Another reason is that the intervention significantly improved the information acquisition process by design, as students could observe the current cutoffs for all the programs in the centralized system, which helped them to assess their chances and find other programs to include in their application. Therefore, we conclude that these information policies can scale up effectively to help students improve their applications and outcomes in the admissions process, especially in highly uncertain environments.

[37] One potential explanation is that students in the low and medium-risk groups get assigned to desirable programs, so they are likely to enroll in their program of assignment. In contrast, students in the high-risk group get assigned to borderline programs compared to the outside option (e.g., going to technical schools, the military, or simply joining the labor force), so any improvement in their assignment may result in a higher chance of enrollment.

7 Discussion

7.1 Learning from the Scale-Up and the Last-Mile Problems

Through our five-year endeavor, we learned several lessons at every stage that helped to guide the design of subsequent steps. Theory allowed us to hypothesize potential mechanisms that linked the existence of information frictions and application mistakes, shedding light on how to measure them and how information policies could mitigate them and improve outcomes. The surveys and administrative data enabled us to confirm the existence of information frictions on several margins and measure the prevalence and relevance of application mistakes. The field experiments showed us how light-touch personalized information interventions could effectively reduce some of these mistakes and how the effects on outcomes could be robust at scale. Finally, the policy implementation allowed us to test the effects of the information policy at scale in a high-uncertainty environment.

Although we consider the process successful and the policy implemented in 2023 helped to reduce application mistakes and significantly improved students' outcomes, we also faced several last-mile challenges that are important to highlight as learning experiences for policymakers and market designers. First, we carefully designed the information policy in collaboration with MINEDUC, but other stakeholders were not directly involved in the design and opposed this type of intervention in a year with many changes to the admissions process. Given the partial support from school officials, MINEDUC had to make several concessions in the design to increase the policy's viability. For instance, we could not show students a range of forecasted cutoff scores instead of current cutoffs, which we believe was an important part of the policy design. Moreover, the insufficient level of coordination between stakeholders led to some confusion during the policy implementation. For instance, universities did not know how we were computing the current cutoff scores, but they had to answer calls from students inquiring about their chances of admission.[38]

Overall, we believe that the lack of alignment among relevant stakeholders, particularly in the design phase, led to a sub-optimal policy design and implementation. Hence, there is significant room for improvement in future iterations of this policy.

7.2 Implications for Market Design

Our previous results indicate that information frictions significantly impact the performance of centralized college admissions systems, even in the absence of clear strategic incentives for students to misreport their preferences. Our findings have three main implications that could apply to other setting with strategy-proof mechanisms in place and that could be of interest for market designers.

First, given that strategy-proof mechanisms are not immune to application mistakes and the prevalence of payoff-relevant mistakes is significant, policymakers may want to implement mechanisms that are more robust to information frictions and behavioral biases. As discussed by Rees-Jones and Shorrer (2023), sequential assignment procedures, such as dynamic implementations of DA (Bó and Hakimov, 2022), can serve as viable alternatives in real-world two-sided matching markets. For example, sequential assignment procedures can enhance the performance of these markets when students lack full information about their preferences (Grenet et al. (2022)), when behavioral biases lead to misrepresentations of preferences (Meisner and von Wangenheim (2021) and Dreyfuss et al. (2022a)), or when costly information acquisition is prevalent (Immorlica et al. (2020)).

Second, our findings indicate that information policies can substantially enhance the performance of centralized college admissions systems. If the primary goal is to support students in their information acquisition process, then the policy interventions discussed in this paper can be implemented at scale.

[38] We believe MINEDUC efficiently addressed these concerns, alleviating a significant part of the implementation issues.

For instance, the personalized website we designed and implemented in 2022 can be easily adapted for use in other countries. In this context, if policymakers prefer not to implement sequential mechanisms like *iterative DA*, they can still introduce light-touch information policies that accomplish similar objectives.

8 Conclusions

In this paper, we present the results of a multi-year collaboration with policymakers in Chile to design and evaluate whether information policies implemented at scale can effectively reduce application mistakes and improve student outcomes. We introduce a new taxonomy that characterizes several types of mistakes depending on information frictions on several margins, including students' biases regarding their admission chances, programs' characteristics, their level of awareness about programs, and their lack of understanding of the assignment mechanism when reporting their preferences. Based on this framework, we designed a series of nationwide surveys to measure the incidence of information frictions and the prevalence and relevance of application mistakes that result in undermatched and unmatched students. Our survey results show that between 5% to 8% of applicants make a payoff-relevant mistake, and we find that these mistakes are primarily caused by biases in their admission probabilities and programs' characteristics. Moreover, we find that students from disadvantaged backgrounds are significantly more likely to make these mistakes.

Based on the surveys' insights, we collaborated with policymakers to design and implement a multi-year outreach policy to reduce information frictions and application mistakes. Using a field experiment where we vary the information provided to students—targeting the main drivers of application mistakes—, we find that showing personalized information about admission probabilities for listed programs and customized messages to guide students depending on their overall admission probability has a causal effect on improving students' outcomes, significantly reducing the risk of being unmatched to the centralized system and the incidence of *over-confidence* mistakes. Moreover, we find that making recommendations at the major level and providing information about programs outside the students' lists can significantly help them improve their assignments, avoiding *under-confidence* mistakes and reducing the fraction of students who are undermatched. Finally, we analyze how pervasive congestion effects are in our setting and find that our intervention policy would be effective if implemented at scale.

Given the positive results of the field experiment, we continued collaborating with MINEDUC to improve the design of our information policy and implement it at scale. By exploiting an encouragement design, we find that showing personalized application advice, information about programs' characteristics, and current cutoff scores for all programs in the centralized system—similar to sequential implementations of the Deferred Acceptance algorithm—has a causal effect on improving students' outcomes, consistent with the field experiment's results. Indeed, by reducing primarily *over-confidence* and *under-confidence* mistakes, we estimate that students who were affected by the policy roughly double the probability of entering the centralized system or improving relative to their initial assignment, which translates to a substantial decrease in unmatched and undermatched students and a significant increase in monetary returns.

Overall, our work demonstrates that information frictions and application mistakes are significant, even in a high-stakes environment like college admissions. However, personalized information policies implemented at scale and sequential mechanisms can effectively alleviate these frictions. By reducing the fraction of students who are unmatched and undermatched, these policies have the potential to decrease inequality in access to higher education. We believe our work provides a model for how researchers and policymakers can collaborate closely to design, test, refine, and implement at scale education policies to improve students' outcomes. As the availability of education data grows, major opportunities exist for policies addressing information frictions to enhance efficiency and equity. We hope our work inspires future efforts in this direction.

