Confidence and Information in Strategy-Proof School Choice*

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Abstract

Contrary to classical theory, we provide experimental evidence that preference reports in a strategy-proof school-choice mechanism systematically depend on beliefs. We employ a "hard-easy gap" to exogenously vary students' beliefs about their priority rank. As predicted, underconfidence induces more manipulation and thus more justified envy than overconfidence. The effect of priority information on justified envy crucially depends on the initial beliefs and the real priority ranks: while top students always gain, non-top students lose from this information. In total, correcting overconfidence/underconfidence increases/decreases justified envy. Finally, we confirm that additionally providing information on school availability through a dynamic implementation of the mechanism reduces justified envy compared to priority information alone.

JEL-Classification: C92, D47.
Keywords: Market design, school choice, overconfidence, strategy-proofness, information.

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1 Introduction

In school choice, market designers typically advocate for centralized assignment mechanisms, in which students (or parents) submit their privately known preferences over schools and schools rank students according to priorities. While it is natural to wonder whether students should learn their priority rank and to what degree it matters if their beliefs about priorities are distorted, strategy-proof mechanisms seem to circumvent these questions: since the straightforward strategy to rank schools according to match values is dominant, it theoretically seems irrelevant what students know or believe about priorities. However, non-straightforward play is prevalent in the field and in the lab (Rees-Jones and Shorrer, 2023). In this light, we revisit seemingly naive questions about strategy-proof mechanisms through the lens of modern behavioral theory, and we experimentally test its implications. Our research questions are: Does over- or underconfidence systematically impact students' actions? Does the arising non-straightforward play impact the stability of assignments? Does it help students if the provided information corrects this misconfidence? Does additional information on remaining capacity help? In short, our answers are: Yes, yes, it depends, and partly yes.

Our study consists of a 2×3 between-subject design laboratory school-choice experiment where test scores determine a priority ranking shared by all schools. Each subject took the role of a different student in a market consisting of eight students and four schools with two seats each. We induced private and heterogeneous preferences over schools, and students submitted a rank-ordered list (ROL) to a centralized mechanism. We described a deferred-acceptance (DA) mechanism, but due to the priority structure, this mechanism collapses to a serial dictatorship. To exogenously induce over- and underconfidence, we employed a "hard-easy gap" (Lichtenstein et al., 1982; Dargnies et al., 2019): students in the Hard condition faced a more difficult priority test (more complicated Raven matrices) than those in the Easy condition. Next, subjects played five rounds of the school-choice game, where a new vector of match values was drawn for each round. In one-third of the sessions, participants did not learn their priority rank (Info-0); in another, they were informed about it (priority information, Info-1); and in the remaining third of sessions, the mechanism was executed sequentially so that participants additionally learned the remaining capacities of schools (availability information, Info-2).

Our hypotheses are founded in the behavioral matching literature that has emerged recently to explain non-straightforward (NSF) behavior, i.e., not ranking schools according to the induced valuations. First, we verify the fundamental assumption of our setup, Hypothesis 0. That is, we find that subjects in Easy have more optimistic beliefs about their relative performance compared to subjects in Hard. Within the taxonomy of overconfidence according to Moore and Healy (2008), we study overplacement, i.e., participants overestimate their position relative to others, and we find this phenomenon to be more prevalent in Easy, whereas subjects in Hard tend to underplace themselves. Behavioral theory predicts that NSF play is more prevalent among students who believe to have low priority, but such manipulations are not necessarily consequential in the sense that they lead to a worse assignment than the straightforward ROL (holding the ROLs of other players constant).

In our setting, any consequential manipulation of a student i leads to justified envy, i.e., another student j with lower priority is assigned to a school that i prefers over the school she is assigned to. Rather than simply studying whether students play the straightforward strategy, we focus on whether the unique stable and efficient assignment is implemented for several reasons. First, as we discuss below, NSF play is already well-documented, but this phenomenon is only economically relevant if it is consequential, which is still debated (see, e.g., Artemov et al., 2020). Second, as a defining feature of stability, the absence of justified envy is not only a property of theoretical interest, but it is also a key desideratum in many applications because it reflects fairness. Third, it is a crucial ingredient of state-of-the-art empirical strategies to estimate preferences so that its interaction with information provision is important to understand.

Guided by the theory, our Hypothesis 1 is that more justified envy occurs in treatment Hard×Info-0 than in Easy×Info-0, while the difficulty condition should not have an effect when priority information is provided. In Hypothesis 2, we predict that providing priority information alone, Info-1, in treatment condition Easy increases justified envy, while it leads to less justified envy in treatment condition Hard. Specifically, the argument here is that beliefs in Easy are pooled at the top so that a correction triggers more NSF play among non-top students (ranked third or worse); in contrast, beliefs in Hard are pooled at the bottom so that a correction triggers more straightforward play among top students (ranked first or second). Finally, Hypothesis 3 predicts that the lowest rates of justified envy are obtained in information condition Info-2, where both priority and availability information are provided, because all students essentially select their assignment under certainty.

In line with Hypothesis 1, we observe significantly more consequential manipulations when uninformed students hold more pessimistic beliefs about their priority. In contrast, justified envy does not vary significantly with the difficulty in information conditions Info-1 and Info-2. Moreover, we document that informing top students about their true priority rank significantly reduced their justified envy in Hard, but not in Easy. As predicted, priority information significantly increased justified envy among non-top students in Easy. Seemingly against our prediction, this effect is, to a lesser degree, also present in Hard, where it dominates the effect on the top students. Nevertheless, when we differentiate students by priority ranks, we find support for our theoretical argument. Perhaps surprisingly, the dynamic implementation of Info-2 did not push justified envy to zero.

Evidence from the field and the lab consistently shows NSF play, see Hakimov and Kübler (2021) and Rees-Jones and Shorrer (2023) for excellent reviews of this literature. The literature on NSF play can be grouped into preference-based and complexity-based explanations. For instance, the former branch suggests disappointment aversion (Dreyfuss et al., 2022; Meisner and von Wangenheim, 2023; Dreyfuss et al., 2025; Chen et al., 2024), ranking-dependent preferences (Meisner, 2023; Kloosterman and Troyan, 2022), reciprocal preferences (Opitz and Schwaiger, 2023), ego utility (Moscariello, 2024), or preference discovery (Grenet et al., 2022) as possible drivers. The latter branch aims to make the strategy-proofness more apparent, e.g., by making the mechanism obviously strategy-proof (Li, 2017) or by employing a different mechanism description (Katuščák and Kittsteiner, 2024; Gonczarowski et al., 2023) because participants may falsely perceive a strategic trade-off as in an immediate-acceptance (Boston) mechanism. Identifying the source of NSF play is *not* the goal of this paper. Rather, we exploit that the systematic belief-based behavior we hypothesize is consistent with most of the theories above.

In contrast to, for example, Rees-Jones and Skowronek (2018) who document a correlation between overconfidence and straightforward play, we can also make causal claims because we randomly assign subjects to treatment conditions that exogenously vary confidence. Pan (2019) studies overconfidence and priority information in centralized assignment mechanisms. However, her focus lies on "aptitude-stability," and she is mainly concerned with the impact of overconfidence on the immediate-acceptance mechanism. In contrast to us, she finds little variance in NSF play and stability in serial dictatorship. We believe her design with common values is conducive to this observation. In the context of expectation-based loss aversion, (Dreyfuss et al., 2025) find belief-dependent play in DA. Laboratory experiments with settings similar to our information condition Info-2 typically study them as a different dynamic mechanism rather than in the context of availability information. In contrast to, e.g., pick-an-object mechanisms (Bó and Hakimov, 2023), we study a dynamic implementation in which participants must still submit complete ROLs.

Confidence has also been studied in the field. Hakimov et al. (2022) combine survey and administrative data to estimate the impact of informing college applicants in France[1] of their real rank in the grade distribution on their college application behavior. Their intervention voids the impact of confidence on applications, making grades the dominant factor. Similarly, Bobba and Frisancho (2022) find that correcting miscalibrated priors of Mexican students influences application behavior. We further discuss the empirical literature on the interplay between overconfidence, availability information, and non-placement in Section 6.

We are not the first to study priority information in matching mechanisms in the lab. To the best of our knowledge, Pais and Pintér (2008) are the first to record that rates of NSF play, efficiency and justified envy vary in different informational settings, but it is not clear how their specific school-choice setting generalizes. Most importantly, their research is rather exploratory than founded in theory because the theory described in the next paragraph did not exist at the time. Since we elicit participants' priority beliefs and differentiate between priority ranks, we are able to provide more nuanced hypotheses. Specifically, follow-up work (Pais et al., 2011; Hu and Yao, 2024) confirms their finding that the amount of information has a negative effect on straightforward play, but we identify that the opposite is true for high-priority students, particularly if they underplaced themselves. In Section 6, we continue to discuss this seminal paper.

Our paper makes four main contributions. First, we establish a causal link between confidence and consequential manipulations in a strategy-proof school choice mechanism by exogenously varying participants' beliefs about their priority rank. Second, we introduce a structured variation in information provision, distinguishing between no information, only priority information, and combined priority and availability information. Third, we document heterogeneous effects of information

[1]French students are allocated via a dynamic implementation of college-proposing DA with constrained lists, where colleges are free to determine their own priority ranking as long as grades are a dominant factor. This mechanism is not strategy-proof.

across participants with different actual priority ranks. Finally, we identify limits to the dynamic implementation of the mechanism, showing that justified envy still exists.

The rest of the paper is structured as follows. Section 2 describes the experimental design. Section 3 explains the theory guiding our hypotheses, which are laid down in the following Section 4. Section 5 introduces the results, which we discuss in more detail in Section 6. We conclude with Section 7.

