Title
Essays on Modeling and Identifying Cognitive Mistakes in Decision Making

Author
WANG, Ao

Publication Date
2022

Peer reviewed|Thesis/dissertation

Essays on Modeling and Identifying Cognitive Mistakes in Decision Making

by

Ao Wang

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Economics

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Stefano DellaVigna, Chair
Professor Christopher Walters
Professor Edward Augenblick

Summer 2022

Essays on Modeling and Identifying Cognitive Mistakes in Decision Making

Abstract

Essays on Modeling and Identifying Cognitive Mistakes in Decision Making

by

Ao Wang

Doctor of Philosophy in Economics

University of California, Berkeley

Professor Stefano DellaVigna, Chair

This dissertation studies modeling and identifying cognitive mistakes in decision making in the context of education system in China. Chapter 1 studies the cognitive distortion in complex school choice problems in Chinese centralized admission system. Chapter 2 studies the impact of education on students' ability of decision making. Chapter 3 studies the psychological impact of religious obligation on education attainment.

In Chapter 1 (coauthored with Shaoda Wang and Xiaoyang Ye), we empirically study an admission system that employs a constrained Deferred Acceptance Algorithm to understand how students construct their lists. Students appear overly cautious with their top choices and most of them do not always put safer choices at a lower-ranked spot on the list. We propose that the Model of Directed Cognition could explain such choices. Applicants using the model myopically focus on the spot they are contemplating and neglect its impact on the rest of the list. To differentiate from alternative hypotheses, we deploy an in-field experiment that pinpoints a core prediction of our model concerning framing effects and find clear evidence of it. Structural estimation suggests that 45%~55% of the sample are better described by our model and that this boundedly rational decision rule explains 83% of outcome inequality across socioeconomic groups.

Chapter 2 (coauthored with Binkai Chen and Wei Lin) intends to investigate the causal impact of collegiate economics courses on individual learning and decision-making under a development context. By exploiting a Chinese college-admission system that quasi-randomly assigns students to economics/business majors given students' preferences and the College Entrance Exam's cutoff scores for economics/business majors, we are able to isolate the treatment effects of an economics education on students' responses to a decision-making survey. Specifically, we compare the survey responses of students who narrowly meet the cutoffs for the economics/business majors to those who do not and find that students educated in economics/business courses are more likely to be risk neutral and less prone to common biases in probabilistic beliefs. While students in

economics/business majors do not show significant changes in social preferences, they appear more inclined to believe that others behave selfishly.

Chapter 3 (coauthored with Shaoda Wang and Xiaoyang Ye). We reports a field experiment that tests the effect of motivated cognition on information acquisition. When the high-stakes College Entrance Exam is held in the month of Ramadan, Chinese Muslim students not only underestimate the cost of fasting when uninformed, but further, misread clear empirical evidence of the cost, which we obtain by analyzing administrative data on past students' exam performance. Inspired by the theory of motivated cognition, we tackle this learning failure by randomly offering a subset of the students reading materials in which well-respected Muslim clerics explain that it is permissible to postpone the fast until after the exam. Students who receive the material are substantially less likely to misread our empirical analysis and more willing to postpone the fast.

The findings in this dissertation can deepen our understanding of the impact of psychological factor and cognitive limitation on decision making, particularly in the context of education.

Contents

List of Figures

List of Tables

Acknowledgments

First and foremost I would like to thank my advisor, Professor Stefano DellaVigna, for his continuous support and invaluable advice. I would not have tided over the pandemic if he did not walk and chat with me every time when progress had stalled. I would not have overcome self-doubt if he did not repeatedly encourage me before, during, and after every seminar presentation. I would not have had a fine draft for the job market if he did not meticulously read every version of the paper that I had sent to him. His guidance is indispensable for any progress I have made in every step of the doctoral study.

I would like to thank my committee member Professor Ned Augenblick for his role in my doctoral study and my life as a graduate student. He was always capable of conceptualizing an interesting phenomenon when exchanging ideas in phone calls. He was always able to help me articulate my own thoughts during casual conversations in a random cafe. He was always there when I needed help most, research or otherwise. His insight and intellectual curiosity is what motivates much of the effort, and what leads to much of the progress in my own research projects.

I benefit tremendously from my third committee member, Christopher Walters. He is extremely knowledgeable and resourceful. When I came to his office with a faint idea about my project four years ago, he was able to write down all the potential related works and advised me how to proceed. When I got stuck in econometric issues, he could always instantaneously come up with multiple sound solutions. It would not have been possible to complete my job market paper without his continuous advice, support, and encouragement.

While not on my dissertation committee, I would like to thank Professor Alex Rees-Jones for his advice and support throughout the entire graduate program. I have learnt a lot while working with him, and I have benefited from countless encouraging, productive, and stimulating conversations with him about my job market paper and other research projects. Without his help, it would not have been possible for me to interact with many researchers outside UC Berkeley who work in related areas.

I would also like to express my gratitude to Professor Dmitry Taubinsky, whose comments always struck me as sharp, refreshing and thought provoking and who had always been encouraging and supportive of my job market paper; to Professor Ted Miguel, whose expertise in survey design and the context of developing countries helped me nail issues in project implementation. I would also like to thank Professors Aaron Bodoh-Creed, Supreet Kaur, Kei Kawai, Patrick Kline, Jesse Rothstein, and many others for their help at various stages of my doctoral study.

The process of pursuing a doctoral degree is complex and unpredictable. I thank my co-authors, Binkai Chen, Wei Lin, Shaoda Wang, Xiaoyang Ye, without whom it would have been impossible to complete this dissertation. In particular, Shaoda and Wei have greatly influenced my way of thinking, and the research topics I have been working on. I am grateful for my parents and grandparents because I would not be who I am without their love, support, and sacrifice. In addition, I would like to thank all my peers and friends, including but certainly not limited to, Chaoran Yu, Yipei Zhang, Jimmy Narang, Zihao Zhou, Maximilian Muller, Alex Steiny, Arlen Guarin, Kaveh Danesh, Dong Wei, Yue Huang, Shelly He, Stephanie Bonds, Qihang Wu, Luisa Cefala, Maximiliano Lauletta, Nicholas Swanson, Woojin Kim, Yikun Jiang, Can Huang, and Yunbo Liu. I would also like to thank the J-PAL PPE Initiative, CEGA, and the National Social Science Foundation of China for generous funding that enables the implementation of the research projects in this dissertation.

Chapter 1

Cognitive Distortions in Complex Decisions: Evidence from Centralized College Admission

1.1 Introduction

Centralized admission systems play an important role in student-to-school matching around the world. In these systems, students are assigned to schools based on the outcome of a matching mechanism that accounts for students' reported preferences (typically called rank-order lists, or ROLs) and their priority scores. In most real-world mechanisms, determining the optimal ROL requires significant sophistication on the part of the student. It consists of multiple risk-reward tradeoff problems that require backward induction, contingent reasoning, and aggregating risk across choices, which applicants may have trouble grasping.[1] Failure to grasp the optimal strategy results in undesirable outcomes at later stages of education, which could ultimately influence career choice and economic mobility.[2]

We empirically investigate the consequences of sub-optimal application choices among college applicants in Ningxia, China. The centralized admission system in Ningxia employs a constrained Deferred Acceptance Algorithm[3] where eligible students can list up

[1]Chade and Smith (2006) and Shorrer (2019) theoretically characterize and develop algorithms for constructing the optimal portfolio in stylized settings of college admission, both of which invoke dynamic programming thinking that is similar to the logic of backward induction. Calsamiglia, Fu, and Güell (2020) show that backward induction solves computationally intractable problems for a wide class of mechanisms that are used in real college admission systems. Camerer et al. (1993) and Johnson et al. (2002) document failure of backward induction in extensive form games. Esponda and Vespa (2016) and Martínez-Marquina, Niederle, and Vespa (2019) document how uncertainty impedes contingent reasoning. Rabin and Weizsäcker (2009) document decision makers' general tendency to evaluate risk in isolation.

[2]Impacts of postsecondary education on labor market income have been documented in many countries, including Chile (Hastings, Neilson, and Zimmerman, 2013), China (Jia and Li, 2016), and the US (Chetty et al., 2020).

[3]As detailed in Section 1.2, this is also a serial dictatorship mechanism.

to four colleges from 239 first-tier colleges. The algorithm works by considering students' demand in order of their scores from the College Entrance Exam (CEE). When the algorithm reaches a given student, it considers the student's first-choice college and assigns the student to this choice if the admission quota of the college has not been filled. The algorithm checks the next choice only when the choice in question has already been filled by higher-scoring students, and repeats this process until the fourth choice. In practice, the student is relegated to a second-tier college outside of the 239 choices if all of the listed colleges have been filled.

Undoubtedly, any student needs to manage the risk of relegation to a second-tier college by listing at least one safe first-tier college on her ROL. The amount of risk that the student should take for the other three choices, however, is less obvious. Intuitively, the cost of not getting into one's first-choice college is less devastating than not getting into one's fourth choice - if the first choice is missed, the student can just move on to the next choice without worrying about a looming relegation. The optimal choice for any spot depends on the consequences of rejection, and the consequences of rejection depend on the schools listed lower on the ROL. This intuition prescribes, that rather than viewing each spot in isolation, the student should formulate a contingency plan to make the most of the entire portfolio by backward induction.

We obtained access to the administrative data on students' application lists. We find that, even with their first choices, 25% of the students choose a safe college – i.e., one that has an estimated unconditional acceptance probability larger than 86.2%,[4] suggesting that many students are not very selective in their first choices. Meanwhile, 55.3% of the students exhibit "risk-taking reversals", defined as ranking a less-selective college above a more-selective college on their ROLs, resembling evidence from many other contexts (Lucas and Mbiti, 2012; Ajayi, 2013; He, 2015; Rees-Jones, Shorrer, and Tergiman, 2020; Larroucau et al., 2021).

These behaviors correlate with demographics and contribute to inequality in admission outcomes. Students coming from disadvantaged areas are substantially more cautious in their first choices and are more likely to exhibit risk-taking reversals on their ROL. Conditional on priority scores, the most disadvantaged students on average end up in colleges whose selectivity, as measured by mean of cutoffs during 2014-2018, are 0.13 standard deviation lower than the most advantaged students.

To explain these empirical patterns, we propose that a boundedly rational decision rule, inspired by the Directed Cognition Model (henceforth the DC Model) (Gabaix et al., 2006), can naturally fit these patterns. In our context, the DC Model predicts that students focus their cognition entirely on the single spot they are contemplating (i.e., they choose a college to maximize improvement of expected utility for a portfolio that consists of that spot and a subjective, perhaps psychological, outside option) and ignore the impact of this choice on the rest of the ROL. This decision rule reduces a portfolio choice problem to repeated discrete choice problems, dispensing with backward induction, contingent reasoning, or the difficulty of aggregating risk across choices.

[4] The unconditional probability is the probability of meeting the admission cutoff of a college.

Because most centralized mechanisms share the feature that optimal risk-taking across different spots is interdependent, our hypothesis, if substantiated, could provide a descriptive model of suboptimal strategic behavior, and have general implications for the design of matching systems. However, testing decision optimality is difficult for this type of problem because applicants may have heterogeneous preferences (Agarwal and Somaini, 2018, 2019). This may be the reason that limited progress has been made on understanding whether decision-makers respond to such interdependency in a systematically suboptimal way[5]. We are able to perform this task with the help of an incentivized survey experiment, conducted among a subset of student applicants right after they have submitted ROLs. Additionally, when analyzing ROLs in the administrative data, the variations in assignment probability across individual students enable us to differentiate our hypothesis from various types of preference heterogeneity. To understand how the DC Model can be differentiated from alternative hypotheses with the help of the aforementioned data, we briefly discuss the four main predictions of the DC Model as well as their empirical support.

Our first prediction, labeled **"Top-Choice Cautiousness"**, says that, compared to the Rational Rule, the DC Model takes substantially less risk for their first choices. However, their fourth choices are similar to their rational counterparts in terms of risk-taking.

Our second prediction, labeled **"Risk-Taking Reversals"**, states that the DC Model is often more likely to rank riskier colleges at a lower position, at times generating dominated choices where the applicant ranks a lower-quality college higher on her list of choices.

While these two predictions seem to be in line with our previous observation from students' risk taking behavior in the administrative data, it is important to note that horizontal preference – i.e., preferences that do not completely align with competitiveness – may also contribute to the seemingly anomalous risk-taking behavior. To that end, the next two predictions play a key role in distinguishing our model from the alternative hypotheses.

Our third prediction, labeled **"Framing Effect"**, indicates that the DC Type takes more risks if the ROL problem is transformed to mathematically equivalent lottery formulation. We analyze the incentivized questions from the online survey to test this prediction. Estimates using our preferred specification suggest that 40.7% (SE=2.4%) of the students behave according to the predictions of the DC Type in our survey sample. Moreover,

[5]Significant progress has been made in understanding mistakes in the absence of strategic concerns and risk-taking consideration. Artemov, Che, and He (2017) posit that, in a strategy-proof environment, the mistakes on the lists are primarily inconsequential. Shorrer and Sóvágó (2018) find that, in Hungary, dominated choices in college applications are more likely to be made when expected cost is lower; they argue that multiple imperfections in decision-making may contribute to their findings. Hastings and Weinstein (2008) document substantial presence of information frictions in school choices, using randomized interventions. In a setting with strategic considerations, Rees-Jones, Shorrer, and Tergiman (2020) find that decision-makers neglect correlation in admission chances, even in settings where correlation is of first-order concern. Kapor, Neilson, and Zimmerman (2020) document mistaken beliefs using survey data in centralized system applications, using survey data. Dreyfuss, Heffetz, and Rabin (2019) proposes that Koszegi-Rabin expectation-based reference dependence may explain the dominated choice in Li (2017)'s experimental data.

echoing our findings in the administrative data, a 1 SD increase in our socioeconomic status index is associated with a 7.7% (SE=2.7%) decrease in propensity to be the DC Type.

Our fourth prediction, labeled **"Upward Movement"**, implies that, if the priority score for a rational type was to increase, any listed colleges will move down along the list. For DC decision-makers, however, any listed college would first move up along the list, and then move down, exhibiting an inverse-U shape as a function of the priority score. Data analysis reveals substantial presence of the non-monotonic movements that are predicted by the DC Model.

To quantify the impact of the DC Model, we structurally estimate a mixture model of college choices, where both DC and Rational Type coexist, using simulated method of moments. We are able to jointly identify preferences and the DC Type by exploiting how risk-taking behaviors in different positions of the list jointly respond to variation in priority scores, which causes a differential rate of change in assignment probabilities for different colleges.

The mixture model fits the data better than the single-type model, even when extra flexibility is corrected by BIC analogues, and it yields substantially better out-of-sample predictions relative to the single-type rational model. A closer look at the fit of a Rational Type-only model suggests that the flexibility of college preference in our model generates less cautiousness and fewer risk-taking reversals, echoing our first two predictions.

The estimated share of the DC Type is substantial, and ranges from 45.1% (SE=0.54%) to 55.1% (SE=0.55%). The estimates are comparable to the share of the DC Type estimated from the survey experiment. Moreover, a 1 SD increase in the socioeconomic index is associated with a decrease that ranges from 3.68% (SE=0.56%) to 6.02% (SE=0.55%) in the share of the DC Type. In a counterfactual scenario where all students act optimally with respect to the mechanism, conditional on priority scores, the outcome gap between the most disadvantaged and advantaged quarter of the sample shrinks by 83.15%, suggesting that behavioral bias is the primary factor that explains the less desirable outcomes among high-achieving disadvantaged.

The DC decision rule also has adverse impact on overall efficiency. The de-biasing intervention is predicted to induce substantial welfare gain among behavioral applicants, which is on average larger than an increase of roughly 0.25 s.d. in the test scores under the old equilibrium. On the other hand, switching to either an unlimited list or a Boston mechanism without de-biasing, while intuitively making the bias less relevant, decreases welfare overall, echoing Chen and Kesten (2017)'s theoretical findings on the benefit of China's parallel mechanisms.

Our paper adds to the literature on behavioral mechanism design (Hassidim, Romm, and Shorrer, 2016; Li, 2017; Rees-Jones and Skowronek, 2018; Dreyfuss, Heffetz, and Rabin, 2019). Our results suggest that decision-makers may fail to achieve optimality even if they recognize the gain of being strategic, an important difficulty discussed by Pathak and Sönmez (2008). We show that, in the presence of choice interdependence, a specific decision heuristic that neglects such a connection can better explain participants' strategies. Our analysis demonstrates the benefits of structural modeling in the analysis of behavioral agents (DellaVigna, 2018).

The paper also is closely related to the recent surge of studies on empirical student-school matching. Song, Tomoeda, and Xia (2020), an important and closely related paper, shows that full rationality with no aggregate uncertainty is incompatible with admission outcome data in China. To tackle aggregate uncertainty, a key component of decision-making in centralized systems, as well as to provide evidence on the specific suboptimal strategy employed in our setting, our paper is similar to papers that employ both rank-order lists and survey data to test for optimal strategic play (De Haan et al., 2015; Kapor, Neilson, and Zimmerman, 2020). Our paper further demonstrates that behavioral decision rules can be easily incorporated into the framework of revealed preference analysis laid out in Agarwal and Somaini (2018, 2019), and can help unmask the mechanisms behind the choice patterns of high-achieving disadvantaged students (Hoxby and Avery, 2012). Our findings suggest that certain cognitive limitations create gaps in admission outcomes among applicants of the same academic ability; thus, our work is related to the literature on the distributional consequences of behavioral biases (Campbell, 2016; Bhargava, Loewenstein, and Sydnor, 2017; Allcott, Lockwood, and Taubinsky, 2019; Rees-Jones and Taubinsky, 2020). While the underlying mechanism is different, this also echoes studies that cover the distributional impact of school choices in decentralized systems (Walters, 2018).

The paper proceeds as follows. Section 1.2 introduces the empirical setting and data sources. Section 1.3 lays out the problem mathematically and discusses both the Rational Rule and the DC Decision Rule. Section 1.4 presents the evidence concerning Cautiousness and Risk-Taking Reversals. Section 1.5 tests Framing Effects using the survey experiment data. Section 1.6 tests Upward Movement using the administrative data. Section 1.7 presents results from the structural estimation. Section 1.8 concludes.

1.2 Empirical Setting

Summary of Timeline

Figure A.1 presents the timeline of the admission procedure[6]. Student applicants are required to take the College Entrance Exam (CEE), a nationwide exam that takes place less than one month before the start of college admissions. As elaborated in Appendix A.5, the exam performance determines students' priority scores in the admission system and thus has a predominant impact on students' application strategies.

When students are notified of their test scores and corresponding provincial rankings, the online college application system opens. At that time, students know whether their score meets the minimum requirement to apply for schools in the 1st-tier college category, which is set by Ningxia Provincial Education Authorities. It has remained quite stable in terms of rankings over time[7].

[6]The timeline in 2020 is different from the years before 2020 because of COVID-19. In 2020 the exam as well as all the related admissions activities were postponed by exactly one month.

[7]See Appendix A.5 for additional details on college category.

The application process is time-constrained and cognitively demanding. Students need to select four colleges from 239 colleges, but have few opportunity to learn about this system by trial and error before submitting their final decisions, despite the novelty of this decision environment. Anecdotal evidence suggests that misunderstanding is not rare. According to several college application advisory platforms on the Chinese Internet, one of the most common mistakes is to treat the four spots on the ROL as separate and equal, effectively ignoring the order in which the ROL is processed[8].

Admission Rule

After the deadline for ROL submission, the centralized admission system assigns students to colleges using a deferred acceptance algorithm based on their priority score and the colleges' preannounced admission quotas. Since the priority score for each student is the same for all colleges, the mechanism is effectively a serial dictatorship mechanism, where colleges only need to specify a priority score cutoff to decide which applicants are admitted, regardless of a college's position on the student's submitted ROL. For a student applicant, she knows her priority score and ranking when she applies, and past cutoff scores are publicly available. The only uncertainty comes from the cutoffs of the current year.

Table A.1 presents examples of how cutoffs determine admission outcomes. In Example 1, the student is admitted to college A because her score exceeds A's cutoff. The system ignores all her lower-ranked choices. In Example 2, the student is admitted to college B because her score does not meet A's cutoff but exceeds B's cutoff. As a result, the system assigns her to B, ignoring C and D. In Example 3, the student is unassigned because she does not meet the cutoff of any college she listed. In Example 4, the student is assigned to college D because she does not meet the cutoffs of A,B, and C but meets the cutoff of D.

After assignment of first-tier colleges has been completed, students are notified of the admission decision within a month. Students who are not admitted to any first-tier colleges will be passed on to the next stage of admission, where the centralized system will assign them to lower-tier colleges using the same priority score and similar algorithms [9].

Data

The first dataset is the administrative data generated by the centralized admission system that records the application behavior of students from 2014 to 2018. This dataset is maintained by the Ningxia Provincial Education Authorities. The second dataset is an online survey experiment that we set up in 2020. It also targets CEE takers in Ningxia applying to first-tier colleges, the same group of students we analyze in the administrative dataset.

[8]See here for an example of comments about mistakes in college applications (in Chinese).

[9]Students can rank four second-tier colleges and four third-tier colleges

(I) Administrative Data We obtained access to administrative data from 2014 to 2018, covering around 9,000 first-tier eligible science track students in each admission cycle. The number is considerably lower for humanities track students, amounting to roughly 2,000. The data contains students' CEE scores, ROLs, admission outcomes, and some demographic information, including the county, city and street of their residence address (available only in 2015, 2017, 2018).

(II) Online Survey In August 2020, we carried out an online survey that targeted high school students who had applied for first-tier colleges in 2020. Four local high schools actively encouraged students to take our survey. As a result, while any first-tier eligible applicants in Ningxia could respond to our online survey, our sample mainly consists of students from these four high schools. We were able to collect 1,412 complete and effective responses, roughly 15% of the total number of first-tier college applicants in 2020. As shown in Figure A.1, the survey was conducted right after students submitted their ROL for first-tier colleges, but before they were notified of the admission outcomes[10]. The survey consisted of three parts. The first part elicited basic information, such as their final ROL, high school, gender, age, parents' education and occupation, CEE score, source of application advice, and preferences over college characteristics. The second part elicited their beliefs about the unconditional admission probability[11] of the colleges on their ROL (0% $\sim$ 100%) and how satisfied they would feel (0-100) if they were admitted to a particular college. The third part consisted of several incentivized risk-taking questions that were presented in the form of a ROL and lottery, which are discussed in detail in Section 1.5.

1.3 Decision Problem

Setup and Mathematical Notations

To mathematically describe the decision problem in our setting, consider a student, Mei, who needs to list four colleges from a set of n colleges on her ROL. After she submits her ROL, along with other students, the centralized admission system will process their ROLs using the Constrained Deferred Acceptance Algorithm (DAA). Then, Mei either will be assigned to one of the four colleges that she listed on her ROL, or will be rejected by all four colleges and end up with her outside option. We assume that the number of students m is much larger than the number of colleges, n.

[10]We choose this particular timing for three reasons. First, in order to best approximate students' information set, the survey had to take place after students had been notified of their score and had spent time researching colleges. Second, high school officials believed that the survey might distract some students from the high-stakes and time-sensitive application process, so we postponed the survey until after the college application deadline.

[11]The question asked students to guess the probability of meeting the admission cutoff of the college in question. Based on feedback from teachers and students in a pilot, admission cutoff is a very basic concept. The most natural way to elicit beliefs about admission probability was to ask students about their belief that their scores would meet the admission cutoffs.

Mei is playing an incomplete information game[12], where she does not know the ROLs submitted by other students and has to form beliefs about what the other lists could be. As discussed in Section 1.2, because students have been notified of their scores and corresponding provincial rankings when they apply, the admission outcomes solely depend on the cutoffs of the colleges that they apply for, unknown at the time of list submission. Hence, instead of thinking about others' lists, Mei only needs to form her beliefs about the distribution of cutoffs.

Based on beliefs about the distribution of cutoffs, Mei will assign probabilities of meeting the cutoffs of the n colleges that she is contemplating. Denote the unconditional assignment probabilities (i.e., probability of meeting the cutoff) of the n colleges by p_1,p_2,...,p_n respectively. For the purpose of presentation and without loss of generality, assume that $p_1 < p_2 < ... < p_n$ (that is, colleges are ranked from the most competitive ones to the least). Denote the utility of admission to colleges by u_1, u_2, ..., u_n respectively. The utility of Mei's outside option is $\underline{u}$. For any single college j, the only two characteristics that Mei needs to care about are its admission utility u_j and unconditional admission probability p_j.

Estimate Probability p_j from Data

As discussed in the previous subsection, one of the two components of this decision problem is the unconditional probabilities p_j, for which we need to construct measures to approximate what students think. To that end, we proxy students' beliefs using admission cutoffs in the past, which are publicly available shortly after the end of each previous admission cycle[13].

The cutoffs in past years can reliably predict the current cutoffs. In Figure 1.1, we plot the cutoff in its converted form in a specific year (e.g., 2018) against the cutoff the previous year (e.g., 2017) for all the colleges that admitted Ningxia students during this time period. We find that the correlation between a cutoff and its past year counterpart is around 0.95; it would be even higher, except that a few outliers significantly drag the correlation down. The median of the distance between realized cutoffs and the college-level average during 2014-2018 is 0.097 of a standard deviation of priority scores among science-track applicants, and 0.146 of a standard deviation of the distribution among humanity-track applicants. Importantly, the uncertainty is larger among less competitive colleges, as the scatter at the bottom left of each graph is more likely to be away from the 45-degree line.

To calculate probabilities, we assume that for college j, in year t, the cutoff c_{jt} is normally distributed:

$$c_{jt} \sim N(\mu_j, \sigma(\mu_j))$$

[12]This treatment is conventional in this literature. Examples include Agarwal and Somaini (2018); Kapor, Neilson, and Zimmerman (2020); Calsamiglia, Fu, and Güell (2020).

[13]See here for a well-known website that documents past admission cutoffs.

where the value of μ_j is the average of admission cutoffs for college j during 2014-2018, which reflects the overall competitiveness of a college across years. The assumptions about the value of μ_j are motivated by the informativeness of past cutoffs. We compute the distribution of $c_{jt} - \mu_j$ in Figure A.2a, and find that the distribution function is a bell-shaped function that is centered around zero, with reasonably thin tails, suggesting that it is possible to approximate the true distribution with a hybrid of normal distributions.

Based on the observation from Section 1.1 that the predictability is heterogeneous across colleges of different competitiveness, we assume that the mean of $ln(\sigma_j)$ is a fourth-order polynomial of μ_j:

$$E[ln(\sigma_j)|\mu_j] = \beta_0 + \sum_{k=1}^{4} \beta_k \mu_j^k$$

We estimate the model using maximum likelihood, for the science and humanity tracks, respectively. We then use the estimated $\hat{\beta}$ to predict σ_j:

$$\hat{\sigma_j} \equiv exp(\hat{\beta_0} + \sum_{k=1}^{4} \hat{\beta_k} \mu_j^k)$$

With the estimation, the probability of meeting the cutoff of college j is:

$$\hat{p}_{ij} \equiv \Phi(\frac{s_i - \mu_j}{\hat{\sigma_j}})$$

where $\Phi(.)$ is the CDF of standard Gaussian.

We follow Kapor, Neilson, and Zimmerman (2020) to validate the estimates of admission probabilities. Specifically, we analyze individual level data on admission outcomes by running the following regression:

$$1(\text{Admitted to First Choices})_i = \alpha_1 + \beta_1 \hat{p}_{ij}$$

where subscript j represents students' first choices. The null hypothesis that the estimated admission probability is accurate implies that $\alpha_1 = 0$ and $\beta_1 = 1$.

The estimation results suggest that our estimates accurately reflect the actual unconditional admission probability. As shown in Columns (1) and (2) of Table A.2, $\hat{\alpha_1} = -0.0043$ (SE=0.0024) and -0.0050 (SE=0.0040) for the science and humanity tracks, respectively, and $\hat{\beta_1} = 1.0015$ (SE=0.0043) and 1.0117 (SE=0.0081) for the science and humanity tracks, respectively. The p-values of the F-tests are 0.074 and 0.328, respectively, meaning that we fail to reject the null hypothesis. In Figure A.2b, we divide colleges into four groups of equal size according to their competitiveness and plot the kernel density estimation of the distribution of $c_{jt} - \mu_j$ for each group respectively. The figure shows that our model can well approximate such empirical patterns.

The Optimal Rank-Order List

Given the beliefs about the unconditional admission probability of colleges, as well as the utility of admission for each college, playing Bayesian Nash Equilibrium in this context reduces to finding out the optimal portfolio for Mei. Specifically, suppose that Mei's ROL is (j_1, j_2, j_3, j_4), and the utilities and probabilities are (u_{j_1}, p_{j_1}), (u_{j_2}, p_{j_2}), (u_{j_3}, p_{j_3}), (u_{j_4}, p_{j_4}), respectively. Given her list, she will be admitted to college j_1 with probability p_{j_1}. Under constrained DAA, she will be considered by college j_2 only when college j_1 has rejected her; thus, the probability of being admitted to college j_2 is $(1 - p_{j_1})p_{j_2}$[14]. Similarly, the probabilities of being admitted to colleges j_3 and j_4 are $(1-p_{j_1})(1-p_{j_2})p_{j_3}$ and $(1-p_{j_1})(1-p_{j_2})(1-p_{j_3})p_{j_4}$, respectively. Her expected utility from the portfolio $\{j_1, j_2, j_3, j_4\}$ is:

$$\begin{aligned} EU([j_1, j_2, j_3, j_4]) \equiv p_{j_1}u_{j_1} + (1 - p_{j_1})p_{j_2}u_{j_2} + (1 - p_{j_1})(1 - p_{j_2})p_{j_3}u_{j_3} + (1 - p_{j_1}) \\ (1 - p_{j_2})(1 - p_{j_3})p_{j_4}u_{j_4} + (1 - p_{j_1})(1 - p_{j_2})(1 - p_{j_3})(1 - p_{j_4})\underline{u} \end{aligned} \tag{1.1}$$

Considering joint assignment probabilities for a group of colleges at the same time substantially complicates the decision problem because the chance of admission at one college depends on the chance of admission at the colleges that the student has ranked above it. This interdependence implies that decisions should not be made by considering each program sequentially, viewed in isolation. Instead, Mei should consider admissions probabilities arising from a complete ROL, and thus optimal decision-making requires picking an optimal portfolio out of a large number[15].

To understand what an optimal portfolio should look like, suppose Mei's best list is $[a^*, b^*, c^*, d^*]$. The risk taking behavior depends on Mei's preference profile

Vertical Preferences In case of vertical preferences (i.e. higher risk is associated with higher desirability), we know that $p_{a^*} < p_{b^*} < p_{c^*} < p_{d^*}$, a qualitative prediction that we can directly test using the administrative data alone. The optimal amount of risk to take is different across positions. For example, p_{d^*} needs to maximize $p_d u_d + (1 - p_d)\underline{u}$, whereas p_{a^*} needs to maximize $p_a u_a + (1 - p_a)EU([b, c, d])$. The fact that $EU([b, c, d]) > EU(\emptyset) = \underline{u}$ implies that Mei needs to worry less about the downside of missing the risky college she pursues (i.e., the utility that follows $(1 - p)$ in each expression). Consequently, it is the colleges that the student ranks below the current choice, not the colleges ranked above it, that matter most for the optimal choices. Hence, backward induction, a decision rule unnatural to human cognition, becomes useful in this process:

- (blank) $\longrightarrow$ (blank) $\longrightarrow$ (blank) $\longrightarrow$ d $\longrightarrow$ (outside option)

[14]Here we are assuming that admission probability is independent because we believe that this is a reasonable approximation in our empirical setting. See Section A.5 for a detailed discussion of why this assumption is justified.

[15]To be precise, the number is $\binom{n}{4}$, which amounts to 4 billion in our context.

- (blank) $\rightarrow$ (blank) $\rightarrow$ c $\rightarrow$ d $\rightarrow$ (outside option)
- (blank) $\rightarrow$ b $\rightarrow$ c $\rightarrow$ d $\rightarrow$ (outside option)
- a $\rightarrow$ b $\rightarrow$ c $\rightarrow$ d $\rightarrow$ (outside option)

Horizontal Preferences In this case, we can no longer deduce $p_{a^*} < p_{b^*} < p_{c^*} < p_{d^*}$ from $u_{a^*} > u_{b^*} > u_{c^*} > u_{d^*}$, because, if any pair of colleges j_1 and j_2 that reflect Mei's horizontal preference are both present on the list, the more competitive one, which is less desirable in terms of Mei's preference, would be ranked lower.

Optimal Decision Rule Regardless of Preferences Chade and Smith (2006) find that a decision rule, Marginal Improvement Algorithm, can achieve the global optimum by selecting one college at a time. In each step, the optimum depends on the colleges that have already been included in the portfolio in previous steps. The procedure coincides with backward induction in the case of strong vertical preferences, while the mapping between step number and list position becomes more complicated for other preference profiles. The commonality, however, is that the decision needs to be converted into a dynamic problem where the current choice is interrelated with the choices in the past steps.

Formulation of the Directed Cognition Model

The rational benchmark in Section 1.3 proposes that the optimal portfolio can be reached by decomposing the problem into four different discrete choice problems. The correct decomposition of this problem requires student applicants to appreciate the interdependence between choices, because it is the colleges that they list below a given rank, not those above that rank, that affect the optimal choice for the given rank. Literature in experimental economics, however, has established that, even in simplified settings, subjects have trouble grasping the concept of backward induction (Camerer et al., 1993; Johnson et al., 2002). Moreover, laboratory evidence suggests that decision-makers lack the ability to cope with uncertainty in simple decisions (Martínez-Marquina, Niederle, and Vespa, 2019), a skill that is necessary to assess the distribution of utility for the lower-ranked choices.

In this subsection, we continue to use the setting in Section 1.3, and introduce an alternative boundedly rational decision rule that is inspired by the Model of Directed Cognition in Gabaix et al. (2006) (the DC Model). We believe that this decision rule explains students' suboptimal strategies when they cannot apply the optimal strategy as prescribed in Chade and Smith (2006). The rule prescribes that, instead of tracking all the information and acting optimally upon it, another student applicant, Hua, due to cognitive limitations, myopically focuses on the spot where he is actively contemplating which college to fill in, and neglects the impact of that choice on the rest of the list. He fills out his ROL in a natural order, from the first choice to the fourth choice. Mathematically, in step i, Hua maximizes

$$p_i u_i + (1 - p_i)\underline{u} \tag{1.2}$$

As a result, in each step Hua is making choices for essentially the *same* decision problem. That is, he maximizes the expected utility of a portfolio that consists of his current choice and the perceived outside option, as described graphically below:

- **Step 1**: j_1 → (blank) → (blank) → (blank) → (Outside Option)
- **Step 2**: j_1 → j_2 → (blank) → (blank) → (Outside Option)
- **Step 3**: j_1 → j_2 → j_3 → (blank) → (Outside Option)
- **Step 4**: j_1 → j_2 → j_3 → j_4 → (Outside Option)

This decision rule requires less cognitive capacity for two reasons. First, Hua is proceeding in a natural order by considering the first choices before the rest of the ROL. Second, because Hua is making choices for each spot in isolation, the source of uncertainty is reduced and he only needs to consider at most two states: admission to a first-tier school, or rejection by all four of his choices and getting the utility of the outside option.

1.4 Summary Statistics on Risk-Taking Behavior

Section 1.3 and 1.3 have introduced both the Rational Rule and the DC Rule. They yield different predictions regarding basic risk taking behavior on the list, which are discussed in Section 1.4. We present data analysis that supports the presence of the DC Rule in Section 1.4 and Section 1.4.

An important feature of our setting is that some students who barely meet the minimum requirement of first-tier colleges have limited options because of their low priority score. Therefore, mechanically they are much more risk-taking than those whose priority score is above the minimum by a comfortable margin. We therefore focus on the top 60% in this and subsequent sections[16].

Predictions: Cautiousness and Risk-Taking Reversal

Compared to the rational decision rule, the DC decision rule yields substantially different predictions about the patterns of risk taking. To mathematically describe the differences between the rational benchmark and the DC Rule, consider Hua, who is

[16]Figure A.3a presents the bin-scatter plot of the mean probability for the four choices conditional on the quantile of priority score. We can see from the figure that, for the bottom 5% in terms of priority score, even the mean probability of the fourth choice is less than 20%, as these students really don't have any safe choices. The mean probability for all the four choices rises simultaneously, with this trend stopping when the priority score quantile is around 40%.

choosing between two colleges, a or b, to list as his choice in the qth spot. College a is more desirable and riskier than b, thus $p_a < p_b$. Hua needs to compare $U_a \equiv p_a u_a + (1 - p_a) PF_q$, the expected utility of choosing a, to $U_b \equiv p_b u_b + (1 - p_b) PF_q$, where PF_q is the perceived expected utility of a portfolio that consists of all the choices below the qth choice, including the outside option. Define the propensity to take risk as a function of spot position (i.e. the qth choice) and the decision rule (Optimal or DC)

$$f(q, \text{decision rule}) \equiv U_a - U_b = p_a u_a - p_b u_b + (p_b - p_a) PF_q$$

Here, the greater $U_a - U_b$ is, the more appealing it is to take risks and choose a.

[Top-Choice Cautiousness] For any choice $q < 4$, the Rational Rule takes more risks in listing their first choice than the DC Rule. Moreover, the gap in risk-taking between the Rational and the DC Type is decreasing in q. .

This prediction holds because Mei can correctly calculate the expected value as $PF_1 > PF_2 > PF_3 > PF_4 = \underline{u}$, whereas Hua ignores the rest of the portfolio and makes decisions as if $PF_1 = PF_2 = PF_3 = PF_4 = \underline{u}$. Mei's cautiousness is as great as Hua's in the fourth spot. As $U_a - U_b$ is increasing in the expected utility of the backup list, Mei is more inclined than Hua to make a risky move for higher-ranked spots. The risk-taking gap between the DC Rule and the Rational Rule will be maximal for the first spot.

Under the assumption of vertical preferences, the DC decision rule also has implications for the order of admission probability for the listed colleges:

[Risk-Taking Reversals] Under vertical preferences, the ROL of the Rational Type always features decreasing utility and increasing admission probability (that is, $u_a > u_b > u_c > u_d$, $p_a < p_b < p_c < p_d$). A DC Type, in contrast, may exhibit "risk-taking reversal" by putting a riskier college in a lower-ranked position, leading to dominated choices.

This prediction holds because, for Mei, riskier colleges should also be put in higher-ranked positions. For Hua, however, $f(i, \text{Rational}) = f(j, \text{Rational})$ because only the outside option is regarded as his backup list. For example, Hua may list c before d, not because c is more desirable, but because its probability is higher: $p_c u_c + (1 - p_c)\underline{u} > p_d u_d + (1 - p_d)\underline{u}$.

Summary Statistics on Admission Probability

Distribution of Admission Probability for the Four Choices Table 1.1, Panel A presents the summary statistics of unconditional admission probability that we construct as described in Section 1.3, for each choice on the lists. The mean probability for the first and fourth choices is 46.91% and 91.01% respectively, consistent with the prediction from the rational benchmark that students should be pursuing more risks for top choices.

However, substantial share of students are not taking risks in their first choices, as the 75th percentile of probability is 86.17%. On the other hand, the heterogeneity of probability is minimal for the fourth choices, where the 25th percentile is 95.66%, suggesting that the vast majority of students are taking little risk for their bottom choices, which makes sense in a high-stakes environment.

Share of "Risk-Taking Reversal" The rational decision rule and vertical preferences jointly predict that the probability should be lower for the higher-ranked choices. To quantify students' strategy in this dimension, we construct risk-taking reversal, namely, a "flip" of the probability of colleges, to quantify the violation of benchmark prediction. As our goal is to capture risk-taking reversal anywhere on the ROL, we consider the following statistics:

$$R \equiv max_{j>i}\{p_i - p_j\}$$

In the expression of R, we take the maximum for the probability gap between any pair of choices to capture the most serious risk-taking reversals on the ROLs. We report the results in Table 1.2, Panel A, and find that 55.3% of the ROLs exhibit risk-taking reversals ($R > 0\%$). Further limiting our scope to the case of "serious" reversals, where an ROL is counted only when R exceeds a certain positive threshold ($R\% > 25\%$,$R\% > 50\%$,$R\% > 75\%$), the share mechanically decreases but remains non-negligible. For example, the share is 24.35% when the restriction is $R\% > 25\%$.

In summary, the data suggests that a substantial proportion of students are quite cautious even for their first choices, and many of them exhibit "risk-taking reversals" on their lists. This clearly rejects the joint hypothesis that students have perfect vertical preferences and are following the rational benchmark.

Socioeconomically Disadvantaged Exhibit More Top-Choice Cautiousness and Risk-Taking Reversals

A large literature documents that socioeconomically disadvantaged students are worse at strategizing in centralized systems (Lucas and Mbiti, 2012; Ajayi, 2013; De Haan et al., 2015; Shorrer and Sóvágó, 2018; Kapor, Neilson, and Zimmerman, 2020). We choose average educational attainment at township level to approximate socioeconomic status[17]. We match the township level educational attainment data to individual students in the administrative dataset, and plot the distribution of this measure in Figure A.4. We split students into four groups according to their SES, and focus on the most advantaged quartile and the most disadvantaged quartile. Because Ningxia accounts for only about 0.7% of China's area, and all but two first-tier colleges are located far outside the province, the difference in township should not significantly alter geographic proximity.

In Figure 1.2a, we compute the statistics in Section 1.4 for the most advantaged and the most disadvantaged quartile, respectively. The mean probability of the first choices among the socioeconomically disadvantaged students (53.8%) is less than their advan-

[17]Roughly speaking, township is equivalent to zip code in the US. We obtain statistics on average years of education among adults between 40-65 at township level from the China Census in 2010, as well as access to students' home addresses in 2015, 2017, and 2018.

taged counterparts (45.7%). However, for their fourth choices, the probability among the disadvantaged (91.3%) is slightly less than their advantaged counterparts (92.9%) [18].

We run the following regression to quantify the difference statistically:

$$\text{Outcome} = \beta\text{Disadv} + f(\text{Priority Score}) + \text{Disadv} * g(\text{Priority Score}) + \text{controls} \qquad (1.3)$$

where Disadv indicates whether students are from a lower SES group, and f(Priority Score) and g(Priority Score) represent a fourth-order polynomial of priority score[19]. The main effect of Disadv, β, is the overall outcome gap between students of different SES groups after priority score has been fully controlled, as well as the heterogeneity in whether the outcome gap changes with the priority score.

As shown in Table 1.1, Panel B, the results confirm our visual perception regarding the first and fourth choices. After a full set of controls is introduced, the gap between the advantaged and the disadvantaged with regard to fourth-first choice probability differences amounts to 8.98% (SE 0.78%). Panel C demonstrates that the gap is robust to priority score.