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8.1 Appendix to Section 3

8.1.1 Surveys questions

In this subsection we describe the main questions used in the analysis.[39]

- ***Current cutoff:*** *We show you now a list of the programs you applied to, in strict order of preference. For each of them, please tell us which do you think will be the value of the cutoff score for the CURRENT Admission Process and how likely do you think your application score will be above the cutoff score. We remind you that this is only a survey, and it DOES NOT affect in any way your application nor your admission probabilities. What do you think will be the value of the cutoff score for the current Admission Process for each of these programs?*

- ***Admission probability to a program:*** *How likely do you think your application score for the following programs will be above the current admission process's cutoff score?*
  On a scale from 0 to 100, where 0 is "completely sure that your application score WILL NOT be above the cutoff score for this program" and 100 is "completely sure that your application score WILL BE above the cutoff score for this program".

- ***Admission probability:*** *Regardless of the admission track. How likely you think that you will be admitted in some preference of your application?*
  On a scale of 0 to 100, where 0 is "completely sure that you WILL NOT be admitted in any of your preferences" and 100 is "completely sure that WILL BE admitted in one of your preferences".

- ***Knowledge about previous cutoffs:*** *It is referred to a cutoff score as the application score of the last admitted students to a given program. Each student is assigned to the highest reported preference for which her application score is greater than or equal to the cutoff score that realizes in the current Admission Process. Do you know which was the cutoff score for the PREVIOUS YEAR for each of the programs you applied to?*

- ***Knowledge about requirements:*** *Do you know the requirements and vacancies for each program in the following list?*
  - *Restricts preference order?*
  - *Requires Science test?*
  - *Number of vacancies?*
  - *Minimum weighted score?*
  - *Minimum math-verbal average?*
  - *Requires HYCS test?*

- ***Knows someone in the program:*** *Among the programs you applied to, do you know someone close to you who is currently studying there (friends, relatives, etc.)?*

- ***True-top:*** *This question aims to know where you would have applied to in the hypothetical case in which your admission did not depend on your scores. We remind you that this is only a hypothetical question and will not affect your application or admission probabilities. If the Admissions Process did not depend on your PSU scores, nor your NEM or Ranking scores. To which program would you have applied?*

- ***True-bottom:*** *Imagine a HYPOTHETICAL scenario in which you were NOT admitted to any program in your application list. Is there any program in the centralized system that you have NOT included in your application but you would prefer than being unassigned?*

- ***Knowledge about income and employment:*** *Regarding the graduation process of higher education and considering your knowledge about characteristics like the average income of the graduates and employment rates, how informed do you think you are about the following programs?*

[39]The complete set of instruments is available upon request.

- *I don't think I am informed*
- *Slightly informed*
- *Moderately informed*
- *Quite informed*
- *Completely informed*

- ***Average income:*** *The objective of this question is to know about your expectations of FUTURE INCOME in some of the programs that you applied to and in some that you did not. What do you think is the average monthly income of graduates at the fourth year of graduation from the following programs? In a scale of $0 to $3.000.000*

8.1.2 Surveys summary statistics

Table 16: Summary Statistics - 2020

	Survey 2020		Applicants 2020	
	Mean	SD	Mean	SD
Public	0.28	0.45	0.28	0.45
Voucher	0.54	0.50	0.54	0.50
Private	0.17	0.38	0.18	0.38
Income Below Median	0.62	0.48	0.62	0.49
Female	0.61	0.49	0.57	0.49
NEM	611.86	110.43	598.10	106.81
Ranking	639.14	129.83	621.89	125.61
Language	533.15	157.15	511.31	173.12
Math	528.21	161.86	504.68	177.12
Average score	530.32	154.80	507.46	171.08
Observations	38,093		146,438	

Note: Summary statistics for the 2020 survey and administrative data. Survey data considers only students who completed the entire survey. Administrative data considers all students who applied to at least one program in the 2020 Adm. Process.

Table 17: Summary Statistics - 2022

	Survey 2022		Applicants 2022	
	Mean	SD	Mean	SD
Public	0.70	0.46	0.70	0.46
Voucher	0.16	0.36	0.16	0.37
Private	0.08	0.28	0.08	0.27
Income Below Median	0.59	0.49	0.59	0.49
Female	0.64	0.48	0.60	0.49
NEM	656.97	108.89	634.64	107.19
Ranking	684.68	123.39	658.01	121.78
Language	527.08	151.63	499.16	164.93
Math	522.42	154.64	493.79	167.64
Average score	524.52	147.34	496.18	160.90
Observations	11,455		105,928	

Note: Summary statistics for the 2022 survey and administrative data. Survey data considers only students who completed the entire survey. Administrative data considers all students who applied to at least one program in the 2022 Adm. Process.

Table 18: Summary Statistics - 2023

	Baseline Survey 2023		All 2023		Endline Survey 2023		Applicants 2023		Both 2023	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Public	0.64	0.48	0.64	0.48	0.68	0.47	0.69	0.46	0.69	0.46
Voucher	0.18	0.39	0.19	0.39	0.17	0.38	0.17	0.37	0.16	0.36
Private	0.09	0.29	0.10	0.29	0.09	0.28	0.08	0.28	0.08	0.27
Income Below Median	0.67	0.47	0.68	0.47	0.66	0.47	0.66	0.47	0.66	0.47
Female	0.65	0.48	0.55	0.50	0.63	0.48	0.60	0.49	0.66	0.47
NEM	725.76	162.62	687.71	160.94	738.99	152.38	724.16	143.82	751.84	158.88
Ranking	751.99	178.81	709.01	176.62	765.75	168.29	747.07	158.87	781.62	174.96
Language	647.70	154.05	606.09	175.98	667.30	152.01	654.88	149.89	680.99	147.87
Math	565.20	153.71	527.04	168.79	585.31	148.70	567.31	143.11	598.45	151.16
Average score	606.45	137.22	566.56	155.34	626.31	130.78	611.09	127.51	639.72	130.39
Observations	19,032		176,856		6,977		132,893		2,503	

Note: Summary statistics for the 2023 surveys and administrative data. Survey data for the Baseline survey (*Baseline Survey 2023*), Endline survey (*Endline Survey 2023*), and matched observations who answered both surveys and participated in the information policy (*Both 2023*), considers all students who answered at least one question in the survey which is used in the analysis. Administrative data considers all students who participated in the Admission Process 2023 and received a link from MINEDUC to answer the Baseline survey (*All 2023*) and all students who applied and were part of the information policy (*Applicants 2023*).