2 Experimental design

In our experiment, we employ a 2×3 between-subject design, where we have two conditions for the exogenous confidence shock, {Easy, Hard}, and three conditions for the information variations in the assignment mechanism, {Info-0, Info-1, Info-2}. Participants are assigned to one condition in each of these dimensions, creating six unique treatment combinations, Easy×Info-0, Easy×Info-1, Easy×Info-2, Hard×Info-0, Hard×Info-1, and Hard×Info-2. In this section, we first explain the general structure of the experimental design, then the hard-easy dimension, and finally the second dimension used for the information variations of our assignment mechanism. We conclude the section with procedural details.

General overview: Our experiment consisted of three parts, 1) the real-effort task, 2) the *school-choice game*, and 3) an exit questionnaire.

In the first part of the experiment, participants solved a total of 24 incentivized Raven's Advanced Progressive Matrices (Raven, 1962) split into two consecutive sections. The first twelve matrices in part 1 were taken from the 12-item short form developed by Bors and Stokes (1998), consisting of (in that order) items 3, 10, 12, 15, 16, 18, 21, 22, 30, 28, 31, and 34.

Next, the participants were informed that they were randomly grouped with seven other participants and took the role of one of eight students facing four schools having a two-seat capacity each. They were instructed about the rules of the assignment mechanism and that their priority rank in their group would be determined by how well they performed in solving the subsequent twelve Raven matrices compared to other group members. The remaining twelve Raven matrices, section 2 of part 1, were assigned based on the group's difficulty condition. After completion of this section, participants answered two belief elicitation questions regarding their relative and absolute performances in this section. The real effort task was incentivized per correctly solved matrix, and we elicited beliefs with a binarized scoring rule (Hossain and Okui, 2013).

Part 2 was the *school-choice game*. Here, the participants took part in five rounds of rank-ordered list (ROL) submission to a given assignment mechanism, whose description was printed out. After the participants read the printout, they were asked to answer four comprehension questions, two about how the assignment mechanism works and two about the construction of their priority rank. In each round, they privately learned a new vector of match values determining their earnings when matched with each of the four schools, and then they submitted their ROL. While the match values were new independent draws for each round, the priority rank of participants (and their group) remained fixed. To prevent learning, participants did not get any feedback about the assignment outcome between rounds.

Finally, in part 3, they answered an exit questionnaire and then received their final monetary payoff. This payoff was the sum of their part 1 earnings, the outcome of one randomly chosen round of the *school-choice game*, and the participation fee.

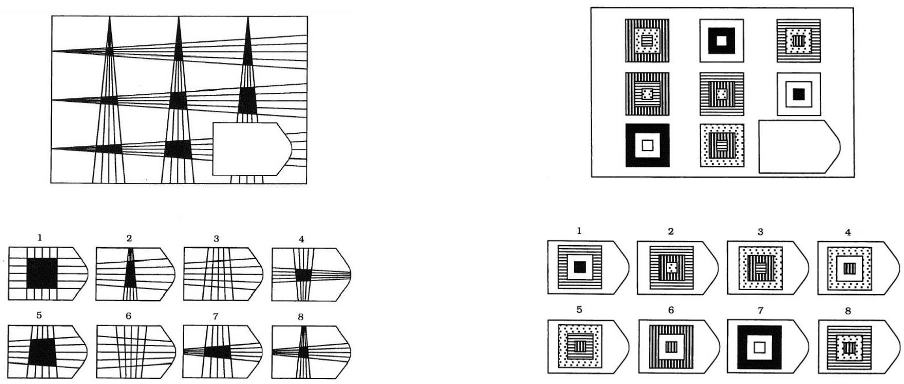

Figure 1: Examples of the Raven matrices in difficulty condition Easy (left) and Hard (right).

Hard-easy gap and priorities: Each student group was randomly assigned to one of the difficulty conditions, Easy or Hard. These two conditions differed in the real effort tasks of the second section of part 1. Compared to the twelve matrices concerning the first section, the twelve matrices in Easy were easier, while the twelve matrices in Hard were more difficult. Specifically, the matrices in Easy were the first 12 items (in order of increasing difficulty) that were not part of the 12-item short-form questionnaire: items 1, 2, 5, 6, 7, 8, 9, 11, 4, 14, 17, and 19. The matrices in Hard were the last 12 items (in order of increasing difficulty) that were not part of the 12-item short-form questionnaire: items 13, 20, 23, 25, 26, 24, 27, 29, 32, 33, 35, and 36.[2] The idea behind the hard-easy gap method (Lichtenstein et al., 1982; Dargnies et al., 2019) is to create overconfidence in the Easy condition and underconfidence in the Hard condition. In this paper, we always mean overplacement (Moore and Healy, 2008) when we mention overconfidence, i.e., the situation in which subjects overestimate their priority rank.

The first section of part 1 is a novelty over the earlier papers, and it served a dual purpose: first, these matrices provide a measure of cognitive ability that is not linked to the priority rank; second, they created a reference point for the difficulty of the matrices subject to priority ranking. In both difficulty conditions, participants received a point for each correct solution in the second section, and their priority ranks were determined according to the total number of points received. The time spent solving matrices served as a tie-breaker. Subjects knew that all other students in their group were presented with the same set of matrices.

***School-choice game* and match values:** A market consisted of eight students (the participants) and four schools. A participant's payoff from a round was determined by the assignment outcome. Each of the schools $s \in S = \{\triangle, \square, \bigcirc, \bigstar\}$ had a capacity for two students. In each round, each student i was presented a new vector of match values $\mathbf{v_i} = (v_{i,s})_{s \in S}$. Students knew the distribution from which all participants' values were drawn, but they only learned their own realization $\mathbf{v_i}$. The match values $v_{i,s}$ were drawn in advance, and the same set of values was used in all sessions. They were i.i.d draws from a uniform distribution on different discrete supports from intervals of integers, $V_s = [\![\underline{V}_s, \overline{V}_s]\!] := [\underline{V}_s, \overline{V}_s] \cap \mathbb{Z}$. Specifically, these intervals were

$$V_\triangle = [\![7, 10]\!], \quad V_\square = [\![5, 10]\!], \quad V_\bigcirc = [\![2, 7]\!], \quad \text{and } V_\bigstar = [\![2, 5]\!].$$

That is, subjects were aware that, for all participants, (ex-ante) the match values for school $\triangle$ tended to be the highest and the match values for school $\bigstar$ tended to be the lowest. Moreover, the possible ordinal preferences ("straightforward ROLs") were

$$(\triangle,\ \square,\ \bigcirc,\ \bigstar),\ (\square,\ \triangle,\ \bigcirc,\ \bigstar),\ (\triangle,\ \square,\ \bigstar,\ \bigcirc),\ (\square,\ \triangle,\ \bigstar,\ \bigcirc),\ (\triangle,\ \bigcirc,\ \square,\ \bigstar).$$

The idea behind this design was to reflect that preferences over schools in real-life

[2]We follow the item reordering suggested by Bors and Stokes (1998), who validated the test with more than 1,000 students.

settings are similar across students, but not perfectly correlated.

Information conditions: Our experiment employed three information conditions, Info-0, Info-1, and Info-2. The difference between the first two information conditions was that in condition Info-1, the participants learned about their own priority rank, while in Info-0, they were not informed. Hence, condition Info-1 aimed to correct misconfidence. In contrast to the other two conditions, the participants in Info-2 were assigned sequentially in priority order among their group, and, at the time of their own ROL submission, they could see the remaining capacity of each school so that this condition eliminated the uncertainty about seat availability.

Assignment Mechanism: In the information conditions Info-0 and Info-1, participants received a description of the static deferred-acceptance (DA) mechanism with common priorities: In step 1 of the mechanism, each student applies to the first-ranked school in their ROL, and schools then temporarily accept the two highest-priority applicants and reject all others; in the next step, all rejected students automatically apply to the next-ranked school in their ROL, and schools again temporarily accept the two highest-priority applicants and reject all others; this process is repeated until no student is rejected and then the assignment is finalized. In Info-2, the participants submitted their ROL sequentially following the priority ranks, and their assignment was finalized immediately afterwards so that the remaining capacity could be updated for the following students to see. Importantly, the participants needed to submit a complete ROL in all conditions, including Info-2. Finally, we did not explain or even mention strategy-proofness in the mechanism description.

Belief elicitation: Before part 2 started, we asked our participants to submit an estimation of their absolute and relative performance in the second section of part 1. Both questions were incentivized. The exact details of incentivization for the relative performance question, i.e., binary scoring rule, were not provided (Danz et al., 2022).

Questionnaire: The experiment concluded with an exit questionnaire. At the end of the experiment, we also provided a free text field for participants to describe their decision process when entering their ROL.

Procedures: We conducted the experiment in November 2023 at the WZB-TU lab of the Technical University of Berlin, and our participants were recruited from the lab's recruitment pool, mainly consisting of undergraduate students ($N =$

384). Approximately 45% of participants self-identified as female, 53% as male, and 2% as non-binary. The average payment per subject was **15.56** Euro. The experiment was programmed in z-Tree (Fischbacher, 2007). Each session took on average **60** minutes.

Each session consisted of 16 subjects, split into two 8-student groups. In total, we ran eight sessions per information condition. The information conditions were determined randomly at the session level. In each session, one group was randomly assigned to the difficulty condition Easy, while the other group was assigned to Hard. Each subject played five rounds of the *school-choice game* corresponding to their randomly assigned treatment. Consequently, we obtained 1.920 observations in total. That is, for each of the six treatment combinations, we have 320 observations comprised of five rounds of 64 participants.

3 Theory

Setting: Let a student i be labeled according to their (true) priority rank, $i \in I := [\![1, 8]\!]$. Each student has a type θ_i, learns some information γ_i depending on the information condition, and submits a rank-ordered list (ROL) r_i over schools $s \in S = \{\triangle, \square, \bigcirc, \bigstar\}$ to the mechanism. We describe type θ_i and information γ_i in more detail later. Let Θ be the type space, Γ be the space of learned information, and let R be the space of possible (complete) ROLs, i.e., the space of all permutations of set S. A (pure) strategy is a mapping $\sigma_i : \Theta \times \Gamma \to R$. We consider different behavior models, but in each one, student i maximizes an expected utility function

$$U(r_i|\theta_i, \gamma_i) = \mathbb{E}_{r_{-i},i}[u_i(r_i, r_{-i}|\theta_i)|\theta_i, \gamma_i].$$

Let us call the two students with the highest priority, $i \in \{1, 2\}$, **top** students, the next two, $i \in \{3, 4\}$, **medium** students, and all other, $i \geq 5$, **bottom** students. Students $i \geq 3$ are called non-top students.