Figure 1.2b plots the share of reversals for the most advantaged and disadvantaged quartiles, respectively. The gap in the share of reversals between disadvantaged and advantaged remains about the same, and is robust to the threshold. When the threshold X% is 25%, for example, the share of reversals among the advantaged is 19.7%, and the weighted share of reversals among the disadvantaged is 26.1%, roughly 40% higher than the advantaged.

In Table 1.2 we plug in “share of reversals" as the outcome variable in Equation 1.3. In each column, we vary the threshold X so that it equals 0, 25, 50, or 75% in Columns 1, 2, 3, and 4, respectively. As reported in Panel B, the results are consistent with the graphical observation, with the gap in the share of reversal remaining at about 5%; as Panel C shows, the results are robust to the level of priority score.

To examine whether the admission outcomes are worse among the disadvantaged, we run the following regressions:

$$\text{Selectivity of Admitting College} = 1(\text{SES Quartile}) + f(\text{Priority Score}) + \text{Controls}$$

where we measure the selectivity of the admitting college by calculating the average admission cutoffs for the colleges during 2014-2018. We report the regression results in Table A.3, Panel A. The results in Column 1, for example, suggest that the most disadvantaged quartile (1st Quartile) on average end up in colleges whose selectivity is 0.1288 (SE=0.0082) of a standard deviation worse compared to the most advantaged quartile (4th Quartile).

[18] All the statistics have been reweighted to take into account any differences in priority score. Please refer to Figure A.3b for a complete breakdown of admission probability by priority score, across students in the most advantaged and most disadvantaged quartiles.

[19] Each of the terms in the polynomial has been demeaned so that the main effect β is the predicted Adv-Disadv Gap at average level of priority score.

1.5 Survey Experiment: Testing Framing Effect

The DC Model generates more first-choice cautiousness and risk-taking reversals. Evidence in Section 1.4 seems to indicate that the risk-taking behavior of many applicants is in line with what is predicted by the DC Model, and that the DC Type may be more prevalent among the disadvantaged. However, risk-taking behavior can also be affected by horizontal preferences, information frictions, or subjective beliefs.

To tackle these issues, we design a survey experiment in which students make college and lottery choices. The monetary incentive and risk of these hypothetical colleges are designed to test the predictions of the DC Rule that are not susceptible to college preferences, beliefs about assignment probability, or information frictions in real college choices.

Prediction: Framing Effect under Arbitrary Preferences

Consider what leads to a DC Type's suboptimal strategy. The correct utility of choosing college a for the rth choice is

$$U_a \equiv p_a u_a + (1 - p_a)U_r$$

where U_r is the expected utility of the portfolio that consists of everything below the rth choice on the list. A DC Type's trouble is that, when the question is presented in a ROL, they fail to calculate U_r and instead treat it as u_0. When this is not presented in the form of a ROL question, but in the form of the mathematically equivalent lottery choice $(p_a, u_a; (1 - p_a), U_r)$, the payoff in the event of rejection has been calculated and presented clearly so that the DC Type cannot distort it. Effectively, choices presented in the form of a lottery can "de-biased" by bringing the utility of backup choices to the decision-makers' attention so that their cognition is no longer directed to a single spot. Let U'_r denote the perceived expected utility of the portfolio that consists of everything below the rth choice. Mathematically, this prediction holds because, for the Rational Type, $U'_r = U_r$, regardless of whether the problem is presented in lottery representation or ROL representation. For the DC Type, $U'_r = U_r$ if it is presented in lottery representation, but $U'_r = u_0 < U_r$ if it is presented in the ROL representation. Hence, we have the following prediction:

[Framing Effect] Suppose all the possible portfolios that are framed as ROL have been correctly transformed into their lottery representation. A Rational Type will behave consistently across ROL and lottery questions. By contrast, the DC Type will be more risk-taking in the lottery questions.

Design of the Survey Experiment

The core of the survey experiment consists of three groups of incentivized questions. The first and third group of questions asked students to choose the amount of risks they prefer in each hypothetical situation. They need to choose between College X and College Y, whose unconditional admission probability and payoffs in the event of an "admission" in the game have been specified in Panels A2 and C2 of Table A.4, respectively, and fill in the first spot of the ROL. For each multiple price list, there are seven questions in total, as presented in the table, where the payoff of X is held constant (admission probability is 50%, get 25 CNY if “admitted" in this scenario), and the payoff of Y is in increasing order (admission probability is 25%, get 30, 35, 40, 45, 50, 55, 60 CNY if “admitted" in this scenario). The second, third, and fourth spots of the ROL have been pinned down, as shown in Panel A1 and C1 of Table A.4, where one of them will definitely “admit" the student applicant if she is not “admitted" to the first spot. The rules of admission for both groups of questions are exactly the same as the real admission procedures, with the only difference being that the payoff of being “admitted" to lower ranked colleges in Question Group 1 is 20 Chinese Yuan (CNY), whereas the amount in Question Group 3 is merely 5 CNY. These binary choice problems feature the core tradeoff in our setting: if students wish to take more risks and choose a more desirable college for their first choices, they must face a greater risk of being admitted to backup choices, whose payoffs are considerably lower.

The second group of questions is mathematically equivalent to the first one, but is asked in the form of its lottery representation. This group has seven questions as well. Lottery X, whose payoff structure is (25 CNY, 50%; 20 CNY, 50%) delivers the same distribution of payoffs as College X in Question Group 1, and is held across questions. The payoff structure of Lottery Y is (30 CNY, 25%; 20 CNY, 75%), (35 CNY, 25%; 20 CNY, 75%), ... (60 CNY, 25%; 20 CNY, 75%), respectively, which is mathematically equivalent to the payoff of College Y in Question Group 1. In terms of framing, however, Question Group 2 differs from Question Group 1 in that the probability and payoff of rejection from the first choice are included in the choices, as shown in Panel B of Table A.4. Because the DC Type in our setting choose their first college in isolation, they ignore the payoff of lower-ranked colleges, and thus fail to translate the ROL problem to its correct lottery representation. The inclusion of downside payment in the lottery precisely mutes the mistake from the DC decision rule.

Students were directed to carefully read through our explanations about the questions and complete comprehension checks before answering these questions. Since the payoff of College/Choice X is held constant, whereas that of College Y is increasing, a coherent response could switch from X to Y at most once. This point is clearly communicated in the instructions and students are allowed to switch from X to Y at most once in their responses.

After the student applicants have submitted their ROLs during the experiment, we asked them more application-related questions. The key questions which we intend to discuss are listed below:

1. The ROL that they submit.
2. For each choice, what do they think is the chance of meeting its cutoff?
3. If only two colleges were allowed to be included in a list, which two would they choose?

4. If only one college was allowed to be included in a list, which college would they choose?

Analysis of Survey Data

Prediction 1.5 states that DC Type students appear to take less risk in ROL questions compared to their (mathematically equivalent) lottery representations. We start our analysis by tabulating the joint distribution of students' responses to Question Groups 1 and 2 in Figure 1.3. About 65.5% of the observations are located in the blue blocks, indicating that students are very cautious[20] in both rank-order list and its lottery equivalent questions, or not very cautious in both questions. Such behavior is consistent with, or does not substantially deviate from the Rational Decision Rule. Meanwhile, 30.8% of the students are very cautious in the college choice problem, but not in the lottery equivalent questions. Such behavior is indicative of the presence of the DC Type.

Table 1.3, Panel A reports whether students with disadvantaged SES backgrounds, as measured by parents' average years of education, is associated with the aforementioned behavior. Linear probability models with various sets of controls demonstrate that one standard deviation of increase in the normalized SES index is associated with 4.1% to 4.7% of increase in the probability of belonging to the red block (i.e. exhibiting substantial framing effect predicted by the DC Decision Rule). This also relates to previous findings from the administrative data in Section 1.4, where socioeconomically disadvantaged students are more cautious only in the top spots of their lists and commit more risk-taking reversals.

To estimate the share of the DC Type in the survey sample, we model survey takers' risk-taking behavior by assuming that students have constant relative risk aversion (CRRA) preferences:

$$u = \frac{(c+B)^{1-\rho} - 1}{1-\rho}$$

where B is background consumption, which we set to be 10 CNY[21]. We allow ρ to vary on Edu, parents' average years of education, and CEE, the quantile of Priority Score:

$$\rho = \alpha_0 + \alpha_1 \text{SES} + \alpha_2 \text{Score} + \epsilon_\rho$$

where $\epsilon_\rho \sim N(0, \sigma_\rho^2)$. Following Von Gaudecker, Van Soest, and Wengstrom (2011), we model the noise in decision-making in terms of its impact on perceived certainty equivalents of a risky choice. Decision noise $\epsilon \sim N(0, \sigma^2)$ is independent across different questions and will govern the choice: for example, if an individual prefers College $X \equiv$

[20]As described in graph, being very cautious means that in terms of risk attitude, the students prefer (50,25%;20,75%) to (25,50%;20,50%). The implied CRRA coeffiient for such risk attitudes is larger than 20. Loss-averse with the fixed choice as reference point under small stakes as in Sprenger (2015) implies that the decision maker's loss aversion is larger than 6.35.

[21]Roughly the value of one meal in China.

$(25, 50\%; 20, 50\%)$ to $Y(m) \equiv (m, 25\%; 20, 75\%)$ when $m = 30$, but switches to $Y(m)$ when $m \geq 35$, this means that:

$$CE(X, \rho) + \epsilon \geq CE(Y(30, \rho))$$

and

$$CE(X, \rho) + \epsilon < CE(Y(35, \rho))$$

where $CE(L, \rho)$ is the certainty equivalent of lottery L when the CRRA coefficient is ρ.

Regardless of type, students with the same ρ in our model will produce the same distribution of choices in the standard lottery question (Question Group 2). However, in Question Groups 1 and 3, different types of individuals behave differently despite having the same risk preferences. Denote the lottery presentation of College X and Y from Question Groups 1 and 3 by $X(b) \equiv (25, 50\%; b, 50\%)$ and $Y(m, b) \equiv (m, 25\%; b, 75\%)$, respectively, where m is the payoff of first choices and b is the payoff of backup colleges. While the Rational Type will correctly translate the ROL question to its lottery representation, a DC Type processes the ROL questions differently:

$$CE_{\text{DC Type}}(X(b)) = CE(X(0))$$

and

$$CE_{\text{DC Type}}(Y(m, b)) = CE(Y(m, 0))$$

In other words, a DC Type distorts the downside of the lottery to a lower level, and appears to be more cautious.

We additionally consider a third type that is established in the literature for some of our specifications: the "sincere type" (Pathak and Sönmez, 2008; Calsamiglia, Fu, and Güell, 2020). The sincere type predicts that, in the ROL presentation, students will always prefer colleges with the highest payoffs, ignoring the probability of admission. In terms of certainty equivalents:

$$CE_{\text{Sincere Type}}(X(b)) = 25$$

and

$$CE_{\text{Sincere Type}}(Y(m, b)) = m$$

We further assume in this mixture model that the share of the DC Type varies with socioeconomic status:

$$Prob(\text{DC Type}|\text{Edu}, \text{Score}) = \frac{exp(\beta_0 + \beta_1 \text{SES} + \beta_2 \text{Score})}{exp(\beta_0 + \beta_1 \text{SES} + \beta_2 \text{Score}) + 1}$$

We estimate this mixture model and present the main results in Table 1.3, Panel B. Including DC Type in the estimation (Column 2) substantially improves the fit (Log Likelihood = -6543.705, 8 parameters) compared to the model in Column 1 that only allows for the Rational Type (Log Likelihood = -6751.161, 5 parameters). The improvement is substantial, such that the Bayesian Information Criterion also favors the mixture model

(BIC metrics = 13145.43) over the single-type rational model (BIC metrics = 13538.59). The impact of including the sincere type (Column 3), while it also improves the fit, is limited compared to the DC Type (Log Likelihood = -6542.287, 9 parameters). In line with this observation, the estimated share of the DC Type is 40.7% in our preferred specification. The marginal effect of socioeconomic status is also statistically significant, where a 1 SD increase in the SES Index is associated with a 7.7% (SE=2.7%) decrease in the share of the DC Type.

Framing Effect Predicts Risk-Taking Behavior in College Choices

If the DC Type indeed exists among the participants of the survey experiment, we would expect their real college choice patterns to exhibit what is described by Prediction 1.4 and 1.4 as well. Moreover, the DC Decision Rule is differentiated from the Rational Decision Rule by prescribing that decision-makers make choices in a forward way rather than doing backward induction when competitive colleges are mostly of higher desirability[22]. As a result, we would expect the DC Type to pick the top colleges, rather than bottom colleges as prescribed by backward induction, had they been allowed to include at most two colleges on their list.

Table 1.3, Panel C presents the mean statistics of students' college choices by whether they exhibit substantial framing effect (i.e. belong to the red block in Figure 1.3). It appears that applicants who are very cautious in the college choice problem but not in its lottery equivalents are substantially more likely to state that they would have picked their top choices in the original problem if the list is shortened such that they can only list up to one or two colleges. Moreover, these students are significantly more likely to believe that they have picked safer colleges for the top spot (i.e., top-choice cautiousness) but not for the bottom spot, and that they have committed risk-taking reversals.

1.6 Testing Upward Movement

With administrative data alone, we show in this section that a particular variation in priority score will help differentiate the DC Model from various forms of horizontal preferences and other alternative hypothesis.

Prediction: Position Movement in ROL

This subsection presents another prediction that distinguishes the two decision rules with minimal parametric assumptions on college preferences. We characterize the choice pattern of any single college, A, as priority score (consequently, the assignment probability of A[23]) changes. For an arbitrary preference profile u (a utility vector that represents

[22] As discussed in Section 1.3, in the presence of substantial horizontal preferences, there are exceptions when the Rational Rule does not imply backward induction

[23] Probability of meeting the cutoff of A.

preferences over *all* colleges), if A appears on the list given a specific priority score s, where would A be if s were higher?

To understand the result intuitively, consider that, under the rational decision rule and preference u, A is a particularly attractive college and will appear on the list for some s. If s is too low, such that being admitted to A is impossible, A will be omitted because listing it wastes a spot. However, as s becomes higher, A appears on the list as soon as the admission probability is high enough. As s continues to move higher, more colleges become possible options for u. Consequently, A will remain in the same place if none of the newly possible options are better than A, and will move down the list if any newly possible option is better than A and has a favorable chance. Figure 1.4a presents an example in which the position of a college evolves as priority score changes. Note that, on the horizontal axis, priority scores have been converted to admission probability of A to make the graph comparable.

To mathematically describe the result, define function $\mathcal{R}$ that maps preference profile u, priority score s, college A to its position κ on the list under the rational decision rule:

$$\mathcal{R} : (u, A, s) \mapsto \kappa$$

where κ is 0 if A is omitted from the list. κ **takes the value of 4, 3, 2, 1 if A is the first, second, third, and fourth choice, respectively.** The following theorem characterizes $\mathcal{R}$:

Jibberish 1. *Under Assumption 1, 2, 3, for any preferences u, college A, and s_0 such that $\mathcal{R}(u, A, s_0) \geq 1$, if $s > s_0$, then $\mathcal{R}(u, A, s) \leq \mathcal{R}(u, A, s_0)$. Moreover, for any $\kappa \geq 1$, the set $C^{RN}_{(u,A,\kappa)} = \{s|\mathcal{R}(u, A, s) = \kappa\}$ is connected.*

The key assumptions, as detailed in Appendix A.4, mean that increases in priority score will make the assignment probability of the safer college increase at a lower rate compared to the riskier one. As discussed in Appendix A.4, this statement is true for any pair of colleges whose cutoff distribution is log-concave[24] and of the same dispersion. The assumption is testable, and largely holds in our setting because we assume the cutoff distribution is normal and colleges of similar competitiveness have comparable dispersion of the cutoff distributions.

Similarly, define function $\mathcal{D}$ that maps preference profile u, priority score s, college A to its position κ on the list under the DC decision rule:

$$\mathcal{D} : (u, A, s) \mapsto \kappa$$

$\mathcal{D}$ is different from $\mathcal{R}$ in that a particular college need not move down the list as s increases. College A is not listed when s is too low, for similar reasons. As s increases, however, A first appears at the fourth choice once its probability is just high enough to exceed the previous fourth choice, which is the least appealing one on the list in terms

[24] Log-concave distributions include normal distribution, uniform distribution, exponential distribution, logistic distribution, extreme value distribution, Pareto distribution, etc.

of $p_A u_A$[25]. If u_A is higher than other listed colleges (despite lower probability, which results in lower $p_A u_A$ overall), A may move ***up*** as s increases, because p_A affects how A is ranked under the DC rule. A starts to move down gradually if s is so high that better colleges are within reach. Figure 1.4b presents an example where the position of a college evolves as priority score changes, where priority scores have been converted to admission probability of A on the horizontal axis. Compared to Figure 1.4a, it becomes apparent that the "climbing up to the top" movement to the left of the peak in Figure 1.4b distinguishes DC from the rational rule. The following theorem mathematically characterizes $\mathcal{D}$, when Assumptions 1, 2, 3, as detailed in Appendix A.4, hold:

Jibberish 2. *Under Assumptions 1, 2, 3, for any preferences u and college A, if there exist* $\underline{s}$ *and* $\bar{s}$ *such that* $\mathcal{D}(u, A, \underline{s}) = 0$ *and* $\mathcal{D}(u, A, \bar{s}) = \kappa$, *then for any integer* $\xi \in [1, \kappa]$, *there exist* $s \in [\underline{s}, \bar{s}]$ *such that* $\mathcal{D}(u, A, \bar{s}) = \xi$.

The derivations of both theorems are detailed in Appendix A.4. Note that the assumption about the tie of expected utility is not essential, because, if we assume that students choose randomly if more than two colleges tie, similar results emerge. When the assumption of both theorems hold, together they imply the following prediction:

[Upward Movement] In response to increase in priority scores, for any preferences, previously listed colleges will only move downward on the list under the rational decision rule, but may move upward under the DC decision rule.

Variations in priority score that are orthogonal to preferences

Conditional on the same exam performance, the same academic ability leads to different provincial rankings of priority scores in different years. As discussed in Section 1.2, the college entrance exam consist of four subjects, and priority score is determined by summing up the *raw scores* of all subjects. However, the difficulty of subjects varies from year to year, such that the dispersion of students' performance does not move in the same direction, as demonstrated in Figure A.6a. Given the unpredictability of subject difficulty, the way in which students' true academic ability is aggregated changes exogenously from year to year. For example, a student who is good at math may have a higher total score in a year where the math test is more difficult. To quantify the impact of such idiosyncratic aggregation, we regress rank-preserving score[26] on the polynomials of percentiles of each subject:

$$\text{Rank-Preserving Score}_i = f(Chinese\%_i, Math\%_i, English\%_i, Comprehensive\%_i) + \nu_i \tag{1.4}$$

If the regression is run within each year, the score should be mechanically predicted by the quantiles perfectly. If we run the regression over the whole sample (i.e., 2014-2018), quantiles cannot perfectly predict the score because of the cross-year change in

[25]For the sake of convenience, the outside option in this subsection is assumed to be 0.

[26]Converting raw CEE score to its 2018 rank-preserving equivalent, as in Section 1.1.

score aggregation, as shown in Figure A.6b. This regression thus decomposes the rank-preserving score into two orthogonal components, the predicted academic ability $\hat{f}_i$ and the residual $\hat{v}_i$. $\hat{v}_i$ is approximately normally distributed, creating additional variation whose standard deviation amounts to roughly 2 points in the priority score. While small, this creates enough variation for us to test the prediction in Section 1.6.

Testing Upward Movement in ROL: Data Analysis

For each student i and its selected college j, we divide the students according to the predicted probabilities in the absence of the score shock $\hat{v}_i$ (i.e., the probabilities converted from the predicted ability $\hat{f}_i$) into ten groups: 0.1% ~ 10%, 10% ~ 20%, 20% ~ 30%, ... 80% ~ 90%, 90% ~ 99.9% and generate ten dummies. This set of variables is aimed at capturing any heterogeneity in preferences that are associated with students' academic ability. In other words, within each probability bin, students have essentially the same academic ability and thus any changes in preferences that are associated with academic ability have been controlled. Armed with the shock $\hat{v}_i$, we run the following random-coefficient regression to test our hypothesis:

$$y_{ij} = \beta_{(j,\text{SES Quarter},\text{Prob Bin})}\hat{\hat{f}}_i + \gamma_{(j,\text{SES Quarter},\text{Prob Bin})}\hat{v}_i + FE_j * FE_{\text{SES Quarter}} * FE_{\text{Prob Bin}} + \epsilon_{ij} \quad (1.5)$$

where y_{ij} is the position of college j on student i's list (i.e., the vertical axis of Figure 1.4a and Figure 1.4b), FE_j is the college fixed effects, which aim to capture average preferences over college j non-parametrically, and $FE_{\text{SES Quarter}}$ is a dummy for the socioeconomic quartile that students belong to. The parameter of interest is the coefficient of score shock $\hat{v}_i$ (i.e., the "slope" of the movement in Figure 1.4a and Figure 1.4b) in each Prob Bin * SES Quarter * College cell, without imposing any restrictions across cells.

Under the assumption that students' preferences with regard to a specific college are homogeneous within each Prob Bin * SES Quarter * College cell, any positive estimates in $\gamma_{(j,SES)}$ that appear in any cluster would be interpreted as evidence of upward movement[27]. In other words, students who have similar socioeconomic status and academic ability, and choose to include the same college in their list (regardless of the position of the college), are assumed to have homogeneous (not necessarily vertical) preferences.

Figure 1.4c summarizes the distribution $\hat{\gamma}_{(j,SES)}$ by predicted admission probability in the absence of score shock. When the probability is lower than 20%, the mean of $\hat{\gamma}_{(j,SES)}$ is positive, suggesting substantial presence of the upward movement. When the predicted admission probability is above 20%, the estimated mean is around zero or negative. This does not indicate absence of the DC Type, because the movement among the DC Type is predicted to become downward when the probability is higher (Figure 1.4b). Moreover, the heterogeneity in $\hat{\gamma}_{(j,SES)}$ across college*SES cluster is not negligible even when the mean estimate of $\hat{\gamma}_{(j,SES)}$ is non negative. This finding also points to the massive presence of

[27] y_{ij} takes the value of 4, 3, 2, 1 if college j is listed as the first, second, third, and fourth choice respectively.

upward movement for higher probability bins. The numeric value of the aforementioned statistics are reported in Table A.5.

1.7 Structural Estimation using Administrative Data

Prediction of the DC Rule & Intuition of Identification

Because we are relying on risk-taking behavior to separate the DC Type from the Rational Type, once heterogeneous and horizontal preferences are incorporated into the model, it becomes less obvious how a structural model can identify the DC Type. The variation we are going to exploit is the variation in priority scores. As discussed in Section 1.6, conditional on the same academic ability, the cross-year variation in ranking and the resulting probabilities are arguably orthogonal to preferences.

How does this variation contribute to identification? A higher priority score increases the assignment probability of all colleges, *at different rates*. As discussed in Agarwal and Somaini (2018), with sufficient variation in assignment probabilities, it becomes possible to identify the distribution of preferences. While we are **not** claiming that the variation we have here is sufficient to identify arbitrary distribution of preferences, it is at least powerful enough to help identify the intensity of preferences, as well as the presence of the DC Type, as illustrated in the simplistic setting below.

Example There are four colleges, $A1$, $A2$, $B1$, $B2$. Let s denote priority score. $A1$ and $A2$ are risky (but potentially more desirable) colleges. Both of them have unconditional assignment probability $p_A(s)$ and admission utility $\delta > 0$. $B1$ and $B2$ are safe colleges. Both of them have unconditional assignment probability $p_B(s) > p_A(s)$; they have admission utility of 2 and 1 respectively. All the probabilities are independent. Students need to select two colleges from the four under a constrained Deferred Acceptance Algorithm. The utility of the outside option is 0.

Identifying Preference Intensity from Score Variation: Binary Choice Suppose the second position must be left blank, and students must choose from $A1$ or $B1$. Then, they choose $A1$ if and only if

$$\delta p_A(s) > 2p_B(s) \iff \frac{p_A(s)}{p_B(s)} > \frac{2}{\delta} \tag{1.6}$$

Figure A.5 shows an example where different s leads to different ratio $\frac{p_A(s)}{p_B(s)}$. When the distributions of cutoffs of the two colleges are normal, as hypothesized in our setting, the ratio of assignment probability of $A1$ to $B1$ will be increasing as the probabilities are increasing at different rates[28]. If we can observe an individual making choices given a

[28]As discussed in Appendix A.4, the ratio will be increasing for any log-concave distributions if the cutoffs of the two colleges have the same dispersion. This scenario is largely in line with our hypothesis that

different priority score s, we would expect her to switch from $B1$ to $A1$ at some point. The earlier she switches, the more she likes $A1$ over $B1$. Consequently, for a group of students, the rate at which students switch from $A1$ to $B1$ identifies the density of δ.

Identifying DC Type Using Joint Changes in ROL When students select only one college, by definition the DC Rule cannot be distinguished from the Rational Rule. With two choices in the list, however, this becomes possible. As we observe how both choices change in response to increasing s, optimality implies that both choices are changing. The changes are jointly restricted because both are responses to the same preferences, as summarized by δ.

There are four possible portfolio choices in this setting, $(A1, A2)$, $(A1, B1)$, $(B1, A1)$, $(B1, B2)$. $(A1, A2)$ features substantial risk-taking, which for convenience is labelled as "reckless". $(A1, B1)$ features differential risk-taking in different positions ("diversifying"). $(B1, A1)$ features safer options before risky ones ("reversal"). $(B1, B2)$ features minimal risk-taking ("cautious"). We have the following results that help separate the DC Type from the Rational Type as the probability ratio $\frac{p_A(s)}{p_B(s)}$ increases: As s increases, only the DC Type switches from "cautious" to "reversal" when $\frac{p_A(s)}{p_B(s)} < \frac{1}{2}$, and only the DC Type switches from "reversal" to "reckless" when $\frac{1}{2} < \frac{p_A(s)}{p_B(s)} < 1$.

Why cannot preference alone explain the switch to and away from "reversals"? The reason is that the probability ratio $\frac{p_A(s)}{p_B(s)}$, which is observable to us, contains information about the magnitude of δ. When the switch happens with low $\frac{p_A(s)}{p_B(s)}$, the switch implies that preference intensity toward $A1$ and $A2$, δ, is high. For the Rational Type, high δ rules out the possibility of a reversal. Moreover, the point at which the switch happens also reveals the preference intensity of the DC Type. The rate at which the list switch happens identifies the probability density of δ.

Setup

Structure of College Preferences We parameterize college preferences to implement the structural estimation. For individual i, the utility of admission to college j is

$$u_{ij} = f(\theta_i^C, C_{ij}, SES_i) + g(\theta_i^d, d_j, SES_i) + h(\theta_i^X, X_j, SES_i) + O_i + \epsilon_{ij} \tag{1.7}$$

where C_{ij} is the competitiveness of college j with respect to student i, and SES_i is the average educational attainment in student i's township of residence. As detailed in Appendix A.3, function $f(.)$ controls the curvature over college preferences and takes the form of the CRRA function, with the curvature parameter being θ_i^C[29]. θ_i^C is normally

the cutoff distribution of any two colleges of similar calibre share a similar standard deviation. When the dispersion is different, the range of the ratio usually will be wider, though not necessarily increasing in s.

[29]We ensure that C_{ij} is positive by taking the difference between itself and the minimally competitive college.

distributed with unknown variance, and the mean is allowed to vary across students of different SES levels. We normalize d_j as the distance between students' home and the location of the college. Note that subscript i is omitted. Since all but two first-tier colleges are located outside Ningxia, and are clustered in metropolitan areas far away, the distances barely differ for students living in different areas in Ningxia. Function $g(.)$ controls preferences for distance and is quadratic with parameter vector θ_i^d. θ_i^d is jointly normally distributed with an unknown diagonal variance matrix, and the mean is allowed to vary across students of different SES levels. $h(\theta_i^X, X_j, SES_i)$ controls the interaction between other college characteristics and students' socioeconomic status, with the interaction parameter θ_i^X permitted to be normally distributed, with unknown variance and SES-specific mean. O_i measures the desirability of first-tier colleges overall relative to outside options. $\epsilon_{ij} \sim N(0, \sigma_\epsilon^2)$ is the individual-college specific shock to admission utility. Appendix A.3 discusses all the details of the specification.

Mixture Model We estimate a mixture model where there are two types of students: DC and rational. In this mixture model, we assume that share of the DC Type among students is a function of the SES status of student i:

$$P(\text{DC Type}|\text{SES}_i) = \frac{exp(\gamma_0 + \gamma_1 * SES_i)}{1 + exp(\gamma_0 + \gamma_1 * SES_i)} \tag{1.8}$$

Admission Probability In the benchmark estimation, we use the estimated probability $\hat{p_{ij}}$, as in Section 1.3. Appendix A.3 discusses the case where one wishes to rely on students' subjective beliefs in the survey experiment to conduct estimation.

Estimation Strategy

Following Section 1.4, we focus on students whose priority score percentile belongs to the top 60%, because these students' choices are less constrained. We split our sample according to students' CEE score and conduct the estimation for 40% ~ 60%, 60% ~ 80%, 80% ~ 100% separately because students at different levels of academic ability tend to choose colleges of different levels of competitiveness, as demonstrated in Figure A.7.

We include moments such as the mean assignment probability of the first, second, third, and fourth choices, as well as the share of reversals to target the moments that are directly related to the predictions of the DC Model[30]. In this parameterized model, the moments on characteristics of the listed colleges (physical distance, as well as share choosing a specific type of college) jointly identify the distribution of choice over colleges, hence the horizontal preferences over observables. As discussed in Section 1.7, curvature

[30]Maximum likelihood estimation using ROLs, as in Agarwal and Somaini (2018); Calsamiglia, Fu, and Güell (2020); Kapor, Neilson, and Zimmerman (2020), is relatively more difficult because the number of portfolios one could construct amounts to billions in our setting. However, it may still be possible to use MLE in a computationally feasible way, according to the techniques introduced in Larroucau and Rios (2018).

over competitiveness can be identified by the mean of the assignment probability of a single choice. Other horizontal preferences are assumed to be idiosyncratic, and thus bounded by the competitiveness of the choice. The moments are constructed separately for four SES quartiles to examine whether the share of the DC Type is higher among the socioeconomically disadvantaged, compared to their counterparts.

We use the Simulated Method of Moments to estimate this model. We simulate each student's choices 50 times, and calculate the simulated moments by averaging across different rounds. The estimation minimizes the weighted distance between simulated moments $m(\theta)$ and data m_0:

$$min\ (m(\theta) - m_0)'W(m(\theta) - m_0)$$

The estimator achieves asymptotic normality, with an estimated variance of:

$$(\hat{G}'W\hat{G})^{-1}(\hat{G}'W(1 + \frac{1}{50})(\hat{\Omega}/N)W\hat{G})(\hat{G}'W\hat{G})^{-1}$$

where 50 corresponds to the number of simulated choices for each observation (Laibson, Repetto, and Tobacman, 2007; DellaVigna et al., 2016), $\hat{G} \equiv \frac{\partial m(\theta)'}{\partial \theta}$ and $\hat{\Omega} = Var(m(\hat{\theta}))$. In our estimation, to enhance the efficiency of estimation, W is selected to be the inverse of the covariance matrix.

Estimation Results

Table 1.4 compares the performance of a model that only allows for the Rational Type to a mixture model that allows for both the Rational Type and the DC Type. While all models are over-identified and rejected, the mixture model decreases the distance by 42.0%, 69.1%, 53.3% for the 40% ∼ 60%, 60% ∼ 80%, 80% ∼ 100% subsamples, respectively. This improvement is substantial. The MMSC-BIC metric[31] (Andrews and Lu, 2001), an analogue of the Bayesian Information Criterion, favors the mixture model over the rational benchmark as well.

The estimated share of the DC Type is 53.1% (SE=0.61%), 45.1% (SE=0.54%), 55.1% (SE=0.55%) for 40% ∼ 60%, 60% ∼ 80%, 80% ∼ 100% subsamples, respectively. These estimates are interestingly close to the estimate we get from the online survey (48.7%, SE=1.8%). The estimates on the marginal effect of SES are negative, where 1 standard deviation of decrease in SES index is associated with a decrease of 3.71% (SE=0.57%), 3.68% (SE=0.56%), 6.02% (SE=0.55%) in the propensity of being a DC Type. This negative effect is slightly less than the estimated effect from the survey (7.3%, SE=1.7%).

The estimated curvature over competitiveness is mild and sometimes positive, ranging from -0.486 to 0.413 (ρ in the CRRA specification), with all standard errors below 0.1. In summary, the levels of estimated curvature in both models are consistent with anecdotal evidence on the perceived importance of competitiveness/cutoffs on college prestige.

[31] The formula for this metric is $d - (m - p)ln(n)$, where d is the distance, m is the number of moments, p is the number of parameters, and n is the sample size. The criteria favor smaller values.

Out-of-sample prediction of the mixture model is also substantially better than the one-type rational model[32]. As demonstrated in Panel B of Table 1.4, compared to the one-type rational model, the mixture model decreases the distance by 14.9%, 43.4%, and 47.2%, respectively, and fits much better in the moments that are related to our predictions about cautiousness and reversals.

The DC Model helps fit the key moments of the data. Figure 1.5 compares the overall data and fit from the one-type rational model and the mixture model, for average risk-taking (Figure 1.5a) and share of reversals with different thresholds (Figure 1.5b), respectively. While both the one-type rational model and mixture model generate substantial reversals by introducing heterogeneous preferences, the one-type model fails to explain the cautiousness in the first choices by a fairly large margin (20%), while it predicts extreme cautiousness (probability > 99%) for the fourth choices, contrary to the data, in which the mean of probability is around 90% for the fourth choices.

Although our model does not directly fit the estimates on upward movement, Figure 1.5c shows that, consistent with our theory, the estimated upward movement coefficient γ obtained by running regression 1.5 on the simulated sample generated by the estimated mixture model, is uniformly higher compared to that generated by the rational one-type model.

Welfare

The monetary measurement of welfare is motivated by the analogy that students' priority scores (determined by their exam performance, not by their college demand or strategy) serve as their WTP for college education, and that the cutoffs of the colleges serve as the prices of such services. We treat students' priority scores as their "budget set", and measure the welfare change as the equivalent variation (EV) in terms of priority score. In other words, for each individual applicant, the EV is the amount of change in its priority score in the current equilibrium that results in the same change in expected utility had the score remain unchanged under the new equilibrium.

To compute the counterfactual, we follow Kapor, Neilson, and Zimmerman (2020) by simulating how everyone would react to others' lists in the following two scenarios: (I) The system switches to Boston Mechanism with four spots without changing its decision rule; (II) All applicants respond optimally under the current system. We thereby obtain the new lists, and then use the new lists to simulate the cutoffs, iterating until convergence.

De-Biasing Conventional wisdom suggests that the sophisticated usually take advantage of the naive in a market setting (Gabaix and Laibson, 2006; Pathak and Sönmez, 2008), such that de-biasing could be a zero-sum game. However, in empirical settings,

[32]The structural estimation only uses the data from 2015, 2017, and 2018, because we have access to township-level student addresses only in these years to measure SES in a more precise way. The measure of SES in 2014 and 2016 is county-level adult educational attainment, which is substantially less precise, and thus is left out of the estimation and used for out-of-sample testing.

the intensity of vertical preferences may differ, and acting strategically will help students communicate such intensity (Abdulkadiroğlu, Che, and Yasuda, 2011). We report our findings in Panel C of Table 1.4. De-biasing improves the welfare of the 3rd, 4th and 5th quintiles of the DC Type, which is equivalent to an improvement of 0.495, 0.253, and 0.082 of a standard deviation of the priority score in the old equilibrium. Interestingly, de-biasing also increases the welfare of the Rational Type whose priority scores belong to the 3rd and 4th quintiles by 0.368 and 0.217 of a standard deviation of the score, and decreases the welfare of those whose scores belong to the highest quintile by 0.080 of a standard deviation. Intuitively, this happens because, in the old equilibrium, there is a mismatch effect between the behavioral type with a higher score and the rational counterpart with a lower priority score. De-biasing eliminates this effect and benefits any rational type whose score is not among the highest.

Effect of De-Biasing on Outcome Gap in Terms of Selectivity We know from Section 1.4 that the most socioeconomically disadvantaged quartile ends up in less selective colleges. In the structural model, this gap is explained by other differences in college preferences, or the behavioral biases. We examine how the gap would change under the counterfactual scenario in which all students are acting optimally. As reported in Table A.3, Panel B, the gap shrinks by at least 83.15%, implying that, rather than heterogeneous preferences, most of the gap is explained by the behavioral biases.

Alternative Mechanisms DAA without limits on the number of choices would remove the advantage of the Rational Type, but students would not be able to express their preference intensity through risk-taking (Abdulkadiroğlu, Che, and Yasuda, 2011). Panel C reports the welfare effects of switching to unlimited DAA, which decreases the welfare of the Rational Type in the 3rd, 4th, and 5th quintiles by 0.329, 0.725, and 0.045 s.d. of the score. It increases the welfare of the DC Type in the 3rd and 4th quintile by 0.231 and 0.447 s.d. of the score, but decreases the welfare of the DC in the 5th quintile by 0.155 s.d. of the score. Overall, DAA without a limit is not welfare-enhancing for the majority of the students. An important caveat is that such evaluation ignores the differential impact of mistaken beliefs across mechanisms (Kapor, Neilson, and Zimmerman, 2020).

An alternative is the Boston Mechanism, which intuitively makes bias less costly because it weakens the Rational Type's advantage of using backup choices as "insurance" to act aggressively for their top choices. Our results suggest that the Boston mechanism leads to a decrease in welfare for students of both types, although the decrease for the DC Type is smaller than the decrease for the Rational Type. Together with the results from DAA with an unlimited list, our analysis provides empirical support for Chen and Kesten (2017, 2019)'s theoretical finding that the Chinese parallel mechanism, as a middle ground between the Boston and DAA mechanism, may be better than both in our context.

Inequality Figure 1.6 presents how de-biasing and mechanism switching affect students of different socioeconomic status. Switching to an unconstrained Deferred Acceptance

Algorithm or a Boston mechanism decreases the average welfare of students of every socioeconomic status. In contrast, de-biasing increases the welfare of all socioeconomic levels, where the welfare increase for the most disadvantaged quartile is equivalent to 0.294 s.d. of the score, and the welfare increase for the most advantaged quartile is equivalent to 0.221 s.d. of the score.

1.8 Discussion

Alternative Considerations Several factors that have been documented in the literature may also affect students' decision making. Appendix A.3 discusses, using the survey data, that to what extent students' subjective beliefs deviate from the estimated probability we construct using administrative data, and how the elicited beliefs affect the results of structural estimation. The primary finding is that using subjective beliefs increase the estimated share of the DC Type because the beliefs reflect higher degree of top-choice cautiousness. Appendix A.5 discusses correlation in the event of admission across colleges and finds that it does not affect the accuracy of estimated admission probability. Additionally, Appendix A.5 discusses to what extent consideration of major could affect decision making, and find that it has minimal impact on risk taking behavior.

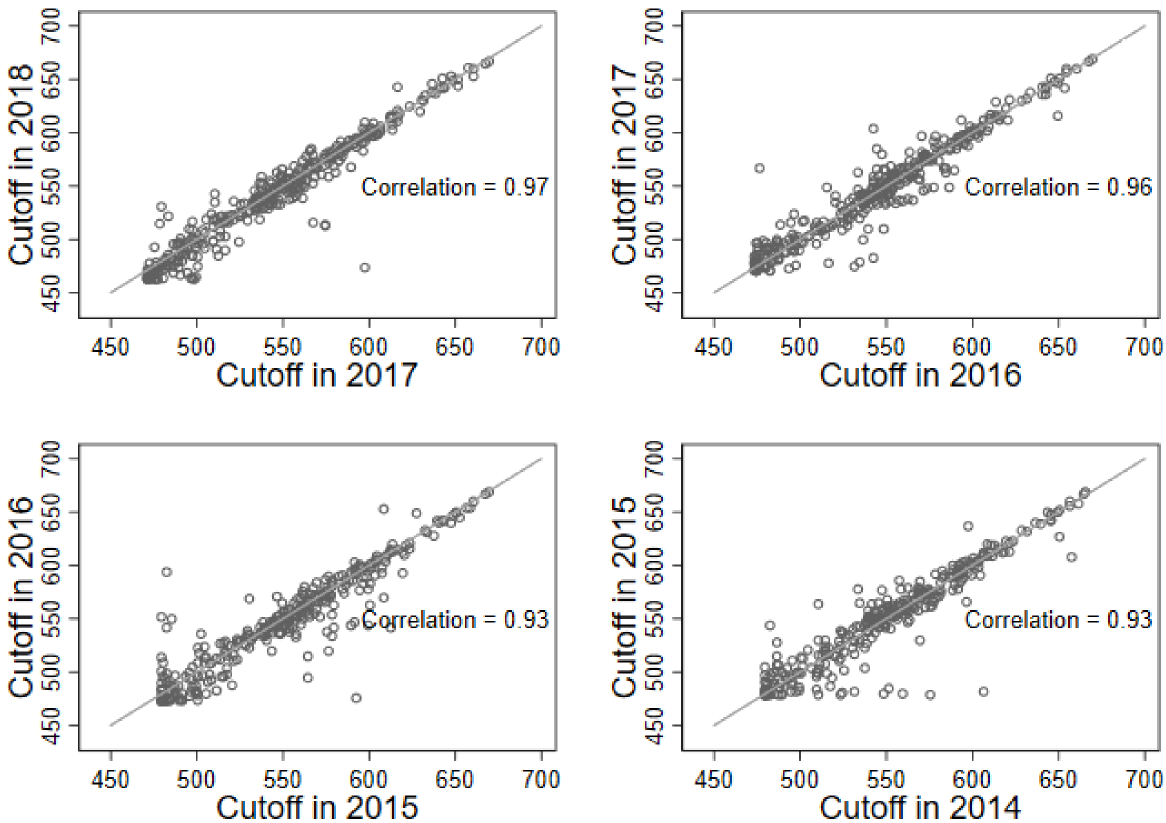
Psychological Mechanism The Model of Directed Cognition can naturally fit all the empirical patterns that we highlight. In addition to the failure of backward induction and contingent reasoning, the DC Model captures the idea that decision-makers tend to ignore the "background" of a problem when making individual choices. This intuition is similar to the theoretical models that describe how variation in choice attributes affects their salience and in turn affects the decision weight placed on them (Bordalo, Gennaioli, and Shleifer, 2012; Kőszegi and Szeidl, 2013; Bordalo, Gennaioli, and Shleifer, 2021). For example, if each spot of the rank-order list is viewed as an attribute of the portfolio, decision-makers may underweight the colleges that are already included when selecting individual colleges for other blank spots.