8.2 Appendix to Section 4

8.2.1 Additional Results

Figure 11: Distributions of the standardized difference between subjective expected cutoffs and *Ratex* and *Adaptive* beliefs

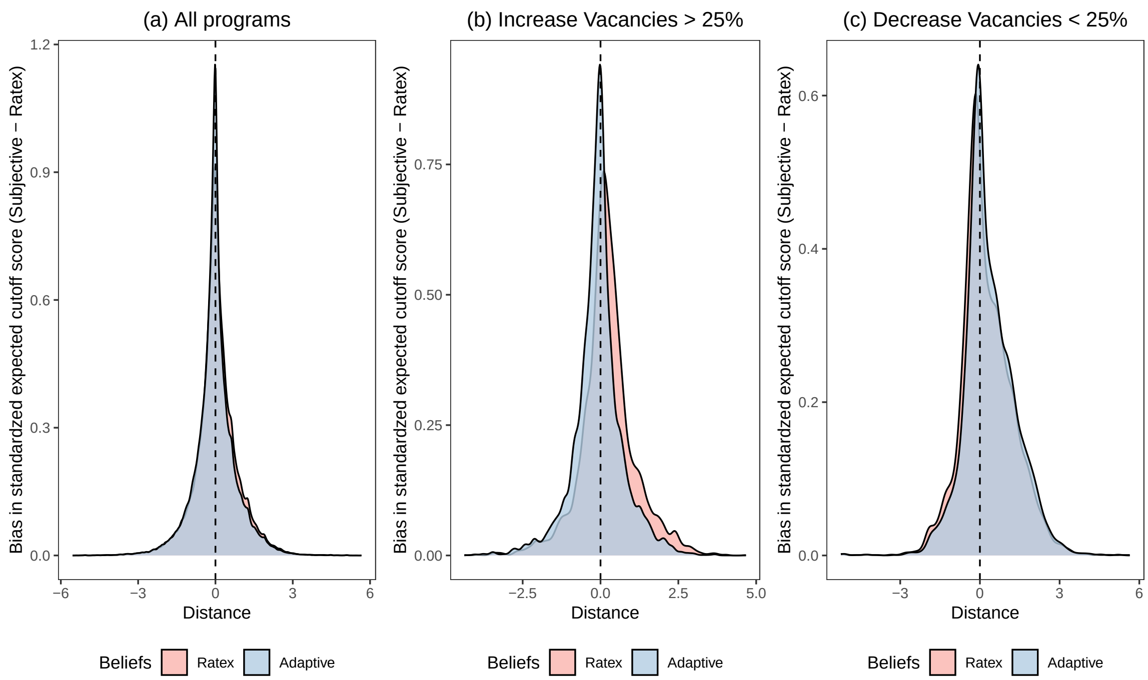


Table 19: OLS regression for the proportional bias on expected cutoff scores

	Estimate	Std. Error	p-value
Distance if positive	0.302	(0.005)	0.000
Distance if negative	-0.336	(0.007)	0.000

Observations: 147218, Adj. R-squared: 0.485

Note: OLS regression for the proportional bias on expected cutoff scores relative to realized cutoff scores in 2020. Each observation is a pair student-program where the student applies and give a response in the 2020 survey. *Distance if positive* (*Distance if negative*) captures the distance between student's application score to the realized 2020 cutoff score. Programs' fixed effects are included. Sample includes all students who are not PACE and completed the survey.

Table 20: Regression Results for Bias on Average Income

	Percentage of Bias in Average Income				
	Top Reported (1)	Top True (2)	Bottom Reported (3)	Bottom True (4)	Random (5)
Application score standardized	-0.102^{***}	-2.459^{***}	-9.675^{***}	-2.650^{***}	-0.040^{***}
	(0.010)	(0.443)	(1.168)	(0.784)	(0.005)
Female	8.944^{***}	4.757^{*}	8.635^{***}	8.257^{*}	8.333^{***}
	(1.989)	(2.783)	(2.181)	(4.366)	(2.859)
Family income below median	−1.357	−3.906	−1.866	−2.077	−2.340
	(1.940)	(2.640)	(2.127)	(4.280)	(2.760)
Public	−2.103	−1.709	−0.196	10.211^{*}	−1.374
	(2.853)	(3.750)	(3.158)	(5.892)	(4.158)
Voucher	−5.303	−3.984	−4.325	6.354	−3.139
	(3.574)	(4.848)	(3.917)	(7.414)	(5.297)
Constant	77.881^{***}	13.214^{***}	22.607^{***}	5.980	29.782^{***}
	(7.042)	(4.238)	(3.588)	(6.399)	(4.921)
Observations	6,854	3,365	6,144	1,606	3,943

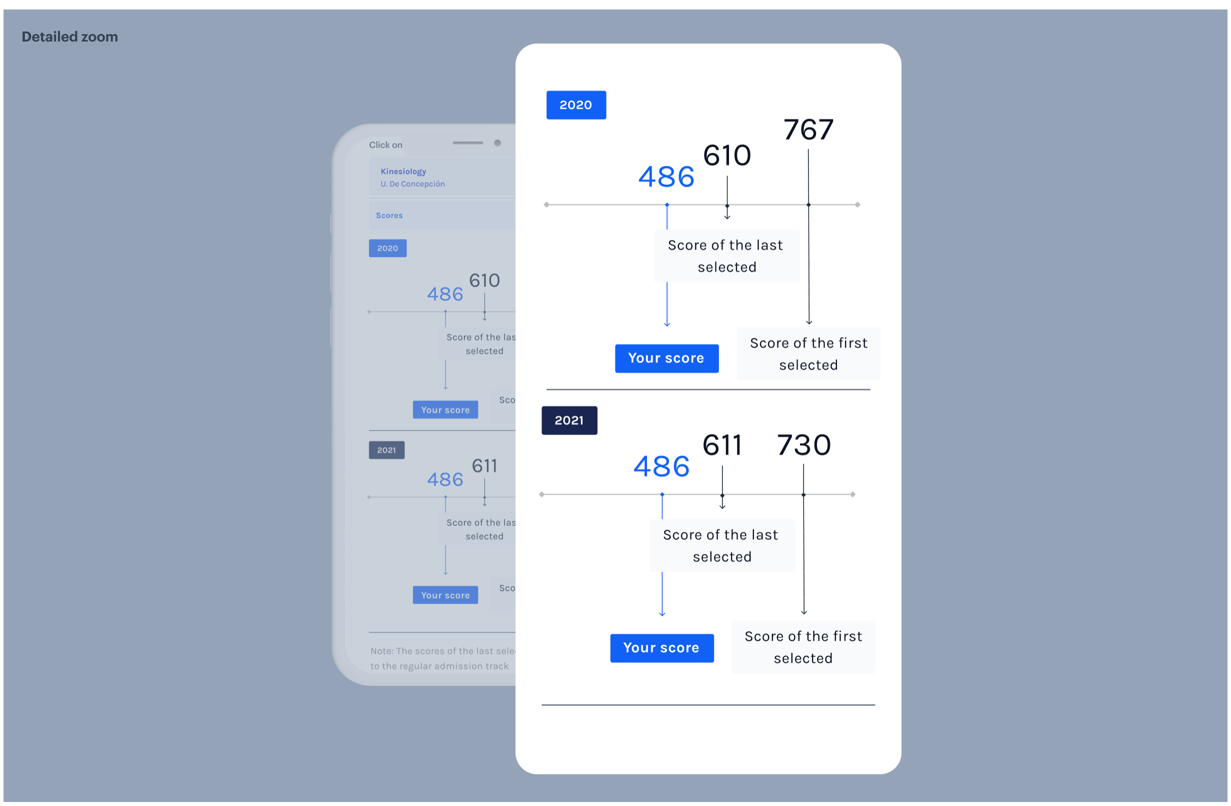
Table 21: Percentage of Absolute Bias on Expected Income After Four Years of Graduation