Type $\boldsymbol{\theta}_\mathbf{i} = (\mathbf{v}_\mathbf{i}, \mathbf{b}_\mathbf{i})$: Participant i privately observes the **value vector** that contains the match payoff for each school, $\mathbf{v}_i = (v_{i,s})_{s \in S}$, and she forms a **priority belief** b_i about the common priority ranking. A priority belief is a probability distribution, $b_i = (b_{i,j})_{j \in I}$ where $b_{i,j}$ is the probability with which i believes to have priority rank j and $\sum_{j \in I} b_{i,j} = 1$. For point beliefs, we abuse notation such that $b_i = j$ means that i believes to have priority rank j with certainty. Note that our experimental setup induces students' preferences $\mathbf{v}_i$ and provides correct beliefs over $\mathbf{v}_{-i}$, but students form their own beliefs b_i. Our experiment does not ask participants for full distributions b_i, but only for an estimated rank.

Assignment: Let M be the space of possible assignments, where an assignment $\mu : I \to S$ maps a student into their assigned school. Our assignment mechanism maps ROLs into assignments, $m : R \to M$. For ease of exposition, we use the notation $\mu_i = \mu(i)$ and $m_i(\mathbf{r}) = m(\mathbf{r})(i)$. Because in our setting all feasible assignments are individually rational and non-wasteful, we call an assignment stable if and only if no student has justified envy. Student i has justified envy in assignment μ if there is another student $j > i$ and $v_{i,\mu_j} > v_{i,\mu_i}$, i.e., i has justified envy when a lower-priority student $j > i$ is assigned to a school μ_j that i prefers over their own assignment μ_i. An assignment μ is efficient if there exists no other assignment $\mu' \neq \mu$ such that for all students i $v_{i,\mu'_i} \geq v_{i,\mu_i}$, i.e., there exists no Pareto-improving trading cycle.

Information conditions $\mathbf{\Gamma}$: Our three information conditions manipulate what information a student i learns. In **Info-0**, $\gamma_i = \emptyset$ for all $i \in I$. That is, no student gets any additional information and submits an ROL solely based on their values $\mathbf{v}_i$ and belief b_i. In information condition **Info-1**, $\gamma_i = i$ for all $i \in I$. That is, each student learns their priority rank (=their index in our labeling convention) and updates their belief to the correct point belief $b_i = i$. In **Info-2**, $\gamma_i = (q^i_s)_{s \in S}$ for all $i \in I$, where q^i_s is the remaining capacity of school s at step i of the mechanism. Clearly, student i can also infer their own priority rank from this information. In this information condition, student i learns which schools are **available** to i, i.e., the set $\overline{S}(\gamma_i) := \{s : q^s_i > 0\}$.

Straightforward ROLs and consequential manipulations: For each student i, the unique **straightforward** ROL is

$$\widehat{r}_i(\mathbf{v}_i) = (s_1, s_2, s_3, s_4), \text{ where } v_{s_1,i} > v_{s_2,i} > v_{s_3,i} > v_{s_4,i}$$

Let $r_i \neq \widehat{r}_i(\mathbf{v}_i)$ be a **consequential manipulation** for student i for a given vector $\mathbf{r}_{-i}$ if and only if

$$m_i(r_i, \mathbf{r}_{-i}) \neq m_i(\widehat{r}_i(\mathbf{v}_i), \mathbf{r}_{-i}),$$

i.e., whenever the assigned school under r_i differs from the assigned school under the straightforward ROL.

Behavior models: We consider four behavior models that differ in the underlying expected utility function U: Classic, IA, EBLA, and RDP. For any belief about r_{-i} (or, alternatively, any belief about σ_{-i} and belief about θ_{-i}) and any priority belief b_i, define the assignment beliefs $p_{i,s}(r_i|b_i,\gamma_i) = \mathbb{E}_{i,r_{-i}}[\Pr(m_i(r_i,r_{-i}) = s)|b_i,\gamma_i]$ as the probability that our mechanism assigns student i to school s when i submits ROL r_i. Similarly, let $p^{IA}_{i,s}(r_i|b_i)$ be the corresponding probability for the Immediate Acceptance (Boston) mechanism. In the **classic** behavior model, students maximize

$$U^{Classic}(r_i|\theta_i,\gamma_i) = \sum_{s\in S} p_{i,s}(r_i|b_i,\gamma_i)v_{i,s}. \tag{1}$$

In the **immediate-acceptance (IA)** behavior model, students maximize

$$U^{IA}(r_i|\theta_i,\gamma_i) = \sum_{s\in S} p^{IA}_{i,s}(r_i|b_i,\gamma_i)v_{i,s}, \tag{2}$$

where the only difference from the classical model is that the student misspecifies how ROLs and priorities map into assignments. In the **expectation-based loss aversion** (EBLA) and the **ranking-dependent preferences** (RDP) behavior models, students maximize

$$U^{EBLA}(r_i|\theta_i,\gamma_i) = \sum_{s\in S} p_{i,s}(r_i|b_i,\gamma_i)u^{EBLA}(s|r_i,\theta_i,\gamma_i) \text{ and} \tag{3}$$

$$U^{RDP}(r_i|\theta_i,\gamma_i) = \sum_{s\in S} p_{i,s}(r_i|b_i,\gamma_i)u^{RDP}(s|r_i,\theta_i,\gamma_i), \text{ respectively.} \tag{4}$$

In both these models, students have a correct understanding of the mechanism. In addition to the classic component, the EBLA model incorporates a gain-loss utility that reflects a possible disappointment with respect to a reference point, and the RDP model incorporates a component that reflects a direct extra utility from an assignment that is ranked high in the submitted ROL. We provide more details in the appendix.

Given a behavior model X, an ROL $r^*(\theta_i,\gamma_i)$ is **optimal** given (θ_i,γ_i) if

$$U^X(r^*(\theta_i,\gamma_i)|\theta_i,\gamma_i) \geq U^X(r|\theta_i,\gamma_i) \quad \text{for all } r\in R. \tag{5}$$

We now state some theoretical insights.

Insight 1. *In our setting, there is a unique stable assignment $\mu^* = m((\widehat{r}(\mathbf{v}_i))_{i\in I})$, and this assignment is also efficient. Therefore, we call μ^* the optimal assignment.*

This insight applies in any setting with a single priority ranking common to all schools. Our mechanism implements this optimal assignment when all students report straightforwardly.

Insight 2. *Student i has justified envy if and only if their ROL r_i is a consequential manipulation.*

This insight follows from the strategy-proofness of our mechanism and the uniqueness of the stable assignment. Since the straightforward ROL leads to the best possible assignment given any r_{-i}, r_i must lead to a worse assignment. Moreover, another student $j > i$ must get this better assignment $m_i(\widehat{r}(\mathbf{v}_i), r_{-i})$. Since j has lower priority, i's envy is justified. Similarly, student $j > i$ can only get a placement that i prefers if i's ROL is a consequential manipulation.

A consequential manipulation is neither necessary nor sufficient for being part of a Pareto-improving trading cycle. That is, there can be another assignment that all students weakly prefer over a given assignment and student i prefers it strictly, but student i reported her preferences straightforwardly. However, since the only assignment that lacks justified envy is efficient, some consequential manipulation must have occurred for inefficiency to arise.

Insight 3. *Consider behavior model Classic and any $\mathbf{v}_i$. The ROL r_i is optimal for every assignment belief $p_{i,s}$ if and only if r_i is straightforward.*

In other words, the assignment mechanism is strategy-proof in the classical model, and the straightforward equilibrium implements the optimal assignment μ^*. It is easy to see that NSF ROLs can be strictly optimal in behavioral models IA, EBLA, and RDP.

Insight 4. *Consider behavior model IA, EBLA, or RDP, and any $\mathbf{v}_i$. If a student believes that a match with their most-preferred school is unlikely, a non-straightforward ROL leads to a strictly larger expected payoff than the straightforward ROL.*

It is well-known that the IA (Boston) mechanism is not strategy-proof (Abdulkadiroğlu and Sönmez, 2003) and that ranking the most-preferred school first is strictly suboptimal if a match is very unlikely. Under EBLA, the insight follows from Meisner and von Wangenheim (2023, Proposition 2). Under RDP, it follows from Meisner (2023, Proposition 1). While such a manipulation by a pessimistic student can be consequential, no potentially consequential manipulation can be optimal for a student who believes to be top.

Insight 5. *Consider information condition* INFO-0 *and any of the behavior models. If b_i assigns a sufficiently large probability to ranks 1 and 2, the straightforward ROL leads to a higher expected payoff than any ROL that does not rank the school with the highest match value first.*

Insight 6. *Consider information condition* INFO-1 *or* INFO-2 *and any of the behavior models. A consequential manipulation is never optimal for any top student* $i \in \{1, 2\}$.

If top students are aware that they are top students, they understand that they are assigned with certainty to whatever school they rank first: because all schools have two seats, it is not possible (neither in DA, nor in IA) that their first-ranked school is taken by higher-priority students. Essentially, submitting an ROL boils down to selecting whatever school they rank first with certainty. In INFO-0, some students may incorrectly believe to be top students and submit the straightforward ROL, while they would have submitted a potentially consequential manipulation otherwise. That is, in behavior models other than the classic model, being overconfident can lead to a preferred assignment.