An alternative consideration is that the sequence in which a rank-order list is processed implies that uncertainty is resolved in multiple stages, which is a compound lottery problem. The established association between ambiguity aversion and failure of reducing compound lottery (Halevy, 2007; Chew, Miao, and Zhong, 2017) suggests that the inability to cope with multistage uncertainty could be caused by either ambiguity or complexity aversion. Regardless of its welfare implications, the model of directed cognition can be viewed as the limit point of the recursive utility specification in this literature. However, the documented framing effect in the survey experiment suggests that this psychological mechanism cannot explain all the suboptimal choices in our setting, because the uncertainty in later stages has been effectively eliminated in the incentivized survey questions.

As our findings could be potentially generalized to other mechanisms and settings where risky choices are interrelated, an important question for future research is to what extent the aforementioned factors influence the descriptive power of the DC Model in other contexts.

Implications for School Choice Systems A large literature documents that disadvantaged students select worse schools in the presence of school choice (Hastings and Weinstein, 2008; Hoxby and Avery, 2012; Walters, 2018). Our analysis suggests a novel channel through which a specific cognitive bias exacerbates inequality in educational attainment. Contrary to the convention wisdom, the documented behavioral biases affect not only equity, but also efficiency. The results suggest that intervening against the biases under the current system outperforms alternative popular mechanisms. This finding depends on the students' preference profiles. Caution needs to be exercised when applying this insight to other contexts.

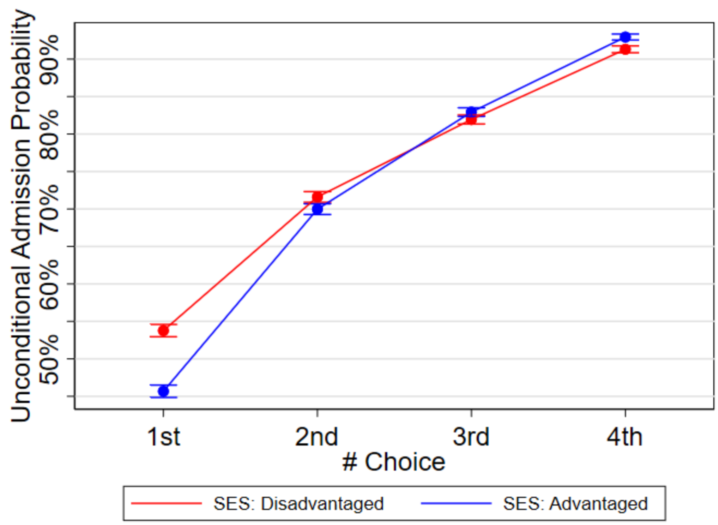
Figure 1.1: Admission Cutoffs Across Years



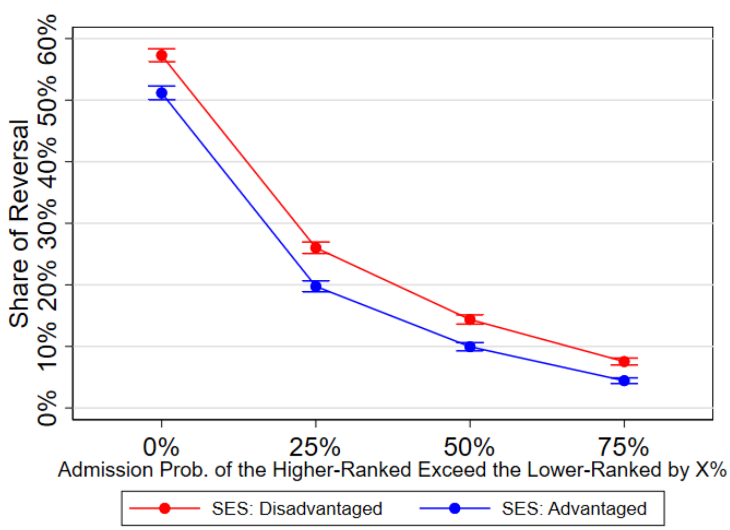
Note: This figure plots the admission cutoffs for all colleges that admit Ningxia students during 2014-2018, for the discussion in Section 1.3. The admission cutoffs are converted to its 2018 rank-preserving equivalents. Each dot represents the admission cutoffs of a specific college in two consecutive years. Top left graph plots the cutoffs in 2018 against 2017. Similarly, top right graph plots the cutoffs in 2017 against 2016; bottom left graph plots the cutoffs in 2016 against 2015; bottom right graph plots the cutoffs in 2015 against 2014.

Figure 1.2: Mean of Admission Probability and Share of Reversals by SES Groups

(a) Mean of Unconditional Admission Probability



(b) Share of Risk-Taking Reversals by Threshold X%



Note: This figure presents statistics for the most advantaged quartile (in blue) and the most disadvantaged quartile (in red), respectively. The sample is restricted to those whose priority score is top 60%, for the discussion in Section 1.4. Subfigure (a) plots the mean of unconditional admission probability. Subfigure (b) plots the share of students whose strategy exhibits at least one pair of risk-taking reversals anywhere on her ROL, as a function of the threshold above which the reversal is counted in the statistic. The statistic we use to classify reversal is $R \equiv max\{p_i - p_j| \text{for any} 1 \leq j < i \leq 4\}$, only those whose R is above threshold $X\%$ will be counted. To maximize comparison, the mean from the least advantaged quartile has been reweighted to account for differences in priority scores.

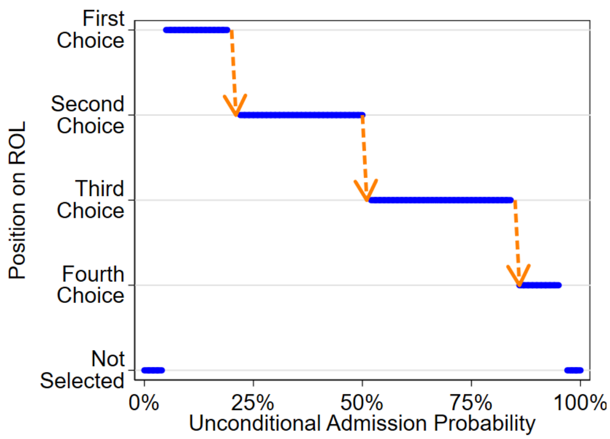
Figure 1.3: Distribution of Response to Incentivized Survey Questions

			Equivalent Lottery: Indifferent Between (25 CNY, 50%; 20 CNY, 50%) (L CNY, 25%; 20 CNY, 75%)		
			Very Cautious L≥50	Not Very Cautious L<50	Total
ROL 1st Choice:Payoff of non-first colleges is 20 CNY Indifferent Between (25 CNY, 50%) (R CNY, 25%)	Very Cautious	R≥50	72 5.1%	435 30.8%	507 35.9%
	Not Very Cautious	R<50	81 5.7%	824 58.4%	905 64.1%
	Total		153 10.8%	1259 89.2%	1412 100%

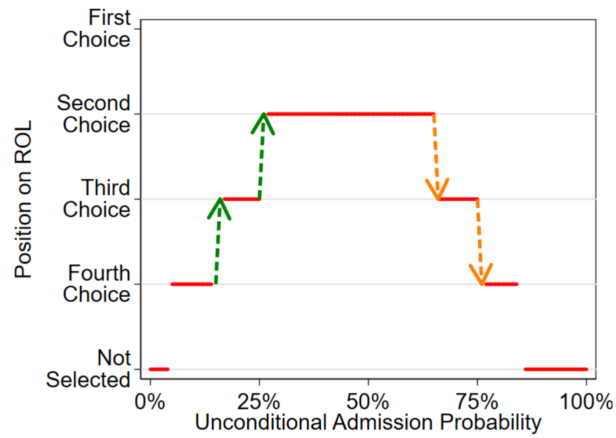
Note: This figure displays a coarsened joint distribution of responses to Question Group 1 (ROL 1st Choice) and Question Group 2 (Equivalent Lottery), for the discussion in Section 1.5.
currentcolor
Blue cells indicate that students' responses are both classified as "not very cautious" in the "ROL first-choice" problem and the equivalent lottery problem, as predicted by Rational Decision Rule.
currentcolor
Red cells indicate that students' responses are very cautious in "ROL first-choice" problem but not very cautious in the equivalent lottery problem as predicted by Directed Cognition (DC) Decision Rule.
currentcolor
Gray cells indicate that students' responses are not very cautious in "ROL first-choice" problem but very cautious in the equivalent lottery problem, which cannot be predicted by neither decision rule.
"ROL first-choice" question (vertical) corresponds to Question Group 1. In this question, students need to choose from college X, whose admission probability is 50% and payoff of admission is 25 CNY, or college Y, whose admission probability is 25% and payoff of admission is R CNY, where R = 30, 35, 40, 45, 50, 55, 60, respectively in the MPL, and put the college of their choice on the top of their list. If students are not admitted to their first choices, they will be admitted to one of the bottom choices in this scenario, which corresponds to the payoff of 20 CNY.
"Equivalent Lottery" question (horizontal) corresponds to Question Group 2. In this question, students need to choose from lottery X, whose payoff is 25 CNY with 50% of chance, 20 CNY with 50% of chance, or lottery Y, whose payoff is L CNY with 25% of chance, 20 CNY with 75% of chance, where L = 30, 35, 40, 45, 50, 55, 60, respectively. Note that "Equivalent Lottery Question" is mathematically equivalent to the aforementioned "ROL first-choice Question".

Figure 1.4: Prediction: Position Movement in ROL as Priority Score Changes

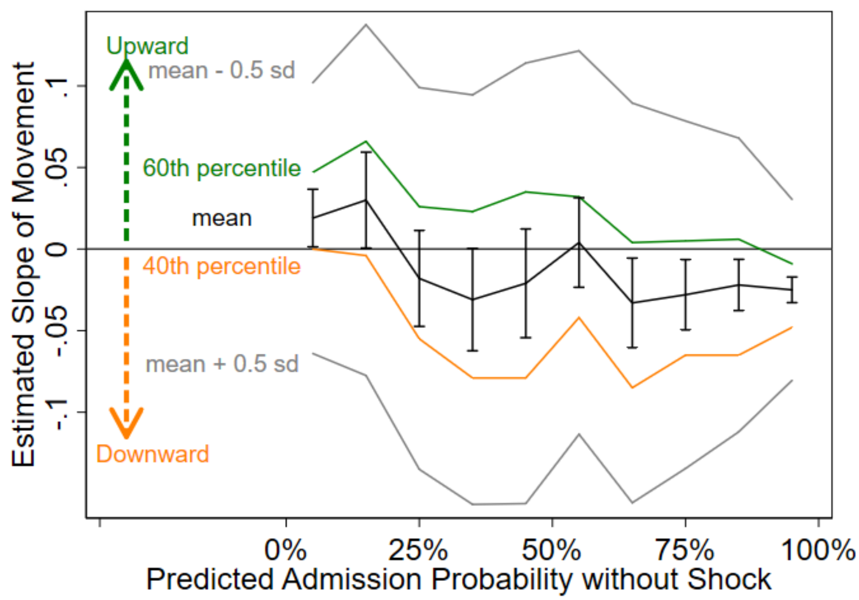
(a) Individual Choice Pattern under Rational Rule



(b) Individual Choice Pattern under DC Rule



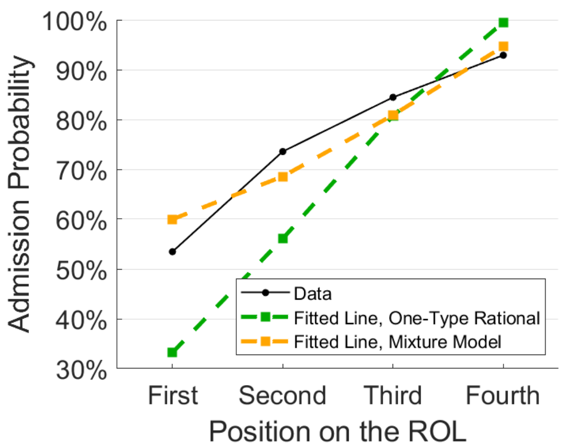
(c) Estimated Average Direction of Movement



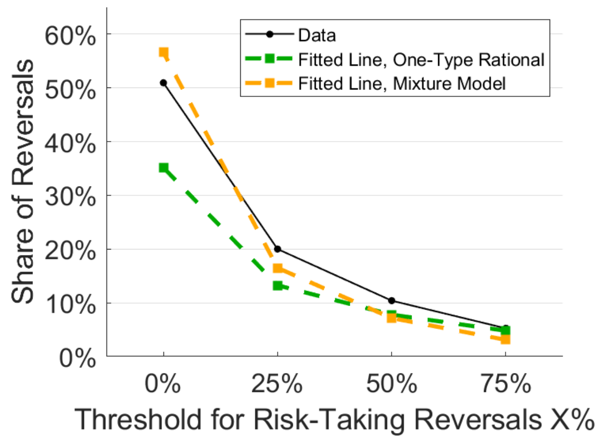
Note: This figure shows the prediction about how any single college move on the list as priority score changes under Rational Decision Rule and DC Decision Rule respectively, as discussed in Section 1.6, and 1.6. Subfigure (a) presents an example where the college in question is put on the list under Rational Decision Rule. Blue dots represent the position of the college given a level of priority score specified on horizontal axis. Subfigure (b) presents an example where the college in question is put on the list under DC Decision Rule. Red dots represent the position of the college given a level of priority score specified on horizontal axis. Subfigure (c) shows the estimated mean, 40th percentile, 60th percentile, mean ± 0.5sd of slope for each probability bin using the regression described in 1.6.

Figure 1.5: Data & Model Prediction: One-Type Rational vs. Mixture Model

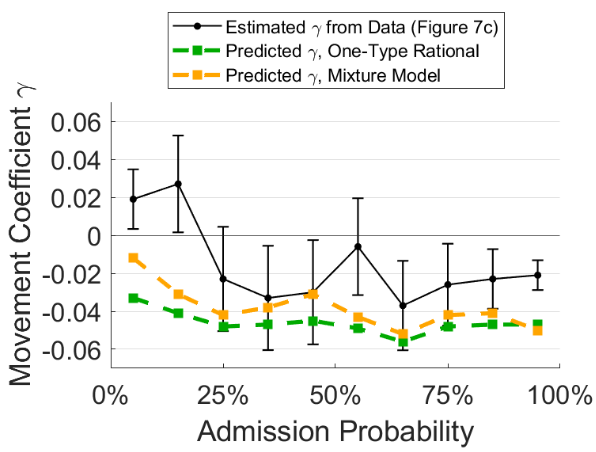
(a) Mean Admission Probability for Each Choice



(b) Share of Risk-Taking Reversals

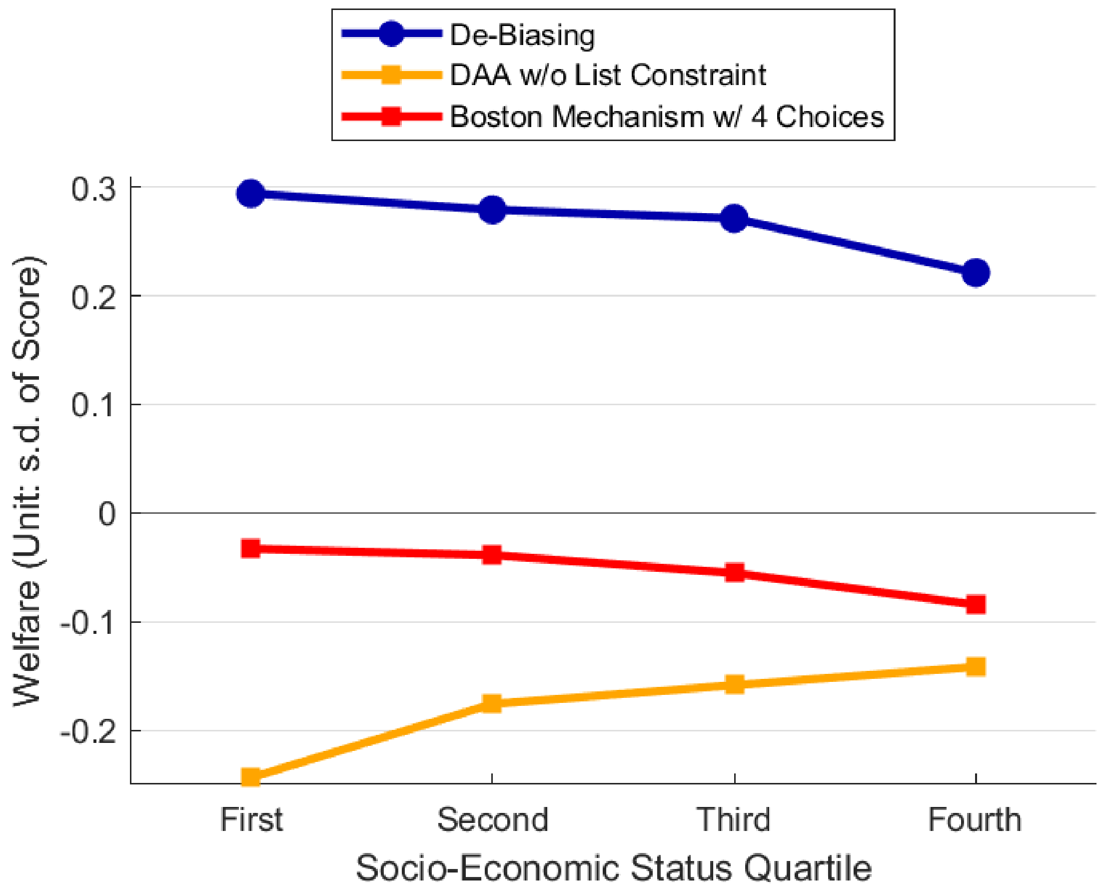


(c) Upward Movement Coefficient γ



Note: This figure compares the fit and data for the key moments in risk-taking strategies, as discussed in detail in Section 1.7. The fit is generated by the structural model that excludes students of the bottom 40% to minimize the impact of the constraint of priority score on college choices in the year of 2015, 2017, and 2018, where township level SES index can be obtained. Green dashed line is generated by the model that excludes the DC Type, but with the same degree of flexibility in preferences. Orange line is generated by our preferred model, the mixture model that allows for both Rational Type and the DC Type. Subfigure (a) plots the mean of unconditional admission probability for the first, second, third and fourth choices. Subfigure (b) plots the share of risk-taking reversals where the unconditional probability of the higher-ranked exceeds that of lower-ranked by more than X%. Subfigure (c) compares the estimated upward movement index, that is, the γ we obtain by running regression 1.5, from Figure 1.4c and Table A.5.

Figure 1.6: Welfare Impact of De-Biasing and Alternative Mechanisms by SES Quartile



Note: This figure presents the mean of welfare impact of de-biasing, as well as switching to alternative mechanisms (Deferred Acceptance Algorithm without list constraints and Boston Mechanism with 4 choices), evaluated separately for students in each quartile of socioeconomic status index. The first quartile is the least advantaged. The fourth quartile is the most advantaged. The discussion of this figure is detailed in Section 1.7.

Table 1.1: Cautiousness of First and Fourth Choices:
Most Advantaged Quartile vs. Most Disadvantaged Quartile

Unconditional Admission Probability	(1) First	(2) Fourth	(3) Fourth-First
Panel A: Summary Statistics of Admission Probability			
Mean	46.91%	91.01%	44.09%
25th Percentile	6.98%	95.66%	7.74%
50th Percentile	43.11%	99.8%	44.18%
75th Percentile	86.17%	100%	83.56%
Panel B: Disadv-Adv Gap - Aggregate Estimate			
Most Disadvantaged Quartile	6.74%	-2.24%	-8.98%
	(0.70%)	(0.40%)	(0.78%)
Benchmark Group		Most Advantaged Quartile	
Predicted Mean: Most Advantaged	44.58%	92.11%	47.53%
	(0.43%)	(0.24%)	(0.48%)
4th-Order Polynomial of Priority Score	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Track (Science or Humanity) Fixed Effects	Yes	Yes	Yes
Panel C: Predicted Mean of Disadv-Adv Gap - by Quantile of Priority Score			
E[Adv-Disadv|Priority Score 40%]	5.77%	-1.66%	-7.43%
	(0.96%)	(0.54%)	(1.08%)
E[Adv-Disadv|Priority Score 60%]	7.72%	-1.97%	-9.69%
	(0.87%)	(0.49%)	(0.97%)
E[Adv-Disadv|Priority Score 80%]	10.36%	-1.88%	-12.23%
	(1.67%)	(0.94%)	(1.87%)
E[Adv-Disadv|Priority Score 99%]	8.76%	-3.72%	-12.48%
	(5.94%)	(3.37%)	(6.65%)

Note: This table reports reduced-form results from administrative data that analyzes the unconditional probability of students' first choices and fourth choices among students from the most disadvantaged quartile and the most advantaged quartile, as described in Section 1.4. Column 1 reports statistics related to the unconditional probability of the first choices. Column 2 reports statistics related to the unconditional probability of the fourth choices. Column 3 reports statistics related to the fourth-first choice gap, in terms of unconditional probability. Panel A reports the summary statistics of aforementioned variables. Statistics in Panel B and C are generated by a fixed effect regression that regresses outcome variables on the interaction of an indicator whether students belong to the most disadvantaged, and a fourth-order polynomial of priority scores. Panel B reports main effect of belonging to the most disadvantaged. Panel C examines the heterogeneity of adv-disadv gap by reporting the predicted mean of gap conditional on priority score quantile.

Table 1.2: Risk-Taking Reversals:
Most Advantaged Quartile vs. Most Disadvantaged Quartile

	(1)	(2)	(3)	(4)
Threshold of Reversal X%: Probability of the Higher-Ranked Exceeds That of the Lower-Ranked by More Than X%	0%	25%	50%	75%
Panel A: Summary Statistics of Share of Risk Taking Reversal				
Mean	55.28%	24.35%	13.47%	6.91%
Panel B: Disadv-Adv Gap - Aggregate Estimate				
Most Disadvantaged Quartile	5.55%	6.64%	4.97%	3.91%
	(0.94%)	(0.80%)	(0.64%)	(0.48%)
Benchmark Group	Most Advantaged Quartile			
Predicted Mean of Benchmark Group	53.12%	21.43%	11.13%	4.94%
	(0.57%)	(0.49%)	(0.39%)	(0.29%)
List of Controls				
4th-Order Polynomial of Priority Score	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Track (Science or Humanity) Fixed Effects	Yes	Yes	Yes	Yes
Panel C: Predicted Mean of Disadv-Adv Gap - by Quantile of Priority Score				
E[Adv-Disadv|Priority Score 40%]	6.71%	7.10%	6.00%	5.79%
	(1.28%)	(1.10%)	(0.87%)	(0.65%)
E[Adv-Disadv|Priority Score 60%]	4.87%	5.89%	4.24%	3.05%
	(1.16%)	(0.99%)	(0.79%)	(0.59%)
E[Adv-Disadv|Priority Score 80%]	3.36%	4.31%	2.23%	0.27%
	(2.23%)	(1.90%)	(1.51%)	(1.13%)
E[Adv-Disadv|Priority Score 99%]	9.78%	8.40%	4.31%	1.58%
	(7.94%)	(6.79%)	(5.4%)	(4.04%)

Note: This table reports reduced-form results from administrative data that analyzes the risk-taking reversal, namely, choosing rank a safer college higher, among students from the most disadvantaged quartile and the most advantaged quartile, as described in Section 1.4. We classify a pair of choices as risk-taking reversal if the gap in terms of admission probability exceeds X%, where X takes the value of 0, 25, 50, 75 in Columns 1, 2, 3, 4, respectively. Panel A reports the summary statistics of share of reversals. Statistics from Panel B and C are generated by a fixed effect regression that regresses outcome variables on the interaction of an indicator whether students belong to the most disadvantaged, and a fourth-order polynomial of priority scores. Panel B reports the main effect of belonging to the most disadvantaged. Panel C examines the heterogeneity of adv-disadv gap by reporting the predicted mean of gap conditional on priority score quantile.

Table 1.3: Testing Framing Effect - Incentivized Survey Responses

Panel A: Framing Effect and Socio-Economic Status			
	(1)	(2)	(3)
	Prob(Red Block) Very Cautious in ROL But Not Very Cautious in Lottery		
SES Index Normalized	-4.7%	-4.7%	-4.1%
	(1.3%)	(1.6%)	(1.4%)
Control: Priority Score	Yes	Yes	Yes
Control: Demographic Variables	No	Yes	Yes
Control: College Preferences	No	No	Yes
Panel B: Estimated Share of Type from Survey Responses			
Share of Directed Cognition Type: Mean	0.0%	40.7%	40.7%
	-	(3.1%)	(2.4%)
Marginal Effect of Normalized SES	0.0%	-7.5%	-7.7%
	-	(3.0%)	(2.7%)
Share of Sincere Type	0.0%	0.0%	1.1%
	-	-	(0.8%)
Number of Parameters	5	8	9
Log Likelihood	-6751.161	-6543.705	-6542.287
Number of Observations	1412	1412	1412
Bayesian Information Criterion	13538.59	13145.43	13149.85
Panel C: Framing Effect and Elicited College Application Behavior			
	Not More Cautious in ROL than Lottery (Red Block)	More Cautious in ROL than Lottery (Blue / Gray Block)	Difference
List the 1st Choice If Only One Spot	27.6%	50.8%	23.2%
	(1.4%)	(2.4%)	(2.7%)
List Top Two Choices If Only Two Spots	27.3%	34.5%	7.2%
	(1.4%)	(2.3%)	(2.6%)
Subject Probability of Meeting the Cutoff of the 1st Choice	48.0%	56.6%	8.5%
	(0.9%)	(1.5%)	(1.7%)
Subject Probability of Meeting the Cutoff of the 4th Choice	72.5%	70.6%	-1.8%
	(1.0%)	(1.6%)	(1.9%)
Share of Reversal: Subjective Probability Higher Ranked - Lower Ranked > 25%	18.4%	23.2%	4.8%
	(1.2%)	(2.0%)	(2.3%)

Note: This table reports empirical analysis from incentivized survey response, as described in Section 1.5. Panel A analyzes to what extent the framing effect, that is, being very cautious in college choice problem than its lottery equivalent, is correlated with students' socioeconomic status and their priority scores, controlling for other variables elicited in the survey. Panel B jointly estimates students' propensity of being a Directed-Cognition Type and their CRRA risk preferences using different specifications. Column (1) excludes any behavioral type. Column (2) estimates a mixture model of the DC and the rational type. Column (3) estimates a model that additionally allows for sincere type. Panel C summarizes whether the average behavior in reported college choices among students who exhibit framing effect differ from those who do not.

Table 1.4: Structural Estimation of Mixture Model using Administrative Data

Panel A: Estimation Results - Rational One-Type vs. Mixture Model						
	(1)	(2)	(3)	(4)	(5)	(6)
Sample (Quantile of Priority Score)	40%-60%		60%-80%		80%-100%	
Model	Rational	Mixture	Rational	Mixture	Rational	Mixture
Estimated Share of DC Type		53.1%		45.1%		55.1%
		(0.61%)		(0.54%)		(0.55%)
Marginal Effect of SES		-3.71%		-3.68%		-6.02%
		(0.57%)		(0.56%)		(0.55%)
Mean Curvature: Rational Type	-0.044	-0.375	0.413	-0.365	0.307	-0.336
	(0.007)	(0.027)	(0.022)	(0.024)	(0.014)	(0.021)
Mean Curvature: DC Type		-0.486		-0.459		-0.478
		(0.055)		(0.033)		(0.021)
Number of Moments	120	120	120	120	120	120
Number of Parameters	20	29	20	29	20	29
Distance	7699.344	4462.128	7008.675	2166.753	4737.862	2213.298
Decrease of Distance in Percentage	42.0%		69.1%		53.3%	
MMSC-BIC (Andrews&Lu, 2001)	6864.396	3702.324	6174.988	1408.098	3903.911	1454.404
Panel B: Out-of-Sample Predictions - Rational One-Type vs. Mixture Model						
Sample (Quantile of Priority Score)	40%-60%		60%-80%		80%-100%	
Model	Rational	Mixture	Rational	Mixture	Rational	Mixture
Distance	5760.837	4903.896	5477.374	3102.324	3947.603	2083.348
Decrease of Distance in Percentage	14.9%		43.4%		47.2%	
Key Moments						
Data: Mean Probability 1st Choices	44.1%		45.6%		63.6%	
Predict: Mean Probability 1st Choices	29.0%	58.4%	32.3%	55.5%	38.5%	67.3%
Data: Mean Probability 4th Choices	85.8%		87.8%		95.0%	
Predict: Mean Probability 4th Choices	99.8%	94.3%	99.3%	92.0%	99.4%	97.4%
Data: Share of Reversals	61.3%		58.0%		51.9%	
Prediction: Share of Reversals	33.0%	52.3%	37.1%	62.2%	36.1%	56.7%
Panel C: Welfare Evaluation Using Mixture Model (Unit: Standard Deviation of Priority Score)						
Sample (Quantile of Priority Score)	40%-60%		60%-80%		80%-100%	
Type	Rational	DC	Rational	DC	Rational	DC
Debiasing	0.368	0.495	0.217	0.253	-0.080	0.082
Deferred Acceptance, Unrestricted List	-0.329	0.231	-0.725	0.447	-0.045	-0.155
Boston Mechanism with 4 Choices	-0.147	-0.066	-0.074	-0.021	-0.152	-0.053

Note: This table reports the results of structural analysis as described in Sections 1.7 and 1.7, for the 40% ∼ 60% (column 1,2), 60% ∼ 80% (column 3,4), and 80% ∼ 100% (column 5,6) subsample. Columns 2,4,6 report estimation of mixture model where DC and Rational Type coexist. Columns 1,3,5 report estimation results of a rational model with flexible structure in college preferences. Standard errors are in parentheses. MMSC-BIC is a model and moments selection criteria for GMM developed by Andrews and Lu (2001). It is analogous to Bayesian Information Criterion in the context of maximum likelihood estimation.

Chapter 2

The Causal Impact of Economics Education on Decision-Making

2.1 Introduction

Does education impact decision-making? Evidence for the causal impact of education on decision-making is mixed. On the one hand, Banks, Carvalho, and Perez-Arce (2019) find that an additional year of required education has no significant impact on the quality of decision-making. On the other hand, a randomly assigned financial education program seems to successfully improve students' decision-making (Kim et al., 2018; Luhrmann, Serra-Garcia, and Winter, 2018). Such mixed results raise the idea that the content of the education—in particular, studying economics—may determine whether education has a causal impact on decision-making.

We follow this line of inquiry by considering a setting in which students educated in the same college are quasi-randomly assigned to different majors. We hypothesize that decision-making skills change as the exposure to an economics curriculum increases, similar to the effect of curriculum in other contexts (Cantoni et al., 2017). While a strand of literature has attempted to distinguish between learning and the selection effects of economics education, primarily on social preferences (Marwell and Ames, 1981; Carter and Irons, 1991; Kagel, Kim, and Moser, 1996; Faravelli, 2007; Fisman, Kariv, and Markovits, 2009; Bauman and Rose, 2011), existing evidence on the presence of causal effects is mixed and could be improved in at least two aspects. First, even with longitudinal data, it is inherently difficult to rule out the selection problem induced by individuals' major choices. Second, while previous studies usually investigate a particular aspect of students' decision-making, an economics education could lead to fundamental changes in risk and several aspects of social preferences, which have not been thoroughly studied.

In this study, we overcome these limitations to evaluate the causal impact of economics education on decision-making by exploiting a unique institutional setting in China where admission is determined by students' scores on the College Entrance Exam (CEE). Due to the assignment rule of this centralized admission system,[1] the distribution of CEE scores among those who end up in the same major are highly concentrated. In the college that we investigated, for example, the standard deviation of scores among the students is less than .1 times the standard deviation of those who took the College Entrance Exam. Thus, those students who ended up studying economics/business within this educational system did so simply because their CEE scores were marginally higher than some competing students. Accordingly, we study the comparable sample created by this environment to evaluate the decision-making behaviors of those students who end up studying economics/business and those who take an alternative major because of marginally lower CEE scores. This key variation allows us to identify the causal effects of an economics education on the decision-making skills of comparable students close to the economics-admission cutoffs.

To capture these decision-making choices, we analyze the data of a survey conducted among students near the economics cutoff scores in a Chinese university, the Central University of Finance and Economics (henceforth CUFE), where a considerable number of students have economics/business majors.[2] The university administrators, who were interested in the impact of majors on students, distributed an online survey to some college students at CUFE via Student Central (an online official campus Learning Management System (LMS) that provides a resource for instructors and students to enrich the teaching and learning experience). To compare students who narrowly met the cutoff of economics/business majors with those who did not, students were invited based on whether their scores were close to the cutoff for whichever economics/business major they applied for.[3] Students who received the invitation used assigned accounts to log in and fill out the survey via computer. The survey asked for students' responses along several dimensions of decision-making—i.e., risk preferences, social preferences, and probabilistic beliefs. These questions were elicited in an incentivized manner.[4]

Our findings convey three main conclusions. First, students in economics/business majors exhibit a significant change in risk preferences. Specifically, students receiving an economics education become more risk neutral compared to those who have the same major preferences but end up in a non-economics/business major. As risk neutrality in small-stakes situations is viewed as an expected utility-maximizing behavior (Rabin, 2000), our findings imply that an economics curriculum may induce students to behave as a consistent expected utility maximizer in small-stake gambles. Second, students in economics/business majors show higher decision-making qualities in the investment game, where probabilistic reasoning is essential. This positive finding in probabilistic beliefs sheds light on the possibility of debiasing statistical reasoning. Third, while our results show that economics/business students' behaviors in small-stake social preference games do not change on the whole, these students' perceptions of others appear to shift significantly. Specifically, economics/business students are more inclined to believe that

[1] See Chen and Kesten (2017) for a detailed introduction to the Chinese college admission system.

[2] We use economics/business to signify majors/programs of study often housed within either economics or business schools.

[3] See Section 2.2 for details about survey distribution.

[4] Students received sign-up compensation and payoffs in each module, based on their responses.

others give less in the Dictator Game, are less engaged in reciprocity in the Trust Game, and share less in the Trust Game. This finding suggests that the economics/business curriculum plays a subtle role in shaping social preferences: while individuals' altruism remains unchanged, an economics education appears to affect how individuals interact with other people, especially in cases where their actions depend on perceptions of others.

The remainder of the paper is structured as follows. Section 2.2 elaborates on institutional details. Section 2.3 discusses our empirical strategy. Section 2.4 presents our main results for the impact of economics education on students' risk preferences, social preferences, and probabilistic beliefs. Section 2.5 conducts a comprehensive set of robustness checks. Section 2.6 offers the conclusions and the limitations of our study.

2.2 Background, Data, and Institutional Details

Enrolling in Majors within Chinese Colleges

The admission process for colleges/universities is centralized in China. Before graduating from high schools, students must take the College Entrance Exam (China's National College Entrance Examination, also known as Gaokao, henceforth CEE). The exam is held once a year on June 7th and 8th, and all students must take it in their province of residence. The CEE includes three mandatory tests—in mathematics, verbal Chinese, and verbal English—and two optional tests in liberal arts and sciences. The maximum score of the CEE is usually 750 points.

After receiving their scores, students are required to declare their college preferences through the centralized system, along with their major preferences within each preferred university. They can rank several universities (the maximum number of which varies from province to province but is usually between four and ten) and subsequently usually rank six choices of major within each university, in order of preference. The deadline for applying to colleges is typically at the end of June, and the admission process follows a college-then-major design wherein universities first admit students based on applicants' scores and college preferences, regardless of their major preferences. Thereafter, the admission process resembles a serial dictatorship, where the priority scores are almost completely determined by students' scores in the CEE [5]. Please see Chen and Kesten (2017) for a thorough description of the mechanism.

Major assignments start after students have been admitted to colleges. Students first need to submit a rank-order list (ROL) that contains up to six majors before the application. The ROL ranks from the most preferred to least preferred major. The assignment rule largely follows the Deferred Acceptance Algorithm (Chen and Kesten, 2019). In other words, each major will first specify an admission cutoff score based on their capacity and student demand (i.e., based on the ROLs that have been submitted), and each student will

[5]Exceptions include minority ethnic groups, an award in the National Olympiad for Math/Physics/Chemistry/Biology/Informatics, an Athletes Award, or demonstrating excellence in some extra exam held by colleges to search for students with special talent.

then be assigned to a major that satisfies the following two conditions: (I) her score is not lower than the admission cutoff score of the major; (II) she ranked the major highest among those that satisfy condition (I).[6] The algorithm starts with students applying to their most preferred majors, and these majors accept students according to capacity and effective score. Majors keep students with the highest score and reject the rest, at which time students who have been rejected by their most preferred major apply to their second choices, again using their effective score. Students who are denied their most preferred major apply to their second most preferred and go through round two selection together. Again, the highest scorers pass through, the remainders are arranged according to third-tier preferences, and so on. The algorithm continues until every student has either been admitted or has exhausted their preference list.[7]

In equilibrium, there will be a sharp admission cutoff (lowest admission score) for each specific major that has imbalance between supply and demand. Given that students' scores are highly homogeneous above and below the admission cutoff scores, conditional on their preferences, this assignment is a quasi-experiment that randomly assigns students to different majors despite their similarity in major preferences and academic ability.

Logistics of Survey Distribution

The rules governing major assignments within this system imply that a causal effect of education on decision-making skills may be sussed out by comparing students who have the same major preferences but end up in different majors due to small differences in their CEE scores. Consequently, the sampling strategy of our online survey closely follows this conceptual comparison. For this study, university administrators categorized majors at CUFE into four groups: economics/business, law/sociology, natural sciences, and humanities/language studies. Each major was classified according to its corresponding curriculum. For example, students from economics/business majors receive far more training in economics compared to students in other non-economics/business majors (see Table B.2 and Table B.3 for details). After students' majors were classified, the administrators started to invite and distribute surveys to students if students had an economics/business major as their most preferred major in their rank-order list and if their scores were not far from the economics/business admission cutoffs.

Upon agreeing to participate, each participant was assigned a personal account to sign into the survey website and was invited to start the survey with any internet-connected

[6]The major assignment is determined by an "effective score". The effective score is exactly the exam score for the vast majority of students, with a few exceptions that are unobserved to us. What we know from conversations with members of the admission committee is that the effective score may be less than the original priority score when students put an extremely competitive major at the bottom of her rank-order list, but such operation is rarely executed and has been gradually abandoned in recent years. Effective scores might be more than the original exam score for reasons including ethnicity, an award in the National Olympiad for Math/Physics/Chemistry/Biology/Informatics, an Athletes Award, or demonstrating excellence in some extra exam held by colleges to search for students with special talent.

[7]At this point, the college will try to accommodate students' preference by assigning them to majors that are not on their list but are as close as possible to their preferences.

computer. To provide participants with reliable access to the survey, the research team built the survey on a collaborative website with CUFE's official website domain.[8] The survey had several modules, including demographic information, risk preferences, social preferences, and probabilistic beliefs. During the survey, backward trace or modifying previously recorded answers was not allowed. In addition, the website system automatically cached the data after each module was finished. In total, the online survey took roughly 50 minutes to complete.

To stimulate participation, we incentivized all the questions we anticipated analyzing, and we awarded students an additional 20 Chinese Yuan (CNY) for participating in the background survey. As incentives varied according to students' responses, the range of payoffs spanned 20 CNY to 422 CNY, with a 50 CNY mean payoff, which is 4-8 times the price of a standard meal at school. The money was directly deposited into students' university accounts at the time they finished the survey. The substantial financial incentive ensured that students had enough impetus to participate in the survey and truthfully report their preferences and beliefs. Together with the school's administrative endorsement, these incentives boosted the response rate—roughly 72% of invited participants finished the survey.

Curriculum at CUFE

Economics/business majors are usually taught as an eight-semester program in the vast majority of Chinese universities. There are around 10,000 students registered in 30 departments and research centers at CUFE, which consists of 80 majors. On average, students in economics and business majors take 47% of their courses in an economics curriculum[9] (roughly 18 courses and 54 credits), followed by general interest courses (27%, roughly 31 credits[10]) and other optional courses. Most economics courses closely follow translated versions of standard US textbooks. Tables B.2 and B.3 summarize how many students take their compulsory courses by the end of each semester if they stick to the curriculum requirements.[11] Among economics students, the most relevant courses—including microeconomics, macroeconomics, and finance—are taken by the end of students' third semester.

Match Between Survey and Administrative Data

To evaluate what impact economics education had on students' decision-making skills, we linked our survey data to university administrative data to obtain students' rank-order lists for preferred majors, enrollment in majors, date of birth, province of origin, and score on the CEE. By matching students' survey responses to the administrative data (the success rate of matching surveys with administrative data was 100% since all the survey

[8] The website address is `prelab.cufe.edu.cn`.

[9] Statistics and probability courses fall under this curriculum.

[10] These courses include English, computer skills, and politics.

[11] Data were taken from the 2017 curriculum schedule for CUFE college students.

samples were drawn from administrative data), we identified some general patterns for applicants' major preferences in our sample.

First, economics/business majors were generally the most preferred majors for the vast majority of students, but many students could not enroll in their preferred economics/business majors due to excessive demand. Panel A in Table B.1 quantifies the distribution of applicants whose 1st, 2nd,..., 6th preferences fell into the categories for economics/business majors, natural sciences, law/sociology, humanities/language studies. Due to our sample's priority for students who listed an economics/business major as their first choice, in Panel A, all 989 selected students had an economics/business major as their most-preferred major preference. Among these 989 applicants, 798 and 687 applicants selected a different economics/business major as their second-ranked and third-ranked major preferences, respectively, whereas other applicants listed a science, law/sociology, or humanities/language major as their next-preferred choice. The last row of Panel A shows how many of our sample's 989 students were finally admitted to an economics/business major, namely 493. This split between those who were admitted to an economics/business major and those who were not provides the quasi-experiment for our analysis.

Second, Panel B of Table B.1 illustrates which ranked major (e.g., first choice, second choice) individuals were ultimately admitted into. This panel demonstrates that 753 out of 989 students were admitted within their top four preferred majors.

2.3 Empirical Strategy

Sample Construction

The empirical strategy we employ is similar to Kirkeboen, Leuven, and Mogstad (2016). Conceptually, our sample consists of students who prefer economics/business majors to other majors but who may have ended up in different types of majors due to small differences between their CEE scores. To understand how we implement this strategy, we provide an example in Panel A of Table 2.1. In our example, two students, Qian and Wu, are both accepted CUFE students, and the college is considering which major to assign the students. We can infer from Panel A that both students prefer economics/business majors to others: Their top three preferred majors all fit under the category of economics/business majors, whereas their bottom three preferred majors are all non-economics/business related. As we have discussed in Section 2.2, despite the same major preferences, Qian will be assigned to her 2nd selected major, accounting, whereas Wu will be assigned to her 4th major, Chinese language/literature. Thus, these students end up in different types of majors merely because Wu's CEE score is slightly lower. The cutoff that contributes to such a difference is the minimum cutoffs of their top three majors. The counterfactual of an individual like Wu would be that she would have been assigned to an economics/business major as long as her CEE score met the cutoff of at least one major in her top three choices. Therefore, the effective cutoff in this case is 631.