		Top Reported		Bottom Reported		Top True		Bottom True		Random	
Received	Open	N	Bias [%]	N	Bias [%]	N	Bias [%]	N	Bias [%]	N	Bias [%]
No	No	2543	44.695	2203	47.094	1407	46.041	650	48.544	0	NaN
			(1.193)		(1.252)		(1.548)		(2.448)		(NA)
Yes	No	4654	53.52	4219	56.323	2101	51.994	1023	60.219	4156	57.011
			(0.979)		(1.044)		(1.319)		(2.227)		(1.043)
Yes	Yes	4061	47.447	3679	50.012	1849	49.515	922	51.072	3535	53.282
			(1.003)		(1.047)		(1.462)		(2.364)		(1.141)

Table 22: Percentage of Bias on Expected Income After Four Years of Graduation

		Top Reported		Bottom Reported		Top True		Bottom True		Random	
Received	Open	N	Bias [%]	N	Bias [%]	N	Bias [%]	N	Bias [%]	N	Bias [%]
No	No	2543	13.708	2203	12.3	1407	16.661	650	16.368	2372	11.362
			(1.461)		(1.583)		(1.925)		(3.035)		(1.625)
Yes	No	4654	18.078	4219	17.475	2101	14.947	1023	20.99	4427	13.936
			(1.226)		(1.33)		(1.709)		(2.842)		(1.303)
Yes	Yes	4061	16.715	3679	16.896	1849	18.078	922	18.665	3814	16.258
			(1.221)		(1.304)		(1.813)		(2.836)		(1.369)

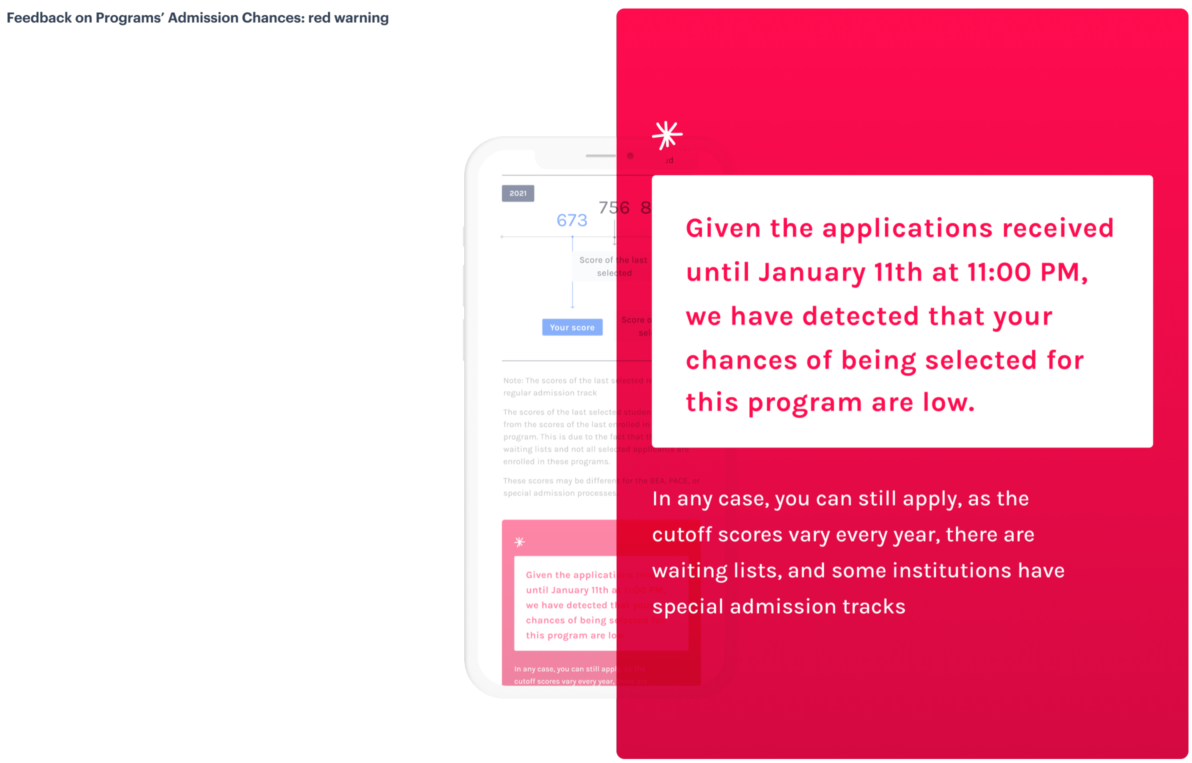
Figure 12: Detailed zoom



8.3 Appendix for Section 5

8.3.1 Intervention Design

Figure 13: Feedback on Programs' Admission Chances: red warning



8.3.2 Treatment Assignment and Stratification

As discussed in Section 5, we assign students to treatments in a stratified way to achieve balance. For the stratification we consider the following observables:

- Female: dummy variable equal to 1 if the student is female, and 0 otherwise.
- Region: categorical variable that takes four 3 levels depending on the region where the student graduated from high-school. Specifically, this variable is equal to 1 for students graduating in the north (regions I, II, III, IV and XVII); 2 for students graduating in the center (regions V, XIII, VI, VII); and 3 for students graduating in the south (regions VIII, IX, X, XI, XII, XIV and XVI).
- Score: categorical variable that takes 4 levels depending on the average score between the PDT tests in Math and Verbal. Specifically, this variable is equal to 1 for students with average score below 450; 2 for students which average score between 450 and 600; and 3 for students with score above 600.
- Overall alert: there are three types of overall alerts: (i) reach, (ii) safety, and (iii) more information. Each student can be assigned to one of these groups, and thus we also use this assignment as part of the stratification.

- Opened scores' intervention: when the scores of the PDT were published, MINEDUC ran an experiment aiming to provide information regarding the relative position of students among their peers (their high-school and their region). Hence, we use a dummy variable equal to 1 if the student received that intervention (and 0 otherwise) as part of our stratification.
- SMS: dummy variable equal to 1 if the student received an SMS encouraging them to open their personalized website, and 0 otherwise.