Insight 7. *Consider information condition* INFO-2 *and any of the behavior models. A consequential manipulation is never optimal for any student. Consequently, the optimal assignment* μ^* *is implemented.*

What only holds for top students in information condition INFO-1, holds for everyone in information condition INFO-2: students choose their school under certainty. They know that they can never be assigned to an unavailable school (one where all seats are taken by higher-priority students), and they know that, at the time of their action, the higher-priority students have already been assigned. Because ranking the final assignment first has an inherent benefit for students under the behavior model RDP, students will rank the most preferred available school first and will be assigned to it. Under the classic or the EBLA model, students know that any manipulation involving an unavailable school will not affect their assignment and so they may rank such a school first. As long as the most-preferred available school is ranked first among the available schools in their ROL, this ROL is a non-consequential manipulation and therefore optimal as it yields the maximal obtainable payoff, equivalent to the straightforward ROL. In the IA model, students may believe that at their turn an IA mechanism is executed just involving them and the available schools, which would be equivalent to our mechanism.

Insight 7 relies not only on the behavioral theories we consider. In fact, the strategic simplicity of the dynamic implementation can be formalized: it is strongly obviously strategy-proof. A strategy is obviously dominant (Li, 2017) if it prescribes at each information set an action such that the worst-case payoff from this action is at least as large as the best-case payoff under any deviation at this information set. Here, this obvious dominance is strong (Pycia and Troyan, 2023) because each student is called to play exactly once and no foresight is required to calculate payoffs. Any strategy that only prescribes weakly straightforward ROLs at each information set is strongly obviously dominant.

Insight 8. *In* INFO-0*, no student has an obviously dominant strategy. In* INFO-1*, the straightforward ROL is an obviously dominant strategy only for top students. In* INFO-2*, the straightforward ROL is an obviously dominant strategy for all students.*

General insights regarding non-top students $i \geq 3$ are more involved because they depend on unobserved parameters of the model, such as Λ in the EBLA model or ρ in the RDP model. Moreover, students will likely make different assumptions about the behavior of higher-priority students in the information conditions INFO-1 and INFO-0. In INFO-1, the behavior of top students is pinned down by the insights above, but they may have incorrect beliefs in information condition INFO-0 and therefore behave differently. Consequently, it is not clear that, for instance, a student who believes to have priority rank 4 in INFO-0 behaves the same as a student who knows to have this rank in information condition INFO-1. However, our theory predicts two students with the same priority to behave the same in EASY×INFO-1 and in HARD×INFO-1. Similarly, the difficulty condition should not matter in information condition INFO-2.

4 Hypotheses

In this section, we spell out our hypotheses and explain how they are guided by the theoretical insights discussed above.

Beliefs: Hypothesis 0 is the "hard-easy gap" in beliefs, a fundamental assumption to develop the arguments behind our main hypotheses. In other words, a verification of Hypothesis 0 is necessary for our predictions on behavior and assignments and thus needs to be tested first. According to the non-classical behavioral theories, submission behavior depends on beliefs. Consequently, a change in beliefs leads to a change in actions, which can be consequential for final assignments.

Hypothesis 0. *Compared to subjects in* Hard*, subjects in* Easy *have more positively biased beliefs about their relative performance, exhibiting greater overplacement and less underplacement.*

The hard-easy gap is a technique to exogenously manipulate participants' beliefs. It is a stylized fact that participants who perform well in an easy test tend to overplace themselves in relative rankings without fully accounting for the fact that all other participants took the same easy test and likely also did well. Vice-versa, the beliefs about the relative performance in a hard test are shifted in the other direction. That is, participants overweight their own absolute performance, and they neglect the correlation with the objective difficulty of the test. In the context of our experiment, this misconfidence translates into a biased perception of students' priority.

Impact of confidence shock: We continue our argument and move from beliefs to actions in information condition Info-0. Since we expect students' beliefs to pool at the top in the difficulty condition Easy, we also expect their actions to be similar. Specifically, and in line with Insight 5, we expect mostly straightforward play. In contrast, in difficulty condition Hard, we expect only a few subjects to believe they have high priority so that, according to Insight 4, fewer straightforward ROLs should be observed. Since we expect (some of) our subjects' behavior to be belief-dependent, priority information should eliminate the differences between the difficulty conditions.

Our hypotheses on assignments are an implication of the predicted behavior. As a consequence of their predicted straightforward reporting in Easy×Info-0, top students are assigned to their destined school μ_i^*. Similarly, we predict medium students to be assigned to one of the two high-value schools, $\triangle$ or $\square$. On the contrary, any manipulation at the top of a top student's ROL immediately leads to justified envy, because they are always assigned to their first-ranked school, see Insight 2. Hence, we predict more justified envy among top students in Hard×Info-0, and the benefactors of these consequential manipulations are necessarily non-top students.[3] Because of the effect of priority information on actions, we predict no difference in justified envy between Easy×Info-1 and Hard×Info-1, nor between Easy×Info-2 and Hard×Info-2.

Hypothesis 1. *In information condition* Info-0, *there is more justified envy in* Hard *than in* Easy. *In information conditions* Info-1 *and* Info-2, *there is no systematic difference in justified envy between* Hard *and* Easy.

Impact of priority information: Because priority information aligns the behavior across the difficulty conditions, its impact depends on the initial beliefs and the true priority of the informed student. In particular, Insight 6 predicts that, regardless of the difficulty condition, all top students in Info-1 (or in Info-2) reveal their true favorite school to be assigned to it. Consequently, it should have little effect on top students' justified envy in the difficulty condition Easy as their initial priority belief is expected to be accurate. The opposite is true in difficulty condition Hard where the positive belief update should lead to a difference in the behavior and assignment of top students in Hard×Info-0 and Hard×Info-1. In contrast, the initial beliefs of non-top students are more accurate in the difficulty condition Hard. In difficulty condition Easy, we predict that priority information reduces straightforward play among non-top students because it corrects their overplacement. In sum, we expect more justified envy in Easy×Info-1 than in Easy×Info-0, and this increase is driven by non-top students. In difficulty condition Hard, we expect the effect of priority information to go in the other direction, and this decrease in justified envy is driven by top students.

Hypothesis 2. *There is more justified envy in* Easy×Info-1 *than in* Easy×Info-0. *There is less justified envy in* Hard×Info-1 *than in* Hard×Info-0.

Impact of availability information: When students obtain availability information, they essentially select schools under certainty. While a non-straightforward ROL can be optimal, a consequential manipulation never is, see Insight 7. Since all our behavioral theories clearly predict the optimal matching μ^* to arise, we expect the lowest rates of justified envy in information condition Info-2. Because the dynamic implementation might be easier to understand, justified envy may even reduce among top students from Info-1 to Info-2, although our models predict

[3] The scope for justified envy among non-top students is smaller. For instance, for student $i = 6$ any manipulation involving $\triangle$ or $\square$ is likely to be inconsequential because they are likely to be taken by higher-priority students $i < 6$.

no consequential manipulations by top students in both information conditions.

Hypothesis 3. *No other information condition exhibits lower rates of justified envy than* Info-2.

Table 1 summarizes our predictions on justified envy (JE) in plain language. To a lesser degree, these predictions also apply to inefficiency. By Insight 1, the assignment of the straightforward equilibrium is efficient. Hence, a consequential manipulation is necessary for inefficiency to arise. However, as discussed, it is not sufficient because the student who obtained a better assignment as a result may not want to trade it away.

	Easy		**Hard**	
	Top	Non-top	Top	Non-top
Info-0	Little JE	Tiny JE	Big JE	Some' JE
Info-1	No JE	Some JE	No JE	Some JE
Info-2	No JE	No JE	No JE	No JE

Table 1: Our predictions about justified envy (JE) are rooted in behavioral theory. The classical model predicts no JE for each cell in the table. Here, No $<$ Tiny $<$ Little $<$ Some $\approx$ Some' $<$ Big.

5 Results

In this section, we test our hypotheses and report the results. We first examine the induced hard-easy gap in beliefs for all information conditions and then whether it translates into a difference in actions and assignments. We begin our analysis with the information condition Info-0, and we then compare these results to the expected null results in the information conditions Info-1 and Info-2. Finally, we test whether increasing the information level mitigates the effects of the hard-easy gap and, in particular, whether these effects differ between top and non-top students. While the main part of our analysis is based on our hypotheses and therefore focuses on justified envy, we also study inefficiency at the end of this section.

Figure 2 shows the induced hard-easy gap pooled over all information conditions. First of all, this figure suggests that we selected appropriate matrices for the difficulty conditions in the sense that participants in condition Easy indeed solved and believed to have solved more matrices correctly. Participants believe that they

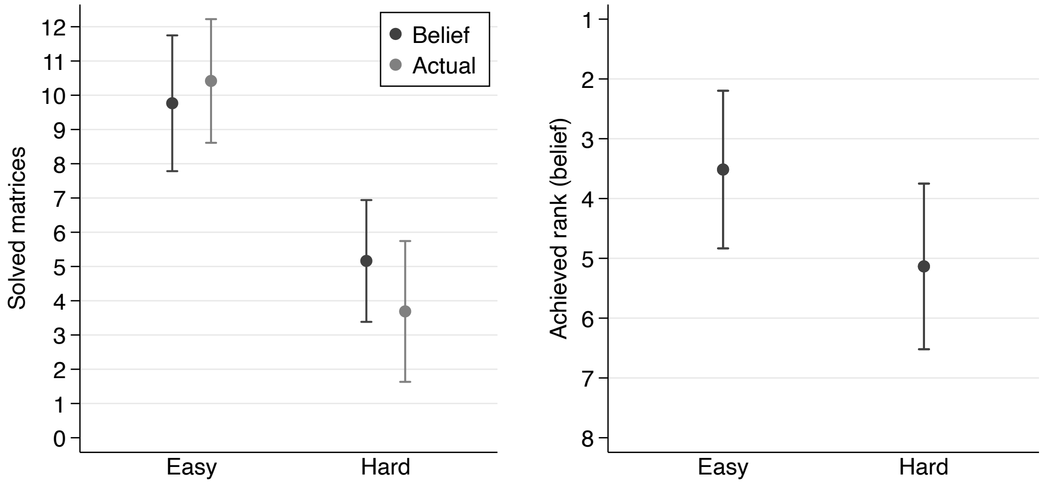

Figure 2: Means of beliefs about number of solved matrices (left) and achieved rank (right) by Easy and Hard with standard deviations for all information conditions.

solved around 10 out of 12 matrices ($M = 9.77$, $SD = 1.98$) correctly in Easy and about 5 out of 12 in Hard ($M = 5.16$, $SD = 1.78$). The difference is significant, $t(382) = 23.96$, $p < 0.01$. For our design, it is not necessary that participants over- or underestimate their *absolute* performance. What is crucial is that their beliefs about their *relative* performance are distorted. As the right sub-figure shows, participants believe that they rank on average between rank 3 and 4 in Easy ($M = 3.51$, $SD = 1.32$) and at rank 5 in Hard ($M = 5.14$, $SD = 1.39$). Again, the difference is significant, $t(382) = -11.74$, $p < 0.01$.[4]

	Underconfident	–	Overconfident
Easy	25.52%	16.67%	57.81%
Hard	56.25%	16.15%	27.60%

Table 2: Under- and overconfidence by Easy and Hard for all information conditions.