To compare students accepted to their preferred major against those not accepted, we have to establish effective cutoffs relevant to their sorted placement. As we have discussed in Section 2.2, the vast majority of CUFE students prefer economics/business majors to other majors. In such cases, the effective cutoff is simply the minimum cutoffs of the economics/business majors that they put on the top of their ROLs. Additional complication arises when students do not always put one category of majors before others. In these cases, we instead consider the local ranking around the major that students are admitted to rather than the whole ROL (global rankings). To clarify what local ranking means, consider another two hypothetical students, Lin and Wang, whose ROLs are presented in Panel B of Table 2.1. Lin and Wang ranked economics/business majors 1st, 3rd, and 5th, and non-economics/business majors 2nd, 4th and 6th. Since Lin is admitted to law (2nd choice), the local ranking for him is finance (1st choice) > law (2nd choice). He would have entered his first ranked major, an economics/business major, if his CEE score met 635. In this case, therefore, the effective cutoff for Lin to enter an economics/business major is 635. However, in Wang's case, he is admitted to his 3rd choice, an economics/business major. The local ranking for him is economics (3rd choice, economics/business major) > marketing (4th choice, economics/business major) > journalism (5th choice, non-economics/business major). He would remain in an economics/business major as long as his score meets 626. In this case, therefore, the effective cutoff for Wang is 626.

To verify that the effective cutoff is indeed the key determinant in one's major, we plot the frequency and admission probability as a function of distance to the cutoff score. In Figure 2.1, we plot the probability of being admitted to an economics/business major against the distance to the cutoff score. Below the cutoff score, the probability of entering economics/business majors is 0, which confirms the admission rule that meeting the cutoff is a necessary condition for admission. The admission probability goes up from zero to nearly 1 after a score passes the admission cutoff. The few exceptions where non-admission occurs even though students' scores met the cutoff are perhaps caused by some special cases we cannot observe in our dataset.[12] Including these small fraction cases within our sample barely affects our results.

Our main analyses of the decision-making survey results focus on students whose CEE scores are close to the effective cutoff. Specifically, a student was included in our sample only if the distance between her CEE score and the effective cutoff was less than 0.1 times the standard deviation of the CEE score's distribution.[13] Within our analyzed sample, all the students preferred an economics/business major to other majors, at least locally. However, despite the same major preferences, some students ended up in an economics/business major simply because their CEE scores were slightly higher than

[12]We talked to administrators on the admission committee and learned there are several possible reasons that could contribute to non-admission. The most important reason relates to instances where students have the same score as the cutoff score but where capacity is constrained. The admission system will use students' score in a particular subject (usually math) to break ties and determine who will be admitted. See Appendix B3 for other possible reasons.

[13]We alter the selection criteria in Section 2.5 to test the robustness of our main results.

others. This arbitrary difference in educational treatment (e.g., economics education or non-economics education) serves as our key independent variable.

Balance Test

Having constructed the target sample, we conducted some balance checks to examine whether there are any systematic correlations between major and other covariates. The results of the balance test are presented in Table 2.2 and show that predetermined covariates are quite balanced in the survey between the treatment group (students who ended up in an economics/business major) and the control group (students who ended up in other majors).[14] Table 2.2 shows that in the survey, 64% of non-economics participants are female, while this number is 62% in general economics fields.[15] The differences in column (4) in Table 2.2 as well as the standard errors in parentheses indicate that there is no substantial difference in these determinants between economics and non-economics students except for pre-college rankings. We find that pre-college rankings for economics students (top 11.86%), on average, is slightly higher than that of non-economics students (top 13.26%). This difference is perhaps driven by the construction of our regression sample: students in economics are on the right-hand side of the admission cutoffs while students in non-economics are on the left-hand side. As a result, economics students on average perform slightly better than non-economics students in high school.

Specification

The aim of our empirical strategy is to estimate and interpret the coefficients of the following equation:

$$y_i = \alpha + \gamma econ_i + \sum_{k=2}^{k=6} \mu_{ki} + \epsilon_i \tag{2.1}$$

Where y_i denotes decision-making choices for individual i. μ_{ki} is a vector of dummies that denotes whether students' k^{th} majors in their rank-order list belong to the economics/business major category.[16] This set of dummies helps us control for the intensity of students' preferences regarding studying in economics/business majors despite the fact that, within this sample, all choose their most preferred major as an economics/business major. $econ_i$ indexes whether student i is in an economics/business major or not based on our selected sample. γ, therefore, is our coefficient of interest, which measures the impact of the economics education on our outcomes of interest. We use this specification

[14] These alternative factors include gender distribution, father's education, mother's education, pre-college ranking, monthly consumption, monthly allowance from parents, and years of boarding before college.

[15] In the administrative data documenting all the students at CUFE, 62% are female.

[16] The first major application is always economics due to the design of our sample selection.

as a benchmark in our empirical analysis. In the following subsections, we analyze the outcomes of risk preferences, social preferences, and probabilistic beliefs, respectively.

2.4 Main Results

Risk Preference

We focus on the two multiple price lists (MPL 1 and MPL 2, hereafter) in the risk-preference module, where students were asked to make choices between two options for a series of questions. In MPL 1, individuals were asked to choose between a series of monotonically increasing certain payoffs, {25 RMB, 30, 35,..., 55, 60} and a fixed lottery {30 with P=0.25 and 60 with P=0.75}. The place where students switch from the fixed lottery to a certain payoff indicates students' risk preferences. For example, students with a switching point equivalent to the certain payoff 35 are more risk averse than those equivalent to 50. To test whether the endowment effect is present—whether students value the lottery more when the question is framed as eliciting Willingness to Accept (WTA) the lottery (Table B.10 in Appendix B1)—we cross-randomized half the participants (480) into the WTA mode of questioning (Table B.10 in Appendix B1) and the other half (509) into Willingness to Pay (WTP) mode (Table B.11 in Appendix B1).

The second scenario used to elicit students' risk preference asked students to decide between two different lotteries. (Hereafter, we call this decision-making problem MPL 2 (Table B.12 in Appendix B1). In this scenario, Option A receives 30 RMB with probability $Pr = 0.25$, and 60 with $Pr = 0.75$. The series of Option B receives 400 with increasing probabilities $Pr(400) = \{0.01, 0.03, 0.05, 0.07, ..., 0.23, 0.25\}$, or receives nothing, with probability $1 - Pr(400)$. Compared to MPL 1, MPL 2 provides subjects with more opportunities to exhibit some risk-loving preferences, which are not encouraged in economics textbooks but quite common in gambling. For example, those who choose Option B given a probability of winning less than 0.13 are opting for the riskier option even when its expected payoff is also lower.

We start by analyzing MPL 1. The proportion of students exhibiting multiple switching in our sample is low (1% for non-economics students and 0.7% for economics students). In our analysis, we exclude students who give multiple switching responses and focus exclusively on students who give consistent answers, with at most one switching. Let p denote the price that students are willing to forgo in exchange for the fixed lottery. The first two columns of Panel A in Table 2.3 summarize the interpretation of students' choices in MPL 1.

We first analyze the difference in the distribution of responses for MPL 1, pooling both WTA and WTP together in Figure 2.3 (the coding of the switching point is shown in the third column of Table 2.3), where the red line in each histogram indicates the switching point that indicates risk neutrality. We can see from the figure that economics students are more risk tolerant, and substantially more economics students are risk neutral.

Based on the interpretation of the switching point in the second column of Table 2.3, students are categorized into four groups: dominated choices, risk loving, risk neutral, and risk averse. We then conduct regression analyses to investigate the causal effect of economics education using the specification in equation (1) in Section 2.3. Columns (1)-(3) of Table 2.4 present the estimation results. Column (1) estimates the impact of an economics/business major ($Econ = 1$) on the proportion of students who appear to be risk neutral. Column (2) measures the impact of an economics/business major on the share of risk-loving students. Column (3) compares the impact of an economics/business major on the proportion of risk-averse students. In these regressions, we exclude people who make dominated or inconsistent choices, and we limit to students who put both an economics and a non-economics/business major in their rank-order list (henceforth, common support of major preference[17]) but who preferred economics/business majors more. Taken together, the results suggest that roughly 11.8% of students who would be risk averse without economics education become risk neutral when making choices in MPL 1.

We additionally examine whether the Willingness to Accept/Willingness to Pay (WTA/WTP) framing affects students' risk preferences. Table 2.5 presents the main analysis using separated sub-samples. The first, third, and fifth columns include students who answer MPL 1 under the WTA mode, whereas the second, fourth, and sixth columns include students who answer MPL 1 under the WTP mode. Under the WTP mode, the proportion of risk-averse and risk-neutral economics students does not significantly differ from non-economics students, suggesting that the economics education does not have a significant impact on students' decision-making when loss aversion is at play. This finding could provide support for the notion that loss aversion is inherent and cannot be eliminated (Chen, Lakshminarayanan, and Santos, 2006). In contrast, under the WTA mode, economics students value the lottery more compared to non-economics students, suggesting that the effect of cation is most salient when the framing effect that stems from loss aversion is muted.

Next we turn to MPL 2 and relate the results to what we have found in MPL 1. The commonality between MPL 1 and MPL 2 is that both lists have two options for students to choose, and Option A is the same fixed lottery (win 30 w.p. 25%, win 60 w.p. 75%). For the purpose of exposition, we denote the winning probability required for students to choose the lottery with payoff of 400 Yuan by p. The first column in Panel B of Table 2.3 describes the choice in MPL 2 and the second column reports the interpretation of the choice. The third column in Panel B of Table 2.3 presents the coding of the choice.

We start with plotting the empirical distribution of the choice for MPL 2 in Figure 2.4, where the vertical red line again indicates the risk-neutral choice. The higher the switching point is, the more risk averse subjects are. There is a more significant spike on risk-neutral choices, particularly among economics students. The primary change in the distribution of the risk preferences manifests among students who used to be risk-loving (i.e., switching point < 15) but who become risk neutral, which we confirm in Columns (4)-(6) of Table 2.4.

[17]Students who had at least a non-economics/non-business major as their choice.

Similar to the findings in MPL 1, in MPL 2, we again find that the proportion of risk-neutral economics students is significantly higher than risk-neutral non-economics students, with the proportion of risk-loving being lower among economics students. Combined, these results suggest that an economics education may decrease students' risk-taking behaviors in an environment where taking more risks would lead to less expected values.

Taken together, both MPL 1 and MPL 2 suggest that economics education induces students to behave as risk-neutral agents. As Rabin (2000) argues, relative to total wealth in a whole life cycle, a modest reward such as what we offered in our study should be viewed as small-stakes gambles. Our preferred interpretation of the finding is that an economics education induces more students to behave like consistent expected-utility maximizers, who arguably have a higher quality of decision-making. There are, of course, other interpretations that are hard to rule out with the data we have. For example, if students believe that there exists "a right answer" and take the survey in "exam mode," those with an economics background might be better at finding the supposed right answer because of their training. Alternatively, students with an economics background are conceivably more likely to try harder when answering such familiar decisions questions thanks to their courses. While we do not take a stand on which interpretation is correct in our case, there is no doubt that students who receive an economics education are more likely to behave as if they are making "consistent" choices, and such changes could possibly affect how decisions are made in other situations.

Social Preference

In the module of social preferences, students were asked to play a series of real-stakes games, wherein they received the payoff promised if their responses were randomly selected for reward.[18] To measure students' social preferences, three social preference games were conducted in the survey: the Dictator Game, the Ultimatum Game, and the Trust Game. For each game, each student was randomly assigned one of three roles: Player A, Player B, or Bystander. In our context, Players A and B play the games and the Bystander answers questions about her beliefs regarding Player A's and/or Player B's actions.

In the Dictator game, Player A corresponds to the Dictator. We asked Player A the following question: How much money out of 500 Yuan are you willing to share with Player B (the "Receiver")? Player B corresponds to the Receiver, and students who were assigned to the role of Player B were informed of the game but did not need to take any action. We asked the Bystander the following question: As a bystander, what do you think is the median value of the Dictator's sharing value in the Dictator Game? In terms of monetary incentive, Player A will get 500 minus the amount she/he sends out, and Player B will get the money that Player A is willing to transfer. For the Bystander, the payoffs depend on the accuracy of her belief. Following the binarized scoring rule (Hossain and Okui, 2013)

[18]The monetary award in this module was designed to be particularly large to boost the survey's response rate. We made it clear in the survey that 20 participants' responses would be randomly selected for award.

as well as previous literature (Krupka and Weber, 2013), the rule is that the closer the belief is to the truth, the more likely the Bystander will be able to win a 500-Yuan award. The probability she wins the award is max{0, 1-(difference between belief and truth/150)}.

The results from this game are summarized in Table 2.6. On average, Dictators share about 190 Yuan out of 500 Yuan (38%) with the other player, and the amount of sharing does not differ significantly across economics and non-economics students, regardless of the regression specifications (Column (1) and (2)). Interestingly, when comparing Columns (1) and (2) (the Dictator's actual sharing) to Columns (3) and (4) (the Bystander's beliefs about the Dictator's sharing), we find that non-economics students' prediction (which is shown at the bottom of Columns (3) and (4)) pretty much aligns with our data on the Dictator's actual sharing (as shown at the bottom of Columns (1) and (2)). Economics students' beliefs about the Dictator's sharing decrease substantially relative to non-economics students, which leads to more inaccuracy and pessimism in beliefs about the Dictator's behavior. We interpret our findings as evidence suggesting that an economics education leads students to believe other people will behave in line with standard game theory predictions but economics education does not change what the student's own social preferences (e.g., altruism, social norms) may be.

In the Ultimatum Bargaining Game, Player A, the Proposer, received 500 Yuan, which she was told to split between herself and Player B in the first step. She could choose any amount to keep (from 0 to 500 Yuan), giving the rest to Player B, the Receiver. In the second step, Player B could choose to accept, which resulted in the same outcome as the Dictator Game, or choose to decline, in which case both players got 0. Player A was asked to propose the amount she would give to Player B, and Player B was asked the minimum payoff he would receive to not decline the proposal. The Bystander was asked to predict the median of the distribution of Player B's rejection threshold. Similar to the Dictator Game, Player A and Player B played the game and received the exact amount of money if their responses were selected. The Bystander was rewarded for the accuracy of her beliefs, with our payout rule saying that the closer the belief was to the truth, the more likely the Bystander would be able to win a 500-Yuan award. The probability she won the award was max{0, 1-(difference between belief and truth/150)}.

The results from this game are summarized in Table 2.7. On average, students' belief about the rejection threshold (Column (4)) is higher than the average of the actual threshold (Column (2)), and the Proposer is willing to share much more (Column (6)) compared to expectations (Column (4)). This result suggests that some Proposers may be extremely averse to being rejected for instrumental or psychological reasons. We do not find significant differences in the rejection threshold, the Bystander's beliefs, and the Proposer's sharing in this game when comparing economics students' behavior to their counterparts.

The Trust Game was approached as follows: Player A, the Sender, could choose to send X amount of 500 Yuan[19] to Player B, the Receiver. She was also informed that what

[19]We intended to say X out of 200 Yuan, but the typo was not identified until the survey had been issued. Consequently, in the reimbursement stage, we paid the selected students the amount stated in the survey.

she sent would be tripled when Player B received the money. Therefore, when Player A shared a value X with Player B, our game would give $3X$ to Player B. Upon receipt of the money from Player A, Player B could choose to send Y amount of $3X$ back to Player A. The Bystander in this game was asked about his beliefs about Player A's and Player B's behaviors, similar to the Dictator Game and the Ultimatum Game. Each student was asked three questions for this game, as detailed below.

In Question 1, students were randomly assigned to play this game as either Player A or the Bystander. Player A was asked to choose the amount of money to send, and the amount X could be 50, 100, or 150. Bystander was asked to predict the mean of the distribution of X.

In Question 2, every student was asked about Player B's choices, namely, how much money Player B would like to give back to Player A upon receiving money from Player A, whose amount was M hypothetically. M could be 50, 100, or 150. For each student, each value of M appeared with equal probability.

In Question 3, every student was asked to predict the average response in Question 2. Specifically, every student needed to predict the median distribution of the amount of money given back if Player A hypothetically gives Player B M Yuan—and M could be 50, 100, or 150. For each student, each value of M appeared with equal probability.

The results of the Trust Game are summarized in Table 2.8. Columns (1) and (2) summarize our findings from Question 1 for the role of Player A and the Bystander, respectively. Columns (3) and (4) present regression analysis for Question 2 and 3, respectively, where we run the following regression:

$$y_i = \beta_0 + \beta_1 econ_i + \beta_2 (M-100)_i + \beta_3 econ_i * (M-100)_i + \sum_{k=2}^{k=6} \mu_{ki} + \epsilon'_i \tag{2.2}$$

Where M is defined as the hypothetical amount of money Player A would like to share. We normalize the amount M by subtracting 100 from the amount, and we denote this normalized amount by M' (hereafter M' indexes $M-100$). The rationale behind this normalization is that in the survey, Player A has only three options for disbursement: 50, 100 or 150 Yuan. We interpret an amount exceeding 100 as a generous action and an amount falling short of 100 as an uncharitable action.

Column (1) in Table 2.8 analyzes how an economics education affects students' sharing behavior as the Proposer in the Trust Game. It reflects the Proposer's beliefs about the other player's trustworthiness and inclination to reciprocate when the Proposer shares more in the first stage. We find that an economics education significantly reduces the amount of sharing, and the magnitude is about 10% of the average sharing among non-economics students. These results are consistent with the finding in Column (4): While there is no significant difference between economics and non-economics students as Player B, when asked how much will be given back if Player A gives 100 Yuan—the middle (and perhaps neutral) action—economics students on average believe that generous sharing by the Proposer has less impact on the other players' reciprocated behavior. To recapitulate, economics students as Player A, on average, share less because they believe that Player

B is less likely to return their generosity. Column (3), on the other hand, informs us that there is actually no significant difference in terms of reciprocity behavior across economics and non-economics students. These three columns together are in line with the Dictator Game findings, where economics students do not change their own social preferences but rather change their views about how fellow students will interact with people. The insignificant result in Column (2) could be interpreted as a second-order belief about subjects' tendency to reciprocate, because the finding reflects beliefs about students' willingness to share, which, in itself, is reflective of the first-order beliefs about Player B's willingness to reciprocate.

Overall, while we do not find strong evidence that economics students change their social preferences (altruism, pro-sociality, norms, etc.), they do change their first-order beliefs about others' social preferences. This observation has significant implications for how economics education could potentially shape human interactions: While economics education may not have a strong effect on people's own social preferences, it may well change how people interact with others in many social activities, as learning economics induces students to regard others as homo-economicus. Due to the limits of our design and sample size, however, the estimates here are not as precise as the results in the risk-preferences module. We hope that future research could pursue this line of inquiry and investigate whether this finding extends to other contexts and larger samples.

Outcomes: Probabilistic Beliefs

The survey also contained three questions on probabilistic beliefs. These three questions were similar in that they asked students to allocate their resources (30 virtual coins for all questions) between two Arrow-Debreu assets, A and B. If event A/B was realized, for each coin allocated to A/B, students would gain a lottery ticket that would yield 200 Yuan with probability 1%.

The first question tested knowledge of the law of large numbers: When flipping a fair coin 1,000 times, event A specifies that the coin's head would appear at least 530 times, and event B complemented event A (less than 530 times). Regardless of preferences, students should always allocate as many of their assets to event B as possible, as the law of large numbers indicates that the probability of event B is almost 1. The second question was a placebo test where event A and B happen with the same probability, and to the best of our knowledge, no psychological heuristics can be related to this question. The third question tested the representativeness heuristic: Flipping a fair coin 100 times, event A specifies that exactly 50 previous flips were heads, and event B is complementary to event A. While the probability of B is almost 1, students who are influenced by "exact representativeness" (Camerer, 1987) may overestimate the probability of A and allocate too many of their assets to event A. More details about these questions are presented in the section on eliciting probabilistic beliefs in Appendix B2.

We can see from Table 2.9 that the results are highly consistent with our hypothesis that economics students are more rational because they allocate significantly more coins to event B for question 1 and 3 (Column (1) and Column (3) in Table 2.9). In contrast, when there is no single optimal strategy, their behavior does not differ much from non-economics students in question 2 (see Column (2) in Table 2.9).

Overall, our results regarding the probabilistic beliefs module suggest that the statistical courses in economics/business majors endow students with the ability to understand and calculate probability. Still, it is unclear whether students can and are willing to apply this acquired skill to their real-life decisions, as students may be responding to the questions in "exam mode." Further study is required.

2.5 Robustness Check

In this section, we conduct three robustness checks to test the validity of our conclusions: (1) discuss heterogeneity in treatment effects and exposure to economic courses; (2) compare non-causal and causal estimates using extra survey samples; (3) test robustness to sample selection criteria. Additional results on all other robustness checks, such as control of financial status, disappointing effects, encoding of major preferences, and gender heterogeneity can be found in Appendix B3.

Exposure and Treatment Heterogeneity

While we cannot tease out the effect of taking a specific course in the economics curriculum, as most students take multiple compulsory courses at the same time, we have sufficient variation in curricula to test changes in decision-making over time because most students take compulsory courses during freshman and sophomore year. Due to the limited power in our design, we will only be focusing on the heterogeneity effect on risk and probabilistic beliefs. Note that the social-preferences module's between-subject design significantly weakens the power, such that there are usually fewer than 400 students in each regression, making it infeasible to divide the sample again by stage-of-education. Therefore, we do not pursue heterogeneity analysis in social preferences for this subsection.

Since our survey was conducted in December, sophomore, junior, and senior students had already finished the first three semesters of their compulsory courses. Among economics students, most relevant courses—including microeconomics, macroeconomics, and finance—are taken by the end of the third semester, as shown in Table B.2 in Appendix B1. Thus, we divided economics students into two groups: freshman vs. post-freshman, and we ran the following regression to examine the heterogeneity in treatment effects among economics students:

$$Y_i = \kappa + \beta_1 econ_i * freshman_i + \beta_2 econ_i * post\_freshman_i + \theta X_i + \epsilon_i \tag{2.3}$$

Where Y is the subjects' response. The constant κ is the average response for non-economics students. $post\_freshman$ indicates whether a student is a freshman or not, where $post\_freshman = 0$ if student i is a freshmen. β_1 and β_2 measure the effects of economic education before and after the main economics courses, respectively. X denotes the control variables, such as major-preference fixed effects under common support of major preferences. If our hypothesis is correct, we would expect that in the presence of significant treatment effects, β_2 would be larger in magnitude (hence, more significant) compared to β_1. Among the regressions with no significant treatment effects, we would expect both β_1 and β_2 to be insignificant.

The results are summarized in Table 2.10. The outcome variable in Columns (1) and (2) is the share of risk-neutral students in MPL 1 and MPL 2. Columns (3), (4), and (5) pertain to the probabilistic belief questions on the law of large numbers (LLN), two identical choices, and the probabilistic belief questions on Exact Representativeness (ER), respectively. Consistent with previous results and our hypothesis, in Table 2.10, β_1s, the education effects on freshmen between economics and non-economics students are barely significant across all the outcomes, as the freshmen have only taken three months of classes. However, β_2 is statistically significant in risk preference (MPL 1 in Column (1) and MPL 2 in (2)), the probabilistic belief questions on the law of large number (Column (3)), the probabilistic belief questions on exact representativeness (Column (5)), and the Bystander's belief in the Dictator Game (Column (7)). Columns (4) and (6) indicate that economics students show no difference relative to non-economics students in the indifferent-choice question (the second question in Section 2.4) and social preference.

Causal vs. Non-causal Estimates

Here, we consider whether causal estimates differ substantially from non-causal estimates and to what extent the magnitude of learning effects compare to those of sorting effects. The construction of our sample limits our ability to assess the full extent of sorting effects, as most students who were invited to participate in the survey were those who chose economics/business majors as their most-preferred majors. To approximate non-causal estimates with existing data, we conducted two types of exercises, both of which are reported in Table 2.11.

In the first exercise, we re-ran the causal specification as reported in Section 2.4, but made two important changes: (1) The university also distributed surveys to those who were not admitted through the college entrance exam (CEE). Among the 1634 students who took the survey, there were 510 who were admitted through non-CEE channels.[20] In Section 2.4, these students were excluded in our causal specification but are included here to form a non-causal sample. (2) We excluded controls on students' major-preference fixed effects in the non-causal specifications here, as these characteristics capture students' preferences toward economics/business majors, a key factor that determines the process of sorting.

In Table 2.11, Panel A reports the results of our non-causal estimates, and we also replicate the corresponding causal estimates from Section 2.4 in Panel C for the convenience of comparison. Different columns correspond to different outcome variables, as indicated at the top of the table. We can see from the comparison that non-causal estimates differ

[20]These channels include, for example, a special college enrollment plan for rural/poor students.

from casual ones, though to a various degree across different outcome variables. The estimates for risk preferences become less statistically significant (Column (1) and (2)), suggesting that sorting effects are present in our context. Similarly, the effect for the ER heuristic becomes smaller and less statistically significant in the causal specification (Column (5)), which is consistent with the fact that non-causal effects are strengthened by both learning effects and sorting effects. In the social-preferences module, the non-causal estimates differ even more from causal estimates. Specifically, we find in non-causal estimates that an economics education is significantly associated with lower sharing (Column (6) of Panel A) but not with Bystander's beliefs (Column (7) of Panel A), which is exactly the opposite of what we find in causal estimates. The statistical significance in our non-causal specification suggests both that some existing findings on economists' selfishness could be explained by sorting effects of major application/assignment and that what economics education really shapes is students' perception about other people's social preferences.

In the second exercise of this section, we considered the following regression to double check the non-causal effects:

$$y_i = \alpha'' + \beta'' * \#PreferredEconMajors_i + \epsilon''_i \tag{2.4}$$

Where variable # *Preferred Econ Majors* represents how many economics/business majors a student i put in her ROL. Since the variable # *Preferred Econ Majors* is a measure for the intensity of students' preferences towards economics/business majors, the coefficient of interest in this specification is β'', because it captures whether students' preference—the key factor that dictates the sorting process—correlates with the outcome variables of interest.

We report the regression results for all major outcome variables in Panel B of Table 2.11. In the risk-preferences module (Columns (1) and (2) of Panel B), stated preferences are significantly and positively associated with the outcome variables, and the sign of the estimates is consistent with the causal effects in Panel C. Similar findings emerge in Columns (3), (4), and (5) for probabilistic beliefs. For the social-preferences module, stated preference is negatively correlated with Dictator's sharing (Column (6) of Panel B), which suggests the existence of substantial sorting in this particular dimension and explains why the estimate is significant in non-causal specification but not in the causal estimations in Section 2.4. In sum, these results suggest that the intensity of preferences for economics/business majors is indeed strongly correlated with our outcome variables of interest and could potentially contribute to sorting effects. Therefore, sorting effects should be carefully controlled for if researchers would like to obtain a causal estimate of the effects of economics education on certain outcome variables.

Criteria of Sample Selection

In Section 2.4, we restrict the regression sample to 0.1 standard deviations within the distribution of CEE to make the students in the treatment and control group more homogeneous and thereby more comparable. In this subsection, we conducted two additional empirical exercises to check whether our results is sensitive to sample selection.

The first exercise was to re-run the regression analysis using an alternative sampling criteria. Specifically, in this robustness test, we limited the sample to students lying in the 0.15 times the standard deviation within the distribution of the CEE score. By applying this new criterion, we obtained a new sample with 963 students, of which 495 were in economics/business majors and 468 were in non-economics/business majors.[21] We then tested the robustness by regressions using equations (1) and (2). The results are shown in Table B.8. We find that the magnitude and significance of most coefficients on economics/business major are robust after the inclusion of the additional sample. Perhaps due to our small sample size, the estimate is somewhat more sensitive to such inclusions in Column (7), the Bystander's belief about reciprocity in the Trust Games.

The second exercise examined RD-type figures that plot outcome variables against the difference between individual's CEE score and the cutoff score for economic majors. Compared to the first exercise, the advantage of this second exercise is that it does not rely on any assumptions on the level of treatment effects as a function of distance-to-cutoff scores. Therefore, the flexibility could unmask the potential heterogeneity in treatment effects and shed light on the sensitivity of regression estimates to bandwidth selection.

We focus on the case of risk preferences, social preferences, and probabilistic beliefs, and our main results are presented in Figures 2.5, 2.6, and 2.7. In Figure 2.5, we plot the share of risk-neutral students against distance-to-cutoff for MPL 1 and 2, respectively, and find that the discontinuity is substantial for both cases around the cutoff score. In Figure 2.6, we plot the outcome variables from the social-preferences module. For the Dictator Game, we can see from the first two plots of the top panel of Figure 2.6 that there is no visible "notch" for Dictator's sharing, but the discontinuity is present for Bystander's beliefs about others' sharing for the Dictator game. These findings are consistent with our causal estimation results from Table 2.6.

In the two graphs of the Trust Game (the third plot in the middle panel and the plot in the bottom panel of Figure 2.6), we find that there is discontinuity for the Proposer's sharing behavior but not for the Bystander's beliefs about sharing. Findings in both the Dictator and Trust Games are in line with our previous regression conclusions from Table 2.6 and Table 2.8.[22] For the Ultimatum Game (the third plot in the top panel and the first two plots in middle panel of Figure 2.6), consistent with our regression results, the discontinuity is absent for Bystander's belief and Proposer's sharing. The discontinuity seems to be significant for the Responder's rejection threshold, where economics education does not seem to have a causal effect on our regression analysis. We believe that this outcome could potentially be caused by our small sample size in the social-preferences module, a limitation of our design. Alternatively, it also could be caused by the differ-

[21] We also restricted students to be in the common support of the rank-order list, limiting the treatment group to students who had at least one non-economics/business major in their preferences. Consequently, in Table B.8, the number of observations is smaller than 963.

[22] We do not plot a RD-type figure for the reciprocity specification because the parameter of interest concerns an interaction term (econ * M').

ences between the two exercises: While the second exercise imposes fewer restrictions on functional forms, it does not include the controls on student-major preferences. Figure 2.7 presents our results on probabilistic beliefs, and visually the findings are in line with our regression estimates.

2.6 Conclusion

Identifying the effect of majors on decision-making is intricate, in part due to students' initial preferences and self-selection in their application. This paper takes advantage of a natural experiment in China to analyze the causal impact of an economics/business major on decision-making. We analyze data by matching survey results for college students near the economics-admission cutoffs to examine the effect an economics education has on peoples' decision-making along several dimensions.

The main results of our paper address three aspects. First, students who receive an economics education are more likely to behave as risk-neutral agents in small-stake choices. On the other hand, our finding that economics and non-economics students are equally sensitive to a loss frame in the Willingness to Pay (WTP) game suggests that education as a debiasing scheme is not guaranteed—even extensive and long-term training may not change some fundamental heuristics.

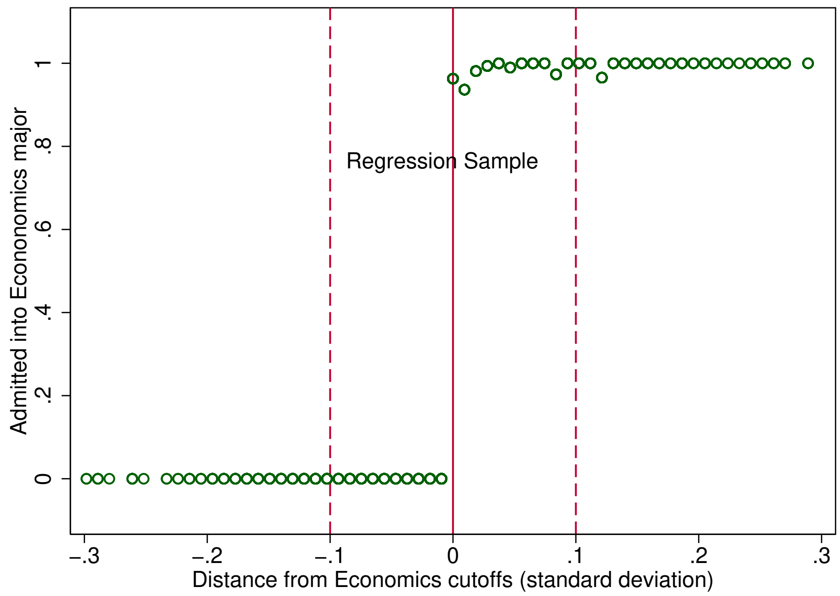
Second, an education in economics/business majors seems to shift individuals' first-order beliefs about others' social preferences (altruism, norms, etc.) more than it shifts students' own social preferences. Individuals who receive an economics education may believe that other people are homo-economicus. This finding may have significant implications for how people who receive an economics education might interact with others.

Third, courses in statistics, which are required in economics/business curricula, successfully endow people with correct probabilistic beliefs.

Taken together, our results show a mixed picture of exposure to economics education: On the one hand, these curricula improve students' decision-making qualities without altering their own social preferences; on the other hand, these curricula lead to substantially biased beliefs about other peoples' social preferences, which could impact social interaction in real life.

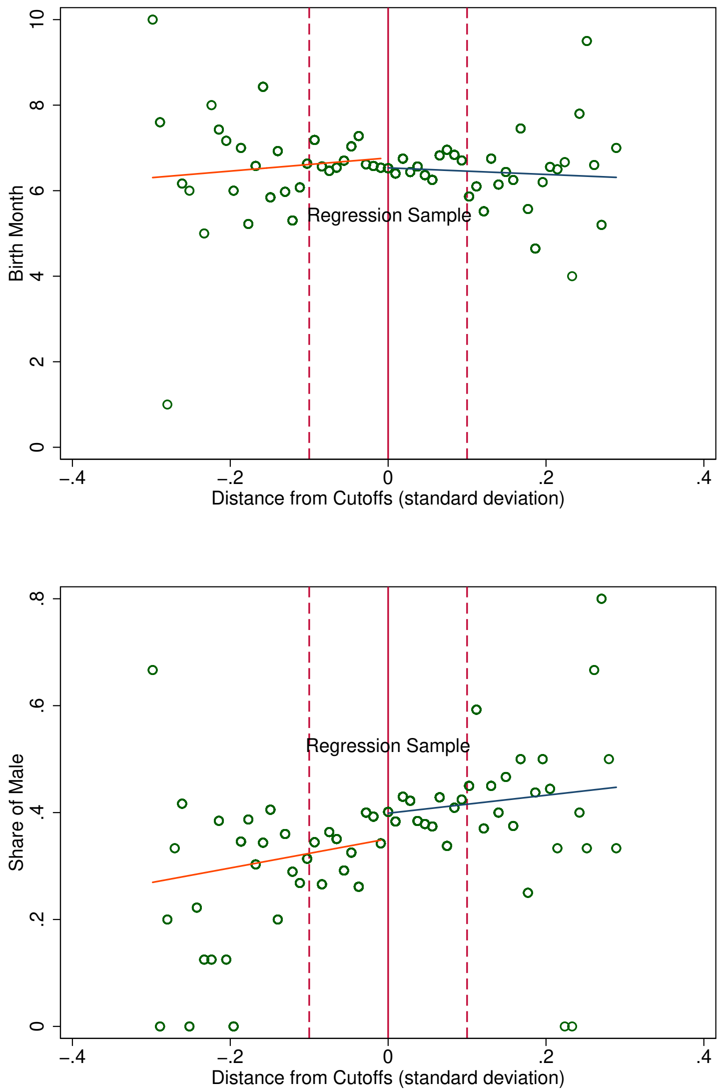
While we have found that the most plausible explanation for our results hinges on the effects of an economics education on students' decision-making, there are three limitations that impede us from drawing stronger conclusions. First, despite the systematic changes in students' survey responses, it remains relatively unclear how much such gaps could affect real-life actions. As we discuss in Section 2.4, students may treat the surveys as exam questions, and economics students might be more capable or exert more effort when responding to these economics-related questions. Second, despite the suggestive findings that within the sample, non-causal specification may overestimate the learning effect in several cases, strictly speaking, we cannot compare the magnitude of this learning effect to the full extent of sorting because most invited survey participants were those whose most preferred choices were economics/business majors. Third, the effects we discovered are limited to one particular university, and the estimates in social-preferences module are not very precise due to the limits of our design. We hope that future research can shed more light on the external validity of our findings and the effects of such preference gaps on real-life decision-making.

Figure 2.1: Probability of being Admitted to Economics and Distance to Cutoff



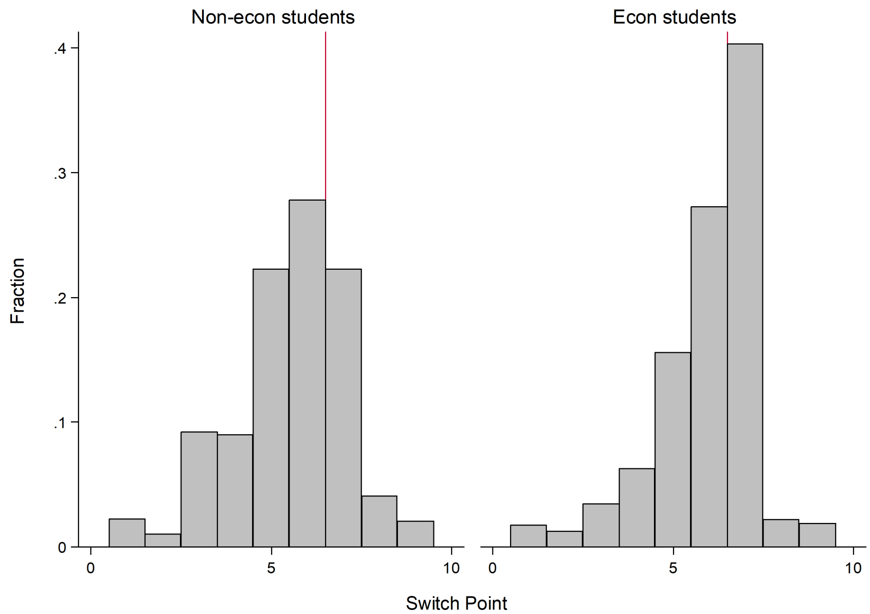
Note: This figure depicts the probability of being admitted to an economics/business major against the distance to the cutoff score using the administrative data of every student. The horizontal axis indicates the distance between an exam score and the corresponding threshold of an economics-admission line. The vertical axis denotes the probability of being admitted to an economics/business major.

Figure 2.2: Birth Month/ Gender Distribution and Distance to Cutoff Using the Administrative Data



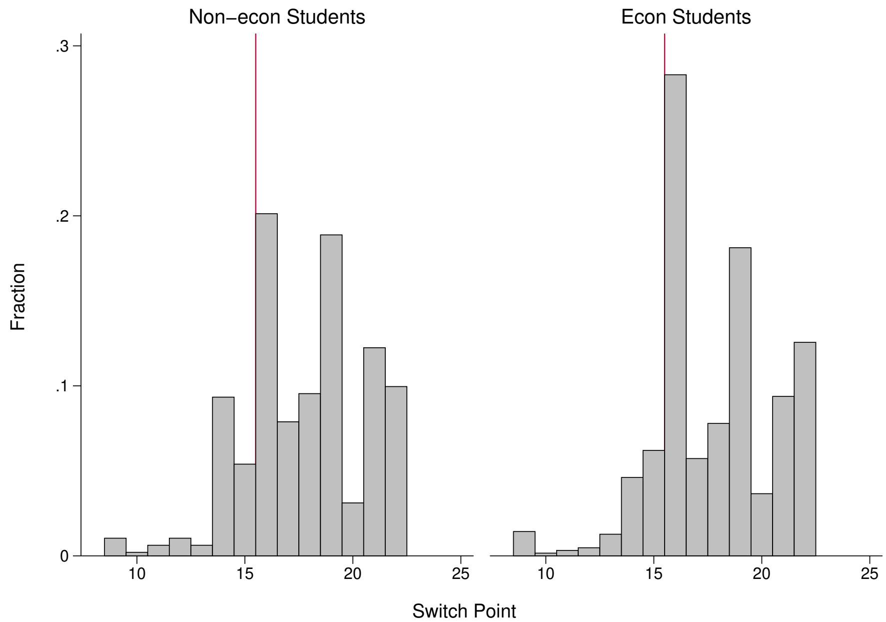
Note: This figure shows the graph of the distribution of pre-determined variables against the distance to the economics/business cutoff score. The vertical axis in the top panel shows the average birth month, and the vertical axis in the bottom panel denotes the share of males conditional on the distance to a cutoff score. The relationship between birth month/ gender distribution and the distance to an economics admission line shows that there is no systematic difference for people near the cutoff scores. The balance test results for the other control variables are shown in Table 2.2

Figure 2.3: Distribution of Switching Points for Risk: MPL 1



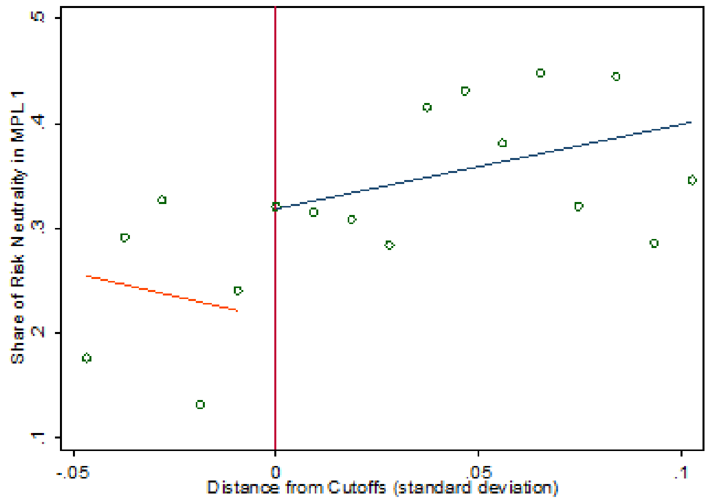
Note: This figure presents the distribution of students' choices in MPL 1, where the red line in each histogram indicates the switching point that indicates risk neutrality. Please see Table 2.3 for the coding of the switching point.