In Table 23 we report the results of a multinomial regression models that consider the treatment assigned as dependent variable and the aforementioned variables as controls. The first three columns report the results considering all observations, while the last three columns report the resulting excluding misfits. We observe that none of the variables considered is significant, which confirms that our treatment assignment is balanced in terms of these covariates.

Table 23: Treatment Assignment: Balance Checks

	Dependent variable: Treatment					
	All observations			Excluding misfits		
	(1)	(2)	(3)	(4)	(5)	(6)
Region - Center	−0.011	−0.007	−0.003	−0.010	−0.007	−0.003
	(0.026)	(0.026)	(0.026)	(0.027)	(0.027)	(0.027)
Region - South	−0.007	−0.004	−0.002	−0.005	−0.004	−0.002
	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)
Female	−0.002	−0.001	−0.001	−0.002	0.00002	−0.0001
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Score - Medium	−0.008	−0.004	−0.002	−0.006	−0.004	−0.002
	(0.032)	(0.032)	(0.032)	(0.033)	(0.033)	(0.033)
Score - High	−0.003	−0.001	−0.001	−0.001	0.0004	0.0001
	(0.036)	(0.036)	(0.036)	(0.037)	(0.037)	(0.037)
Overall Alert - Safety	−0.014	−0.008	−0.004	−0.012	−0.008	−0.004
	(0.038)	(0.038)	(0.038)	(0.039)	(0.039)	(0.039)
Overall Alert - Information	−0.017	−0.010	−0.005	−0.017	−0.011	−0.007
	(0.031)	(0.031)	(0.031)	(0.032)	(0.032)	(0.032)
Received SMS	0.007	0.005	0.003	0.007	0.005	0.002
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Received Previous Intervention	0.037	0.023	0.011	0.034	0.022	0.010
	(0.036)	(0.036)	(0.037)	(0.038)	(0.038)	(0.039)
Constant	0.031	0.018	0.010	0.029	0.018	0.010
	(0.049)	(0.049)	(0.049)	(0.051)	(0.051)	(0.051)
Observations	107,837	107,837	107,837	106,100	106,100	106,100

8.3.3 Admission Probabilities

To compute the admission probabilities, we use a bootstrap procedure similar to that in Agarwal and Somaini (2018) and Larroucau and Ríos (2018). The main difference is that these approaches use complete information regarding the applications. In our case, we only have the application list of close to 2/3 of the students that ended up applying, so running the bootstrap procedure on this sample would considerably underestimate the cutoffs. For this reason, our first task is to estimate the total number of students that would apply in 2022 based on the applications received so far. To accomplish this, we divide the population into three segments based on their average score between Math and Verbal (the two mandatory exams of the PSU/PDT). Then, using data from 2020 and 2021, we estimate which fraction of all students that take the national exam would apply to at least one program in the centralized system taking the average between these two years. Finally, comparing this number with the actual fraction of students in each score bin that have applied so far, we quantify the number of students that have not applied yet.

Based on the number of applicants missing, we perform 1000 bootstrap simulations, each consisting of the following steps:

1. Sample with replacement the number of students missing in each bin score, and incorporate the sampled students to the pool of applications received so far.
2. Run the assignment mechanism used in the Chilean system. See Rios et al. (2021) for a detailed description of the mechanism used in Chile to solve the college admissions problem.
3. Compute the cutoff of each program for both the regular and BEA admission processes.

As a result of this procedure, we obtain two matrices (for the regular and BEA processes) with 1000 cutoffs for each program. Hence, the next step is to estimate the distribution of the cutoff of each program in each admission track. To accomplish this, we estimate the parameters of a truncated normal distribution for each program and admission track via maximum likelihood. Then, using the estimated distributions, we evaluate the CDF on the application score of the student to obtain an estimate of the admission probability, taking into account whether the student participates only in the regular process or also in the BEA track.

8.3.4 Recommendations

The recommendation algorithms works as follows.

1. Find the most and the second most popular majors based on the preferences included in the student's ROL.
2. For each pair of majors, and considering the most and the second most preferred major of each student, compute a transition matrix that returns the probability that a given major is followed by another major as the most preferred ones.
3. For each student, compute the set of feasible majors considering the student's scores and her admission probabilities (obtained as described in the previous section).
4. For students with high scores (i.e., average between Math and Verbal above 600), choose four majors according to the following rule:
   (a) Choose most preferred major according to the student's list of preferences,
   (b) Choose the second most preferred major according to the student's list of preferences,
   (c) Choose the major with the highest average wage[40] among all majors considering the transition matrix previously computed,
   (d) Choose the major with the highest average wage among all feasible majors (i.e., majors for which the student has a positive probability of assignment) considering the transition matrix previously computed.
5. For students with low scores (i.e., average between Math and Verbal below 600), choose four majors according to the following rule:
   (a) Choose the most preferred major according to the student's list of preferences,

[40] Average wages are measured at the fourth year after graduation. This statistic is computed by SIES and provided to us by MINEDUC.

(b) Choose the second most preferred major according to the student's list of preferences,

(c) Choose the major with the highest expected wage among all majors belonging to IPs or CFTs,

(d) Choose the major with the highest expected wage among all feasible majors (i.e., majors for which the student has a positive probability of assignment) considering the transition matrix previously computed.

8.3.5 Additional Results

Table 24: Summary Statistics by Group and Reception

			Application			Assignment					
Treatment	Opened	N	Modified [%]	Increased [%]	Decreased [%]	Assigned [%]	Inc. Prob. [%]	Entered [%]	Left [%]	Improved [%]	Persisted [%]
T1	No	18960	10.211	8.966	1.429	77.727	1.334	2.824	0.171	3.108	1.319
			(0.22)	(0.207)	(0.086)	(0.302)	(0.083)	(0.252)	(0.034)	(0.143)	(0.174)
T1	Yes	7443	13.167	11.34	1.975	79.833	1.572	3.677	0.136	3.496	1.419
			(0.392)	(0.368)	(0.161)	(0.465)	(0.144)	(0.478)	(0.048)	(0.239)	(0.301)
T2	No	19060	10.414	9.307	1.522	77.665	1.312	3.116	0.197	2.831	1.398
			(0.221)	(0.21)	(0.089)	(0.302)	(0.082)	(0.263)	(0.037)	(0.137)	(0.178)
T2	Yes	7486	14.748	12.036	2.137	79.361	1.643	4.08	0.288	3.631	1.82
			(0.41)	(0.376)	(0.167)	(0.468)	(0.147)	(0.496)	(0.07)	(0.244)	(0.335)
T3	No	19053	10.654	9.432	1.69	77.872	1.37	3.255	0.17	3.091	1.385
			(0.224)	(0.212)	(0.093)	(0.301)	(0.084)	(0.27)	(0.034)	(0.143)	(0.178)
T3	Yes	7459	14.922	12.24	1.998	79.421	2.051	5.053	0.222	4.201	2.807
			(0.413)	(0.38)	(0.162)	(0.468)	(0.164)	(0.547)	(0.062)	(0.262)	(0.413)
T4	No	18951	10.517	9.266	1.704	77.627	1.467	3.238	0.178	2.734	1.493
			(0.223)	(0.211)	(0.094)	(0.303)	(0.087)	(0.268)	(0.035)	(0.135)	(0.184)
T4	Yes	7516	13.797	11.15	1.996	79.417	1.45	3.287	0.286	3.556	1.454
			(0.398)	(0.363)	(0.161)	(0.466)	(0.138)	(0.448)	(0.069)	(0.24)	(0.301)