From the believed ranks, we deduce whether participants over- or underplace themselves. To this end, we define the variable *confidence* as the difference between real and believed rank. That is, a positive realization reflects overconfidence, i.e., overplacement. Figure 3 shows the distribution of believed ranks and the corresponding confidence for both difficulty conditions. Table 2 presents the proportion

[4] All tests hold with $p < 0.01$ for each information condition, respectively.

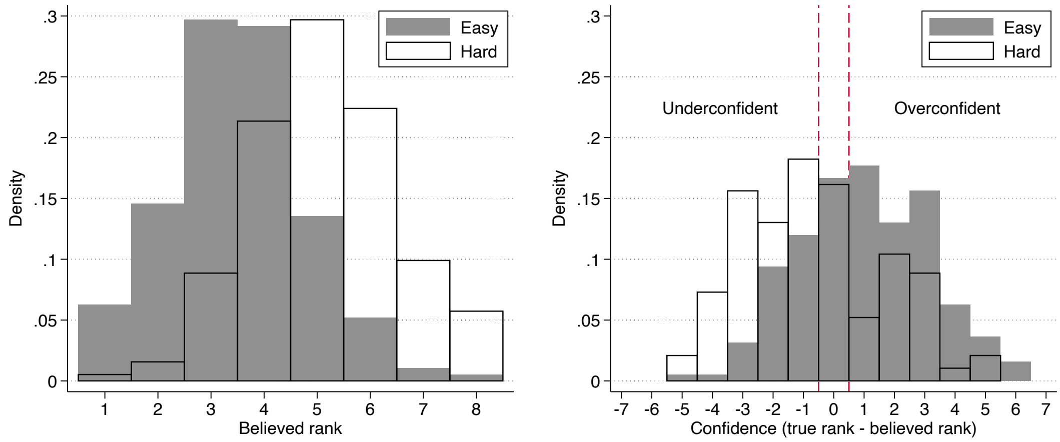

Figure 3: Distribution of believed priorities and confidence by Easy and Hard for all information conditions.

of overconfident and underconfident participants. While the majority of participants in the Easy condition were overconfident ($M = 0.98$, $SD = 0.16$), the majority in the Hard condition was underconfident ($M = -0.64$, $SD = 0.17$). Indeed, we confirm that the Easy (Hard) condition induces significant overplacement (underplacement), as evidenced by the mean confidence being significantly below (above) zero, confirmed by a two-sided t-test for both (Easy: $t = 3.64$, $p < 0.01$; Hard: $t = -3.80$, $p < 0.01$).[5]

Result 0. *Participants in* Easy *believe to have significantly better priority ranks than participants in* Hard*.* Easy *(*Hard*) induces significant overplacement (underplacement).*

We now turn our attention to differences in justified envy (JE) in the difficulty conditions. In keeping with Insight 2, we define the variable JE to be 1 for each individual participant who submitted a consequential manipulation and 0 otherwise. Table 3 provides descriptive evidence that the hard-easy gap in beliefs corresponds to a gap in JE in information condition Info-0, but not in information condition Info-1. This observation is in line with our theory, which predicts behavior (and thus the resulting assignments) to be belief-dependent, because the priority information eliminates the belief distortions triggered by the difficulty conditions. That is, our evidence is consistent with Hypothesis 1. Surprisingly, this alignment seems to be less pronounced in information condition Info-2, where priority in-

[5] All tests hold with $p < 0.01$ for each information condition, respectively.

formation is also provided along with the availability information. However, this difference is not significant as discussed below. Nevertheless, the presence of JE in Info-2 is puzzling, and we return to it later in this text.

	Info-0	Info-1	Info-2
Easy	8.13%	15%	6.56%
Hard	15.31%	16.25%	10.00%

Table 3: Share of consequential manipulations (JE) by Easy and Hard and the information conditions.

In Table 4, we show by means of a random-effects regression in specifications (1) and (2) of *Panel A* that there is a direct treatment effect of Hard in Info-0 (without controls: $p = 0.048$; with controls: $p = 0.044$). Specifications (3) to (6) in all panels show that the unveiled effects vanish when priority information is provided, i.e., the effects are not present in the information conditions Info-1 (without controls: $p = 0.799$; with controls: $p = 0.911$) and Info-2 (without controls: $p = 0.262$; with controls: $p = 0.303$).[6] In *Panel B*, we employ an instrumental variable approach to show that the difficulty conditions causally affect justified envy via the confidence measure in Info-0 (without controls: $p = 0.051$; with controls: $p = 0.051$). Again, this neither holds in Info-1 (without controls: $p = 0.797$; with controls: $p = 0.911$) nor in Info-2 (without controls: $p = 0.270$; with controls: $p = 0.327$).[7]

Result 1. *In information condition* Info-0*, there is significantly more justified envy in* Hard *than in* Easy*. In information conditions* Info-1 *and* Info-2*, there is no significant difference between justified envy in* Hard *and* Easy*.*

From the perspective of Hypothesis 2, the verdict of Table 3 appears to be unclear at first glance: while an increase in information is accompanied by an increase in JE in difficulty condition Easy, there does not seem to be an effect in difficulty condition Hard (if anything, this effect would be in the "wrong" direction). That is, the evidence is consistent with the first part of Hypothesis 2, but we do not seem to see the predicted decrease in JE in Hard. To get a better understanding, let us delve into the details of our argument behind Hypothesis 2, which predicts the increase in Easy to be driven by non-top students and the decrease in Hard

[6] Results hold qualitatively when analyzing the intensive margin, i.e., the loss of payoffs due to JE, instead of the extensive margin, i.e., the presence of JE, see Table 8 in the appendix.

[7] Results are consistent when instrumenting the believed rank instead of the confidence.

	Info-0		Info-1		Info-2	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Random-effects model						
Hard	0.082**	0.080**	0.014	0.006	0.039	0.036
	(0.042)	(0.040)	(0.056)	(0.054)	(0.035)	(0.035)
Constant	0.093***	0.153	0.171***	0.312***	0.075***	0.058
	(0.031)	(0.104)	(0.038)	(0.079)	(0.017)	(0.083)
Baseline	Easy	Easy	Easy	Easy	Easy	Easy
Controls	No	Yes	No	Yes	No	Yes
Observations	560	560	560	560	560	560
Panel B: Instrumental variable model						
Confidence	-0.048*	-0.048*	-0.008	-0.003	-0.027	-0.024
	(0.025)	(0.024)	(0.032)	(0.029)	(0.024)	(0.024)
Constant	0.121***	0.220***	0.176***	0.319***	0.090***	0.104
	(0.023)	(0.079)	(0.029)	(0.075)	(0.016)	(0.100)
Controls	No	Yes	No	Yes	No	Yes
Observations	560	560	560	560	560	560

The dependent variable is Justified Envy $\in \{0, 1\}$. Estimation by random-effects regression in Panel A and by two-stage least squares random-effects estimator in Panel B, where Believed rank is instrumented by Hard. Standard errors clustered on group-level in parentheses. Controls are performance in baseline Raven test, gender, and the true ROL. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table 4: The effect of confidence on JE by information condition.

to be driven by top students.

Table 5 differentiates between top and non-top students. It juxtaposes the observed rates of JE with the predictions in Table 1. Let us first consider top students, i.e., the left side of each table in both difficulty conditions. As predicted, priority information reduced consequential manipulations among top students, and, also as predicted, this reduction is large in Hard and small in Easy. Surprisingly, the rates of JE among top students in both difficulty conditions in Info-1 remain larger than zero. Specifications (3) to (6) of *Panel A* in Table 6 further scrutinize this discussion. All in all, the observed effect of priority information on top students' justified envy is consistent with our theory: there is no significant effect of Info-1 in Easy (specifications (3) and (4), $p = 0.806$ and $p = 0.950$), whereas this effect is (marginally) significant in Hard (specifications (5) and (6), $p = 0.090$ and $p = 0.084$). Note that significance only at the 10% level may in part be due to the small sample size of top students.

On the other hand, Table 5 suggests that priority information increases JE for non-

	Easy			Hard	
	Top	Non-top		Top	Non-top
Info-0	18.75% (Little)	5.50% (Tiny)	Info-0	26.25% (Big)	14.00% (Some')
Info-1	16.25% (None)	17.50% (Some)	Info-1	8.75% (None)	22.50% (Some)
Info-2	2.50% (None)	9.50% (None)	Info-2	2.50% (None)	15.00% (None)

Table 5: Instances of consequential manipulations (justified envy) for top and non-top ranked students with theoretical predictions in brackets.

top students, and, notably, this is the case in both difficulty conditions. While we predicted a stark increase in Easy, we only expected a negligible effect in Hard. Figure 4 suggests that this observation is driven by medium students, while bottom students barely seem to be affected by priority information.