Figure 2.4: Distribution of Switching Points for Risk: MPL 2

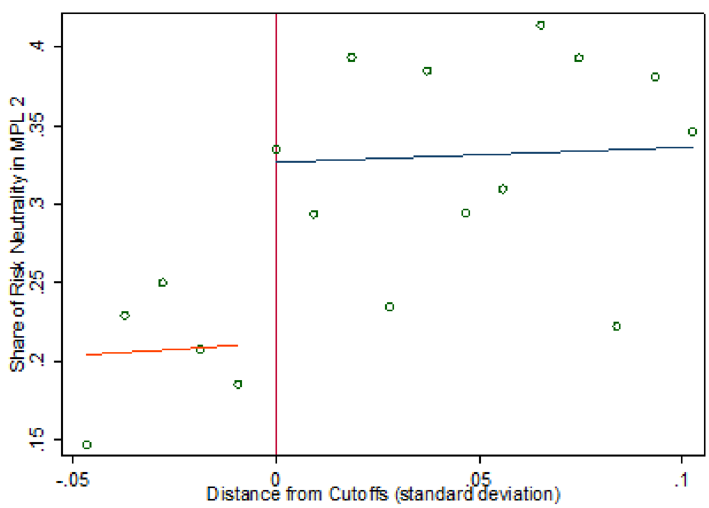


Note: This figure presents the distribution of students' choices in MPL 2. Please see Table 2.3 for the coding of the switching point.

Figure 2.5: Share of Risk Neutrality in MPL 1 and MPL 2 against Distance-to-cutoff



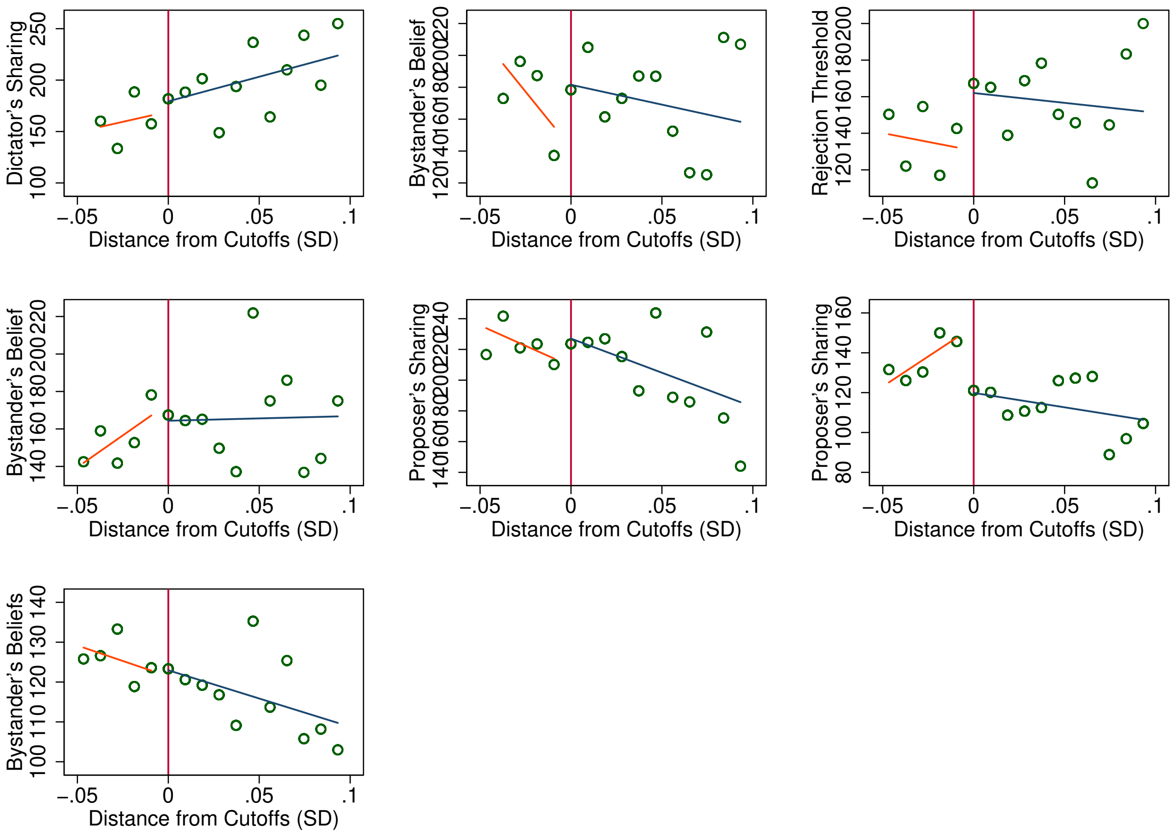
(a) Share of Risk Neutrality in MPL 1



(b) Share of Risk Neutrality in MPL 2

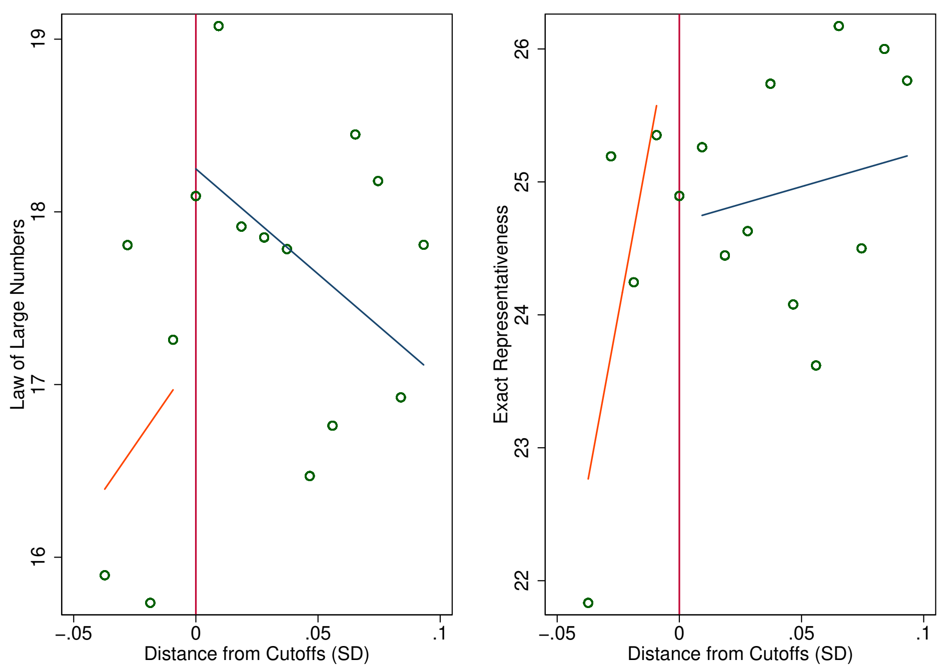
Note: This RD-type figure presents the share of risk neutrality in MPL 1 and MPL 2 against the distance-to-cutoff score of an economics/business major. Consistent with Table 2.4, the shares of risk neutrality are discontinuous around the cutoff.

Figure 2.6: Social Preferences in Three Games against Distance-to-cutoff



Note: This figure shows the outcome variables of the Dictator's Game, Ultimatum Bargaining Game, and Trust Game against the distance to an economics/business cutoff score. The first two plots of the top panel present the Dictator's sharing and Bystander's belief in the Dictator Game (Table 2.6). The third plot in the top panel and the first two plots in middle panel show the patterns of the Rejection Threshold, Bystander's belief and Proposer's sharing in the Ultimatum Bargaining Game (Table 2.7). And the third plot in the middle panel and the plot in the bottom panel display the patterns of the Proposer's sharing and Bystander's belief in the Trust games (Table 2.8).

Figure 2.7: Probabilistic Beliefs against Cutoff Scores



.

Note: This figure demonstrates the patterns of the law of large number and exact representativeness against the distance to an economics/business cutoff score (Table 2.9).

Table 2.1: Cutoff Construction and Identification Strategy: Four Examples

Rank Order	Ranked Major	Field	Admission Cutoff
Panel A: Qian and Wu's ROL			
1st best	Finance	Economics & Business	635
2nd best	Accounting	Economics & Business	631
3rd best	Industry Management	Economics & Business	632
4th best	Chinese language & Literature	Humanity & Language	629
5th best	Law	Law & Sociology	628
6th best	Sociology	Law & Sociology	616
Qian's CEE Score = 631		Admitted Field: Economics & Business	
Wu's CEE Score = 629		Admitted Field: Humanity & Language	
Panel B: Lin and Wang's ROL			
1st best	Finance	Economics & Business	635
2nd best	Law	Sociology	631
3rd best	Economics	Economics & Business	629
4th best	Marketing	Economics & Business	626
5th best	Journalism	Humanity & Language	622
6th best	Sociology	Law & Sociology	616
Lin's CEE Score = 632		Admitted Field: Sociology	
Wang's CEE Score = 629		Admitted Field: Economics & Business	

Table 2.2: Summary Statistics

Variables	Non economics	Economics	Difference
Gender (Female=0, Male=1)	0.36	0.38	-0.02
			(0.02)
Father's education	13.63	13.79	-0.16
			(0.19)
Mother's education	13.15	13.40	-0.24
			(0.21)
Pre-college ranking	13.26	11.86	1.40*
			(0.83)
6-Month consumption	13304.60	13068.56	236.03
			(709.81)
6-Month allowance	13139.96	12321.44	818.52
			(786.36)
Years of boarding before college	2.05	2.02	0.03
	496	493	(0.13)

This table presents the summary statistics of characteristics between economics/business and non-economics/business students. The first column shows the name of the variables for which we conduct the balance test.

Table 2.3: Interpretation of Choice for MPL 1 and MPL 2 under CRRA

Choice	Interpretation	Switching point (question #)
Panel A: Multiple Price List 1		
Always choose certain payment	Dominated choice	1
Choose the fixed lottery iff $p = 25$	Dominated choice	2
Choose the fixed lottery iff $p \leq 30$	Risk averse	3
Choose the fixed lottery iff $p \leq 35$	Risk averse	4
Choose the fixed lottery iff $p \leq 40$	Risk averse	5
Choose the fixed lottery iff $p \leq 45$	Risk averse	6
Choose the fixed lottery iff $p \leq 50$	Risk neutral	7
Choose lottery iff $p \leq 55$	Risk loving	8
Always choose the fixed lottery	dominated choice	9
Panel B: Multiple Price List 2		
Never choose B	Risk averse	9
Choose B iff $p = 0.25$	Risk averse	10
Choose B iff $p \geq 0.23$	Risk averse	11
Choose B iff $p \geq 0.21$	Risk averse	12
Choose B iff $p \geq 0.19$	Risk averse	13
Choose B iff $p \geq 0.17$	Risk averse	14
Choose B iff $p \geq 0.15$	Risk neutral	15
Choose B iff $p \geq 0.13$	Risk neutral	16
Choose B iff $p \geq 0.11$	Risk loving	17
Choose B iff $p \geq 0.09$	Risk loving	18
Choose B iff $p \geq 0.07$	Risk loving	19
Choose B iff $p \geq 0.05$	Risk loving	20
Choose B iff $p \geq 0.03$	Risk loving	21
Always choose B	Risk loving	22

This table presents the interpretation of choices for MPL 1 and MPL 2 under the assumption that students have CRRA preferences. The second column shows the interpretation (risk averse/neutral/loving) corresponding to each potential choice in the first column. The third column presents the encoding of the responses in the first column.

Table 2.4: Risk Preference and Distribution in MPL 1 and MPL 2

	MPL 1 (1) - (3)			MPL 2 (4) - (6)		
	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	Risk Neutral	Risk Loving	Risk Averse	Risk Neutral	Risk Loving	Risk Averse
Econ=1	0.118***	-0.019	-0.099***	0.060**	-0.051*	0.003
	(0.035)	(0.015)	(0.036)	(0.031)	(0.027)	(0.036)
Constant	0.032**	0.034**	0.800***	0.150***	0.035*	0.816***
	(0.039)	(0.017)	(0.041)	(0.044)	(0.019)	(0.046)
Major-Preference FX	X	X	X	X	X	X
Inconsistent Choice Excluded	X	X	X	X	X	X
Common Support of Major Preference	X	X	X	X	X	X
Observations	765	765	765	813	813	813
R-squared	0.041	0.013	0.031	0.039	0.010	0.030
Mean outcome of non-econ	0.23	0.04	0.72	0.20	0.18	0.57
	(0.42)	(0.21)	(0.45)	(0.40)	(0.39)	(0.50)

This table reports the regression results using equation (1) for risk preferences in MPL 1 (column (1)-(3)) and MPL 2 (column (4)-(6)). The leftmost column is the name of our key variables added to the regression. *Econ* is taking the value of one if a students was in an economics/business related major. Columns (1) and (4) estimate the impact of an economics/business major (Econ=1) on the share of students who appear to be risk neutral. Columns (2) and (5) estimate the impact of an economics/business major on the share of risk-loving students. Columns (3) and (6) estimate the impact of an economics/business major on the proportion of risk-averse students.

All Columns control for a vector of dummies that denotes whether students' majors in their rank-order list belong to the economics/business major category (*Major-Preference FX*). In these columns, we also exclude individuals with inconsistent choices, and limit to students who put both economics and non-economics/business majors in the rank-order list (*Common Support of Major Preference*). Columns (1)-(3) additionally exclude people who make dominated choices.

Standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.5: Risk Preference and Distribution in MPL 1

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	Risk Neutral		Risk Loving		Risk Averse	
	WTA	WTP	WTA	WTP	WTA	WTP
Econ=1	0.184***	0.027	-0.034	-0.014	-0.150***	-0.013
	(0.051)	(0.047)	(0.027)	(0.012)	(0.054)	(0.048)
Constant	0.199***	0.131**	0.069**	0.003	0.732***	0.865***
	(0.059)	(0.052)	(0.031)	(0.014)	(0.062)	(0.053)
Major-Preference FX	X	X	X	X	X	X
Inconsistent Choice Excluded	X	X	X	X	X	X
Common Support of Major Preference	X	X	X	X	X	X
Observations	374	391	374	391	374	391
R-squared	0.079	0.032	0.020	0.014	0.048	0.037

This table reports the regression results for risk preferences using Willingness to Pay/Willingness to Accept (WTP/WTA) subsamples. Columns (1), (3), and (5) describe the treatment effects of receiving economics/business education on subjects who make choices under WTA framing. Columns (2), (4), and (6) detail the treatment effects on subjects who make choices under WTP framing. Columns (1) and (2) estimate the impact of an economics/business education ($Econ = 1$) on the share of students who appear to be risk neutral. Columns (3) and (4) estimate the impact of an economics/business education on the share of risk-loving students. Columns (5) and (6) estimate the impact of an economics/business education on the share of risk-averse students.
All columns control for a vector of dummies that denotes whether students' majors in their rank-order list belong to the economics/business major category (*Major-Preference FX*), and exclude people who make dominated or inconsistent choices. We additionally limit the regression sample to students who put both economics and non-economics/business majors in their rank-order list (*Common Support of Major Preference*).
Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 2.6: Social Preferences in Dictator Game

Dep. Var	(1) Dictator's Sharing	(2) Dictator's Sharing	(3) Bystander's Belief	(4) Bystander's Belief
Econ=1	2.591	0.239	-26.550*	-22.990*
	(14.181)	(13.885)	(14.080)	(13.861)
Common Support of Major Preference		X		X
Observations	335	274	344	275
R-squared	0.036	0.050	0.018	0.018
Mean of Non-Econ	186.42	187.87	174.31	179.19
	(104.89)	(101.95)	(112.54)	(106.11)

This table presents the regression results using equation (1) for social preferences in the Dictator Game. The dependent variable is the Dictator's sharing in columns (1) and (2), and the Bystander's belief regarding the mean of Dictator sharing in columns (3) and (4).
All columns control for a vector of dummies that denotes whether students' majors in their rank-order list belong to the economics/business major category (*Major-Preference FX*). In columns (2) and (4), we additionally limit the regression sample to students who put both economics and non-economics/business majors in the rank-order list (*Common Support of Major Preference*).
Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 2.7: Social Preferences in the Ultimatum Game

Dep. Var.	(1) Rejection Threshold	(2) Rejection Threshold	(3) Bystander's Belief	(4) Bystander's Belief	(5) Proposer's Sharing	(6) Proposer's Sharing
Econ=1	9.694	13.851	0.710	-4.101	-11.512	-10.370
	(12.303)	(12.574)	(12.546)	(12.820)	(10.269)	(9.587)
Mean of Non-econ	147.18	147.18	160.96	147.18	228.12	228.12
	(87.41)	(87.40)	(87.66)	(87.41)	(62.51)	(62.51)
Common Support of Major Preference		X		X		X
Observations	336	274	316	262	337	266
R-squared	0.018	0.027	0.012	0.014	0.041	0.039

This table presents the regression results using equation (1) for social preferences in the Ultimatum Game. The dependent variable is the Rejection Threshold of Player B in columns (1) and (2), the Bystanders' belief regarding the mean amount of Player A's sharing in columns (3) and (4), the actual mean of Player A's sharing in columns (5) and (6).
All columns control for a vector of dummies that denotes whether students' majors in their rank-order list belong to the economics/business major category (*Major-Preference FX*). In columns (2), (4), and (6), we additionally limit the regression sample to students who put both an economics and non-economics/business major in their rank-order list (*Common Support of Major Preference*).
Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 2.8: Social Preferences in the Trust Game

VARIABLES	(1) Proposer's Sharing	(2) Bystander's Belief	(3) Reciprocity of Player 2	(4) Bystander's Belief about Reciprocity
Econ* M' (-50, 0, 50)			-0.031	-0.383**
			(0.090)	(0.154)
Econ=1	-13.575**	0.122	-0.992	-1.114
	(6.541)	(6.428)	(3.905)	(6.794)
M'=(-50, 0, 50)			1.201***	1.151***
			(0.056)	(0.098)
Common Support of Major Preference	X	X	X	X
Mean of Dependent Variable	122.13	121.92	97.78	97.62
Observations	409	393	801	801
R-squared	0.020	0.007	0.486	0.190

This table presents the regression results using equation (2) for social preferences in the Trust Game. Column (1) analyzes how an economics education affects students' sharing behavior as a Proposer in the Trust Game, which could be interpreted as students' beliefs regarding the amount that the other players would like to reciprocate. The dependent variable is Bystanders' belief regarding the mean amount of Player A's sharing in the Trust Game in column (2). Columns (3) and (4) ask Player B the amount they would like to give back if Player A gives her 50, 100, 150 RMB and the Bystander's belief regarding the mean amount of Player B's giving back, respectively.
All columns control for a vector of dummies that denotes whether students' majors in their rank-order list belong to the economics/business major category (*Major-Preference FX*), and limit the regression sample to students who put both economics and non-economics/business major in their rank-order list (*Common Support of Major Preference*).
Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 2.9: Probabilistic Beliefs

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.: Coins on asset B	Law of Large Numbers		Indifferent Choices		Exact Representativeness	
Econ=1	0.995**	1.219***	-0.251	-0.331	0.952**	1.134**
	(0.457)	(0.462)	(0.362)	(0.379)	(0.481)	(0.474)
Major-Preference FX	X	X	X	X	X	X
Common Support of Major Preference		X		X		X
Constant	17.102***	17.092***	15.409***	15.413***	24.299***	24.305***
	(0.297)	(0.273)	(0.235)	(0.224)	(0.313)	(0.280)
Observations	989	802	989	802	989	802
R-squared	0.013	0.022	0.006	0.008	0.015	0.023

This table presents the results using equation (1) for probabilistic belief outcomes. Column (1) reports the treatment effect of an economics education on question 1 for the probabilistic beliefs (testing their knowledge of the law of large numbers). Column (2) reports the treatment effects on question 2 for the probabilistic beliefs where no psychological heuristics are linked to this question and any allocation is optimal. Column (3) reports the treatment effects on question 3 for the probabilistic beliefs (testing knowledge of exact representativeness).
All columns control for a vector of dummies that denotes whether students' majors in their rank-order list belong to the economics/business major category (*Major-Preference FX*). In columns (2), (4) and (6), we additionally limit the regression sample to students who put both economics/business and non-economics/business majors in their rank-order list (*Common Support of Major Preference*).
Standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.10: Exposure and Learning Effects

Dep. Var.	(1) MPL1 Risk Neutral	(2) MPL2 Risk Neutral	(3) Law of Large Numbers	(4) Identical Choices	(5) Exact Representiveness	(6) Dictator's Sharing	(7) Bystander's Belief
Econ*Freshman	0.095*	0.051	0.131	-0.529	0.667	17.980	8.269
	(0.051)	(0.052)	(0.679)	(0.558)	(0.699)	(19.408)	(20.388)
Econ*Post Freshman	0.177***	0.084**	1.991***	-0.512	1.680***	-19.103	-27.888*
	(0.036)	(0.037)	(0.480)	(0.395)	(0.494)	(13.998)	(14.619)
Common Support of Major Preference	X	X	X	X	X	X	X
Constant	0.220***	0.252***	17.004***	15.484***	24.210***	191.141***	185.934***
	(0.020)	(0.020)	(0.265)	(0.218)	(0.273)	(7.963)	(8.175)
Observations	802	802	802	802	802	274	275
R-squared	0.030	0.007	0.022	0.003	0.014	0.013	0.016

This table examines the heterogeneity of treatment effects with regard to exposure to an economics education. It presents the average differences between economics students (in a particular year) and non-economics students in the responses on risk preferences, probabilistic beliefs and social preferences, introducing two interaction terms of $Econ \times Freshman$ and $Econ \times Post\_Freshman$ using equation (3). The outcome variable in Columns (1) and (2) is the share of risk-neutral students in MPL 1 and MPL 2. Columns (3), (4), and (5) pertain to the probabilistic belief questions on the law of large numbers (LLN), two identical choices, and the probabilistic belief questions on Exact Representativeness (ER), respectively. The outcome variables in columns (6) and (7) are the Dictator's actual sharing and Bystander's beliefs regarding Dictator's sharing in the Dictator Game.
All columns limit the regression sample to students who put both economics and non-economics/business major in their rank-order list (*Common Support of Major Preference*).
Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 2.11: Non-Causal Effects v.s. Causal Effects of Economics Education

	Risk Preferences (1)-(2)		Probabilistic Beliefs (3)-(5)			Dictator Game (6)-(7)		Trust Game (8)-(11)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	MPL1	MPL2	Law of Large	Two Indifferent	Exact	Dictator's	Bystander's	Proposer's	Bystander's	Reciprocity of	Bystander's Belief
Dep. Var.	Risk Neutral	Risk Neutral	Numbers	Choices	Representiveness	Sharing	Belief	Sharing	Belief	Player 2	about Reciprocity
Panel A: Non-causal Regression: Non-Gaokao Sample Included											
Econ=1	0.153***	0.053**	1.324***	-0.104	1.729***	-16.217*	-13.950	-7.686*	5.547	7.052	-2.636
	(0.023)	(0.023)	(0.301)	(0.249)	(0.323)	(9.120)	(9.850)	(4.273)	(4.980)	(12.695)	(3.242)
M′=(-50, 0, 50)										1.199***	
										(0.246)	
Econ*M′(-50, 0, 50)										0.258	
										(0.315)	
M′=(-50, 0, 50)											1.054***
											(0.063)
Econ*M′(-50, 0, 50)											-0.018
											(0.079)
Observations	1,634	1,634	1,634	1,634	1,634	551	528	825	806	1,631	1,630
R-squared	0.026	0.003	0.012	0.000	0.017	0.006	0.004	0.004	0.002	0.046	0.313
Panel B: Non-causal Regression: No. of Stated Preferences Using Non-causal Sample											
# Preferred Econ Majors	0.031***	0.017**	0.247***	-0.015	0.437***	-5.698**	-2.171	-2.576**	-2.507*	-5.946	-0.510
among Six Preferences	(0.007)	(0.006)	(0.085)	(0.070)	(0.091)	(2.557)	(2.842)	(1.212)	(1.393)	(3.662)	(1.102)
Observations	1,634	1,634	1,634	1,634	1,634	551	528	825	806	1,631	1,630
R-squared	0.013	0.004	0.005	0.000	0.014	0.009	0.001	0.005	0.004	0.002	0.000
Panel C: Causal Regression: Table 2.4,2.6,2.8, 2.9											
Econ=1	0.118***	0.060**	1.219***	-0.331	1.134**	0.239	-26.550*	-13.575**	0.122	-0.992	-1.114
	(0.035)	(0.031)	(0.462)	(0.379)	(0.474)	(13.885)	(14.080)	(6.541)	(6.428)	(3.905)	(6.794)
M′=(-50, 0, 50)										1.201***	
										(0.056)	
Econ*M′(-50, 0, 50)										-0.031	
										(0.090)	
M′=(-50, 0, 50)											1.151***
											(0.098)
Econ*M′(-50, 0, 50)											-0.383**
											(0.154)

This table reports the regression results using a non-causal sample (panel A and B) and the causal regression sample used by this paper (panel C). The dependent variables in columns (1) and (2) are the share of risk neutral students in MPL 1 and MPL 2, in which we pool sample from both Willingness to Accept (WTA) and Willingness to Pay (WTP) mode. Columns (3), (4), and (5) report results on the law of large numbers (LLN), two indifferent choice question, and the probabilistic belief question on Exact Representativeness (ER), respectively. The outcome variables in columns (6) and (7) are the Dictator's actual sharing and Bystander's beliefs regarding the Dictator's sharing in the Dictator Game. Column (8) analyzes how an economics education affects students' sharing behavior as a Proposer in the Trust Game, which could be interpreted as students' beliefs regarding the amount that the other players would like to reciprocate. The dependent variable is Bystanders' beliefs regarding the mean amount of Player A's sharing in the Trust Game in column (9). Columns (10) and (11) ask Player B the amount they would like to give back if Player A gives 50, 100, 150 Yuan and Bystander's beliefs regarding the mean amount of Player B's giving back.

Panel B reports the results using regression specification (4), # *Preferred Econ Majors* represents how many economics/business majors a student i put in her ROL.

Panel C copies the previous coefficients in Section 2.4 for comparison. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Chapter 3

When Information Conflicts With Obligations: the Role of Motivated Cognition

3.1 Introduction

Do people react differently to the same objective information if it conflicts with, rather than conforms to, their fundamental values? In the presence of such conflicts, numerous observational studies document learning failures that lead to belief polarization, suggesting a correlation between fundamental values and information acquisition.[1] A causal interpretation of such correlations comes from the theory of motivated cognition: to gain psychological utility, individuals actively distort their beliefs to conform with their fundamental values (Bénabou, 2015; Bénabou and Tirole, 2011). However, a spurious correlation between fundamental values and information acquisition might arise under standard Bayesian updating, where information spillovers exist among individuals who share similar values. To isolate the causal effect of fundamental values on information acquisition, one needs to randomly assign fundamental values without altering information sets, a task seemingly impossible in common settings.

In this paper we attempt to answer whether religious norms, a core aspect of fundamental values, can causally shape information acquisition as predicted by the theory of motivated cognition.[2] We focus on a unique setting where the month of Ramadan overlapped with the extremely high-stakes, once-a-year Chinese College Entrance Exam (CEE) between 2016 and 2018. In concordance with previous literature (Oosterbeek and van der Klaauw, 2013), we document that Ramadan fasting has substantial negative impacts on

[1]For example, despite overwhelming scientific evidence, public opinion is polarized on topics such as GMO foods (Priest, 2000), evolution (Plutzer and Berkman, 2008), and global warming (Hart and Nisbet, 2012).

[2]The theoretical literature on motivated cognition in economics (Bénabou, 2015; Bénabou and Tirole, 2011) is also closely related to an older psychology literature on motivated reasoning, as summarized by Kunda (1990).

the exam performance of Muslim students, using administrative data. Consequently, Muslim students who were about to take the CEE during Ramadan in 2018 were facing a conflict between the fundamental value that Ramadan fasting is morally desirable and the empirical evidence that the secular cost of such practice can be significant.

Leveraging this empirical setting, we conducted a field experiment in 2018, which investigates how Muslim CEE takers' fundamental values attached to Ramadan fasting might affect their processing of information on how Ramadan fasting affects exam performance. Specifically, we present students with a previously unreleased graph, Figure 3.1a, which shows (based on administrative data) that the CEE performance gap between Muslim and non-Muslim students remained stable between 2011 and 2015, but suddenly enlarged substantially in 2016, when the CEE started to fall in the month of Ramadan. We then ask these students, in an incentivized manner, to read from this graph the magnitude of the 2016 CEE performance gap between Muslim and non-Muslim students. It is worth noting that these students are being asked to answer a purely objective question. In the absence of motivated cognition, whether they "trust" or "like" this graphical information should not affect *what information is being presented in this graph*.

Our core experimental innovation is that, prior to showing students this graphical information, we randomly offer a subset of the students reading materials in which well-respected Muslim clerics use Quranic reasoning to explain that it is permissible for students to be exempted from fasting until after the exam. While this "pro-exemption" reading material could substantially reduce the psychological costs of postponing the fast, from the perspective of classical Bayesian theory, such a change in fundamental values should not affect how these students read the objective graphical information on the cost of fasting.[3] In contrast, the theory of motivated cognition predicts that, in the presence of stringent religious norms, Muslim students attach high fundamental values to practicing Ramadan fasting, and thus might engage in "reality denial" (Bénabou and Tirole, 2016): they could try to rationalize their own fasting practice by intentionally misreading clear signals about the high cost of Ramadan fasting on exam performance. In this case, the "pro-exemption" reading material, by reducing the fundamental value that students attach to Ramadan fasting, could potentially help them more accurately interpret how Ramadan fasting affects exam performance when reading Figure 3.1a.[4]

We find that, without receiving our graphical information based on administrative data, Muslim students tend to severely under-appreciate the potential cost of Ramadan fasting on CEE performance. When clearly presented with such information, Muslim stu-

[3] In two control arms, we also show that the reading material alone does not change students' beliefs about the cost of fasting. This confirms our premise that the reading material does not change the information set and is thus irrelevant in Bayesian updating. We explain this in greater detail in Section 3.4.

[4] To state the student's tradeoff more specifically, he is incurring a psychological cost of self-deception in exchange for anticipatory utility generated by expecting better performance in the upcoming CEE. Therefore, if the student gains utility from the observance of Ramadan (or gets disutility for breaking the fast), he is incentivized to underestimate the cost of fasting, so that he can adhere to fasting without suffering from the undesirable belief about having poor performance in the CEE. When we provide the student with the pro-exemption reading material, however, the utility from observance is decreased, thereby alleviating his incentive to manipulate his own belief about the cost of fasting.

dents who have not read the “pro-exemption" article (control group) show strong patterns of motivated cognition: when reading Figure 3.1a, they systematically underestimate the 2016 CEE score gap between Muslim and non-Muslim students, despite the fact that this information is purely objective and clearly presented in front of their eyes. In contrast, when asked to estimate the same gap from the same graph, Muslim students who have read the “pro-exemption" article (treatment group) are 40% more accurate, consistent with predictions of the theory of motivated cognition.[5] Using a “list experiment" approach, we also provide suggestive evidence that alleviating motivated cognition makes students better informed about the costs of Ramadan, and thus more willing to delay the fast until after the CEE.

We conduct a series of additional analyses to better understand the underlying mechanisms. First, when students are asked to guess the magnitude of the 2016 CEE performance gap between Muslim and non-Muslim students without seeing our graphical information, reading the “pro-exemption" article alone does not change their prior on the cost of Ramadan fasting. Second, we show that the bias in information cognition is most salient among students who strictly practiced Ramadan fasting in the past, and they are also the ones who respond strongly to our provision of “pro-exemption" reading materials. This suggests that the baseline findings are indeed driven by students’ fundamental values attached to Ramadan fasting. Third, in a placebo test, we find that receiving the “pro-exemption reading material" does not affect information acquisition on issues unrelated to Ramadan fasting.

Our paper speaks to two strands of literature. First, it provides a direct experimental test for motivated cognition in a field setting with high-stakes information. Our paper complements existing laboratory studies that have established the existence of motivated cognition,[6] and also adds to the field evidence on motivated beliefs by addressing the identification challenge with a randomized experiment.[7] Specifically, we find that motivated cognition can take place at the very beginning of the decision-making process, before information storage (Chew, Huang, and Zhao, 2019; Zimmermann, 2020) and the potentially complex process of (non-)Bayesian updating (Eil and Rao, 2011; Mobius et al., 2011).[8] Such “reality denial" is yet to be widely documented, despite being a distinct prediction generated by the theory of motivated cognition (Bénabou, 2015; Bénabou and Tirole, 2011, 2016).

[5] It is worth noting that, after the survey, all students are provided access to the “pro-exemption" article. This means that, beyond the survey, which took place a month before the CEE, our intervention should not create any subsequent difference in CEE performance between the treatment and control groups. This is later confirmed by the eventual CEE score in 2018, which shows no significant correlation with our intervention.

[6] For example, see Eil and Rao (2011); Mobius et al. (2011); Di Tella et al. (2015); Ambuehl (2017); Exley and Kessler (2018); Chew, Huang, and Zhao (2019); Zimmermann (2020).

[7] For observational studies on motivated beliefs, see Di Tella, Galiant, and Schargrodsky (2007); Oster, Shoulson, and Dorsey (2013); Huffman, Raymond, and Shvets (2019); Schwardmann, Tripodi, and Van der Weele (2019)

[8] This complements existing studies that investigate the role of motivation in beliefs or decision making (Dana, Weber, and Kuang, 2007; Di Tella et al., 2015; Exley, 2016; Exley and Kessler, 2018).

Our paper also sheds light on the costs and benefits of religious participation. In addition to confirming the costs of Ramadan fasting (Almond and Mazumder, 2011; Oosterbeek and van der Klaauw, 2013; Schofield, 2014; Almond, Mazumder, and Van Ewijk, 2015; Majid, 2015), we also show that such significant costs are severely under-appreciated by practicing Muslims (due to motivated cognition), which is consistent with conjectures in the literature (Kuran, 2018).[9] More broadly, such under-appreciation of the costs of religious activities, when combined with a "rational choice" framework of religious behaviors,[10] could help explain the prevalence of religious participation.

3.2 Background

In this paper, for both the analysis of administrative data and the survey experiment, we focus on the Ningxia Hui Autonomous Region (henceforce Ningxia), which is a provincial unit in the northwest of China, with a population of 6.3 million and a GDP pc of $7103.

Among the 6.3 million residents in Ningxia, 38% are Hui, a Muslim minority ethnic group in China, and the rest are mainly Han, the majority ethnic group in China, who are non-Muslim. Due to the large presence of Hui people, Islam is the dominant religion in Ningxia. There are currently more than 3300 major mosques and more than 4000 certified Imams in Ningxia. In comparison, there are fewer than 200 religious sites for all the other religions combined, including churches, Buddhist temples, Taoist temples, etc.

In this section, we introduce the background of our empirical setting: the College Entrance Exam in China, Muslim Ramadan fasting, and how the overlap between Ramadan and the exam affected the performance of Muslim students.

Muslim Ramadan Fasting

Ramadan is the 9th month in the Islamic Calendar, and is observed by Muslims around the world as the holy month of fasting (Sawm) to commemorate the first revelation of the Quran to Muhammad. Fasting during Ramadan is regarded as one of the "five pillars of Islam." It requires abstinence from food and liquids (including water) from dawn to sunset, and is obligatory for practicing Muslims.

The Quran specifies certain subjects for whom exemptions from the fast can be granted, which include children, the ill, the elderly, travelers, and breastfeeding women. However, many other conflicts between secular activities and religious practices are not explicitly discussed in the Quran, and, under these conditions, practicing Muslims typically rely on

[9] On the benefit side of the equation, Augenblick et al. (2016) find that religious followers sincerely attach high pecuniary values to their religious beliefs, and Campante and Yanagizawa-Drott (2015) find that Ramadan fasting increases happiness. Our paper complements these papers by investigating the cost side of the equation.

[10] For example, Azzi and Ehrenberg (1975); Iannaccone (1992, 1998); Montgomery (1996); Stark and Finke (2000); Berman (2000).

a local expert in Islamic jurisprudence (Faqih) to decide whether they may be exempted from fasting.[11]

Due to the difference between the Islamic (lunar) calendar and the commonly used Gregorian calendar, Ramadan shifts 11 days forward every year and has a 33-year cycle. The detailed fasting schedule changes every year and is different across regions based on each location's latitude, which is publicized locally by the Imams before the start of the month of Ramadan.

Ramadan and Exams

Between 2016 and 2018, the month of Ramadan fell in May and June, which are popular times for final exams and high school and college entrance exams around the world. As a result, millions of Muslim students worldwide faced a dilemma between practicing the Ramadan fasting and excelling in academic exams. For example, as described in an information paper by the Association of School and College Leaders, 2016 was the first time Ramadan had clashed with major exams and tests in the UK since the 1980s, and this overlap would continue until 2019/20.[12]. Existing evidence suggests that taking exams during Ramadan has significant negative impacts on the performance of Muslim students (Oosterbeek and van der Klaauw, 2013; Schofield, 2014).

The problem was particularly severe for Chinese Muslim students: between 2016 and 2018, the extremely high-stakes College Entrance Exam in China, which is fixed on June 7th and 8th for all students, fell in the month of Ramadan. When deciding how they observe Ramadan, students need to take into consideration: (1) the great importance of the CEE for their future, (2) the negative impact of fasting on CEE performance, and (3) any flexibility to postpone the fast until after the CEE. While there is little doubt that most CEE-takers are well aware of the importance of this exam, neither (2) nor (3) is fully clear in the Chinese context: no empirical evidence exists regarding the cost of Ramadan on CEE performance, and little information regarding "whether the fast could be delayed until after the exam" could be found on the Chinese internet or other media outlets.[13]

The Costs of Taking the CEE During Ramadan

To identify the causal impact of taking the CEE during Ramadan on students' academic performance, we obtained administrative data on the exam performance of every

[11] For instance, the Egyptian national soccer team qualified for the FIFA World Cup in 2018, but the game was scheduled to start right after the end of the month of Ramadan. Seeing this potential conflict, the Grand Mufti of Egypt, Shawki Allam, granted the Egyptian national squad permission to postpone their Ramadan fasting obligations. On the contrary, the Tunisian national team faced the same problem, but did not get such an exemption, and the players kept fasting while preparing for the World Cup.

[12] "Ramadan: Exams and Tests, 2018", visited on Aug 5, 2018

[13] Two pieces of relevant information could be found through online search engines: an article written by an Imam arguing that students should keep fasting during the CEE, and a translated piece based on the statement of the Egyptian Grand Mufti, suggesting students could delay their fast under certain circumstances.

urban student in Ningxia who took the CEE between 2011 and 2016. This information is maintained by the Ningxia Educational Examination Institute, and is the predominant criterion of college admission. This administrative dataset contains the exam score of every urban CEE-taker in Ningxia during the six-year period, as well as their basic background information, such as ethnicity, gender, age, etc.

Exploiting the fact that the CEE began falling in the month of Ramadan in 2016, and the fact that Ramadan is expected to mostly affect the performance of Muslim students, we illustrate the impact of taking the exam during Ramadan by measuring how the Hui-Han gap in exam scores changed in 2016, relative to the pre-existing gaps between 2011 and 2015. As shown in Figure 3.1a, the Hui-Han gap in exam scores was stable between 2011 and 2015: on average Hui students score 15 points lower than their Han counterparts.[14] However, the Hui-Han gap almost doubled in 2016, suggesting that taking the exam during Ramadan had salient negative impacts on the relative performance of Muslim students.

We formalize these graphical patterns in Figure 3.1b and Table C.2, in which we estimate Difference-in-Differences specifications controlling for a rich set of fixed effects. Our results suggest that the empirical patterns documented in Figure 3.1a are highly robust, both qualitatively and quantitatively.

In this context, a score loss of roughly 15 points is a huge burden for the Muslim students, and would very likely lead to admission by a lower-ranked college, or at least a less desirable major within the same college.[15] It is also worth noting that our DiD model estimates an "Intention to Treat (ITT)" effect, rather than a "Treatment on the Treated (TOT)" effect, given the fact that not all Hui students are practicing Muslims, and some of them might not fast during the exam. Therefore, the real impact of fasting during the exam would be even larger.[16]

3.3 Experiment and Hypotheses

In this section, we explain the design and implementation of our field experiment, and lay out the main testable hypotheses that will guide the subsequent empirical investigations.

[14]The enlarged gap in 2014 was driven by the fact that more Hui students chose the humanities track rather than the STEM track, and the humanities track exam was relatively difficult in 2014. This fluctuation disappears once we control for a Track-by-Year Fixed Effect in the regression analysis.

[15]To put the magnitude in context, in Ningxia, winning in the highly prestigious National Mathematics Olympiad Competition would be rewarded with only 5 bonus CEE points.

[16]As shown in Table C.3, in our experimental sample, around 54% of high school students never broke a fast. If the sample is representative of Ningxia, this would suggest that the TOT effects could be nearly twice as large as the ITT estimates.

Experiment

We partnered with a large urban Hui Muslim high school in Ningxia to conduct a survey experiment.[17] The high school is the second largest in its prefecture city, with 24 classes in its senior cohort (students who were about to take the CEE in June 2018). The majority of students are Hui Muslim, and the average CEE score in the school is comparable to the provincial average. More than 80% of the students board at school on the weekdays, making a student's religious compliance generally observable to other students.

Our survey experiment took place on May 4th, 2018 (about one month before the CEE in 2018), during a 40-minute afternoon class on Friday, simultaneously for the entire senior cohort. The 533 Hui students who were present constitute our population for this study. Our survey questions were answered carefully by the majority of students, as reflected by the fact that most of them correctly answered our multiple choice questions based on a 1000-word reading material.

As summarized in Table C.1, our survey experiment has a 2-by-2 design. Randomly, half of the students read an article arguing for exemptions to delay the fast for the CEE (*Exemption*); the other half read a placebo article on art and philosophy (*No Exemption*). In the meantime, we cross-randomized the graphical information received by students: half of the students were incentivized to read a graph about the Hui-Han CEE score gap (*Information*), while the other half were incentivized to read a graph about the Sino-Janpanese income gap (*No Information*). The 533 Muslim students participating in the study were randomized into one of these four arms: *Exemption*Information*; *Exemption*No Information*; *No Exemption*Information*; and *No Exemption*No Information*. The students were unaware of this randomization during the survey experiment.

In our treatment reading material, we summarized statements from well-respected Chinese Muslim leaders as an article of about 1000 words, which clearly explained that it would be permissible to delay the fast until after the CEE. Specifically, we interviewed an established Muslim scholar, the Imam of an historic mosque, who explicitly said that "*Muslim students should delay their fast until after the CEE is finished*." We also interviewed a famous religious leader, who is the vice president of the provincial Islamic Association, and were told that "*we should interpret the Quran in the modern context and allow the CEE participants to delay their fast*." The two Imams also explained the Quranic reasoning behind their arguments in greater detail. We also collected similar exemptions given in Egypt and France to further demonstrate the case. For the control reading material, we edited an article from a famous Chinese writer, which is about different perspectives in appreciating art, and has roughly the same length as the religious reading. For both treatment and control readings, to ensure that students understood the materials correctly, we asked three multiple choice reading comprehension questions after the main texts, and students got monetary rewards if they answered these questions correctly.

[17] "Hui Muslim high schools" are public schools set up by the government in regions with high concentrations of Hui population, which provide accommodations for the dietary and other religious needs of the Hui students.