Note: Opened is a binary variable equal to 1 if the student opened the personalized website, 0 otherwise. Modified is a binary variable equal to 1 if the student modified its application after the personalized websites were sent, 0 otherwise. Increased (decreased) is a binary variable equal to 1 if the student increased (decreased) the number of valid applications in their list, 0 otherwise. Assigned is a binary variable equal to 1 if the student resulted assigned at the end of the process, 0 otherwise. Inc. Prob. is a binary variable equal to 1 if the student increased her admission probability at the end of the process, 0 otherwise. Entered (left) is a binary variable equal to 1 if the student resulted unassigned (assigned) given their list of preferences before the intervention and (un)assigned given their preferences after the intervention, 0 otherwise. Improved is a binary variable equal to 1 if the student improved on their preference of assignment, 0 otherwise. Persisted is a binary variable equal to 1 if the student entered the assignment and persisted for at least two years in that program, 0 otherwise. Standard errors reported in parentheses.

Table 25: Regression Results among Openers in Safety Group OLS

	Applications			Assignment				
	Modified (1)	Increased (2)	Decreased (3)	Inc. Prob. (4)	Entered (5)	Left (6)	Improved (7)	Persisted (8)
Treatment 2	0.029**	0.0004	0.003	0.006	0.011*	0.010	−0.011	0.009*
	(0.012)	(0.009)	(0.003)	(0.007)	(0.007)	(0.013)	(0.016)	(0.005)
Treatment 3	0.012	0.005	0.002	0.004	0.016**	0.010	−0.001	0.014***
	(0.012)	(0.009)	(0.003)	(0.007)	(0.007)	(0.013)	(0.017)	(0.005)
Treatment 4	−0.007	−0.016*	0.003	−0.013*	−0.001	0.010	0.019	0.001
	(0.012)	(0.009)	(0.003)	(0.007)	(0.007)	(0.013)	(0.017)	(0.005)
Constant	0.115***	0.068***	0.003*	0.044***	0.029***	−0.000	0.011	0.010***
	(0.009)	(0.006)	(0.002)	(0.005)	(0.005)	(0.009)	(0.012)	(0.003)
Observations	6,062	6,062	6,062	6,062	5,677	385	385	5,677

Note: Modified is a binary variable equal to 1 if the student modified its application after the personalized websites were sent, 0 otherwise. Increased (decreased) is a binary variable equal to 1 if the student increased (decreased) the number of valid applications in their list, 0 otherwise. Inc. Prob. is a binary variable equal to 1 if the student increased her admission probability at the end of the process, 0 otherwise. Entered (left) is a binary variable equal to 1 if the student resulted unassigned (assigned) given their list of preferences before the intervention and (un)assigned given their preferences after the intervention, 0 otherwise. Improved is a binary variable equal to 1 if the student improved on their preference of assignment, 0 otherwise. Persisted is a binary variable equal to 1 if the student entered the assignment and persisted for at least two years in that program, 0 otherwise. Standard errors reported in parentheses. Significance reported: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 26: Regression Results among Openers in Reach Group OLS

	Applications			Assignment		
	Modified (1)	Increased (2)	Decreased (3)	Inc. Prob. (4)	Left (5)	Improved (6)
Treatment 2	0.003	−0.016	0.003	−0.002	0.000	−0.005
	(0.018)	(0.018)	(0.007)	(0.002)	(0.001)	(0.009)
Treatment 3	0.022	−0.012	0.004	−0.002	0.002	0.002
	(0.018)	(0.018)	(0.007)	(0.002)	(0.001)	(0.009)
Treatment 4	0.004	−0.022	−0.003	0.001	−0.000	−0.005
	(0.018)	(0.018)	(0.007)	(0.002)	(0.001)	(0.009)
Constant	0.103***	0.130***	0.015***	0.002	0.000	0.028***
	(0.013)	(0.013)	(0.005)	(0.001)	(0.001)	(0.006)
Observations	2,563	2,563	2,563	2,563	2,563	2,563

Note: Modified is a binary variable equal to 1 if the student modified its application after the personalized websites were sent, 0 otherwise. Increased (decreased) is a binary variable equal to 1 if the student increased (decreased) the number of valid applications in their list, 0 otherwise. Inc. Prob. is a binary variable equal to 1 if the student increased her admission probability at the end of the process, 0 otherwise. Entered (left) is a binary variable equal to 1 if the student resulted unassigned (assigned) given their list of preferences before the intervention and (un)assigned given their preferences after the intervention, 0 otherwise. Improved is a binary variable equal to 1 if the student improved on their preference of assignment, 0 otherwise. Standard errors reported in parentheses. Significance reported: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 27: Regression Results among Openers in Explore Group OLS

	Applications			Assignment			
	Modified (1)	Increased (2)	Decreased (3)	Inc. Prob. (4)	Entered (5)	Left (6)	Improved (7)
Treatment 2	0.014**	0.012*	0.001	−0.0005	−0.067**	0.002	0.003
	(0.007)	(0.007)	(0.003)	(0.002)	(0.031)	(0.001)	(0.004)
Treatment 3	0.019***	0.013**	−0.001	0.006***	−0.016	0.001	0.008**
	(0.007)	(0.007)	(0.003)	(0.002)	(0.031)	(0.001)	(0.004)
Treatment 4	0.011	0.005	0.00002	0.002	−0.035	0.002	0.001
	(0.007)	(0.007)	(0.003)	(0.002)	(0.032)	(0.001)	(0.004)
Constant	0.140***	0.124***	0.025***	0.009***	0.115***	0.002**	0.036***
	(0.005)	(0.005)	(0.002)	(0.001)	(0.023)	(0.001)	(0.003)
Observations	21,279	21,279	21,279	21,279	651	20,628	20,628

Note: Modified is a binary variable equal to 1 if the student modified its application after the personalized websites were sent, 0 otherwise. Increased (decreased) is a binary variable equal to 1 if the student increased (decreased) the number of valid applications in their list, 0 otherwise. Inc. Prob. is a binary variable equal to 1 if the student increased her admission probability at the end of the process, 0 otherwise. Entered (left) is a binary variable equal to 1 if the student resulted unassigned (assigned) given their list of preferences before the intervention and (un)assigned given their preferences after the intervention, 0 otherwise. Improved is a binary variable equal to 1 if the student improved on their preference of assignment, 0 otherwise. Standard errors reported in parentheses. Significance reported: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 28: Regression Results for Bias on Average Income