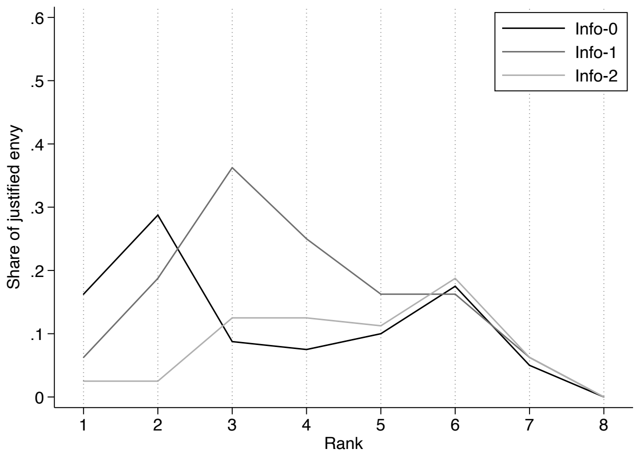

Figure 4: Prevalence of justified envy for each rank for all information conditions.

Focusing on non-top students, specifications (3) and (4) of *Panel B* in Table 6 document the predicted increase in justified envy due to priority information, and this effect is statistically significant in Easy ($p < 0.01$ both with and without controls), while specifications (5) and (6) show that it is not statistically significant in Hard ($p = 0.172$ and $p = 0.166$, respectively).

Taken together, these findings qualitatively confirm our theoretical arguments both for top and non-top students. However, quantitatively, we underestimated the effect of priority information on medium students in the difficulty condition Hard. Put in the words of Table 1 and 5, we seem to have misjudged the difference between *Some* and *Some'*. In Section 6, we continue the discussion of this observation. In light of this caveat, we present our results regarding Hypothesis 2 separately for top and non-top students.

	Easy and Hard		Only Easy		Only Hard	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Top students						
Info-1	-0.100	-0.075	-0.025	-0.007	-0.175*	-0.164*
	(0.073)	(0.073)	(0.102)	(0.104)	(0.103)	(0.095)
Info-2	-0.200***	-0.176***	-0.163**	-0.138	-0.237***	-0.208**
	(0.057)	(0.056)	(0.075)	(0.087)	(0.086)	(0.093)
Constant	0.225***	0.445***	0.188**	0.432**	0.262***	0.404***
	(0.056)	(0.138)	(0.074)	(0.219)	(0.085)	(0.139)
Baseline	Info-0	Info-0	Info-0	Info-0	Info-0	Info-0
Controls	No	Yes	No	Yes	No	Yes
Observations	480	480	240	240	240	240
Postestimation Wald tests:						
H_0: Info-1 = Info-2	$p = 0.03$	$p = 0.04$	$p = 0.06$	$p = 0.04$	$p = 0.30$	$p = 0.44$
Panel B: Non-top students						
Info-1	0.103***	0.103***	0.120***	0.125***	0.085	0.087
	(0.038)	(0.037)	(0.037)	(0.034)	(0.062)	(0.063)
Info-2	0.025	0.024	0.040	0.045*	0.010	0.004
	(0.030)	(0.029)	(0.029)	(0.025)	(0.047)	(0.047)
Constant	0.097***	0.106**	0.055***	0.094*	0.140***	0.108
	(0.018)	(0.047)	(0.018)	(0.051)	(0.024)	(0.079)
Baseline	Info-0	Info-0	Info-0	Info-0	Info-0	Info-0
Controls	No	Yes	No	Yes	No	Yes
Observations	1200	1200	600	600	600	600
Postestimation Wald tests:						
H_0: Info-1 = Info-2	$p = 0.06$	$p = 0.05$	$p = 0.04$	$p = 0.04$	$p = 0.29$	$p = 0.25$

The dependent variable is Justified Envy $\in \{0, 1\}$. Estimation by random-effects regression. Standard errors clustered on group-level in parentheses. Controls are performance in baseline Raven test, gender, and the true ROL. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table 6: The effect of information on JE for top and non-top students.

Result 2. *Among top students there is less justified envy in* Info-1 *than in* Info-0 *in the difficulty condition* Hard. *Among non-top students there is significantly more justified envy in* Info-1 *than in* Info-0 *in the difficulty condition* Easy.

We now study the effect of the availability information provided in condition Info-2. A glance at Table 5 reveals that this information reduces JE close to zero for top students, but surprisingly not for non-top students. Returning to Table 6 allows us to elaborate further on this observation. Here, we see in specifications (1) and (2) of *Panel A* that there is significantly less JE under Info-2 compared to Info-0 and Info-1 for top students. Unraveling the effect of availability information for non-top students requires extra care in reading *Panel B* of Table 6. Here, we see small positive coefficients in all specifications. While this suggests a null effect over baseline Info-0, it is important to understand that the coefficient reflects the effect of *both* priority *and* availability information. Accordingly, the post-estimation Wald test comparing Info-1 and Info-2 is significant in specifications (1) to (4), indicating that the effect is most pronounced in difficulty level Easy. In other words, we can interpret the result on non-top students' JE as the addition of availability information in Info-2 partially undoing the harm of priority information seen in the coefficient of Info-1.

Result 3. *Among top students, there is significantly less justified envy in* Info-2 *than in* Info-0 *and* Info-1 *in both difficulty conditions. Among non-top students there is significantly less justified envy in* Info-2 *than in* Info-1 *in the difficulty condition* Easy.

Efficiency: There are multiple ways to measure efficiency. To this end, we define the dummy variable PITC $\in \{0, 1\}$ that takes value 1 for a given student if there exists a Pareto-improving trading cycle involving this student, and zero otherwise. That is, this binary variable is defined on an individual level for a fixed group and round. Note that this measure is related to (but distinct from) JE, since a Pareto-improving trading cycle always implies that at least one participant involved in the trading cycle has JE (but not vice versa). Moreover, the mere existence of Pareto-improving trading cycles would be remarkable because we constructed students' preferences to be quite similar. For instance, if all students had the same ordinal preferences, such cycles would be impossible. That is, in addition to a misallocation we also need a double coincidence of wants in the preferences.

We check for the occurrences of Pareto-improving trading cycles and the impact of information on PITC for top and non-top students separately. We present the regression results in Table 10 in the appendix. First of all, the significant constants in specifications (1) and (2) (combined sample Easy and Hard) in both panels (top and non-top students) reveal that Pareto-improving trading cycles exist. Next, we see in specifications (5) and (6) in Panel A that the observed reduction of justified envy of top students in Hard due to priority information does not translate into significant efficiency gains. In contrast, specifications (1) and (2) of Panel B show that priority information creates significant inefficiency for non-top students in the combined sample, and, as predicted, this inefficiency is driven by the difficulty condition Easy. Surprisingly, the additional provision of availability information in Info-2 only reverses this effect, but does not eliminate inefficiency. This is puzzling because it means that multiple students have not selected their most preferred school from a certain choice menu.

6 Discussion

We did not only choose a setting with a central test score for simplicity, but also because such a priority structure is prevalent in real-life matching markets (see, e.g., Fack et al., 2019, Table 1). It is well-known that the pattern of NSF play that we document is not only an artifact of laboratory behavior, but it also occurs in the field. Because the true preferences are not observable, only "obvious manipulations" (Artemov et al., 2020; Shorrer and Sóvágó, 2023) are identifiable in the data, e.g., when preferences are expressed that rank attending school s and paying full tuition higher than attending the same school with a tuition waiver. Even when limiting attention to this lower bound, significant costs of manipulation can be observed. Moreover, survey data (Rees-Jones, 2018) is an additional piece of evidence suggesting that NSF play is a phenomenon not limited to the lab. Finally, Rees-Jones and Skowronek (2018) find that participants of the real-life National Resident Matching Program, which matches doctors to residency positions in hospitals in the United States exhibit the same behavior pattern in the lab as our participants.

To estimate participants' preferences in strategy-proof mechanisms, state-of-the-art econometrics (Fack et al., 2019; Artemov et al., 2023) does not rely on the assumption of straightforward play. Instead, these approaches rely on the (asymptotic) stability of the assignment, and they allow estimations that are robust to non-consequential manipulations. Here, it is crucial that students are able to identify the set of schools available to them (Che et al., 2023, show how to account for uncertainty in the priorities). While the prevalence of justified envy in our small-market setting does not necessarily cast doubt on this identifying assumption, our study shows that biased (and usually unobserved) beliefs can trigger unstable allocations, and it is not obvious that these biases vanish in large markets. However, econometricians can leverage this systematic dependence on the beliefs to improve preference estimation techniques.

Our results suggest that providing (certain) availability information leads to the most desirable assignments, even if it does not work perfectly. However, a dynamic implementation is necessary to provide this information, and, in large markets, such an implementation may not be practicable. With Croatian data, Kovač et al. (2025) analyze a setting with fuzzy availability information and document that applicants tend to overreact to the information. The German students in Grenet et al. (2022) also overreact to early offers instead of waiting for an acceptance from a preferred program. Kang et al. (2025) study a dynamic matching mechanism in Inner Mongolia where students are ranked according to a centralized score, are grouped into batches accordingly, and can revise their choices with fuzzy availability information before a deadline. The higher-scoring batches finalize their choices earlier because the deadlines are staggered. Kang et al. (2025) observe that the students just below a batch cutoff who have almost perfect availability information are more likely to apply to "reach schools," while those just above a cutoff who face more availability uncertainty do so less. These insights emphasize that our results on certain availability information have to be treated with caution, as they may not extend to fuzzy availability information.

In our setting, availability information makes the mechanism obviously strategy-proof. However, an obviously strategy-proof stable mechanism is not guaranteed to exist (Ashlagi and Gonczarowski, 2018), and also the robustness to expectation-based loss aversion relies on a particular priority structure (Dreyfuss et al., 2025). Our intention behind including this treatment was to understand the impact of eliminating uncertainty in the choice. The gap between the belief-based predictions and the actual assignment outcomes suggests further work on mechanism descriptions along the lines of Katuščák and Kittsteiner (2024), Gonczarowski et al. (2023), and Nagel and Saitto (2023) is required, because alternative mechanism descriptions seem to be more promising to improve the performance of the mechanism compared to simply giving advice (Ding and Schotter, 2017, 2019; Guillen and Hakimov, 2018).