Our main outcome of interest is whether a student could accurately acquire the information regarding the cost of taking the CEE during Ramadan. To measure such cognitive accuracy, for students in *Exemption*Information* and *No Exemption*Information*, we randomly presented them with Figure 3.1a, which documents how the Hui-Han gap in CEE score was stable between 2011 and 2015, but enlarged abruptly in 2016. The scale of Figure 3.1a was intentionally labeled in a coarse way, where we only showed the max (0) and min (-40) values, but omitted all the intermediate scales, so that the students had to read carefully to accurately report the enlarged Hui-Han gap in 2016.

We explicitly told these students that "*between 2011 and 2015, the CEE did not overlap with Ramadan, and the Hui-Han CEE gap was relatively stable (-14.7 in 2011, and -16.6 in 2015); however, in 2016, the CEE fell in the month of Ramadan, and the Hui-Han CEE gap enlarged in this year. Please read and report the Hui-Han gap in 2016 from the graph.*" In order to incentivize careful reading of the gap, we offered cash rewards to students whose estimates were in the top 50% in terms of accuracy. The main hypothesis is that if students think they need to fast during the CEE (*No Exemption*Information*), they would be motivated to underestimate the cost of fasting, and therefore would tend to have downward biases when reading the gap from the graph. On the contrary, when aware of the pro-exemption arguments from Muslim leaders (*Exemption*Information*), some students would think that they do not have to fast during the CEE, and would thus be able to absorb the same graphical information with less influence from psychological motivations, and therefore get more precise estimates.

Both our anecdotal knowledge and the recent literature suggest that Muslim students might not be fully aware of the negative impacts of fasting (Kuran, 2018). To verify whether this is the case in our context, for students in *Exemption*No Information* and *No Exemption*No Information*, we did not show them the "Hui-Han CEE gap" graph (Figure 3.1a). Instead, we just told them "*between 2011 and 2015, the CEE did not fall in the month of Ramadan, and the average Hui-Han CEE gap was -16.4; however, in 2016, the CEE fell in the month of Ramadan,*" and then we asked the students to guess the 2016 Hui-Han CEE gap, in an incentivized way. By doing so, we could elicit students' priors regarding the Hui-Han CEE gap, in the absence of any intervention.

For the students who did not read the "Hui-Han CEE gap" graph (*Exemption*No Information* and *No Exemption*No Information*), we also conducted a placebo test, where we asked them to read a graph on the Sino-Japanese income gap, as illustrated in Figure 3.1c. Since exemptions to delay the fast should not affect motivations to distort beliefs about the Sino-Japanese income gap, we expect no difference in reading the gap in this graph between those who received the exemption reading and those who did not.

For students in all four arms, in addition to the randomized arm-specific contents (pro-exemption vs. placebo reading; Hui-Han vs. Sino-Japanese graph), we also asked them a common set of questions on basic individual characteristics, including age, gender, parental education, access to computer/internet, academic track, whether boarding at school, whether the student prays daily, whether the student ever broke a fast during high school, etc.

At the end of the questionnaire, we also conducted a "List Experiment" for every student, where we provided five statements about the CEE, four of which were subjective and unrelated to religion, including "*(1) learning alone is more effective than learning in groups, (2) we should care about what we have actually learned more than the CEE score itself, (3) playing sports is good for exam preparation, (4) the CEE mainly tests on familiarity with the material rather than actual intelligence;*" and one statement was about Ramadan fasting, "*(5) delaying the fast until after the CEE is acceptable*." We asked each student *how many* of the five statements they agree with, without having to specify which statements in particular. By comparing the number of statements agreed with in each experimental arm, we could estimate the impacts of our experimental interventions on fasting attitudes.

Given the 2-by-2 design, we prepared four different types of questionnaires: *No Exemption*No Information*, *Exemption*No Information*, *No Exemption*Information*, *Exemption*Information*. All questionnaires have an identical cover letter explaining that the survey data is confidential and will be used for purely academic purposes. We pre-randomized the order of the questionnaires before distributing them in each classroom; as a result, the 533 Muslim students were randomly assigned one of the four types of questionnaires. Given that the cover letters were identical and the students were not able to communicate with each other during the survey, the students did not realize that they were assigned differentiated questionnaires until the end of the survey experiment.

In Appendix Table C.3, we conduct balance tests across the four different arms for all the baseline characteristics. The four arms are well balanced with each other, suggesting that the randomization was well-executed.

Testable Hypotheses

To rationalize the experimental design and guide the empirical analysis, we propose a simple conceptual framework based on the theory of motivated cognition. In this model, a subject jointly chooses two parameters: (1) his belief about the average cost of Ramadan on CEE performance; and (2) whether or not to postpone the fast during the CEE. By doing so, he maximizes his own utility, which consists of three components: (a) anticipatory utility of exam results; (b) benefits from sticking to the religious practice; and (c) the cognitive cost of manipulating his own beliefs.[18]

In this section, we lay out the main testable hypotheses derived from the model, and briefly explain the underlying intuition. The details of the model, including its setup, mathematical proofs, and full propositions, are elaborated in Appendix C.2.

When reading the 2016 Hui-Han CEE score gap from Figure 3.1a, in the absence of the pro-exemption reading material, Muslim students would underestimate the true gap. Students who stick to fasting due to stringent religious norms are motivated to underestimate the cost of fasting.

When presented with the 2016 Hui-Han CEE score gap from Figure 3.1a, students who received the pro-exemption reading material would read the graphical information

[18]In a related paper, Schwardmann (2019) presents a theoretical model with conceptually similar tradeoffs (in the context of healthcare investment), and proposes potential empirical tests of the model's predictions, which we are able to execute with our field experiment.

more accurately. This is the main test of our paper. Receiving the exemption relaxes the religious constraint, which should alleviate the motivation to underestimate the cost of Ramadan on exam performance, and lead to more accurate readings of Figure 3.1a.

When Muslim students receive the graphical information (on the cost of Ramadan on CEE performance) and the pro-exemption reading material at the same time, they will be more willing to delay the fast. Receiving the pro-exemption reading material directly enables students to delay the fast, while also helping them better appreciate the graphical information on the cost of fasting. Both effects would result in increased willingness to delay the fast.

3.4 Results

In this section, we analyze the experimental data to test the the theory of motivated cognition, and discuss whether alternative explanations could rationalize our findings.

Hypotheses 1 and 2: Fundamental Values and Motivated Cognition

Hypothesis 1 predicts that Muslim students would distort their own beliefs when learning about the cost of taking the exam during Ramadan, which leads to an underestimation of the true cost. And according to Hypothesis 2, such cognitive bias can be alleviated by relaxing the stringency of the religious norm (*Exemption*).

To test these two hypotheses, we examine the accuracy of graph-reading by Muslim students in "*No Exemption*Information*" and in "*Exemption*Information*." Specifically, for all the Muslim students who were asked to read the Hui-Han CEE gap (Figure 3.1a), we estimate:

$$Gap_i = \alpha \cdot Exemption_i + X_i' \cdot \beta + \gamma + \epsilon_i \tag{3.1}$$

where Gap_i is student i's estimate of the Hui-Han CEE gap in 2016 based on reading Figure 3.1a. $Exemption_i$ is a dummy variable, which equals 1 if student i received the pro-exemption reading material, and 0 otherwise. X_i is a vector of individual characteristics, γ is a constant, and ϵ_i is the error term.

Since the constant term γ reflects students' estimation of the gap in the absence of any exemption, if Muslim students distort the objective graphical evidence presented to them (Hypothesis 1), γ should be significantly smaller than the true gap (-29.4). Since α reflects the extent to which the pro-exemption reading material could reduce the students' bias in graph-reading, α should be negative and significant, as predicted by Hypothesis 2.

As shown in Table 3.1, for those who did not receive the "pro-exemption" reading material, the average estimated gap is -24.4, which understates the true gap by about 5 points (statistically significant). When randomly assigned the "pro-exemption" reading material, the students' reading of the 2016 CEE gap enlarged by 2 to 2.2 points, eliminating roughly 40% of the baseline cognitive bias. In Columns 1 and 2, the coefficient of interest remains highly robust as we control for class fixed effects and a rich set of individual controls. These empirical patterns confirm the main hypothesis of this paper: the stringency of religious practices leads to motivated cognition regarding the cost of religious behaviors (Hypothesis 1), and the relaxation of religious norms could help alleviate such cognitive bias (Hypothesis 2).

The theory of motivated cognition also implies that our intervention will have heterogeneous treatment effects: students who strictly followed Ramadan fasting in the past would attach higher fundamental values to this religious norm, which means they have stronger incentives to manipulate their beliefs to underestimate the cost of Ramadan, but they should also be more responsive to the provision of pro-exemption reading materials.

In the survey, we asked each student "whether you strictly practiced Ramadan fasting (never broke a fast) throughout high school." Roughly 54% of the students answered "Yes" to this question, and the ratio is balanced across the four arms due to random assignment. In Columns 3 and 4 of Table 3.1, we interact "whether a student strictly followed Ramadan fasting in the past" with "whether the student received the pro-exemption reading material." Consistent with our hypothesis, the baseline findings of the initial cognitive bias among Muslim students, and the subsequent de-biasing effect of providing pro-exemption reading materials, are both stronger among the more religious students, with the caveat that the de-biasing effect is only marginally significant.[19]

Hypothesis 3: Fasting Decisions

As predicted by Hypothesis 3, when both graphical information on the cost of fasting on CEE performance and pro-exemption reading materials are provided to students simultaneously, the students would become more willing to delay the fast: the exemption not only mechanically reduces the students' mental cost of postponing the fast, but also helps the students better appreciate the cost of fasting on CEE performance, which further increases their perceived return to delaying the fast.

As explained in Section 3.3, directly eliciting students' willingness to postpone the fast might be deemed "sensitive" and lead to mis-reporting. To circumvent this issue, we follow the literature to conduct a "list experiment," in which we present students with five statements related to the CEE, one of which says *"delaying the fast until after the CEE is acceptable"* and the other four are unrelated to students' religious beliefs. Students only need to report *how many* of the statements they agree with, and do not need to indicate specifically *which* statements they agree with, which alleviates the social image concerns related to directly admitting to one's willingness to postpone the fast.

In this list experiment, if, on average, students in a certain experimental arm agree with more statements than students in other arms do, we can infer that the corresponding intervention causally increased students' willingness to postpone the fast for the CEE. As shown in Table 3.2, relative to the control group (*No Exemption*No Information*), just showing students the Hui-Han CEE gap alone (*No Exemption*Information*) barely changes students'

[19]We also define an alternative outcome variable $Deviation_i$, which directly measures how far each student's reading deviates from the true value (-29.4), thus taking into account that some students might over-estimate the gap. All the main findings remain with this alternative outcome variable, as shown in Appendix Table C.4.

willingness to delay the fast, while just providing students with the pro-exemption reading (*Exemption*No Information*) makes them more willing to delay the fast. Importantly, the combination of both exemption and information (*Exemption*Information*) persuades the most students to postpone the fast for the CEE, which is consistent with our hypothesis that *Exemption* complements *Information* by alleviating motivated cognition.

Ideally, it would be interesting to also investigate the subsequent impacts of our interventions on the actual fasting behaviors and CEE performance of these students. However, to ensure that our interventions could potentially benefit more students, upon finishing the survey, we provided all students access to the "pro-exemption reading material" and the "Hui-Han CEE gap graph." As a result, beyond the survey experiment, we no longer have any experimental variation to identify the eventual impacts on fasting behaviors and exam outcomes.

Mechanisms for Cognitive Bias

We now investigate the underlying mechanisms behind our baseline findings.

Direct Impacts of Exemption

A potential concern is that, in addition to alleviating the students' religious constraints, the pro-exemption reading material itself might carry some information on the cost of Ramadan: for example, students might infer from the Imam's statements that fasting could hurt exam performance, which makes the information presented in the Hui-Han CEE figure more credible. In principle, this interpretation should not confound our main findings, because our main test focuses entirely on the students' reading of the objective information presented in Figure 3.1a, and whether or not they find such information credible should be of no relevance.

Nevertheless, we explicitly investigate whether the pro-exemption reading material itself directly affects students' priors on how Ramadan affects CEE performance. Specifically, for students who do not receive graphical information on the Hui-Han CEE gap (arms *"No Exemption*No Information"* and *"Exemption*No Information"*), we first informed them about the benchmark Hui-Han CEE gap between 2011 and 2015, and then asked them, in an incentivized manner, to make their most accurate guess on the 2016 Hui-Han CEE gap when the exam happened during Ramadan.[20]

By comparing the elicited guesses on the enlarged 2016 Hui-Han gap between "*No Exemption*No Information*" and "*Exemption*No Information*," we can test whether the exemption itself affects the students' priors about the cost of Ramadan on exam performance. As shown in Table C.5, in the absence of the pro-exemption reading material, students guess that the 2016 Hui-Han CEE gap was -17.9, which is statistically indistinguishable from the average gap between 2011 and 2015 (-16.4). This is consistent with conjectures

[20]We tell the students "*Between 2011 and 2015, the CEE was held outside of the month of Ramadan, and the average score gap between Hui and Han students was -16.4 points. In 2016, the CEE was held in the month of Ramadan. Please give us your most accurate guess: what was the average Hui-Han CEE score gap in 2016?*"

in the literature that many practicing Muslims are not fully aware of the cost of their religious activities (Kuran, 2018). Importantly, when students receive the pro-exemption reading material, their elicited guess of the 2016 Hui-Han CEE gap barely changes at all, confirming that providing the exemption alone does not change the students' priors on the 2016 Hui-Han gap.

Placebo Test

As motivated cognition is generated by the fundamental values attached to Ramadan fasting, receiving an exemption to delay the fast should not affect the cognitive accuracy regarding topics unrelated to either the CEE or Ramadan fasting.

To further rule out alternative mechanisms, we conduct a placebo test, where some students read the religious article (exemption) and were required to estimate the Sino-Japanese income gap from Figure 3.1c (*Exemption*No Information*), and others read the placebo article (about art) and were required to estimate the Sino-Japanese income gap from the same graph (*No Exemption*No Information*).

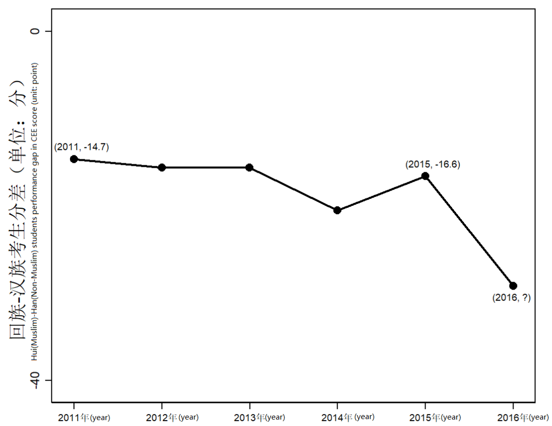
As can be seen in Table C.6, students in general tend to underestimate the Sino-Japanese income gap.[21] But, importantly, reading about the religious exemption has no statistically meaningful impact on the accuracy of reading the Sino-Japanese income gap, suggesting that our findings are indeed driven by religion-induced motivated cognition, rather than alternative mechanisms.

3.5 Conclusion

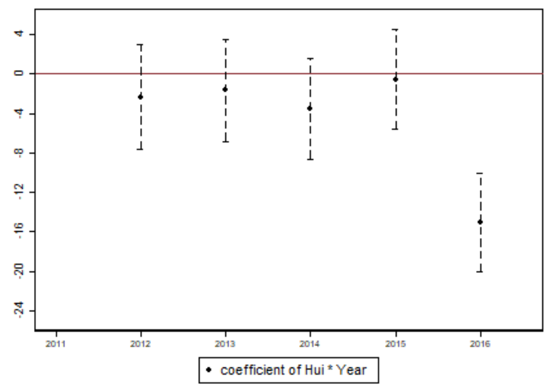
In this paper, we find that, when information conflicts with one's fundamental values, an individual may exhibit strong patterns of motivated cognition by significantly distorting the "undesirable" information in his learning process, even if the information is purely objective and of very high stakes. These findings suggest that, in order to effectively disseminate important information on polarized issues (e.g., climate change, vaccination, etc.), it is crucial to first identify and intervene against the underlying fundamental values that might prevent individuals' accurate digestion of the high-stakes information.

[21]The true gap is -30771, while the students in the control group on average read -28434.

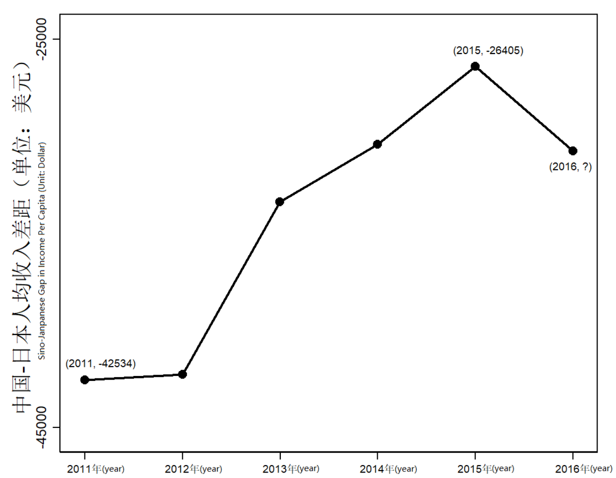
Figure 3.1: Graphical Information



(a) Hui-Han CEE Gap (2011-2016)



(b) DiD Estimates: Hui-Han CEE Gap



(c) Sino-Japanese Income Gap (2011-2016)

Note: Panel 3.1a displays the Hui-Han CEE score gap between 2011 and 2016. Panel 3.1b displays the DiD coefficients of the Hui-Han CEE gap (controlling for Track-by-Year FE), with 95% confidence intervals. Panel 3.1c displays the Sino-Japanese income gap between 2011 and 2016. Panels 3.1a and 3.1c are the graphs presented to students in our survey experiment, with English translations of the Chines labels.

Table 3.1: Motivated Cognition in Reading Graphical Information

	(1)	(2)	(3)	(4)
	Perceived Hui-Han CEE Score Gap in 2016			
Exemption	-1.9032***	-2.1985***	-0.5822	-0.8654
	(0.7387)	(0.7451)	(1.0672)	(1.0935)
Fast			2.5805**	2.9742***
			(1.0425)	(1.0753)
Exemption*Fast			-2.6181*	-2.5483*
			(1.4617)	(1.5348)
Constant	-24.3954***		-25.6952***	
	(0.5289)		(0.7399)	
Mean of Control	-24.395	-24.395	-24.395	-24.395
Class FE	No	Yes	No	Yes
Control Variables	No	Yes	No	Yes
Number of Observations	277	274	277	274
R squared	0.024	0.233	0.045	0.242

Note: Columns 1 and 2 present the effects of receiving exemption to delay fast on the accuracy of reading the 2016 enlarged Hui-Han gap in CEE performance. As shown, the average gap read by students is -25.4, 4 points smaller than the true value of -29.4; receiving an exemption would make the guess 2 points closer to the true value. Columns 3 and 4 present heterogeneous treatment effects of exemption based on fasting history. As shown, students who strictly followed the Ramadan fasting during high school had larger downward bias to start with, and responded to the religious intervention by eliminating such cognitive bias. On the contrary, students who did not strictly follow Ramadan fasting were not responsive to the exemption. Robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 3.2: Fasting Attitudes Revealed in List Experiment

	(1)	(2)	(3)
	Agreed Statements in List Experiment		
Exemp*No Info	0.1769*	0.1924*	0.2168*
	(0.1065)	(0.1085)	(0.1107)
No Exemp*Info	0.0383	0.0540	0.0485
	(0.1051)	(0.1074)	(0.1089)
Exemp*Info	0.2936***	0.2988***	0.3216***
	(0.1038)	(0.1063)	(0.1075)
Constant	1.3543***		
	(0.0754)		
Mean of Control	1.354	1.354	1.354
Class FE	No	Yes	Yes
Control Variables	No	No	Yes
Number of Observations	532	531	528
R squared	0.019	0.053	0.088

Note: This table presents the effects of the graphical information, the pro-exemption reading material, and their interaction on the number of statements one agreed with in the list experiment. The results suggest that receiving the exemption alone makes one more willing to delay fast, receiving the information does not have any significant impact, and receiving both the religious and information interventions have the most powerful persuasion effects. Robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Bibliography

Abdulkadiroğlu, Atila, Yeon-Koo Che, and Yosuke Yasuda. 2011. "Resolving conflicting preferences in school choice: The" boston mechanism" reconsidered." *American Economic Review*, 101(1): 399–410.

Agarwal, Nikhil, and Paulo Somaini. 2018. "Demand Analysis using Strategic Reports: An application to a school choice mechanism." *Econometrica*, 86(2): 391–444.

Agarwal, Nikhil, and Paulo Somaini. 2019. "Revealed Preference Analysis of School Choice Models." *Annual Review of Economics*, 12.

Ajayi, Kehinde F. 2013. "School choice and educational mobility: Lessons from secondary school applications in Ghana." *Institute for Economic Development Working Paper*, 259.

Allcott, Hunt, Benjamin B Lockwood, and Dmitry Taubinsky. 2019. "Regressive sin taxes, with an application to the optimal soda tax." *The Quarterly Journal of Economics*, 134(3): 1557–1626.

Almond, Douglas, and Bhashkar Mazumder. 2011. "Health capital and the prenatal environment: the effect of Ramadan observance during pregnancy." *American Economic Journal: Applied Economics*, 3(4): 56–85.

Almond, Douglas, Bhashkar Mazumder, and Reyn Van Ewijk. 2015. "In utero Ramadan exposure and children's academic performance." *The Economic Journal*, 125(589): 1501–1533.

Ambuehl, Sandro. 2017. "An Offer You Can't Refuse? Incentives Change How We Inform Ourselves and What We Believe." CESifo Group Munich.

Andrews, Donald WK, and Biao Lu. 2001. "Consistent model and moment selection procedures for GMM estimation with application to dynamic panel data models." *Journal of econometrics*, 101(1): 123–164.

Arteaga, Felipe, Adam J Kapor, Christopher A Neilson, and Seth D Zimmerman. 2021. "Smart matching platforms and heterogeneous beliefs in centralized school choice." National Bureau of Economic Research.

Artemov, Georgy, Yeon-Koo Che, and Yinghua He. 2017. "Strategic 'mistakes': Implications for market design research." *NBER Working Paper*.

Augenblick, Ned, Jesse M Cunha, Ernesto Dal Bó, and Justin M Rao. 2016. "The economics of faith: using an apocalyptic prophecy to elicit religious beliefs in the field." *Journal of Public Economics*, 141: 38–49.

Azzi, Corry, and Ronald Ehrenberg. 1975. "Household allocation of time and church attendance." *Journal of Political Economy*, 83(1): 27–56.

Banks, James, Leandro S Carvalho, and Francisco Perez-Arce. 2019. "Education, Decision Making, and Economic Rationality." *Review of Economics and Statistics*, 101(3): 428–441.

Bauman, Yoram, and Elaina Rose. 2011. "Selection or indoctrination: Why do economics students donate less than the rest?" *Journal of Economic Behavior & Organization*, 79(3): 318–327.

Bénabou, Roland. 2015. "The economics of motivated beliefs." *Revue d'économie politique*, 125(5): 665–685.

Bénabou, Roland, and Jean Tirole. 2011. "Identity, morals, and taboos: Beliefs as assets." *The Quarterly Journal of Economics*, 126(2): 805–855.

Bénabou, Roland, and Jean Tirole. 2016. "Mindful economics: The production, consumption, and value of beliefs." *Journal of Economic Perspectives*, 30(3): 141–64.

Berman, Eli. 2000. "Sect, subsidy, and sacrifice: an economist's view of ultra-orthodox Jews." *The Quarterly Journal of Economics*, 115(3): 905–953.

Bhargava, Saurabh, George Loewenstein, and Justin Sydnor. 2017. "Choose to lose: Health plan choices from a menu with dominated option." *The Quarterly Journal of Economics*, 132(3): 1319–1372.

Bordalo, Pedro, Nicola Gennaioli, and Andrei Shleifer. 2012. "Salience theory of choice under risk." *The Quarterly journal of economics*, 127(3): 1243–1285.

Bordalo, Pedro, Nicola Gennaioli, and Andrei Shleifer. 2021. "Salience." National Bureau of Economic Research.

Calsamiglia, Caterina, Chao Fu, and Maia Güell. 2020. "Structural estimation of a model of school choices: The Boston mechanism versus its alternatives." *Journal of Political Economy*, 128(2): 642–680.

Camerer, Colin F. 1987. "Do biases in probability judgment matter in markets? Experimental evidence." *The American Economic Review*, 77(5): 981–997.

Camerer, Colin F, Eric Johnson, Talia Rymon, and Sankar Sen. 1993. "Cognition and framing in sequential bargaining for gains and losses." *Frontiers of game theory*, 104: 27–47.

Campante, Filipe, and David Yanagizawa-Drott. 2015. "Does religion affect economic growth and happiness? Evidence from Ramadan." *The Quarterly Journal of Economics*, 130(2): 615–658.

Campbell, John Y. 2016. "Restoring rational choice: The challenge of consumer financial regulation." *American Economic Review*, 106(5): 1–30.

Cantoni, Davide, Yuyu Chen, David Y Yang, Noam Yuchtman, and Y Jane Zhang. 2017. "Curriculum and ideology." *Journal of Political Economy*, 125(2): 338–392.

Card, David, Stefano DellaVigna, Patricia Funk, and Nagore Iriberri. 2020. "Are referees and editors in economics gender neutral?" *The Quarterly Journal of Economics*, 135(1): 269–327.

Carrell, Scott E, Marianne E Page, and James E West. 2010. "Sex and science: How professor gender perpetuates the gender gap." *The Quarterly Journal of Economics*, 125(3): 1101–1144.

Carter, John R, and Michael D Irons. 1991. "Are economists different, and if so, why?" *The Journal of Economic Perspectives*, 5(2): 171–177.

Chade, Hector, and Lones Smith. 2006. "Simultaneous search." *Econometrica*, 74(5): 1293–1307.

Chen, M Keith, Venkat Lakshminarayanan, and Laurie R Santos. 2006. "How basic are behavioral biases? Evidence from capuchin monkey trading behavior." *Journal of Political Economy*, 114(3): 517–537.

Chen, Yan, and Onur Kesten. 2017. "Chinese college admissions and school choice reforms: A theoretical analysis." *Journal of Political Economy*, 125(1): 99–139.

Chen, Yan, and Onur Kesten. 2019. "Chinese college admissions and school choice reforms: An experimental study." *Games and Economic Behavior*, 115: 83–100.

Chetty, Raj, John N Friedman, Emmanuel Saez, Nicholas Turner, and Danny Yagan. 2020. "Income segregation and intergenerational mobility across colleges in the United States." *The Quarterly Journal of Economics*, 135(3): 1567–1633.

Chew, Soo Hong, Bin Miao, and Songfa Zhong. 2017. "Partial ambiguity." *Econometrica*, 85(4): 1239–1260.

Chew, Soo Hong, Wei Huang, and Xiaojian Zhao. 2019. "Motivated false memory." *Available at SSRN 2127795*.

Dana, Jason, Roberto A Weber, and Jason Xi Kuang. 2007. "Exploiting moral wiggle room: experiments demonstrating an illusory preference for fairness." *Economic Theory*, 33(1): 67–80.

De Haan, Monique, Pieter A Gautier, Hessel Oosterbeek, and Bas Van der Klaauw. 2015. "The performance of school assignment mechanisms in practice."

DellaVigna, Stefano. 2018. "Structural behavioral economics." In *Handbook of Behavioral Economics: Applications and Foundations 1*. Vol. 1, 613–723. Elsevier.

DellaVigna, Stefano, John A List, Ulrike Malmendier, and Gautam Rao. 2016. "Voting to tell others." *The Review of Economic Studies*, 84(1): 143–181.

Di Tella, Rafael, Ricardo Perez-Truglia, Andres Babino, and Mariano Sigman. 2015. "Conveniently Upset: Avoiding Altruism by Distorting Beliefs about Others' Altruism." *American Economic Review*, 105(11): 3416–42.

Di Tella, Rafael, Sebastian Galiant, and Ernesto Schargrodsky. 2007. "The formation of beliefs: evidence from the allocation of land titles to squatters." *The Quarterly Journal of Economics*, 122(1): 209–241.

Dreyfuss, Bnaya, Ori Heffetz, and Matthew Rabin. 2019. "Expectations-based loss aversion may help explain seemingly dominated choices in strategy-proof mechanisms." National Bureau of Economic Research.

Eil, David, and Justin M Rao. 2011. "The good news-bad news effect: Asymmetric processing of objective information about yourself." *American Economic Journal: Microeconomics*, 3(2): 114–38.

Esponda, Ignacio, and Emanuel Vespa. 2016. "Contingent preferences and the sure-thing principle: Revisiting classic anomalies in the laboratory." working paper.

Exley, Christine, and Judd B Kessler. 2018. "Motivated Errors."

Exley, Christine L. 2016. "Excusing selfishness in charitable giving: The role of risk." *The Review of Economic Studies*, 83(2): 587–628.

Faravelli, Marco. 2007. "How context matters: A survey based experiment on distributive justice." *Journal of Public Economics*, 91(7-8): 1399–1422.

Fisman, Raymond, Shachar Kariv, and Daniel Markovits. 2009. "Exposure to ideology and distributional preferences." *Unpublished paper*.

Gabaix, Xavier, and David Laibson. 2006. "Shrouded attributes, consumer myopia, and information suppression in competitive markets." *The Quarterly Journal of Economics*, 121(2): 505–540.

Gabaix, Xavier, David Laibson, Guillermo Moloche, and Stephen Weinberg. 2006. "Costly information acquisition: Experimental analysis of a boundedly rational model." *American Economic Review*, 96(4): 1043–1068.

Halevy, Yoram. 2007. "Ellsberg revisited: An experimental study." *Econometrica*, 75(2): 503–536.

Hart, P Sol, and Erik C Nisbet. 2012. "Boomerang effects in science communication: How motivated reasoning and identity cues amplify opinion polarization about climate mitigation policies." *Communication Research*, 39(6): 701–723.

Hassidim, Avinatan, Assaf Romm, and Ran I Shorrer. 2016. "" Strategic" Behavior in a Strategy-Proof Environment." 763–764.

Hastings, Justine S, and Jeffrey M Weinstein. 2008. "Information, school choice, and academic achievement: Evidence from two experiments." *The Quarterly journal of economics*, 123(4): 1373–1414.

Hastings, Justine S, Christopher A Neilson, and Seth D Zimmerman. 2013. "Are some degrees worth more than others? Evidence from college admission cutoffs in Chile." National Bureau of Economic Research.

He, Yinghua. 2015. "Gaming the Boston school choice mechanism in Beijing."

Holt, Charles A, and Susan K Laury. 2002. "Risk aversion and incentive effects." *American economic review*, 92(5): 1644–1655.

Hossain, Tanjim, and Ryo Okui. 2013. "The binarized scoring rule." *Review of Economic Studies*, 80(3): 984–1001.

Hoxby, Caroline M, and Christopher Avery. 2012. "The missing" one-offs": The hidden supply of high-achieving, low income students." National Bureau of Economic Research.

Huffman, David, Collin Raymond, and Julia Shvets. 2019. "Persistent overconfidence and biased memory: Evidence from managers." *Pittsburgh: University of Pittsburgh*.

Iannaccone, Laurence R. 1992. "Sacrifice and stigma: reducing free-riding in cults, communes, and other collectives." *Journal of political economy*, 100(2): 271–291.

Iannaccone, Laurence R. 1998. "Introduction to the Economics of Religion." *Journal of economic literature*, 36(3): 1465–1495.

Jia, Ruixue, and Hongbin Li. 2016. "Access to elite education, wage premium, and social mobility: the truth and illusion of China's college entrance exam." *Stanford Center for International Development, Working Paper*, , (577).

Johnson, Eric J, Colin Camerer, Sankar Sen, and Talia Rymon. 2002. "Detecting failures of backward induction: Monitoring information search in sequential bargaining." *Journal of Economic Theory*, 104(1): 16–47.

Kagel, John H, Chung Kim, and Donald Moser. 1996. "Fairness in ultimatum games with asymmetric information and asymmetric payoffs." *Games and Economic Behavior*, 13(1): 100–110.

Kapor, Adam J, Christopher A Neilson, and Seth D Zimmerman. 2020. "Heterogeneous beliefs and school choice mechanisms." *American Economic Review*, 110(5): 1274–1315.

Kim, Hyuncheol Bryant, Syngjoo Choi, Booyuel Kim, and Cristian Pop-Eleches. 2018. "The role of education interventions in improving economic rationality." *Science*, 362(6410): 83–86.

Kirkeboen, Lars J, Edwin Leuven, and Magne Mogstad. 2016. "Field of study, earnings, and self-selection." *The Quarterly Journal of Economics*, 131(3): 1057–1111.

Kőszegi, Botond, and Adam Szeidl. 2013. "A model of focusing in economic choice." *The Quarterly journal of economics*, 128(1): 53–104.

Krupka, Erin L, and Roberto A Weber. 2013. "Identifying social norms using coordination games: Why does dictator game sharing vary?" *Journal of the European Economic Association*, 11(3): 495–524.

Kunda, Ziva. 1990. "The case for motivated reasoning." *Psychological bulletin*, 108(3): 480.

Kuran, Timur. 2018. "Islam and economic performance: Historical and contemporary links." *Journal of Economic Literature*, 56(4): 1292–1359.

Laibson, David, Andrea Repetto, and Jeremy Tobacman. 2007. "Estimating discount functions with consumption choices over the lifecycle." National Bureau of Economic Research.

Larroucau, Tomás, and Ignacio Rios. 2018. "Do "short-list" students report truthfully? Strategic behavior in the Chilean college admissions problem." *Preprint, submitted September*, 1(10.13140).

Larroucau, Tomás, and Ignacio Rios. 2020. "Dynamic college admissions and the determinants of students' college retention." Technical Report 2020. and,"Do "Short-List" Students Report Truthfully.

Larroucau, Tomás, Manuel Martínez, Christopher Neilson, and Ignacio Rios. 2021. "Application Mistakes and Information frictions in College Admissions."

Li, Hongbin, Lingsheng Meng, Kai Mu, and Shaoda Wang. 2021. "English Language Requirement and Educational Inequality: Evidence of 16 Million College Applicants in China."

Li, Shengwu. 2017. "Obviously strategy-proof mechanisms." *American Economic Review*, 107(11): 3257–87.

Lucas, Adrienne M, and Isaac M Mbiti. 2012. "The determinants and consequences of school choice errors in Kenya." *American Economic Review*, 102(3): 283–88.

Luhrmann, Melanie, Marta Serra-Garcia, and Joachim Winter. 2018. "The impact of financial education on adolescents' intertemporal choices." *American Economic Journal: Economic Policy*, 10(3): 309–32.

Majid, Muhammad Farhan. 2015. "The persistent effects of in utero nutrition shocks over the life cycle: Evidence from Ramadan fasting." *Journal of Development Economics*, 117: 48–57.

Martínez-Marquina, Alejandro, Muriel Niederle, and Emanuel Vespa. 2019. "Failures in Contingent Reasoning: The Role of Uncertainty." *American Economic Review*, 109(10): 3437–74.

Marwell, Gerald, and Ruth E Ames. 1981. "Economists free ride, does anyone else?: Experiments on the provision of public goods, IV." *Journal of public economics*, 15(3): 295–310.

Mobius, Markus M, Muriel Niederle, Paul Niehaus, and Tanya S Rosenblat. 2011. "Managing self-confidence: Theory and experimental evidence." National Bureau of Economic Research.

Montgomery, James D. 1996. "Contemplations on the economic approach to religious behavior." *The American economic review*, 86(2): 443–447.

Oosterbeek, Hessel, and Bas van der Klaauw. 2013. "Ramadan, fasting and educational outcomes." *Economics of Education Review*, 34: 219–226.

Oster, Emily, Ira Shoulson, and E Dorsey. 2013. "Optimal expectations and limited medical testing: evidence from Huntington disease." *American Economic Review*, 103(2): 804–30.

Pathak, Parag A, and Tayfun Sönmez. 2008. "Leveling the playing field: Sincere and sophisticated players in the Boston mechanism." *American Economic Review*, 98(4): 1636–52.

Plutzer, Eric, and Michael Berkman. 2008. "Trends: Evolution, creationism, and the teaching of human origins in schools." *Public Opinion Quarterly*, 72(3): 540–553.

Priest, Susanna Hornig. 2000. "US public opinion divided over biotechnology?" *Nature biotechnology*, 18(9): 939.

Rabin, Matthew. 2000. "Risk Aversion and Expected-Utility Theory: A Calibration Theorem." *Econometrica*, 68(5): 1281–1292.

Rabin, Matthew, and Georg Weizsäcker. 2009. "Narrow bracketing and dominated choices." *American Economic Review*, 99(4): 1508–43.

Rees-Jones, Alex, and Dmitry Taubinsky. 2020. "Measuring "schmeduling"." *The Review of Economic Studies*, 87(5): 2399–2438.

Rees-Jones, Alex, and Samuel Skowronek. 2018. "An experimental investigation of preference misrepresentation in the residency match." *Proceedings of the National Academy of Sciences*, 115(45): 11471–11476.

Rees-Jones, Alex, Ran Shorrer, and Chloe J Tergiman. 2020. "Correlation Neglect in Student-to-School Matching." National Bureau of Economic Research.

Sarsons, Heather. 2017. "Recognition for group work: Gender differences in academia." *American Economic Review*, 107(5): 141–45.

Schofield, Heather. 2014. "The economic costs of low caloric intake: Evidence from india." *Unpublished Manuscript*.

Schwardmann, Peter. 2019. "Motivated health risk denial and preventative health care investments." *Journal of health economics*, 65: 78–92.

Schwardmann, Peter, Egon Tripodi, and Joël J Van der Weele. 2019. "Self-persuasion: Evidence from field experiments at two international debating competitions."

Shorrer, Ran I. 2019. "Simultaneous search: Beyond independent successes." 347–348.

Shorrer, Ran I, and Sándor Sóvágó. 2018. "Obvious mistakes in a strategically simple college admissions environment: Causes and consequences." *Available at SSRN 2993538*.

Song, Yan, Kentaro Tomoeda, and Xiaoyu Xia. 2020. "Sophistication and Cautiousness in College Applications." *Available at SSRN 3711118*.

Sprenger, Charles. 2015. "An endowment effect for risk: Experimental tests of stochastic reference points." *Journal of Political Economy*, 123(6): 1456–1499.

Stark, Rodney, and Roger Finke. 2000. *Acts of faith: Explaining the human side of religion.* Univ of California Press.

Von Gaudecker, Hans-Martin, Arthur Van Soest, and Erik Wengstrom. 2011. "Heterogeneity in risky choice behavior in a broad population." *American Economic Review*, 101(2): 664–94.

Walters, Christopher R. 2018. "The demand for effective charter schools." *Journal of Political Economy*, 126(6): 2179–2223.

Wang, Ao, Shaoda Wang, and Xiaoyang Ye. 2021. "Cognitive Distortions in Complex Decisions: Evidence from Centralized College Admission."

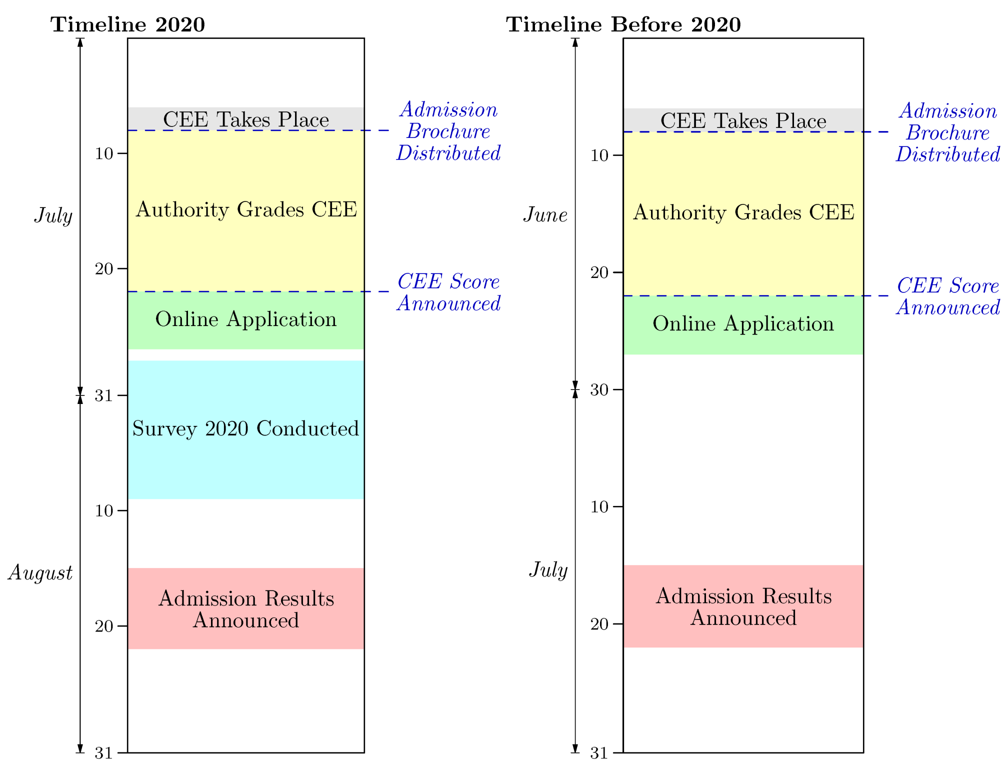
Zimmermann, Florian. 2020. "The dynamics of motivated beliefs." *American Economic Review*, 110(2): 337–61.