	Top-Reported (1)	Bottom-Reported (2)	Top-True (3)	Bottom-True (4)	Random Program (5)
Treatment 2	5.192	0.576	−7.842	8.849	−1.093
	(3.518)	(3.780)	(5.204)	(8.161)	(3.969)
Treatment 3	1.064	0.969	−7.759	10.931	−1.620
	(3.532)	(3.756)	(5.278)	(7.996)	(3.938)
Treatment 4	1.973	−3.156	−8.407	−9.143	−6.546*
	(3.526)	(3.745)	(5.295)	(8.161)	(3.920)
Constant	14.481***	17.103***	24.228***	15.486***	18.464***
	(2.519)	(2.669)	(3.806)	(5.777)	(2.795)
Observations	3,940	3,567	1,799	890	3,699

Note: The analysis employs OLS regression models to examine the proportional change in each student's subjective bias towards the average earnings of graduates of a given program by the fourth year of graduation. The sample is limited to students who responded to the survey and opened the intervention, with exclusions for misfits. Programs with well-defined cutoff scores are included in the sample. Standard errors are reported in parentheses. Significance reported: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

8.4 Appendix to Section 6

8.4.1 Background

As in the previous year, students participated in a national exam that provided them with test scores that the system uses to compute their application scores in each program they listed in their preference list. However, MINEDUC introduced a series of changes to the admission process. First, they completely redesigned the admission exam by changing its focus (moving from knowledge-based to attitude-based) and adding a math-specific exam. In addition, MINEDUC changed the normalization rules and, more importantly, the range of possible scores, moving from a $[210, 850]$ to a $[100, 1000]$ scale.

Second, MINEDUC introduced the option to take the national exam twice per year and changed the rules on how to compute application scores for students that took the exam several times and thus have multiple pools of scores.[41] Specifically, they moved from a pool-based approach, in which the application score is computed considering the best pool among all the ones available, to a test-specific approach, in which the application score is computed considering the best score for each specific exam, potentially combining different pools of scores.

Finally, given all the changes mentioned above and the advice from the research team, MINEDUC decided to increase the constraint on the length of preference lists from ten to twenty programs.

A critical consequence of all these changes is that the previous year's cutoffs were not as informative as in previous years. Indeed, many students had no idea how to assess their chances of admission, as they had no reference point, and the uncertainty was considerably higher. As a result, MINEDUC decided it was crucial to provide students with as much guidance as possible, and thus decided to implement our information policy for all students nationwide. Hence, students who opened their personalized websites received the same information fields, so we do not have proper treatment and control groups as described in Section 5. Nevertheless, as we later discuss, we can still estimate the effect of the intervention using an encouragement design.

8.4.2 Instrument Validity

For an instrument to be valid, we need to satisfy two conditions: (i) relevance, and (ii) exclusion. The former states that the instrument is correlated with the endogeneous variable of interest. In our case, we need to confirm that receiving a whatsapp is correlated with opening the intervention. To check these, there are two possible approaches: (i) check the F-statistic of the first stage regression

$$O_i \sim W_i + X_i + \epsilon_i$$

where $O_i = 1$ if the student opens the intervention and zero otherwise; $W_i = 1$ if student i receives a Whatsapp encouragement message and zero otherwsie; X_i is a vector of control variables (in this case, the risk level group); and ϵ_i is an error term. The results of this first-stage regression are reported in the next table:

Table 29: Regression results: First Stage

	Dependent variable:
	Open
Receive Whatsapp	0.175***
	(0.003)
Constant	0.485***
	(0.003)
Risk group	Yes
Observations	132,893
R^2	0.028
F Statistic	1,282.581*** (df = 3; 132889)

Note: Significance reported: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

As we observe from this table, the variable W_i is positive and significant, and the F-statistic is well above 10, so this provides evidence that our first-stage is significant and that the instrument is relevant. To get further evidence, we cna perform a Weak instruments' test, which results in a p-value < $1e^{-6}$, rejecting

[41] Moreover, MINEDUC had to introduce conversion tables to transform scores from the previous scale to the new one.

the null-hypothesis that the instrument is weak. Hence, we conclude that the instrument considered is relevant.

To assess whether the instrument satisfies the exclusion condition, we must ensure that the variable W_i is exogenous. This condition holds by design since we randomized who receives the encouragement message. In Table 30, we report regression results that consider whether the student received the encouragement as the dependent variable, and we control for the risk group, score variables (different categories of average between Verbal and Math), demographics (including gender, region of residence, whether they have NEM score) and whether the student participated in the BEA/PACE processes. We observe that none of the controls significantly affected all cases, confirming that the encouragement messages were properly randomized.

Table 30: Randomization of Encouragement

	Dependent variable: Received Whatsapp		
	(1)	(2)	(3)
Risk - Medium	-0.001	-0.001	-0.001
	(0.005)	(0.005)	(0.005)
Risk - High	0.001	0.001	0.001
	(0.003)	(0.003)	(0.003)
$LM \in (545, 574]$	-	0.000	0.000
	-	(0.004)	(0.004)
$LM \in (574, 604]$	-	0.000	0.000
	-	(0.004)	(0.004)
$LM \in (604, 640]$	-	-0.000	-0.000
	-	(0.004)	(0.004)
$LM \in (640, 685]$	-	-0.000	-0.000
	-	(0.004)	(0.004)
$LM \in (685, 758]$	-	-0.001	-0.001
	-	(0.004)	(0.004)
$LM \in (758, 1000]$	-	-0.001	-0.001
	-	(0.005)	(0.005)
No NEM	-	-0.000	-0.000
	-	(0.003)	(0.003)
Female	-	-0.000	-0.000
	-	(0.003)	(0.003)
Region - Center	-	-0.000	-0.000
	-	(0.004)	(0.004)
Region - South	-	-0.000	-0.000
	-	(0.004)	(0.004)
BEA	-	-	0.001
	-	-	(0.005)
PACE	-	-	-0.001
	-	-	(0.004)
Constant	0.270^{***}	0.271^{***}	0.271^{***}
	(0.003)	(0.005)	(0.005)
Risk group	Yes	Yes	Yes
Demographics	No	Yes	Yes
Bea/PACE	No	No	Yes
Observations	132,893	132,893	132,893

Note: LM represents the average between the highest Verbal and Math scores obtained by the student. Significance reported: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

8.4.3 Additional results

Table 31: Summary Statistics across Groups

			Applications			Assignment					
Open	Risk level	N	Modified[%]	Increased[%]	Decreased[%]	Assigned[%]	Inc. Prob [%]	Entered [%]	Left [%]	Improved [%]	Enrolled [%]
No	High	14178	14.734	8.570	1.185	4.606	4.542	3.498	0.014	0.092	2.102
	Medium	6310	16.450	9.271	1.712	73.772	5.674	1.632	0.745	1.743	38.605
	Low	35831	19.341	8.733	2.975	99.587	0.234	0.006	0.274	5.841	73.649
Yes	High	16150	28.830	19.108	1.802	9.969	11.028	8.545	0.000	0.074	5.065
	Medium	7540	27.334	15.491	3.263	76.048	10.623	2.653	0.995	3.276	41.724
	Low	52884	28.048	11.240	5.147	99.399	0.291	0.019	0.414	8.226	76.658

Note: Includes all students eligible to receive the intervention, i.e., who applied during the first half of the application time window.