A common takeaway from Pais and Pintér (2008) is that providing participants with more information leads them to attempt to utilize this information to strategize without noticing that NSF behavior can only be costly in a strategy-proof mechanism. In contrast, uninformed participants have no starting point for any attempt to game the system and revert to a straightforward heuristic. Since we observe that the difficulty condition has an impact on participants' ROLs in Info-0, this logic cannot explain all of the variation and belief-based explanations appear plausible. However, Figure 4 documents that students ranked third and fourth in Info-1 or those ranked second in Info-2 were specifically prone to consequential manipulations. Therefore, the strategic use of priority information seems too excessive to be explained only by belief-based theories.

We prohibit the submission of incomplete ROLs, and, hence, our study abstracts from non-placement. However, this problem has received considerable attention in the empirical literature, and the risk of non-placement is clearly associated with overconfidence. Arteaga et al. (2022) combine school choice with a search model and test their predictions using data from the Chilean national school-choice system (unconstrained DA) and the school-choice system in New Haven, Connecticut (truncated DA). The value of search crucially depends on admission chances, such that overestimating admission can reduce search and increase the risk of remaining unassigned. They find that targeted warnings about this risk to respective applicants are an effective intervention leading to longer rankings and a reduction in non-placement. While they stress that the strategy-proofness of the mechanism does not extend to strategy-proofness in the broader choice process, we stress that strategy-proof mechanisms may fail even absent such search costs and absent the risk of non-placement. Similarly, Fabre et al. (2021) and Kovač et al. (2025) record in Chilean and Croatian data, respectively, that applicants' risk of non-placement can be reduced by providing personalized information.

Finally, we find it valuable to highlight gender differences in our setting. Result 1, which documents the effect of confidence on justified envy, is primarily driven by women in the Info-0 condition. That is, in the absence of information, women who are underconfident are more likely to consequentially manipulate their preference reports. In contrast, men in the same condition do not exhibit a similarly systematic relationship between confidence and JE. For the information conditions Info-1 and Info-2, the results hold for both men and women (see Appendix Table 11). Moreover, the result that priority information increases JE among non-top students is mainly driven by men. Lastly, our observation that availability information in Info-2 mitigates the harm done by priority information holds for both top-performing men and women (see Appendix Tables 13 and **??**). These ex-post findings reveal potential systematic gender differences in how confidence and information interact with preference reports in a strategy-proof school-choice mechanism.

7 Conclusion

The purpose of this research should not be misunderstood as attempting to demonstrate how poorly strategy-proof mechanisms perform, as they fail to fully induce the predicted straightforward play and do not implement the desirable assignments they are designed for. On the contrary, we believe they often work very well, just not as well as classically predicted. We use modern theory to predict behavior and to inform about policy interventions where classical theory is silent. That is, while we want to shatter confidence in perfectly working strategy-proof market design, we also want to emphasize the hope to further improve assignment outcomes through seemingly irrelevant details of the market design.

Our experiment suggests that submission behavior in a strategy-proof assignment mechanism systematically depends on beliefs about priority. By means of a hard-easy gap, we exogenously induce participants to overplace or underplace themselves in a common priority ranking. Behavioral theory predicts that overconfident students are less likely to manipulate their preferences compared to underconfident students. We confirm this prediction, and we also document that this pattern leads to significantly more justified envy in the underconfident treatment condition. Moreover, we show that informing top students about their true priority rank significantly reduces their justified envy in the underconfident treatment condition. In contrast, this information significantly increases justified envy among non-top students in the overconfident condition. Surprisingly, making the assignment mechanism obviously strategy-proof by providing availability information in a dynamic implementation does not eliminate justified envy entirely.

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Appendix

Details of the behavioral models EBLA and RDP

Our version of the **expectation-based loss aversion** (EBLA) model is based on Meisner and von Wangenheim (2023) and Kőszegi and Rabin (2007), where students maximize

$$U^{EBLA}(r_i|\theta_i,\gamma_i) = \sum_{s\in S} p_{i,s}(r_i|b_i,\gamma_i)u^{EBLA}(s|r_i,\theta_i,\gamma_i), \text{ with}$$

$$u^{EBLA}(s|\cdot) = \sum_{x:\, v_{i,x}\leq v_{i,s}} p_{i,x}(r_i|\cdot)(v_{i,s} + (v_{i,s} - v_{i,x})) + \sum_{x:\, v_{i,x}>v_{i,s}} p_{i,x}(r_i|\cdot)(v_{i,s} + \lambda(v_{i,s} - v_{i,x})).$$

Here, $\lambda > 2$ captures a degree of loss aversion with respect to a reference school x, and all pairwise comparisons are weighted by the rational assignment probabilities. If λ is sufficiently large, a student can have preferences that violate first-order stochastic dominance. The classical model obtains when $\lambda = 0$.

The **ranking-dependent preferences** (RDP) model is based on Meisner (2023), where students maximize

$$U^{RDP}(r_i|\theta_i,\gamma_i) = \sum_{s\in S} p_{i,s}(r_i|b_i,\gamma_i)u^{RDP}(s|r_i,\theta_i,\gamma_i), \text{ with}$$

$$u^{RDP}(s|\cdot) = v_{i,s} + \rho(s|r_i),$$

and $\rho(s|r_i)$ is a function that is decreasing in the rank that ROL r_i assigns to school s. A strategic trade-off immediately arises when the ranking-dependent component for first-ranked options is larger than for second-ranked options so that the student would not want to rank first their most-preferred school first if an assignment is unlikely. The classical model obtains when ρ is a constant function.

Additional regressions

	Easy and Hard		Only Easy		Only Hard	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel C: Top and non-top						
Info-1	0.045	0.047	0.079	0.087*	0.011	0.009
	(0.036)	(0.035)	(0.049)	(0.047)	(0.049)	(0.047)
Info-2	-0.039	-0.035	-0.018	-0.008	-0.061	-0.058
	(0.029)	(0.029)	(0.035)	(0.032)	(0.040)	(0.042)
Constant	0.134***	0.200***	0.093***	0.177**	0.175***	0.219***
	(0.023)	(0.050)	(0.031)	(0.081)	(0.027)	(0.055)
Baseline	Info-0	Info-0	Info-0	Info-0	Info-0	Info-0
Controls	No	Yes	No	Yes	No	Yes
Observations	1680	1680	840	840	840	840
Postestimation Wald tests:						
H_0: Info-1 = Info-2	$p = 0.01$	$p = 0.01$	$p = 0.02$	$p = 0.02$	$p = 0.16$	$p = 0.19$

The dependent variable is Justified Envy $\in \{0, 1\}$. Estimation by random-effects regression. Standard errors clustered on group-level in parentheses. Controls are performance in baseline Raven test, gender, and the true ROL. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table 7: The effect of information on JE for all students.

Intensive margin regressions

	Info-0		Info-1		Info-2	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B: Random-effects model						
Hard	0.318**	0.315**	0.196	0.171	0.171*	0.162*
	(0.153)	(0.147)	(0.198)	(0.193)	(0.100)	(0.098)
Constant	0.179*	0.529*	0.407***	0.895***	0.121***	0.182
	(0.091)	(0.305)	(0.125)	(0.320)	(0.030)	(0.278)
Baseline	Easy	Easy	Easy	Easy	Easy	Easy
Controls	No	Yes	No	Yes	No	Yes
Observations	560	560	560	560	560	560
Panel D: Instrumental variable model						
Confidence	-0.187**	-0.187**	-0.112	-0.093	-0.116*	-0.107
	(0.091)	(0.091)	(0.110)	(0.103)	(0.070)	(0.070)
Constant	0.289***	0.791***	0.471***	1.083***	0.188***	0.390
	(0.074)	(0.257)	(0.094)	(0.313)	(0.042)	(0.330)
Controls	No	Yes	No	Yes	No	Yes
Observations	560	560	560	560	560	560

The dependent variable is the loss in payoffs due to Justified Envy $\in \{1, \ldots, 8\}$. Estimation by random-effects regression in Panel A and by two-stage least squares random-effects estimator in Panel B, where Believed rank is instrumented by Hard. Standard errors clustered on group-level in parentheses. Controls are performance in baseline Raven test, gender, and the true ROL. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table 8: The effect of confidence on payoffs by information condition.

	Easy and Hard		Only Easy		Only Hard	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Top students						
Info-1	-0.219	-0.132	0.075	0.153	-0.512	-0.450
	(0.311)	(0.298)	(0.332)	(0.342)	(0.516)	(0.512)
Info-2	-0.619***	-0.521**	-0.350	-0.234	-0.887**	-0.738*
	(0.216)	(0.209)	(0.223)	(0.248)	(0.350)	(0.419)
Constant	0.662***	1.577***	0.388*	1.342*	0.937***	1.714***
	(0.215)	(0.462)	(0.222)	(0.694)	(0.348)	(0.552)
Baseline	Info-0	Info-0	Info-0	Info-0	Info-0	Info-0
Controls	No	Yes	No	Yes	No	Yes
Observations	480	480	240	240	240	240
Postestimation Wald tests:						
H_0: Info-1 = Info-2	$p = 0.08$	$p = 0.08$	$p = 0.09$	$p = 0.07$	$p = 0.33$	$p = 0.38$
Panel B: Non-top students						
Info-1	0.322**	0.323**	0.290**	0.304***	0.355	0.355
	(0.134)	(0.131)	(0.113)	(0.109)	(0.227)	(0.228)
Info-2	0.065	0.065	0.060	0.069	0.070	0.052
	(0.093)	(0.091)	(0.057)	(0.050)	(0.160)	(0.159)
Constant	0.208***	0.345**	0.095**	0.255**	0.320***	0.372
	(0.061)	(0.163)	(0.040)	(0.105)	(0.103)	(0.303)
Baseline	Info-0	Info-0	Info-0	Info-0	Info-0	Info-0
Controls	No	Yes	No	Yes	No	Yes
Observations	1200	1200	600	600	600	600
Postestimation Wald tests:						
H_0: Info-1 = Info-2	$p = 0.06$	$p = 0.06$	$p = 0.04$	$p = 0.04$	$p = 0.23$	$p = 0.21$

The dependent variable is the loss in payoffs due to Justified Envy $\in \{1, \ldots, 8\}$. Estimation by random-effects regression. Standard errors clustered on group-level in parentheses. Controls are performance in baseline Raven test, gender, and the true ROL. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table 9: The effect of information on JE for top and non-top students (intensive margin).