Appendix A

Chapter 1 Supplementary Materials

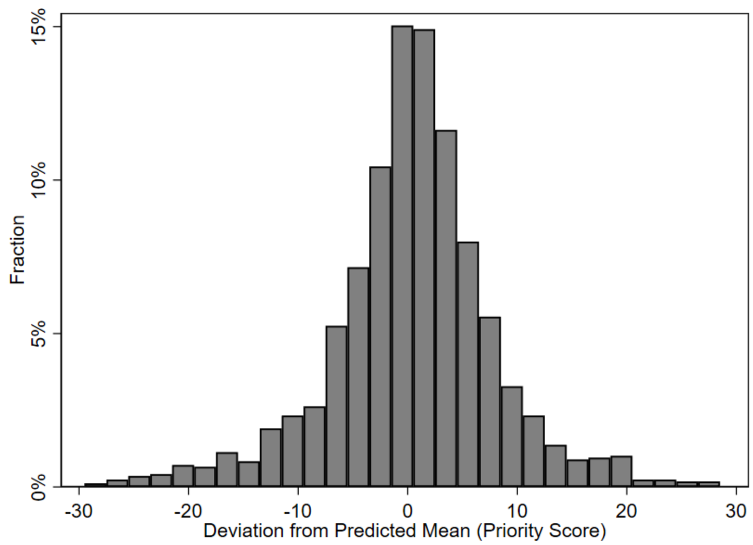
A.1 Supplementary Figures

Figure A.1: Timeline of College Admission Process

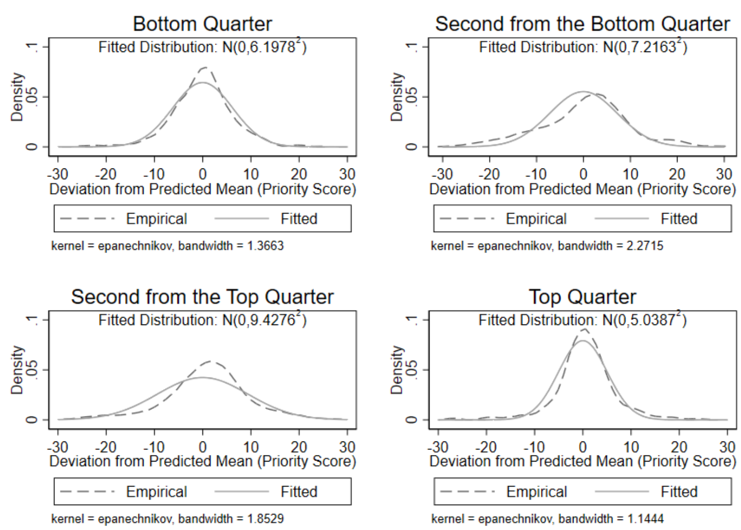


Note: This figure presents the timeline of college admission process, as detailed in Section 1.2. Note that the timeline remains unchanged until 2020, when the college entrance exam (CEE) was postponed by exactly one month due to COVID-19. As a result, all the admission procedures thereafter were postponed by exactly one month as well. As shown in the 2020 timeline, the survey was conducted right after application deadline, but before students were informed of their admission outcomes.

Figure A.2: Distribution of Realized Cutoff-Predicted Mean Differences



(a) Distribution of Differences: All Colleges

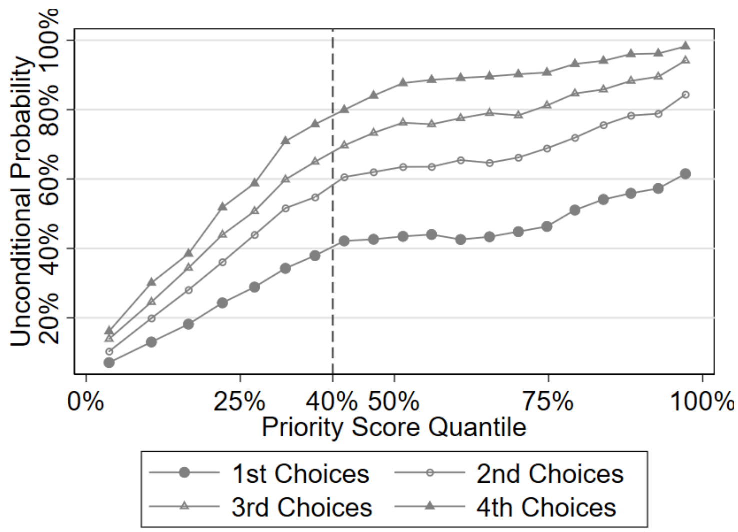


(b) Distribution of Differences by College Competitiveness

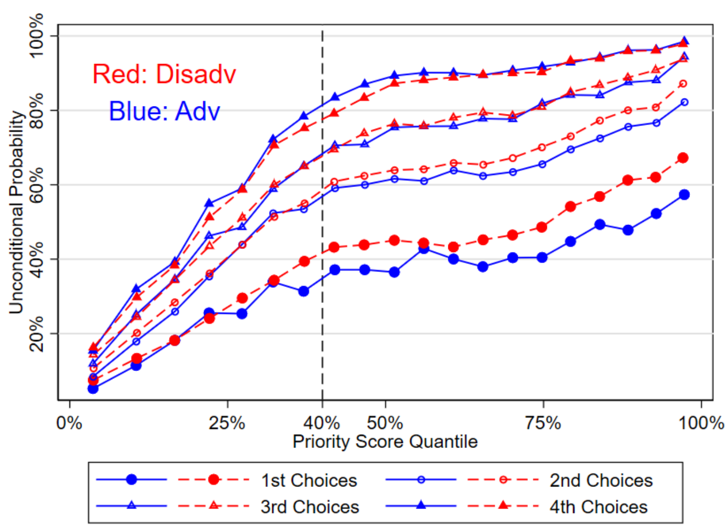
Note: This figure plots the distribution of distance between realized cutoffs and predicted mean as defined in Section 1.3. Subfigure (a) plots the distribution of differences between admission cutoffs and predicted mean among all colleges. Subfigure (b) plots the distribution of differences by college competitiveness, where the dashed lines in the top left, top right, bottom left, and bottom right graph are the empirical distribution for the least competitive quarter of colleges, second from the least competitive quarter, second from the most competitive quarter, and the most competitive quarter of colleges, respectively. The solid lines in subfigure (b) are fitted distributions using the median estimate of the standard deviation of cutoff-mean difference for corresponding quarter of colleges. Both subfigures omit outliers which is more than 30 points away from the predicted mean.

Figure A.3: Mean of Admission Probability Conditional on Priority Score

(a) Full Sample

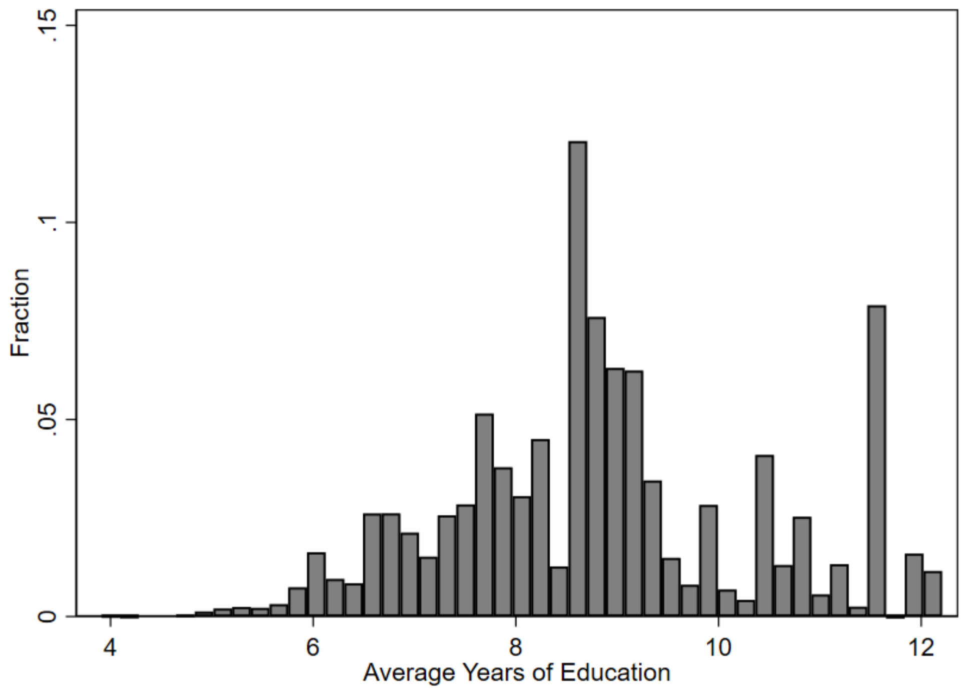


(b) Advantaged cs. Disadvantaged



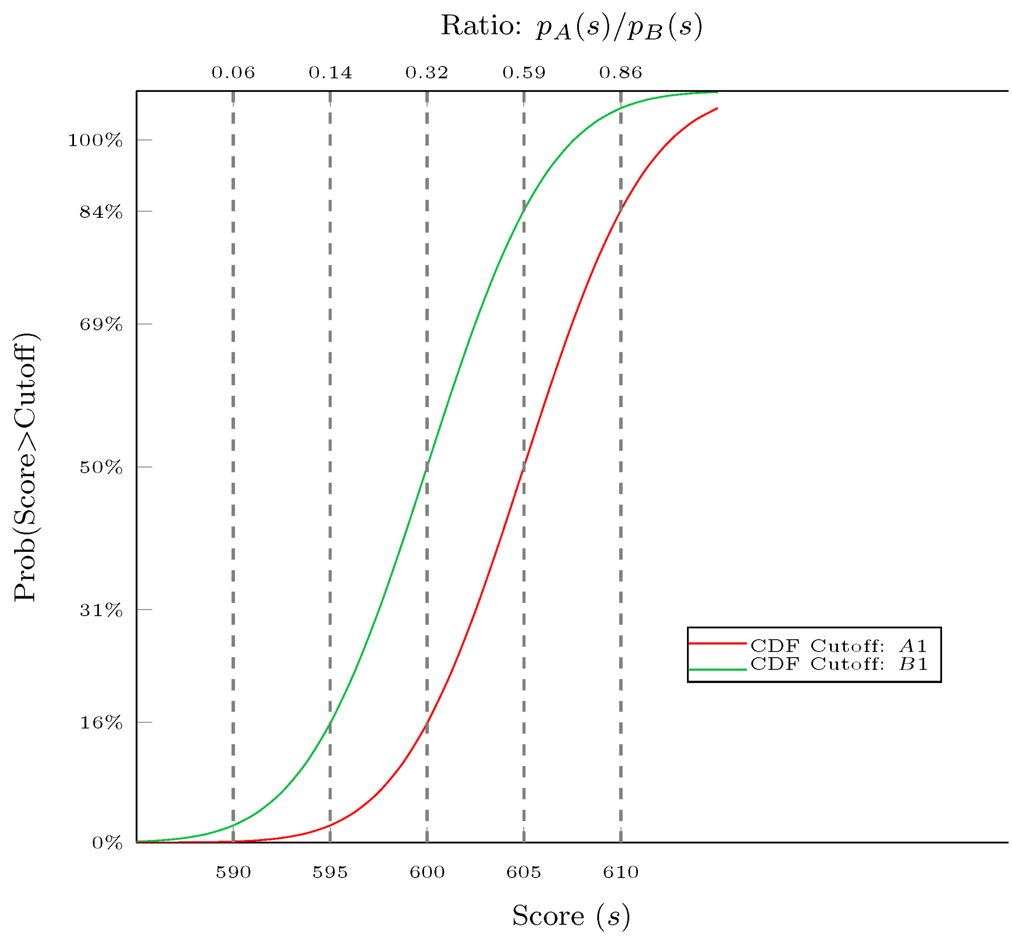
Note: This figure plots the mean of admission probability conditional on the quantile of priority score for student applicants' first choices, second choices, third choices and fourth choices, respectively. Subfigure (a) plots the statistics for the entire sample. Subfigure (b) plots the statistics for the advantaged students (the most advantaged quartile in terms of township-level education attainment) in blue, and for the other students in red. This graph is helpful for the discussion in Section 1.4.

Figure A.4: Average Years of Education in Administrative Data (Township Level)



Note: This figure plots the cumulative distribution of probability of meeting cutoffs for students' first (in red), second (in yellow), third (in green) and fourth (in blue) choices, respectively, during 2014-2018. The figure demonstrates sizable heterogeneity in terms of educational attainment across different townships in Ningxia (25th percentile: 7.80 years; median: 8.73 years; 75th percentile: 9.88 years).

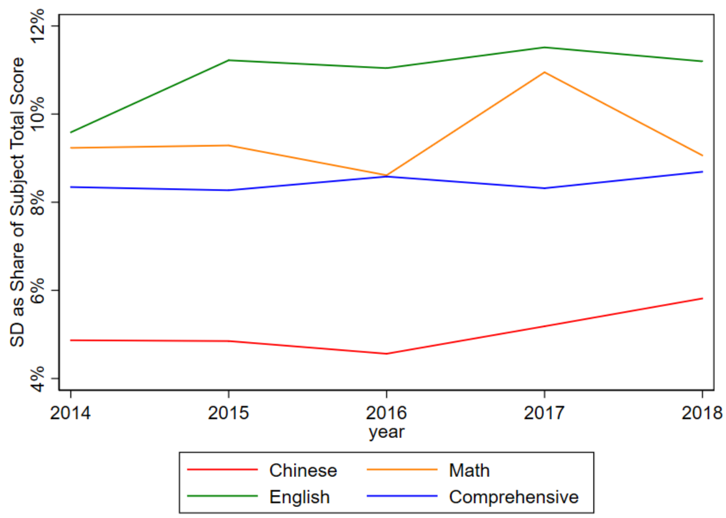
Figure A.5: Example of Change in Priority Score Leading to Change in Ratio of Assignment Probability



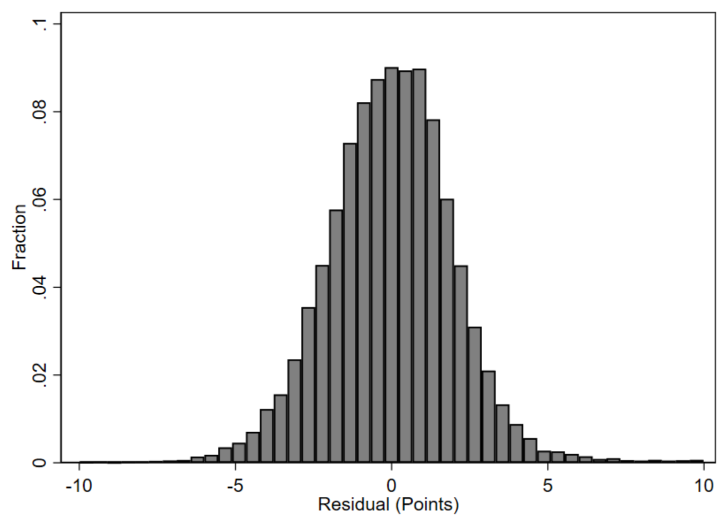
Note: This figure plots an example of the cumulative distributions of the cutoffs for two colleges, $A1$ and $B1$. Both distributions are normal distribution with standard deviation of 5, and the mean being 600 and 605 respectively. As shown in the upper horizontal axis, changes in priority scores lead to change in the ratio of assignment probability of $A1$ to $B1$. This graph is for the discussion in Section 1.7.

Figure A.6: Impact of Subject Difficulty Variation on Admission Probability

(a) Difficulty of Subjects Varies Across Years

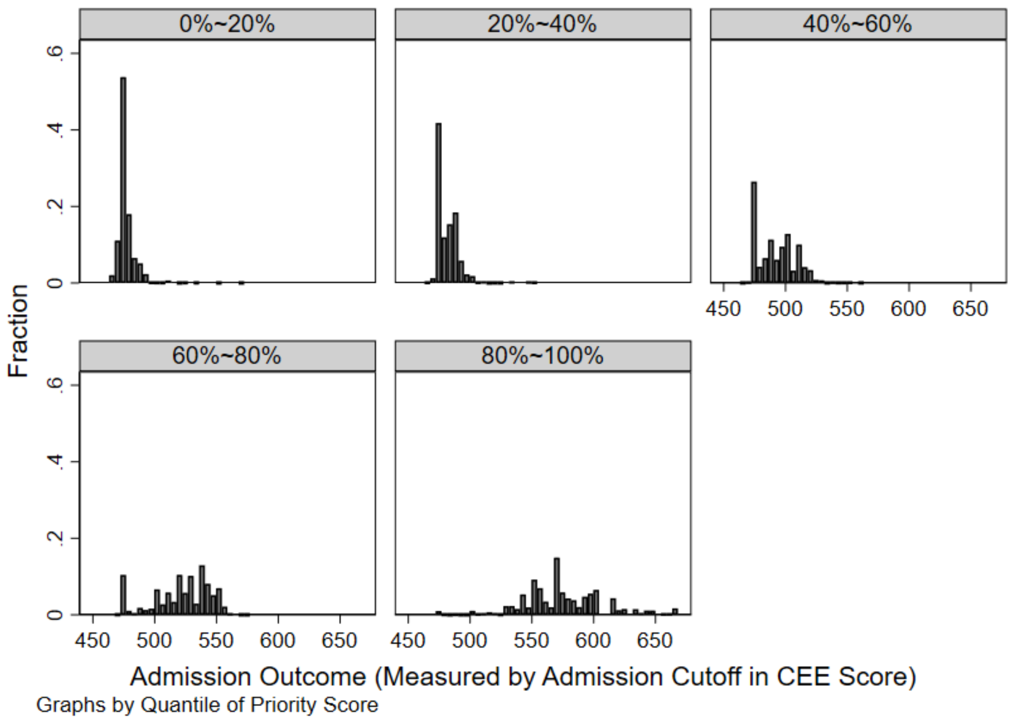


(b) Magnitude of Shock to Admission Probability is Sizable



Note: This figure explains the source of exogenous variation in admission probability. Subfigure (a) presents the standard deviation of raw exam scores as share of subject total scores for each subject during 2014-2018. Subfigure (b) is a histogram of the estimated distribution of shocks, rescaled in terms of the predicted standard deviation of cutoff of students' first choices. This graph is helpful for the discussion in Section 1.6.

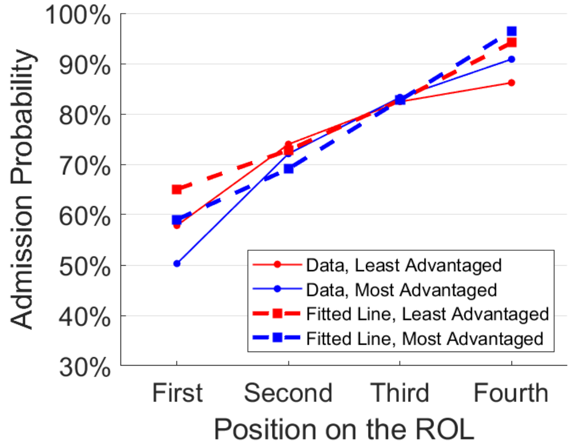
Figure A.7: Competitiveness of Admitting College: by CEE Score Quantile



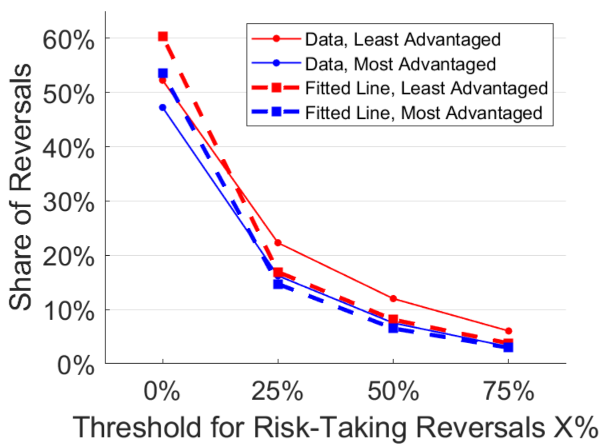
Note: This figure plots the distribution of colleges that admit students during 2014-2018. We split the entire sample of STEM applicants into five groups according to their CEE Score, with the first quintile being the group with lowest CEE Score and the fifth being the highest. This graph is helpful for the discussion in Section 1.7.

Figure A.8: Data & Fit of Mixture Model: Advantaged vs. Disadvantaged

(a) Data vs. Fit: Mean Admission Probability for Each Choice



(b) Data vs. Fit: Share of Risk-Taking Reversals



Note: This figure compares the fit and data for the key moments in risk-taking strategies, for the most advantaged quartile (4th quartile) and the least advantaged quartile (1st quartile) respectively, as discussed in detail in Section 1.7. The fit is generated by the structural model that excludes students of the bottom 40% to minimize the impact of the constraint of priority score on college choices in the year of 2015, 2017, and 2018, where township level SES index can be obtained. To maximize comparability across different SES groups, data have been reweighted to account for differences in priority score. Solid lines represent moments from data. Dashed lines represents values simulated by the estimated parameters from the mixture model. Blue lines represent the most advantaged quartile. Red lines represent the least advantaged quartile. Subfigure (a) plots the mean of unconditional admission probability for the first, second, third and fourth choices. Subfigure (b) plots the share of risk-taking reversals with different levels of threshold X%.

A.2 Supplementary Tables

Table A.1: Examples of Admission Rules

(1)	(2)	(3)	(4)	(5)	(6)	(7)
Ex. Number	Priority Score	Admission Cutoffs				Admission Outcome
		A	B	C	D	
1	600	595	590	587	580	A
2	580	581	572	583	550	B
3	577	595	590	587	580	None
4	580	595	590	587	580	D

Note: This table presents four examples in which students with different priority scores applying for different sets of colleges in their ROLs. The serial number of examples is in Column (1). Each example is associated with a hypothetical student. The priority scores of the students are recorded in Column (2). Columns (3)-(6) present the admission cutoffs of the colleges that the students put in their ROLs. Column (7) presents the admission outcome of each hypothetical student as a result of their scores and ROLs. This table is helpful for the discussion in Section 1.2.

Table A.2: Validity of Estimates of Unconditional Probability

	(1)	(2)	(3)	(4)
	Admitted to First		Admitted to Second	
Estimated Uncond. Prob. to First	1.0015***	1.0117***		
	(0.0043)	(0.0081)		
Estimated Uncond. Prob. to Second			0.9883***	1.0252***
			(0.0060)	(0.0107)
Constant	-0.0043*	-0.0050	0.0033	-0.0094
	(0.0024)	(0.0040)	(0.0037)	(0.0060)
Subsample (Science/Humanity)	Science	Humanity	Science	Humanity
Share of Admitted	.41	.314	.481	.419
Predicted Share of Admitted	.413	.316	.484	.418
F-Test: $\alpha = 0, \beta = 1$	.074	.328	.085	.059
Number of Observations	36104	8230	20929	5571
R squared	0.605	0.652	0.566	0.624

Note: This table reports the empirical exercise that tests the validity of probability estimates that we construct in Section 1.3. Columns 1 and 2 present the results of the regression where we regress the outcome of being admitted to the first choices on the estimated probability of meeting the cutoff of first choices, using the full sample of science-track and humanity-track students, respectively. Columns 3 and 4 present the results of the regression where we regress the outcome of being admitted to the second choices on the estimated probability of meeting the cutoff of second choices using the subsample of students who are not admitted to their first choices, for science-track and humanity track, respectively. This table is helpful for the discussion in 1.3.

Table A.3: Admission Outcome and Score-Cutoff Gap

Panel A: Regression Analysis Using Administrative Data			
	(1)	(2)	(3)
	Selectivity of Admission Outcome (Normalized)		
Most Disadvantaged Quartile	-0.1288***	-0.1061***	-0.0962***
	(0.0082)	(0.0126)	(0.0209)
Benchmark Group		Most Advantaged Quartile	
CEE Score	Yes	Yes	Yes
Demographic Variables	No	Yes	Yes
County Fixed Effects	No	No	Yes
Panel B: Regression Analysis Using Simulated Data after De-Biasing			
	(1)	(2)	(3)
	Selectivity of Admission Outcome (Normalized)		
Most Disadvantaged Quartile	-0.0217*	-0.0106	0.0001
	(0.0125)	(0.0142)	(0.0162)
Benchmark Group		Most Advantaged Quartile	
Decrease in Outcome Gap%	**83.15%**	**90.01%**	**100%**
CEE Score	Yes	Yes	Yes
Demographic Variables	No	Yes	Yes
County Fixed Effects	No	No	Yes

Note: Panel A compares the selectivity of admitting college, as measured by cutoffs during 2014-2018 (normalized), for the most advantaged quartile to the least advantaged quartiles, with different sets of controls in different columns. Only the most advantaged and most disadvantaged quartile are included in the regression. Students' CEE scores have been controlled in all columns. Demographic variables are added as controls for Columns 2,3. County Fixed Effects are controlled in Columns 3. Panel B conducts exactly the same analysis using simulated data that are generated by the estimated college preferences in a counterfactual scenario where all students have learned how to apply optimally. Standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%. This table is helpful for the discussion in Section 1.4.

Table A.4: Design of Incentivized Questions in Survey

	Panel A1: ROL for Question Group 1			Panel C1: ROL for Question Group 3		
	ROL #	Payoffs	Prob(Meeting Cutoffs)	ROL #	Payoffs	Prob(Meeting Cutoffs)
	1st	?	?	1st	?	?
	2nd	20 CNY	100%	2nd	5 CNY	100%
	3rd	20 CNY	100%	3rd	5 CNY	100%
	4th	20 CNY	100%	4th	5 CNY	100%

	Panel A2: MPL for Question Group 1		Panel C2: MPL for Question Group 3	
Question #	College X	College Y	College X	College Y
(1)	25 CNY, 50%	30 CNY, 25%	25 CNY, 50%	30 CNY, 25%
(2)	25 CNY, 50%	35 CNY, 25%	25 CNY, 50%	35 CNY, 25%
(3)	25 CNY, 50%	40 CNY, 25%	25 CNY, 50%	40 CNY, 25%
(4)	25 CNY, 50%	45 CNY, 25%	25 CNY, 50%	45 CNY, 25%
(5)	25 CNY, 50%	50 CNY, 25%	25 CNY, 50%	50 CNY, 25%
(6)	25 CNY, 50%	55 CNY, 25%	25 CNY, 50%	55 CNY, 25%
(7)	25 CNY, 50%	60 CNY, 25%	25 CNY, 50%	60 CNY, 25%

	Panel B: MPL for Question Series 2	
Question #	Choice X (Payoff, Prob)	Choice Y (Payoff, Prob)
(1)	(25 CNY, 50%; 20 CNY, 50%)	(30 CNY, 25%; 20 CNY, 75%)
(2)	(25 CNY, 50%; 20 CNY, 50%)	(35 CNY, 25%; 20 CNY, 75%)
(3)	(25 CNY, 50%; 20 CNY, 50%)	(40 CNY, 25%; 20 CNY, 75%)
(4)	(25 CNY, 50%; 20 CNY, 50%)	(45 CNY, 25%; 20 CNY, 75%)
(5)	(25 CNY, 50%; 20 CNY, 50%)	(50 CNY, 25%; 20 CNY, 75%)
(6)	(25 CNY, 50%; 20 CNY, 50%)	(55 CNY, 25%; 20 CNY, 75%)
(7)	(25 CNY, 50%; 20 CNY, 50%)	(60 CNY, 25%; 20 CNY, 75%)

Note: This table presents the content of the three groups of incentivized MPL questions. Panels A1 and C1 present the ROL for Question Groups 1 and 3, respectively. Names of colleges for the second, third, and fourth spot are replaced with safe colleges that students are familiar with. Panels A2 and C2 present the MPL for Question Groups 1 and 3, respectively. In both question groups, students need to select either College X or College Y from each row, and the selected college will be put at the first spot in the corresponding ROL. Panel B presents the MPL for Question Group 2. Similarly, students need to choose one from Lottery X and Y in each row. This table is helpful for the discussion in Section 1.5.

Table A.5: Testing Upward Movement of College Positions on ROLs

Predicted Probability of College of Choice if No Score Shock	Estimated Mean Movement μ_γ	Estimated Standard Deviation of Movement σ_γ	# Obs	# Cluster
0%~10%	0.019 (0.008)	0.143 (0.009)	4769	324
10%~20%	0.027 (0.013)	0.193 (0.014)	2342	235
20%~30%	-0.023 (0.014)	0.211 (0.015)	2173	234
30%~40%	-0.033 (0.014)	0.222 (0.016)	2087	239
40%~50%	-0.030 (0.014)	0.231 (0.016)	2263	248
50%~60%	-0.006 (0.013)	0.208 (0.014)	2533	274
60%~70%	-0.037 (0.012)	0.212 (0.012)	3156	313
70%~80%	-0.026 (0.011)	0.197 (0.013)	4116	352
80%~90%	-0.023 (0.008)	0.168 (0.009)	5772	435
90%~100%	-0.021 (0.004)	0.103 (0.007)	14952	632

Note: This table reports the estimated mean (μ_γ) and standard deviation (σ_γ) of the estimated slope of score shock, $\hat{\gamma}_{(j,SES)}$. The table corresponds to the empirical analysis in Section 1.6. We split the sample into ten bins according to whether the predicted academic ability, converted to admission probability in the absence of score shock falls into the category of 0.1% ~ 10%, 10% ~ 20%, 20% ~ 30%, ... , 80% ~ 90%, 90% sim 99.9%, and exclude samples where the probability is lower than 0.1% or higher than 99.9%, so that the score shock cannot generate substantial variation in admission probabilities. In terms of socio-economic status, we split students into four groups as we do previous sections. We run the specification in 1.5 for each college*SES quarter*probability bin separately, excluding the cell where there are less than 5 observations. The outcome variable is the position of a college on students' lists, which takes the value of 4 if listed as the first choice, 3 if listed as the second choice, 2 if listed as the third choice, 4 if listed as the fourth choice. For cells belonging to the same probability bin, we report the point estimate of the mean of the estimated slope for the score shock $\hat{\gamma}_{(j,SES)}$ and its standard error (Column 2), estimated standard deviation and of the estimated slope for the score shock $\hat{\gamma}_{(j,SES)}$ and its standard error (Column 3), number of college-student pair for each subsample (Column 4), number of clusters (coefficient of the score shock allowed vary across clusters) within each subsample (Column 5), where the score shock is defined as in Section 1.6. Point estimates for mean, standard deviation and their standard errors for each probability bin are calculated using bootstrap, with the replacement draw at the level of cells, weighted by the size of cell.

Table A.6: Subjective Beliefs

	(1)	(2)	(3)	(4)
	Belief-Est. Prob.		| Belief-Est. Prob.|	
SES Index (Normalized)	1.00%	1.85%	1.08%	1.12%
	(0.51%)	(0.64%)	(0.36%)	(0.45%)
Mean	12.1%		31.0%	
Priority Score	Yes	Yes	Yes	Yes
Demographic Variables	No	Yes	No	Yes
College Preferences	No	Yes	No	Yes

Note: This table presents analysis of student applicants' subjective beliefs about the unconditional admission probability for relevant colleges. All columns present regressions that examine whether subjective beliefs differ systematically from estimated admission probability, and whether such difference is correlated with SES Index. The outcome variable in Columns 1 and 2 is the difference between subjective beliefs and estimated admission probability. Columns 3 and 4 are the absolute difference between subjective beliefs and estimated admission probability. The mean of bias and absolute biases have been calculated below the coefficient estimates. Besides SES Index, the only additional covariate in Columns 1 and 3 is the fourth-order polynomial of priority score, whereas in Columns 2 and 4 we additionally control for demographics and stated college preferences. This table is helpful for the discussion in Section A.3.

Table A.7: Alternative Specifications of Structural Estimation

Panel A: Alternative Specifications - Less Flexible Preferences						
	(1)	(2)	(3)	(4)	(5)	(6)
Sample (Priority Score)	40%-60%		60%-80%		80%-100%	
	Rational	Mixture	Rational	Mixture	Rational	Mixture
Estimated Share DC Type		49.3%		47.8%		54.0%
		(0.25%)		(0.23%)		(0.30%)
Marginal Effect of SES		0.99%		-2.31%		-5.03%
		(0.33%)		(0.26%)		(0.31%)
Mean Curvature: Rational	-0.296	-0.312	-0.133	-0.500	0.259	-0.500
	(0.015)	(0.019)	(0.011)	(0.021)	(0.013)	(0.017)
Number of Moments	120	120	120	120	120	120
Number of Parameters	8	11	8	11	8	11
Distance	11647.42	4586.36	9181.853	4171.124	7247.197	3102.797
Decrease of Distance %	60.6%		54.6%		57.2%	
MMSC-BIC	10712.27	3676.267	8246.711	3261.031	6312.054	2192.703
Panel B: Alternative Specifications - Heterogeneous Beliefs						
Sample (Priority Score)	40%-60%		60%-80%		80%-100%	
	Rational	Mixture	Rational	Mixture	Rational	Mixture
Estimated Share DC Type		92.3%		96.2%		97.8%
		(0.51%)		(0.28%)		(0.26%)
Marginal Effect of SES		6.45%		-1.00%		-0.97%
		(0.48%)		(0.33%)		(0.40%)
Mean Curvature: Rational	-0.318	0.125	-0.100	4.529	0.090	10.445
	(0.016)	(0.045)	(0.011)	(1.171)	(0.005)	(9.113)
Mean Curvature: DC		-0.494		-0.478		-0.431
		(0.022)		(0.017)		(0.011)
Number of Moments	120	120	120	120	120	120
Number of Parameters	20	29	20	29	20	29
Distance	10026.09	4586.656	30492.2	4331.319	54968.45	3360.928
Decrease of Distance %	54.3%		85.8%		93.9%	
MMSC-BIC	9191.145	3826.853	29657.25	3571.516	54133.5	2601.125

Note: This table reports the results of structural analysis as described in Section 1.7 and A.3, for the 40% ∼ 60% (column 1,2), 60% ∼ 80% (column 3,4), and 80% ∼ 100% (column 5,6) subsample using alternative specifications. Panel A reports the estimation results where the model is the same as the single-type/mixture model in Table 1.4 except that only preferences over competitiveness and distance is taken into account, and is homogeneous across types. Panel B reports the estimation results where the parameters are the same as in Table 1.4 but we additionally introduce perturbation in beliefs with the parameters of the perturbation distribution calibrated using data from the online survey.

Table A.8: Major Preference and its Impact on Risk Taking

	(1)	(2)	(3)	(4)	(5)	(6)
	Major Top Concern		Estimated Prob		Subjective Beliefs	
SES Index (Normalized)	-2.24%**	-2.66%**				
	(0.92%)	(1.17%)				
Major Top Concern			3.25%	4.09%	8.94%***	8.01%***
			(2.85%)	(3.00%)	(2.43%)	(2.51%)
Share of Major Top Concern	13.7%					
Implied Impact on 1st Choices			0.4%	0.6%	1.2%	1.1%
Priority Score	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Variables	No	Yes	No	Yes	No	Yes
College Preferences	No	Yes	No	Yes	No	Yes
Number of Observations	1412	1412	1386	1386	1412	1412
R squared	0.009	0.091	0.138	0.201	0.035	0.087

Note: Columns 1 and 2 present regressions that examine whether share of survey takers who think major is their top concern is correlated with SES Index. Columns 3 and 4 present the regression that examines whether those who declare major to be of top concern take different amount of risks on their first choices, in terms of the estimated unconditional admission probability. Columns 5 and 6 examine whether those who declare major to be of top concern take different amount of risks on their first choices, in terms of subjective probability. Columns 1,3,5 only control for priority score, whereas Columns 2,4,6 additionally control for demographics as well as stated college preferences. The share of people who think major is most important have been reported below the estimate for Columns 1 and 2. The implied impact of major consideration on the risk-taking of first choices is reported below the estimated coefficient in Columns 3,4,5,6. This is calculated as the product of the share of people who think major is the most important, and the average of additional cautiousness for the first choices among this group of people. This table is helpful for the discussion in Section A.5.

A.3 Additional Details and Results about Structural Estimation

Specification

This section lays out the details on the specification and the moments we use for the structural estimation in Section 1.7.

Remember in in the mixture model there are two types of decision maker, the Rational Type and the DC Type. We will use superscript RN to declare that the parameter is only relevant for the Rational Type, and DC to declare that the parameter is only relevant for the DC Type.

Remember in Section 1.7 we state that the utility specification we use is:

$$u_{ij} = f(\theta_i^C, C_{ij}, SES_i) + g(\theta_i^d, d_j, SES_i) + h(\theta_i^X, X_j, SES_i) + O_i + \epsilon_{ij}$$

In the benchmark mixture model (Table 1.4, parameters differ across types. Below we detail the specification we use for each type, for student i and college j:

$$u_{ij}^{RN} = f_{ij}^{RN} + g_{ij}^{RN} + h_{ij} + O^{RN} + \epsilon_{ij}$$

$$u_{ij}^{DC} = f_{ij}^{DC} + g_{ij}^{DC} + h_{ij} + O^{DC} + \epsilon_{ij}$$

where $\epsilon \sim N(0, \sigma_\epsilon^2)$, with σ_ϵ being the same across types.

Details of f_{ij}^{RN} and f_{ij}^{DC} First let's discuss the details of f_{ij}^{RN} and f_{ij}^{DC}. The specification for f_{ij}^{RN} is

$$f_{RN} = \frac{C_j^{1-\theta_i^C} - 1}{1 - \theta_i^C}$$

Let NC_j denote the normalized average cutoffs in 2014-2018 for college j. Then $C_j \equiv 10 * (NC_j - min_{j \in \textbf{elite college}}\{NC_j\})$ to ensure that C_j is non-negative. And

$$\theta_i^C \sim N(\mu_C^{RN} + \mu_\beta^{RN} SES_i, (\sigma_C^{RN})^2)$$

Similarly, for the DC Type we have

$$f_{DC} = \frac{C_j^{1-\theta_i^C} - 1}{1 - \theta_i^C}$$

$$\theta_i^C \sim N(\mu_C^{DC} + \nu_C^{DC} SES_i, (\sigma_C^{DC})^2)$$

Details of g_{ij}^{RN} and g_{ij}^{DC} We next discuss the details of g_{ij}^{RN} and g_{ij}^{DC}. For g_{ij}^{RN} we have,

$$g_{ij}^{RN} = \theta_i^d (d_j, d_j^2)'$$

where $\theta_i^d = (\theta_i^{d1}, \theta_i^{d2})$, and the distribution of linear coefficient is assumed to be the same across types to make the utility across types of similar scales:

$$\theta_i^{d1} \sim N(\mu_{d1} + \nu_{d1} SES_i, (\sigma_{d1})^2)$$

the distribution of quadratic coefficient is allowed to be different across types. For the Rational Type we have:

$$\theta_i^{d2} \sim N(\mu_{d2}^{RN} + \nu_{d2}^{RN} SES_i, (\sigma_{d1}^{RN})^2)$$

For the DC Type we have:

$$\theta_i^{d2} \sim N(\mu_{d2}^{DC} + \nu_{d2}^{DC} SES_i, (\sigma_{d2}^{DC})^2)$$

Details of h_{ij} Lastly, we discuss h_{ij}. X_j contains three variables: whether the college is a Science&Technology oriented college X_j^{STEM}, whether the college is a Finance oriented college X_j^{FIN}, and whether the college is a Medical School X_j^{MED}. These categories are mutually exclusive. h_{ij} is

$$h_{ij} = \theta_i^{X,STEM} X_j^{STEM} + \theta_i^{X,FIN} X_j^{FIN} + \theta_i^{X,MED} X_j^{MED}$$

where

$$\theta_i^{X,STEM} \sim N(\mu^{X,STEM} + \nu^{X,STEM} SES_i, (\sigma_{X,STEM})^2)$$

$$\theta_i^{X,FIN} \sim N(\mu^{X,FIN} + \nu^{X,FIN} SES_i, (\sigma^{X,FIN})^2)$$

$$\theta_i^{X,MED} \sim N(\mu^{X,MED} + \nu^{X,MED} SES_i, (\sigma^{X,MED})^2)$$

The probability of being

$$P(\text{DC Type}|\text{SES}_i) = \frac{exp(\gamma_0 + \gamma_1 * SES_i)}{1 + exp(\gamma_0 + \gamma_1 * SES_i)}$$

Subjective Beliefs

Evidence from Survey A number of recent studies have (Kapor, Neilson, and Zimmerman, 2020; Arteaga et al., 2021) documented that students may not accurately estimate the probability of admission, especially when the structure of priority scores is more complicated. While in our context the only uncertainty comes from the variation in cutoffs, a one-dimensional object, it is quite unlikely that students' beliefs are perfectly accurate. We elicited students' beliefs regarding the unconditional probability of the four colleges on their lists, and compare the elicited beliefs to the estimated probability that we construct. Subjective beliefs are positively correlated with estimated probability, at a correlation of 0.50. We define belief error as the difference between subjective beliefs and estimated probability, and regress the error on students' socioeconomic index, controlling for other variables including priority score and college preferences. As demonstrated in Table A.6, students on average overestimate the chance of admission by 12.1%, and the mean of absolute error is 31.0%.

Despite substantial differences in contexts, the level of these statistics are not far from estimates in Kapor, Neilson, and Zimmerman (2020). While socioeconomically advantaged students seem to be somewhat more optimistic, 1 SD of increase in SES is associated with less than 1.85% increase in subjective beliefs. The estimated mean of absolute error is also slightly larger among the advantaged (Column 3 and 4), but the magnitude is also small (1 SD of increase in SES is associated with around 1% increase in absolute errors). Taken together, the belief data suggests that belief errors, while substantial, is at best weakly correlated with the demographics.

Accounting for Heterogeneous Beliefs in Structural Estimation The specification of the perceived probability admission of college j for student i is:

$$p_{ij} = max\{0, min\{1, \hat{p_{ij}} + \tau_{ij}\}\}$$

where $\tau_{ij} \sim N(\mu_{\tau 0} + \mu_{\tau 1}\text{SES}_i, exp(\mu_{\tau 2} + \mu_{\tau 3}\text{SES}_i)^2)$. $\mu_{\tau 0} + \mu_{\tau 1}\text{SES}_i$ dictates student i's overall optimism; $exp(\mu_{\tau 2} + \mu_{\tau 3}\text{SES}_i)^2$ dictates the standard deviation of student i's idiosyncratic beliefs about admission probability on top of the student's overall optimism. In the estimation, we calibrate the parameter by referring to the estimation results from survey. Specifically, the value of $\mu_{\tau 0} = 12.1\%$ and $\mu_{\tau 1} = 1.85\%$ is taken from results in Table A.6. $\mu_{\tau 2} = -0.889$ and $\mu_{\tau 3} = 0$ is based on the variance of beliefs and the finding that belief error in its square term does not change in a quantitatively significant way across students of different SES status.

In Panel B of Table A.7, we consider the impact of heterogeneous beliefs about admission probability on our estimation. We re-run the same specifications in Table 1.4. In terms of fit, the mixture model outperforms the one-type rational model by an even larger margin (54.3%, 85.8%, 93.9% for the 40% ∼ 60%, 60% ∼ 80%, 80% ∼ 100% respectively), with the MMSC-BIC metric favoring the mixture model even more. The fit with heterogeneous beliefs is not as good as our benchmark estimate in Table 1.4, potentially because our model of belief errors is unable to perfectly capture students' beliefs. Another surprising finding is that the estimated share of the DC Type is more than 90% regardless of the subsample we focus on. The overall overconfidence and substantial idiosyncrasies in beliefs ensure that students have large positive belief errors about many colleges, such that, even if students eliminate the risk by only choosing colleges whose subjective probability is close to 100%, the objective probability may actually be much lower. This is particularly a problem for the rational models, as the first choices under the rational rule are the most preferred, and thus more likely to be the competitive colleges mistakenly chosen due to large positive belief errors.

A.4 Mathematical Proofs

In this section we detail the derivation described in Section 1.6 and 1.7. Let's begin with several basic notations.