Table 32: Regression results: Instrumental Variables

	Assignment		
	Decr. Prob.	Leave	Worsen
	(1)	(2)	(3)
Open	0.001	-0.002	0.001
	(0.003)	(0.003)	(0.004)
Constant	0.001	0.006^{***}	0.012^{**}
	(0.002)	(0.002)	(0.005)
Risk group	Yes	Yes	Yes
Observations	132,893	99,151	99,151

Note: Significance reported: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

Table 33: Regression results: Instrumental Variables on Enrollment

	Enroll		
	High	Medium	Low
	(1)	(2)	(3)
Open	0.041^{***}	0.034	0.287
	(0.012)	(0.043)	(0.308)
Constant	0.013^{**}	0.028	-0.151
	(0.006)	(0.023)	(0.209)
Observations	29,939	3,642	161

Note: Significance reported: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

8.4.4 Search tools

In this section, we analyze the effect of the search engine embedded in the personalized websites. To make a fair comparison, we focus on students who opened the information policy, and evaluate the effect of the variable *Search*, which is equal to 1 if the student used the search engine (i.e., did a search) and zero otherwise.

In Table 34 we report summary statistics for the same outcomes of interest discussed in Section 6, separating by risk level and by whether the student did any search. We observe that using the search engine is correlated with increasing the number of applications, valid applications, and also is positively correlated with increasing the overall probability of admission and entering the assignment. These results suggest that using the search is correlated with improving application and admission outcomes.

Table 34: Summary Statistics across Groups

			Applications		Valid Applications		Overall probability			Assignment	
Search	Risk level	N	Inc.	Dec.	Inc.	Dec.	Inc.	Dec.	Change	Enter	Leave
No	High	13074	0.167	0.018	0.151	0.016	0.084	0.002	0.066	0.066	0.000
	Medium	6254	0.126	0.030	0.124	0.029	0.082	0.018	0.018	0.021	0.008
	Low	44349	0.091	0.048	0.092	0.047	0.002	0.007	-0.004	0.000	0.004
Yes	High	3076	0.393	0.033	0.362	0.029	0.223	0.004	0.170	0.168	0.000
	Medium	1286	0.302	0.051	0.303	0.048	0.222	0.035	0.052	0.052	0.020
	Low	8535	0.220	0.075	0.219	0.074	0.006	0.011	-0.007	0.000	0.007

Note: Each unit of observation is a subject.

One possible explanation for the aforementioned effect is that student who use the search engine may be more likely to add new programs to their application, increasing its length and their chances of admission. To rule out this effect, in Table 35 we analyze the results on admission outcomes considering only students who opened the intervention and added a program, and we analyze the outcomes of interest separating by whether the student did any search and also by whether the student added at least one program that resulted from their search.

Table 35: Summary Statistics across Groups (among students who add programs)

				Overall probability			Assignment	
Risk	Search	Add from search	N	Inc.	Dec.	Change	Enter	Leave
High	No	No	2499	0.391	0.007	0.315	0.315	0.000
High	Yes	No	868	0.371	0.007	0.273	0.268	0.000
High	Yes	Yes	970	0.494	0.006	0.374	0.369	0.000
Medium	No	No	1030	0.470	0.051	0.126	0.123	0.020
Medium	Yes	No	282	0.496	0.057	0.134	0.113	0.018
Medium	Yes	Yes	321	0.539	0.065	0.130	0.128	0.047
Low	No	No	6593	0.015	0.027	-0.014	0.001	0.014
Low	Yes	No	1510	0.016	0.021	-0.011	0.001	0.010
Low	Yes	Yes	1718	0.017	0.024	-0.014	0.002	0.015

Note: Each unit of observation is a subject.

We observe that adding a program that resulted from the search is positively correlated with increasing the overall chances of admission and entering the assignment.

8.5 Drivers

To examine whether the policy influences behavior through changes in beliefs on admission probabilities, we use the panel of respondents from the baseline and endline surveys conducted in 2023. For each student in the panel, we calculate a measure of bias in expected cutoffs and a measure of bias in admission probabilities by taking the difference between students' subjective beliefs (elicited in the baseline and endline surveys) and the rational expectations of expected cutoffs and admission probabilities. We then compute the difference between the absolute value of the bias in beliefs for baseline and end-line measures across the top-reported, bottom-reported, and true top programs declared in the baseline survey, and over their overall admission probability.

Table 36 presents the results of OLS regressions for our measure of reduction in absolute bias. We include students' risk group, their baseline beliefs about their expected PAES scores, and their realized PAES scores as controls. Our identification assumption posits that after controlling for individual risk levels, baseline beliefs, and baseline biases, the policy's effect on students' beliefs is uncorrelated with their decisions to access the personalized website. We find that the policy has a positive effect on reducing bias in expected cutoff scores across all three programs. Notably, the effects appear to be larger—relative to the baseline reduction in absolute bias—for students' top-reported and true top programs. However, when examining the reduction in absolute bias concerning admission probabilities, results are only significant for the bottom-reported program. These findings are consistent with the outcomes of the 2022 intervention, indicating that providing personalized warnings and real-time information about admission probabilities effectively reduces biases in beliefs at the bottom of students' preferences.

Table 36: Regression results: OLS Before-After (Biased beliefs)

	Cutoffs			Adm. Probs.		
	Top-true (1)	Top-reported (2)	Bottom-reported (3)	Top-true (4)	Top-reported (5)	Bottom-reported (6)
Open	15.036***	13.310***	9.923*	1.346	1.828	3.480**
	(4.986)	(4.948)	(5.263)	(1.433)	(1.455)	(1.482)
Constant	27.079**	31.282**	40.186***	-7.850^{**}	-5.691	-3.288
	(12.757)	(12.673)	(13.442)	(3.658)	(3.721)	(3.783)
Observations	2,699	2,670	2,613	2,384	2,356	2,309
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: The sample considers all students who answered the questions of cutoffs or admission probabilities in the 2023 baseline and endline surveys. Significance reported: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.