Efficiency regressions

	Easy and Hard		Only Easy		Only Hard	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Top students						
Info-1	0.006	0.018	0.037	0.047	-0.025	-0.035
	(0.044)	(0.051)	(0.077)	(0.084)	(0.035)	(0.032)
Info-2	-0.056*	-0.042	-0.075	-0.064	-0.037	-0.025
	(0.029)	(0.033)	(0.051)	(0.072)	(0.028)	(0.025)
Constant	0.069**	0.181***	0.088*	0.231**	0.050*	0.088**
	(0.028)	(0.068)	(0.049)	(0.114)	(0.026)	(0.040)
Baseline	Info-0	Info-0	Info-0	Info-0	Info-0	Info-0
Controls	No	Yes	No	Yes	No	Yes
Observations	480	480	240	240	240	240
Postestimation Wald tests:						
H_0: Info-1 = Info-2	$p = 0.07$	$p = 0.08$	$p = 0.06$	$p = 0.03$	$p = 0.64$	$p = 0.66$
Panel B: Non-top students						
Info-1	0.067**	0.068**	0.095*	0.098**	0.040	0.039
	(0.031)	(0.030)	(0.050)	(0.047)	(0.036)	(0.036)
Info-2	0.002	0.002	0.005	0.006	0.000	-0.002
	(0.030)	(0.030)	(0.045)	(0.043)	(0.040)	(0.040)
Constant	0.108***	0.126***	0.095***	0.131**	0.120***	0.119**
	(0.018)	(0.036)	(0.030)	(0.052)	(0.022)	(0.046)
Baseline	Info-0	Info-0	Info-0	Info-0	Info-0	Info-0
Controls	No	Yes	No	Yes	No	Yes
Observations	1200	1200	600	600	600	600
Postestimation Wald tests:						
H_0: Info-1 = Info-2	$p = 0.06$	$p = 0.05$	$p = 0.09$	$p = 0.08$	$p = 0.36$	$p = 0.35$

The dependent variable is part of Pareto-improving Trading Cycle $\in \{0, 1\}$. Estimation by random-effects regression. Standard errors clustered on group-level in parentheses. Controls are performance in baseline Raven test, gender, and the true ROL. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table 10: The effect of information on efficiency for top and non-top students.

Gender regressions

	Info-0		Info-1		Info-2	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Random-effects model (Female Sample)						
Hard	0.121**	0.120**	0.078	0.074	0.030	0.028
	(0.059)	(0.056)	(0.077)	(0.077)	(0.059)	(0.062)
Constant	0.095**	0.229**	0.100***	0.161*	0.104***	-0.042
	(0.039)	(0.106)	(0.037)	(0.083)	(0.033)	(0.136)
Baseline	Easy	Easy	Easy	Easy	Easy	Easy
Controls	No	Yes	No	Yes	No	Yes
Observations	225	225	275	275	255	255
Panel B: Random-effects model (Male Sample)						
Hard	0.052	0.055	-0.050	-0.072	0.052	0.057
	(0.054)	(0.052)	(0.079)	(0.083)	(0.047)	(0.048)
Constant	0.091**	0.106	0.243***	0.420***	0.048***	0.120*
	(0.037)	(0.121)	(0.069)	(0.155)	(0.016)	(0.068)
Baseline	Easy	Easy	Easy	Easy	Easy	Easy
Controls	No	Yes	No	Yes	No	Yes
Observations	335	335	285	285	305	305
Panel C: Instrumental-variable model (Female Sample)						
Confidence	-0.060*	-0.059**	-0.043	-0.040	-0.032	-0.026
	(0.031)	(0.030)	(0.044)	(0.042)	(0.065)	(0.058)
Constant	0.127***	0.306***	0.103***	0.245*	0.111***	0.017
	(0.033)	(0.095)	(0.035)	(0.132)	(0.029)	(0.111)
Baseline	Easy	Easy	Easy	Easy	Easy	Easy
Controls	No	Yes	No	Yes	No	Yes
Observations	335	335	285	285	305	305
Panel D: Instrumental-variable model (Male Sample)						
Confidence	-0.036	-0.038	0.029	0.042	-0.026	-0.030
	(0.036)	(0.035)	(0.049)	(0.052)	(0.026)	(0.027)
Constant	0.114***	0.151	0.212***	0.378***	0.072***	0.170*
	(0.026)	(0.098)	(0.038)	(0.128)	(0.023)	(0.101)
Baseline	Easy	Easy	Easy	Easy	Easy	Easy
Controls	No	Yes	No	Yes	No	Yes
Observations	335	335	285	285	305	305

The dependent variable is justified envy, $JE \in \{0,1\}$. Estimation by random-effects regression. Standard errors clustered on group-level in parentheses. Controls are performance in baseline Raven test, and the true ROL. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table 11: The effect of confidence on JE by information condition (split by female and male sample).

	Easy and Hard		Only Easy		Only Hard	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Top students						
Info-1	-0.167*	-0.159	-0.175	-0.217	-0.160	-0.093
	(0.091)	(0.104)	(0.130)	(0.139)	(0.130)	(0.169)
Info-2	-0.202***	-0.200**	-0.160	-0.175	-0.230**	-0.145
	(0.078)	(0.080)	(0.130)	(0.127)	(0.106)	(0.143)
Constant	0.244***	0.259*	0.200	-0.016	0.280***	0.477***
	(0.074)	(0.134)	(0.127)	(0.126)	(0.095)	(0.136)
Baseline	Info-0	Info-0	Info-0	Info-0	Info-0	Info-0
Controls	No	Yes	No	Yes	No	Yes
Observations	205	205	110	110	95	95
Postestimation Wald tests:						
H_0: Info-1 = Info-2	$p = 0.54$	$p = 0.54$	$p = 0.69$	$p = 0.33$	$p = 0.48$	$p = 0.60$
Panel B: Non-top students						
Info-1	0.029	0.030	0.059	0.058	0.012	0.007
	(0.064)	(0.065)	(0.058)	(0.060)	(0.110)	(0.111)
Info-2	0.007	0.011	0.071	0.071	-0.050	-0.048
	(0.051)	(0.050)	(0.059)	(0.054)	(0.073)	(0.075)
Constant	0.139***	0.152*	0.071**	0.053	0.200***	0.221*
	(0.036)	(0.085)	(0.029)	(0.088)	(0.048)	(0.118)
Baseline	Info-0	Info-0	Info-0	Info-0	Info-0	Info-0
Controls	No	Yes	No	Yes	No	Yes
Observations	550	550	270	270	280	280
Postestimation Wald tests:						
H_0: Info-1 = Info-2	$p = 0.74$	$p = 0.76$	$p = 0.88$	$p = 0.86$	$p = 0.58$	$p = 0.61$

The dependent variable is Justified Envy $\in \{0, 1\}$. Estimation by random-effects regression. Standard errors clustered on group-level in parentheses. Controls are performance in baseline Raven test and the true ROL. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table 12: The effect of information on JE for top and non-top students (female sample).

	Easy and Hard		Only Easy		Only Hard	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Top students						
Info-1	-0.032	-0.061	0.117	0.048	-0.221*	-0.210
	(0.119)	(0.108)	(0.177)	(0.153)	(0.129)	(0.131)
Info-2	-0.206***	-0.182**	-0.183**	-0.180*	-0.238*	-0.228*
	(0.074)	(0.073)	(0.083)	(0.104)	(0.127)	(0.122)
Constant	0.217***	0.621***	0.183**	0.634**	0.255**	0.447
	(0.073)	(0.227)	(0.083)	(0.315)	(0.126)	(0.336)
Baseline	Info-0	Info-0	Info-0	Info-0	Info-0	Info-0
Controls	No	Yes	No	Yes	No	Yes
Observations	275	275	130	130	145	145
Postestimation Wald tests:						
H_0: Info-1 = Info-2	$p = 0.07$	$p = 0.09$	$p = 0.05$	$p = 0.05$	$p = 0.59$	$p = 0.74$
Panel B: Non-top students						
Info-1	0.164***	0.161***	0.177***	0.184***	0.149***	0.138**
	(0.039)	(0.038)	(0.059)	(0.058)	(0.054)	(0.056)
Info-2	0.039	0.034	0.017	0.016	0.064	0.049
	(0.041)	(0.038)	(0.023)	(0.018)	(0.079)	(0.075)
Constant	0.064***	0.090*	0.043***	0.131**	0.086**	0.055
	(0.020)	(0.049)	(0.015)	(0.052)	(0.036)	(0.087)
Baseline	Info-0	Info-0	Info-0	Info-0	Info-0	Info-0
Controls	No	Yes	No	Yes	No	Yes
Observations	650	650	330	330	320	320
Postestimation Wald tests:						
H_0: Info-1 = Info-2	$p = 0.01$	$p < 0.01$	$p < 0.01$	$p < 0.01$	$p = 0.30$	$p = 0.30$

The dependent variable is Justified Envy $\in \{0, 1\}$. Estimation by random-effects regression. Standard errors clustered on group-level in parentheses. Controls are performance in baseline Raven test and the true ROL. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table 13: The effect of information on JE for top and non-top students (male sample).

Instructions

Both translated and original instructions are available as an online appendix.