- For college A, the utility of admission is denoted by u_A. The assignment probability of A given priority score s is denoted by $p_A(s)$. Similarly, the assignment probability of B, C is denoted by $p_B(s)$ and $p_C(s)$ and so on.

For convenience, throughout the derivation outside option $\underline{u}$ is normalized to 0. If a ROL is shorter than 4, it means that students leave the rest of it blank.

For a pair of college A, B, if there exists $\delta > 0$ such that $p_A(s) = p_B(s+\delta)$ for any $s \in [\underline{s}, \bar{s}]$ and $p_B(s)$ is continuous and log-concave, then

$$\frac{p_A(s)}{p_B(s)}$$

is decreasing in s when $s \in [\underline{s}, \bar{s}]$.

Proof. Let

$$g(s) \equiv \frac{p_A(s)}{p_B(s)} = \frac{p_B(s+\delta)}{p_B(s)}$$

We have

$$\text{sgn } g'(s) = \text{sgn } \left(\frac{p'_B(s+\delta)}{p_B(s+\delta)} - \frac{p'_B(s)}{p_B(s)}\right)$$

As $p_B(.)$ is log-concave, $\frac{d(ln(p_B(x)))}{dx} = \frac{p'_B(x)}{p_B(x)}$ decreases over x.

Thus,

$$g'(s) < 0$$

□

In our setting, as colleges which has comparable competitiveness are assumed to have normally distributed cutoffs with similar dispersion, this lemma becomes applicable when the derivation involves the pairwise ratio of assignment probability.

Several assumptions we often make in derivations are discussed below:

Assumption 1. *For any pair of college X, Y where at least one appears on the list, $u_X \neq u_Y$*

This assumption effectively says that choices matter for students as it rules out indifference among listed colleges.

Assumption 2. *For any pair of college X, Y, there does not exist a pair of priority score s_1 and s_2 such that*

$$sgn((p_X(s_1) - p_Y(s_1)) \neq sgn((p_X(s_2) - p_Y(s_2))$$

This assumption is testable if we regard assignment probability as observables. Intuitively it says that the relationship where X is riskier/safer than Y does not change with one's priority score s. It holds in our setting because cutoffs are assumed to be normally distributed, where those with comparable competitive have similar dispersion in cutoff distribution.

Assumption 3. *for any pairs of college A, B,*

$$\frac{min\{p_A(s), p_B(s)\}}{max\{p_A(s), p_B(s)\}}$$

increases for $s \in [\underline{s}, \bar{s}]$*,*

This assumption trivially holds if the condition in Proposition A.4 holds. Intuitively it says that the assignment probability of the riskier college increases at a faster rate than the safer college, a property that is true for any pair of colleges with log-concave distributions,and similar dispersion of cutoffs.

Important notations before we derive the theorems:

- Let u denote utility vector $(u_1, u_2, .., u_n)$, preferences over colleges.
- The position of a college on the list is encoded by κ. $\kappa = 0$ if the college is omitted from the list; $\kappa = 1$ if the college is listed as the fourth choice; $\kappa = 2$ if the college is listed as the third choice; $\kappa = 3$ if the college is listed as the second choice; $\kappa = 4$ if the college is listed as the first choice.
- Function $\mathcal{R} : (u, A, s) \mapsto \kappa$ maps utility u, college A, priority score s into position κ under Rational Decision Rule.
- Function $\mathcal{D} : (u, A, s) \mapsto \kappa$ maps utility u, college A, priority score s into position κ under DC Decision Rule.
- Set $C^{RN}_{(u,A,\kappa)} = \{s|\mathcal{R}(u, A, s) = \kappa\}$ is the contour set of priority score s where A is listed as at position κ under Rational Decision Rule and preference u.
- Set $C^{DC}_{(u,A,\kappa)} = \{s|\mathcal{D}(u, A, s) = \kappa\}$ is the contour set of priority score s where A is listed as at position κ under Rational Decision Rule and preference u.

If Assumption 1, 2, 3 hold, for any $\underline{s} < s < s + \delta < \bar{s}$ the following three scenarios (forms of upward movement) are impossible under Rational Decision Rule (if length of list is shorter than 4, it means that the rest is left blank):

1. Choose (X, Y) when priority score is s; choose (Y, Z) when priority score is $s + \delta$;

2. Choose (X, Y, Z) when priority score is s; choose (Y, Z, W) when priority score is $s + \delta$;
3. Choose (X, Y, Z, W) when priority score is s; choose (Y, Z, W, M) when priority score is $s + \delta$;

Scenario 1 The optimality implies that

$$p_X(s)u_X + (1 - p_X(s))p_Y(s)u_Y \geq p_Y(s)u_Y + (1 - p_Y(s))p_Z(s)u_Z$$

and

$$p_X(s+\delta))u_X + (1 - p_X(s+\delta)))p_Y(s+\delta))u_Y \leq p_Y(s+\delta))u_Y + (1 - p_Y(s+\delta)))p_Z(s+\delta))u_Z$$

Reorganize these two equations we get

$$\frac{p_X(s)}{p_Z(s)} \geq \frac{1 - p_Y(s)}{u_X - p_Y(s)u_Y}u_Z$$

$$\frac{p_X(s+\delta)}{p_Z(s+\delta)} \leq \frac{1 - p_Y(s+\delta)}{u_X - p_Y(s+\delta)u_Y}u_Z$$

As optimality also requires that $u_X \geq u_Y \geq u_Z$, we know that $p_X(s+\delta) < p_Z(s+\delta)$. Thus,

$$\frac{p_X(s+\delta)}{p_Z(s+\delta)} \geq \frac{min\{p_X(s), p_Z(s)\}}{max\{p_X(s), p_Z(s)\}} = \frac{p_X(s)}{p_Z(s)}$$

On the other hand, $\frac{1-p_Y(s)}{u_X - p_Y(s)u_Y}$ is decreasing in s, which leads to contradiction. The optimality implies that

$$p_X(s)u_X + (1 - p_X(s))p_Y(s)u_Y + (1 - p_X(s))(1 - p_Y(s))p_Z(s)u_Z \geq$$
$$p_Y(s)u_Y + (1 - p_Y(s))p_Z(s)u_Z + (1 - p_Y(s))(1 - p_Z(s))p_W(s)u_W$$

and

$$p_X(s+\delta)u_X + (1 - p_X(s+\delta))p_Y(s+\delta)u_Y + (1 - p_X(s+\delta))(1 - p_Y(s+\delta))p_Z(s+\delta)u_Z \leq$$
$$p_Y(s+\delta)u_Y + (1 - p_Y(s+\delta))p_Z(s+\delta)u_Z + (1 - p_Y(s+\delta))(1 - p_Z(s+\delta))p_W(s+\delta)u_W$$

Reorganizing these two equations in a similar way we get

$$\frac{p_X(s)}{p_W(s)} \geq \frac{(1 - p_Y(s))(1 - p_Z(s))}{u_X - u_Y + (1 - p_Y(s))(u_Y - p_Z(s)u_Z)}u_W$$

and

$$\frac{p_X(s+\delta)}{p_W(s+\delta)} \leq \frac{(1 - p_Y(s+\delta))(1 - p_Z(s+\delta))}{u_X - u_Y + (1 - p_Y(s+\delta))(u_Y - p_Z(s+\delta)u_Z)}u_W$$

As $u_X > u_Y > u_Z > u_W$, we have $p_X(s+\delta) < p_W(s+\delta)$. THus,

$$\frac{p_X(s+\delta)}{p_W(s+\delta)} > \frac{min\{p_X(s), p_W(s)\}}{max\{p_X(s), p_W(s)\}} = \frac{p_X(s)}{p_W(s)}$$

On the other hand, we have

$$\frac{(1-p_Y(s))(1-p_Z(s))}{u_X - u_Y + (1-p_Y(s))(u_Y - p_Z(s)u_Z)} = \frac{1}{\frac{u_X-u_Y}{(1-p_Y(s))(1-p_Z(s))} + \frac{u_Y - p_Z(s)u_Z}{1-p_Z(s)}}$$

where the first term of the denominator is positive and increasing in s, and the second term is also increasing in s per derivation in Scenario 1. Thus $\frac{(1-p_Y(s))(1-p_Z(s))}{u_X-u_Y+(1-p_Y(s))(u_Y-p_Z(s)u_Z)}$ is decreasing in s, which leads to contradiction. Similar to the manipulation above, we obtain

$$\frac{p_X(s)}{p_M(s)} \geq \frac{(1-p_Y(s))(1-p_Z(s))(1-p_W(s))}{u_X - u_Y + (1-p_Y(s))[u_Y - u_Z + (1-p_Z(s))(u_Z - p_W(s)u_W)]} u_M$$

and

$$\frac{p_X(s+\delta)}{p_M(s+\delta)} \leq \frac{(1-p_Y(s+\delta))(1-p_Z(s+\delta))(1-p_W(s+\delta))}{u_X - u_Y + (1-p_Y(s+\delta))[u_Y - u_Z + (1-p_Z(s+\delta))(u_Z - p_W(s+\delta)u_W)]} u_M$$

Since

$$\frac{(1-p_Y(s))(1-p_Z(s))(1-p_W(s))}{u_X - u_Y + (1-p_Y(s))[u_Y - u_Z + (1-p_Z(s))(u_Z - p_W(s)u_W)]} = \frac{1}{\frac{u_X-u_Y}{(1-p_Y(s))(1-p_Z(s))(1-p_W(s)} + \frac{u_Y-u_Z+(1-p_Z(s+\delta))(u_Z-p_W(s+\delta)u_W)}{(1-p_Z(s))(1-p_W(s))}}$$

where the first term of denominator is increasing in s, and the second term is as well per derivation in Scenario 2. Thus $\frac{(1-p_Y(s))(1-p_Z(s))(1-p_W(s))}{u_X-u_Y+(1-p_Y(s))[u_Y-u_Z+(1-p_Z(s))(u_Z-p_W(s)u_W)]} u_M$ is decreasing s, which leads to contraction. □

If Assumption 1, 2, 3 hold, and $\mathcal{R}(u, A, s) \geq 1$, then for any s and $\delta > 0$, $\mathcal{R}(u, A, s) \geq \mathcal{R}(u, A, s+\delta)$ or $\mathcal{R}(u, A, s+\delta) = 0$.

Proof. Proof. It suffices to show that $1 \leq \mathcal{R}(u, A, s+\delta) < \mathcal{R}(u, A, s)$ would lead to contradiction. Namely, it is impossible that college A is selected and appears at a lower position when priority score is lower. Below we show that this would lead to contradiction in every possible scenario.

Scenario 1 There exists a college Y such that $\mathcal{R}(u, Y, s) = 4$ and $\mathcal{R}(u, Y, s + \delta) = 3$

To prove by contradiction, suppose the optimal list is (X, Y, V, W) when priority score is s and the optimal list is (Y, Z, M, N) when priority score is $s + \delta$, where M, N, V, W are just other colleges that could be identical or different. Let $U_{(M,N)}(s)$ and $U_{(V,W)}(s)$ denote the expected utility of sub-portfolio (M, N), (V, W) when priority score is s respectively.

X and Z cannot be the same college, which leads to contradiction immediately. The optimality implies that

$$\begin{aligned} p_X(s)u_X + (1 - p_X(s))p_Y(s)u_Y + (1 - p_X(s))(1 - p_Y(s))U_{(M,N)} \geq \\ p_Y(s)u_Y + (1 - p_Y(s))p_Z(s)u_Z + (1 - p_Y(s))(1 - p_Z(s))U_{(M,N)} \end{aligned}$$

Similarly,

$$\begin{aligned} p_X(s+\delta)u_X + (1 - p_X(s+\delta))p_Y(s+\delta)u_Y + (1 - p_X(s+\delta))(1 - p_Y(s+\delta))U_{(V,W)} \geq \\ p_Y(s+\delta)u_Y + (1 - p_Y(s+\delta))p_Z(s+\delta)u_Z + (1 - p_Y(s+\delta))(1 - p_Z(s+\delta))U_{(V,W)} \end{aligned}$$

Reorganizing these two inequalities, we have

$$\frac{p_X(s)}{p_Z(s)} \geq \frac{(1 - p_Y(s))u_Z - (1 - p_Y(s))U_{(M,N)}(s)}{u_X - p_Y(s)u_Y - (1 - p_Y(s))U_{(M,N)}(s)}$$

and

$$\frac{p_X(s+\delta)}{p_Z(s+\delta)} \leq \frac{(1 - p_Y(s+\delta))u_Z - (1 - p_Y(s+\delta))U_{(V,W)}(s+\delta)}{u_X - p_Y(s+\delta)u_Y - (1 - p_Y(s+\delta))U_{(V,W)}(s+\delta)}$$

The optimality implies that $u_X > u_Y > u_Z$. This in turn implies that $p_X(s) < p_Z(s)$, because otherwise it is never optimal to list (Y, Z) as top two choices. When $V \neq Z$ and $W \neq Z$, $U_{(V,W)}$ cannot be larger than $U_{(M,N)}$, because this would imply that (V, W) is a better sub-portfolio than (M, N) when priority score is s. Thus,

$$\frac{p_X(s)}{p_Z(s)} < \frac{p_X(s+\delta)}{p_Z(s+\delta)}$$

However, we have

$$\begin{aligned} &\frac{(1 - p_Y(s+\delta))u_Z - (1 - p_Y(s+\delta))U_{(V,W)}(s+\delta)}{u_X - p_Y(s+\delta)u_Y - (1 - p_Y(s+\delta))U_{(V,W)}(s+\delta)} \\ \leq &\frac{(1 - p_Y(s+\delta))u_Z - (1 - p_Y(s+\delta))U_{(M,N)}(s+\delta)}{u_X - p_Y(s+\delta)u_Y - (1 - p_Y(s+\delta))U_{(M,N)}(s+\delta)} \\ < &\frac{(1 - p_Y(s+\delta))(u_Z - U_{(M,N)}(s))}{u_X - p_Y(s+\delta)u_Y - (1 - p_Y(s+\delta))U_{(M,N)}(s)} \\ < &\frac{(1 - p_Y(s))(u_Z - U_{(M,N)}(s))}{u_x - p_Y(s)u_y - (1 - p_Y(s))U_{(M,N)}(s)} \end{aligned}$$

which leads to contradiction.

When $V = Z$, the list is (X, Y, Z, W) when priority score is s and (Y, Z, M, N) when priority score is $s+\delta$. The optimality condition thus implies that $(X, Y, Z, W) \succ (Y, Z, M, W)$ when priority score is s, $(Y, Z, M, N) \succ (X, Y, Z, N)$ when priority score is $s + \delta$. In this case let $U_N(s)$, $U_W(s)$ denote the expected utility of N and W when priority score is s respectively. We have $U_W(s) \le U_N(s) < u_M$, as otherwise n would be suboptimal when score is s. From the preference ordering above (after similar algebraic manipulations) we have,

$$\frac{p_X(s)}{p_M(s)} \ge \frac{(1-p_Y(s))(1-p_Z(s))(u_M - U_W(s))}{u_X - u_Y + (1-p_Y(s))[u_Y - p_Z(s)u_Z - (1-p_z(s))U_W(s)]}$$

and

$$\frac{p_X(s+\delta)}{p_M(s+\delta)} \le \frac{(1-p_Y(s+\delta))(1-p_Z(s+\delta))(u_M - U_N(s+\delta))}{u_X - u_Y + (1-p_Y(s+\delta))[u_Y - p_Z(s+\delta)u_Z - (1-p_z(s+\delta))U_N(s+\delta)]}$$

Since $u_X > u_Y > u_Z > max\{u_W, u_M, u_N\}$, we have $p_X(s) < p_M(s)$. Thus $\frac{p_X(s+\delta)}{p_M(s+\delta)} > \frac{p_X(s)}{p_M(s)}$. Also the expression on the right is decreasing in U, thus

$$\begin{aligned}
&\frac{(1-p_Y(s))(1-p_Z(s))(u_M - U_W(s))}{u_X - u_Y + (1-p_Y(s))[u_Y - p_Z(s)u_Z - (1-p_z(s))U_W(s)]} \\
\ge\ &\frac{(1-p_Y(s))(1-p_Z(s))(u_M - U_N(s+\delta))}{u_X - u_Y + (1-p_Y(s))[u_Y - p_Z(s)u_Z - (1-p_z(s))U_N(s+\delta)]} \\
\ge\ &\frac{(1-p_Y(s+\delta))(1-p_Z(s+\delta))(u_M - U_N(s+\delta))}{u_X - u_Y + (1-p_Y(s+\delta))[u_Y - p_Z(s+\delta)u_Z - (1-p_z(s+\delta))U_N(s+\delta)]}
\end{aligned}$$

which leads to contradiction.

When $W = Z$, the list is (X, Y, V, Z) when priority score is s, and (Y, Z, M, N) when priority score is $s + \delta$. Optimality implies that $(V, Z) \succ (Z, M)$ when priority score is s, and $(Z, M, N) \succ (V, Z, N)$ when priority score is $s + \delta$. Mathematically, the latter is equivalent to

$$\begin{aligned}
&p_Z(s+\delta)u_Z + (1-p_Z(s+\delta))p_M(s+\delta)u_M + (1-p_Z(s+\delta))(1-p_M(s+\delta))p_N(s+\delta)u_N \ge \\
&\quad p_V(s+\delta)u_V + (1-p_V(s+\delta))p_Z(s+\delta)u_Z + (1-p_V(s+\delta))(1-p_Z(s+\delta))p_N(s+\delta)u_N
\end{aligned}$$

Since optimality also implies $u_X > u_Y > u_V > u_Z > u_M > u_N$, it implies that $p_M(s+\delta) > p_V(s + \delta)$. Thus $(Z, M, N) \succ (V, Z, N)$ implies that

$$p_Z(s+\delta)u_Z + (1-p_Z(s+\delta))p_M(s+\delta)u_M \ge p_V(s+\delta)u_V + (1-p_V(s+\delta))p_Z(s+\delta)u_Z$$

which is equivalent to $(z, m) \succ (v, z)$. As Proposition A.4 has proved, this choice pattern cannot be generated by the rational type.

Proof of Scenario 1 concludes.

Scenario 2 There exists a college Y such that $\mathcal{R}(u, Y, s) = 2\ \mathcal{R}(u, Y, s + \delta) = 3$ and

In other words, a rational type chooses (V, X, Y, W) when priority score is s, chooses (M, Y, Z, N) when priority score is $s + \delta$. When $W \neq Z$, the analysis of this case becomes essentially the same as Scenario 1.

When $W = Z$, $M \geq X$ because the choice pattern otherwise has been proved to be impossible in Proposition A.4. Thus the scenario implies that $(X, Y, Z) \succ (Y, Z, N)$ when priority score is s, but $(Y, Z, N) \succ (X, Y, Z)$ when priority score is $s + \delta$, an impossible pattern again according to Proposition A.4.

Proof of Scenario 2 concludes.

Scenario 3 There exists a college Y such that $\mathcal{R}(u, Y, s) = 1$ and $\mathcal{R}(u, Y, s + \delta) = 2$ and

In other words, a rational type chooses (V, W, X, Y) when priority score is s, (M, N, Y, Z) when priority score is $s + \delta$. If $M \neq X$ and $N \geq X$, the optimality condition requires that $(X, Y) \succ (Y, Z)$ when score is s, but $(Y, Z) \succ (X, Y)$ when score is $s + \delta$, which according to Proposition A.4 are impossible to hold at the same time.

If $N = X$, $M \neq W$ because of proposition A.4. Consequently, $(W, X, Y) \succ (X, Y, Z)$ when score is s, $(X, Y, Z) \succ (W, X, Y)$ when score is $s + \delta$, again impossible thanks to Proposition A.4.

If $M = X$, a rational type chooses (V, W, X, Y) when priority score is s, (X, N, Y, Z) when priority score is $s + \delta$. The optimality requires that when score is s, $(W, X, Y) \succ (X, N, Y)$, which is equivalent to

$$
\begin{aligned}
&p_W(s)u_W + (1 - p_W(s))p_X(s)u_X + (1 - p_W(s))(1 - p_X(s))p_Y(s)u_Y > \\
&\quad p_X(s)u_X + (1 - p_X(s))p_N(s)u_N + (1 - p_X(s))(1 - p_N(s))p_Y(s)u_Y
\end{aligned}
$$

The optimality also implies that $u_V > u_W > u_X > u_N > u_Y > u_Z$, and consequently $max\{p_V, p_W\} < min\{p_X, p_Y, p_N, p_Z\}$. As a result, we can infer that $(1-p_W(s)) > (1-p_N(s))$, and consequently $(W, X, Y, Z) \succ (X, N, Y, Z)$ when score is s.

Together with $(X, N, Y, Z) \succ (W, X, Y, Z)$ when score is $s+\delta$, this scenario can be dealt with using the derivation in Scenario 1.

Proof of Scenario 3 concludes.

Scenario 4 There exists college Y such that $\mathcal{R}(u, Y, s) = 2$ and $\mathcal{R}(u, Y, s + \delta) = 4$.

In other words, the optimal list is (X, V, Y, W) when score is s, (Y, M, Z, N) when score is $s + \delta$. The optimality condition implies that $u_X > u_V > u_Y > u_M > u_Z > u_N$ and $u_Y > u_W$, $max\{p_X, p_W\} < min\{p_Y, p_M, p_Z, p_N\}$.

When score is $s + \delta$, we have $(Y, M, Z, N) \succ (V, Y, Z, N)$. Mathematically,

$$
\begin{aligned}
&p_Y(s + \delta)u_Y + (1 - p_Y(s + \delta))p_M(s + \delta)u_M + (1 - p_Y(s + \delta))(1 - p_M(s + \delta))EU[(Z, N)] > \\
&\quad p_V(s + \delta)u_V + (1 - p_V)(s + \delta)p_Y(s + \delta)u_Y + (1 - p_V(s + \delta))(1 - p_Y(s + \delta))EU[(Z, N)]
\end{aligned}
$$

where $EU[(Z, N)] \equiv= p_Z(s + \delta)u_Z + (1 - p_Z(s + \delta))p_N(s + \delta)u_N$.

If $W \neq M$, the optimality condition when score is $s + \delta$ implies that $(Z, N) \succ (W)$, which is equivalent to $EU[(Z, N)] > EU[(W)] \equiv p_W(s + \delta)u_W$. As $(1 - p_M) < (1 - p_V)$, we have

$$p_Y(s + \delta)u_Y + (1 - p_Y(s + \delta))p_M(s + \delta)u_M + (1 - p_Y(s + \delta))(1 - p_M(s + \delta))EU[(W)] >$$
$$p_V(s + \delta)u_V + (1 - p_V(s + \delta))p_Y(s + \delta)u_Y + (1 - p_V(s + \delta))(1 - p_Y(s + \delta))EU[(W)]$$

which implies $(Y, M, W) \succ (V, Y, W)$ when score is $s + \delta$. On the other hand, $(V, Y, W) \succ (Y, M, W)$ when is score s. As shown in Scenario 1, this is impossible.

If $W = M$, the optimal list is (X, V, Y, W) when score is s, (Y, W, Z, N) when score is $s + \delta$. In this case all the other colleges have to be different from each other: $u_X > u_V > u_Y > u_W > u_Z > u_N$. This in turn implies that $max\{p_X, p_V\} < min\{p_Y, p_Z, p_N, p_W\}$. The optimality condition when score is s implies that $(V, Y, W) \succ (Y, Z, N)$. The optimality condition when score is $s + \delta$ implies that $(Y, W, Z, N) \succ (V, Y, W, N)$. Mathematically:

$$EU[(Y, W, Z)] + (1 - p_Y(s + \delta))(1 - p_W(s + \delta))(1 - p_Z(s + \delta))p_N(s + \delta)u_N \geq$$
$$EU[(V, Y, W)] + (1 - p_V(s + \delta))(1 - p_Y(s + \delta))(1 - p_W(s + \delta))p_N(s + \delta)u_N$$

where $EU[(Y, W, Z)] \equiv p_Y(s + \delta)u_Y + (1 - p_Y(s + \delta))p_W(s + \delta)u_W + (1 - p_Y(s + \delta))(1 - p_W(s + \delta))p_Z(s + \delta)u_Z$, $EU[(V, Y, W)] \equiv p_V(s + \delta)u_V + (1 - p_V(s + \delta))p_Y(s + \delta)u_Y + (1 - p_V(s + \delta))(1 - p_Y(s + \delta))p_W(s + \delta)u_W$ As $(1 - p_Z(s + \delta)) < (1 - p_V(s + \delta))$, we have $EU[(Y, W, Z)] > EU[(V, Y, W)]$, which implies that $(Y, W, Z) \succ (V, Y, W)$ when score is $s + \delta$. Impossible according to Proposition A.4.

Proof of Scenario 4 concludes.

Scenario 5 There exists a college Y such that $\mathcal{R}(u, Y, s) = 1$ and $\mathcal{R}(u, Y, s + \delta) = 3$

In other words, the optimal list is (X, V, W, Y) when score is s, (M, Y, Z, N) when score is $s + \delta$. The optimality condition requires that $u_X > u_V > u_W > u_Y > u_Z > u_N$ and $u_M > u_Y$. This in turn implies that $max\{p_X, p_V, p_W\} < min\{p_M, p_Y, p_Z, p_N\}$. If all the letters here denote different colleges, the optimality condition implies that $(Y, Z, N) \succ (W, Y, N)$ when score is $s + \delta$. As $(1 - p_Z) < (1 - p_W)$, we have $(Y, Z) \succ (W, Y)$ when score is $s + \delta$. When score is s, however, we have $(W, Y) \succ (Y, Z)$, which is impossible according to Proposition A.4.

If some letters denote the same college, the only possibility is that M could be X, V or W. If $M = X$ or $M = V$, the same derivation can be applied as well. If $M = W$, the optimal list is (W, Y, Z, N) when score is s, and (X, V, W, Y) when score is s', which appears to be the same Scenario 4.

Proof of Scenario 5 concludes.

Scenario 6 There exists college Y such that $\mathcal{R}(u, A, s) = 1$ and $\mathcal{R}(u, Y, s + \delta) = 4$.

In other words, the optimal list is (Y, M, N, L) when score is $s + \delta$, (W, V, X, Y) when score is s. The optimality condition implies that $u_W > u_V > u_X > u_Y > u_M > u_N > u_L$, and $max\{p_W, p_V, p_X\} < min\{p_M, p_N, p_L, p_Y\}$. As (W, V, X, Y) is the optimal list when score is s, $(X, Y) \succ (Y, M)$ when score is s.

Moreover, as the optimality list is (Y, M, N, L) when score is $s+\delta$. We have $(Y, M, N, L) \succ (X, Y, N, L)$ when score is $s + \delta$. Mathematically this is equivalent to

$$p_Y(s+\delta)u_Y + (1 - p_Y(s+\delta))p_M(s+\delta)u_M + (1 - p_Y(s+\delta))(1 - p_M(s+\delta))EU[(N, L)] \geq$$
$$p_X(s+\delta)u_X + (1 - p_X(s+\delta))p_Y(s+\delta)u_Y + (1 - p_X(s+\delta))(1 - p_Y(s+\delta))EU[(N, L)]$$

where $EU[(N, L)] \equiv p_N(s+\delta)u_N + (1 - p_N(s+\delta))p_L(s+\delta)u_L$ As $(1 - p_M(s+\delta)) < (1 - p_X(s+\delta))$, we have

$$p_Y(s+\delta)u_Y + (1 - p_Y(s+\delta))p_M(s+\delta)u_M \geq p_X(s+\delta)u_X + (1 - p_X(s+\delta))p_Y(s+\delta)u_Y$$

which is equivalent to $(Y, M) \succ (X, Y)$. This pattern has been proved to be impossible in Proposition A.4.

Proof of Scenario 6 concludes.

□

Under Assumption 1, 2, 3, for any preference profile u, college A, $C^{RN}_{(u,A,\kappa)} \equiv \{s | \mathcal{R}(u, A, s) = \kappa\}$ is connected if $\kappa \geq 1$.

Scenario 1 $\kappa = 4$ It suffices to show that if college X is listed in a specific position when priority score is $\bar{s}$ and $\underline{s}$, then it is the best candidate for that position as well for any s such that $\underline{s} < s < \bar{s}$. Mathematically For any college $Y \neq X$, we have

$$p_Y(\bar{s})u_Y + (1 - p_Y(\bar{s}))U(\bar{s}) \leq p_X(\bar{s})u_X + (1 - p_X(\bar{s}))U(\bar{s})$$

$$p_Y(\underline{s})u_Y + (1 - p_Y(\underline{s}))U(\underline{s}) \leq p_X(\underline{s})u_X + (1 - p_X(\underline{s}))U(\underline{s})$$

where $U(s)$ represent the utility of the list of colleges chosen below the current position when priority score is s. Since $\underline{s} < s < \bar{s}$, we have $U(\underline{s}) \geq U(s) \geq U(\bar{s})$. Importantly, note that this holds because X cannot be any of the non-top choices $(1 \leq \mathcal{R}(u, X, s) < 4)$ when $s \in [\underline{s}, \bar{s}]$ thanks to Proposition A.4. The two equations above are equivalent to

$$\frac{p_X(\bar{s})}{p_Y(\bar{s})} \geq \frac{u_Y - U(\bar{s})}{u_X - U(\bar{s})}$$

$$\frac{p_X(\underline{s})}{p_Y(\underline{s})} \geq \frac{u_Y - U(\underline{s})}{u_X - U(\underline{s})}$$

Next we show that it is true that

$$\frac{p_X(s)}{p_Y(s)} \geq \frac{u_Y - U(s)}{u_X - U(s)}$$

We analyze whether X is a better choice when probability is $p_X(s)$, case by case. Case (I): $p_X > p_Y$, $u_X < u_Y$. In this case we have

$$\frac{p_X(s)}{p_Y(s)} \geq \frac{p_X(\bar{s})}{p_Y(\bar{s})} \geq \frac{u_Y - U(\bar{s})}{u_X - U(\bar{s})} \geq \frac{u_Y - U(s)}{u_X - U(s)}$$

Case (II): $p_X < p_Y$, $u_X > u_Y$. In this case we have

$$\frac{p_X(s)}{p_Y(s)} \geq \frac{p_X(\underline{s})}{p_Y(\underline{s})} \geq \frac{u_Y - U(\underline{s})}{u_X - U(\underline{s})} \geq \frac{u_Y - U(s)}{u_X - U(s)}$$

Case (III): $p_X > p_Y$, $u_X > u_Y$, obviously X is better regardless of probability. Case (IV): $p_X < p_Y$, $u_X < u_Y$, Y must be chosen regardless of probability, which leads to contradiction. Proof of Scenario 1 concludes. $\kappa \leq 3$ The proof of Scenario 1 can be largely recycled, with the only complication being whether X could be in a position where $\mathcal{R}(u, X, s) > \kappa$. This is again impossible thanks to Proposition A.4. □

Remark Proposition A.4 and A.4 together imply Theorem 1.

Suppose Assumption 1, 2, 3 hold, and that the tie of expected utility among listed colleges is limited to at most two colleges. For any u and A, if there exists $\underline{s} < \bar{s}$ such that $\mathcal{D}(u, A, \underline{s}) = 0$ and $\mathcal{D}(u, A, \bar{s}) = \kappa \geq 2$, then there exists $\underline{s} < s < \bar{s}$ such that $1 \leq \mathcal{D}(u, A, s) \leq \kappa - 1$.

Proof. Proof. Let $f_A(s) = p_A(s)u_A$. $f_A(s)$ is continuous for any college A. Thus for any pairs of A, X, function $\Delta_{AX}(s) \equiv f_A(s) - f_X(s)$ is continuous. Under the assumptions, $\Delta_{AX}(s)$ switches signs at most once. If $p_A(s) > p_X(s)$, it can possibly switch from positive to negative; it $p_X(s) > p_A(s)$, it can possibly switch from negative to positive.

Define $s_4 \equiv sup_s\{s|\mathcal{D}(u, A, s) = 0\}$. According to the continuity we know that there exists college B such that $\Delta_{AB}(s_4) = 0$. This college must be listed at the fourth place, and $\Delta_{AB}(s_4)$ is switching from negative to positive, because otherwise given the assumptions A will not be moved in the neighborhood of s_4. Thus $\mathcal{D}(u, A, s) = 1$ in the neighborhood of s_4.

If $\kappa = 3$, the key is to consider $s_2 \equiv sup_s\{s|\mathcal{D}(u, A, s) > 1\}$. We can infer using the same method that when s is in the neighborhood of s_2, $\mathcal{D}(u, A, s) = 2$.

If $\kappa = 4$, the key is to consider $s_3 \equiv sup_s\{s|\mathcal{D}(u, A, s) > 2\}$. We can infer using the same method that when s is in the neighborhood of s_3, $\mathcal{D}(u, A, s) = 3$.

□

Remark Proposition A.4 implies Theorem 2.

In the setting as detailed in Section 1.7, we have:

- For Rational Decision Rule:

1. When $\delta > 2$, the optimal list is $(B1, B2)$ if $\frac{p_A}{p_B} < \frac{1-p_B}{\delta-2p_B}$; the optimal list is $(A1, B1)$ if $\frac{1-p_B}{\delta-2p_B} < \frac{p_A}{p_B} < \frac{2}{\delta}$; the optimal list is $(A1, A2)$ if $\frac{p_A}{p_B} > \frac{2}{\delta}$.
2. When $1 < \delta < 2$, the optimal list is $(B1, B2)$ if $\frac{p_A}{p_B} < \frac{1}{\delta}$; the optimal list is $(B1, A1)$ if $\frac{p_A}{p_B} > \frac{1}{\delta}$.
3. When $\delta < 1$, the optimal list is $(B1, B2)$.

- For the DC Decision Rule:

1. When $\delta > 2$, the optimal list is $(B1, B2)$ if $\frac{p_A}{p_B} < \frac{1}{\delta}$; $(B1, A1)$ if $\frac{1}{\delta} < \frac{p_A}{p_B} < \frac{2}{\delta}$; $(A1, A2)$ if $\frac{p_A}{p_B} > \frac{2}{\delta}$;
2. When $1 < \delta < 2$, the optimal list is $(B1, B2)$ if $\frac{p_A}{p_B} < \frac{1}{\delta}$; $(B1, A1)$ if $\frac{p_A}{p_B} > \frac{1}{\delta}$;
3. When $\delta < 1$, the optimal list is $(B1, B2)$.

Proof. The expected utility of $(A1, A2)$ is

$$(2p_A - p_A^2)\delta$$

The expected utility of $(A1, B1)$ is

$$p_A\delta + 2(1 - p_A)p_B$$

The expected utility of $(B1, B2)$ is

$$3p_B - p_B^2$$

Thus we have,

$$(A1, B1) \succ (B1, B2) \iff \frac{p_A}{p_B} > \frac{1-_B}{\delta - 2p_B}$$

$$(A1, A2) \succ (A1, B1) \iff \frac{p_A}{p_B} > \frac{2}{\delta}$$

$$(A1, A2) \succ (B1, B2) \iff \frac{p_A}{p_B} > \frac{3 - P_B}{2 - P_A}\frac{1}{\delta}$$

$$(B1, A1) \succ (B1, B2) \iff \frac{p_A}{p_B} > \frac{1}{\delta}$$

□

Remark Proposition A.4 provides the intermediate results for Theorem 1.7.

A.5 Additional Institutional Details

College Application

The admission process is stratified according to college quality. Before 2019, the colleges were classified into three tiers of decreasing quality: elite (first-tier), public non-elite (second-tier) and private non-elite (third-tier). The latter two categories merged starting in 2019, but the elite category remains unchanged. In this paper, we focus on the admission to elite colleges, which are argued to play a central role in upward mobility in China because of the tremendous value placed on education and the huge return in labor markets (Jia and Li, 2016).

The share of students who are eligible for 1st-tier colleges is roughly 20 ∼ 25% of the exam takers. The eligible students on the science track can choose up to four colleges from among 239 elite colleges. For those who are on the humanities track, the total number of elite colleges is 150. Science track students account for more than 80% of the first-tier applicants.

Priority Score

The priority score is almost completely determined by the College Entrance Exam (CEE)[1]. The CEE is a nationwide closed-book written exam held once a year on June 7th and 8th, with the rare exception that the exam was postponed to July 7th and 8th in 2020 due to COVID-19. To apply for colleges in an admission cycle, all students must take the CEE of the same cycle. In each province, students on the same track (Humanities or Sciences) will take the same exam.[2] As demonstrated in Figure A.1, it will take up to two weeks for Ningxia Provincial Education Authorities to grade students' exams. Students will be notified of their exam score and ranking in Ningxia around the 20th-25th of the month in which the exam takes place.

Exams for both tracks include Chinese, Mathematics[3] and English. For each of these subjects, students get an integer score, with the maximum (best) possible being 150 and the minimum being 0. Additionally, students on the Humanities Track take a comprehensive exam on history, politics, and geography, whereas students on the Sciences Track take another exam on physics, chemistry, and biology. This track-specific exam accounts for 300 points. Thus the total score of the CEE (sum of the scores from the four subjects) is 750 points. In case of a tie in total score, ranking will be determined by the score in the comprehensive exam in the respective track, Mathematics, and English, in a lexicographic way.

[1]Exceptions include winners of international Olympiad contests, students who win sports scholarships, students with exceptional art talent, students who belong to certain minority ethnic groups, etc. These exceptions are also quantified and added to priority scores on top of the exam scores, and are observable in our data.

[2]See Wang, Wang, and Ye (2021) and Li et al. (2021) for more institutional details about the CEE.

[3]Mathematics for the Humanities Track differs from that for the Science Track.

Correlation in Admission Events

If the probability of meeting the cutoff of one college is correlated with meeting that of another, our assumptions about independence are defied. In this case, assuming independence of admission probability alone may result in suboptimal portfolio choices (Shorrer, 2019; Rees-Jones, Shorrer, and Tergiman, 2020). However, in our case students know their priority score and ranking by the time of application, and researchers have assumed independence in similar settings (Larroucau and Rios, 2018, 2020).

Nevertheless, to test the potential presence of pairwise correlation, we conduct an empirical test with the same specification as we used in Section 1.3 on the administrative dataset. The difference is that, in this new empirical exercise, the dependent variable is students' second choices, and the regression is run on those who were not admitted by their first choice:

$$1(\text{Admitted to Second Choices})_i = \alpha_2 + \beta_2 \hat{p}_{ij}$$

If the admission to the first choices does not correlate with the admission probability of the second choices, we would expect $\alpha_2 = 0$ and $\beta_2 = 1$, which is the null hypothesis of this exercise.

The estimation results suggest that our estimates of admission probability remain accurate conditional on the rejection of the first choices, suggesting that in our setting pairwise correlation does not significantly alter admission probability. As shown in Columns 3 and 4 of Table A.2, $\hat{\alpha_2} = 0.0033$ (SE=0.0037) and -0.0094 (SE=0.0060) for the science and humanity tracks, respectively, whereas $\hat{\beta_2} = 0.9883$ (SE=0.0060) and 1.0252 (SE=0.0107) for the science and humanity tracks, respectively. The p-values of the F-test are 0.085 and 0.059, respectively, meaning that we fail to reject the null hypothesis.

College Preferences vs. Major Preferences

Major preference does not affect assignment of college; essentially the Chinese system is a "college-then-major" system (Chen and Kesten, 2017; Calsamiglia, Fu, and Güell, 2020). Major studies typically begin in the second year of bachelor education, and since the 2010s, the Ministry of Education (MoE) of PRC has successfully pushed for a lower barrier in major switching [4]. To assess whether major concerns affect risk taking, we asked students which factor they were most concerned about in college applications. We report the relevant statistical analysis in Table A.8. In Columns 1 and 2, we regress the dummy indicating whether students consider major to be their top concern on a normalized SES index. Only 13.7% of the survey takers consider major to be their top concern, and the share is slightly lower among the advantaged students.

Consideration of major could affect students' strategy if students think ahead, and want to outcompete their peers who are admitted to the same college in terms of academic ability. To examine its quantitative impact on risk-taking, we regress the estimated

[4] The link here is an example. Since this campaign by the MoE, major distinctions have become more coarse and less of a concern to students.

unconditional probability of the first choices (Columns 3-4) and beliefs about being admitted to the first choices (Columns 5-6) on those who consider major to be of top concern. As expected, students tend to be more cautious if they consider major to be of top concern, but its quantitative impact on the probability of first choices is less than 10% compared to those who do not regard major as their top consideration. We calibrate its impact on the full sample average of first choice probabilities by calculating the product of the average impact due to major concerns and the share of students who consider major to be important. Reassuringly, it contributes at most a 1.2% increase in the mean unconditional probability of the first choices.

Appendix B

Chapter 2 Supplementary Materials

B.1 Supplementary Tables and Figures

Table B.1: Distribution of Students' Preferences and Admitted Majors

	Economics / Business	Science	Law / Sociology	Humanities / Languages	Total
Panel A: Overall Preferences in Different Ranks					
1st Preference	989				989
2nd Preference	798	78	73	34	989
3rd Preference	687	71	112	81	989
4th Preference	570	98	163	94	989
5th Preference	505	110	148	110	989
6th Preference	442	107	143	93	989
Admitted to the Major	493	197	176	123	989
Panel B: Students Admitted to Their Ranked-preference Majors					
Rank	1 st	2nd	3rd	4th	
Count	243	229	173	108	989

This table describes the distribution of preferred majors and ultimate admissions among 989 students by matching the survey data to the university administrative admission database. Panel A quantifies the distribution of applicants whose 1st, 2nd,..., 6th preferences fell into the categories for economics/business majors, natural sciences, law/sociology, humanities/language studies. Panel B illustrates which major (e.g., first choice, second choice) individuals were ultimately admitted into.

Table B.2: Proportion of Students Taking Compulsory Courses: Concepts in Economics

Course	Micro		Macro		Finance	
Semester	Econ	Non-econ	Econ	Nonecon	Econ	Eonecon
1	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%
2	100.00%	40.70%	0.00%	0.00%	0.00%	0.00%
3	100.00%	40.70%	100.00%	37.63%	61.26%	15.34%
4	100.00%	40.70%	100.00%	37.63%	65.98%	15.34%
5	100.00%	40.70%	100.00%	37.63%	65.98%	15.34%
6	100.00%	40.70%	100.00%	37.63%	65.98%	15.34%
7	100.00%	40.70%	100.00%	37.63%	65.98%	15.34%
8	100.00%	40.70%	100.00%	37.63%	65.98%	15.34%

Table B.3: Proportion of Students Taking Compulsory Courses: Concepts in Statistics

Course	Probability		Statistics		Econometrics	
Semester	Econ	Non-econ	Econ	Non-econ	Econ	Nonecon
1	0.00%	0.00%	4.57%	11.45%	0.00%	0.00%
2	15.28%	11.45%	4.57%	11.45%	0.00%	0.00%
3	100.00%	53.99%	4.57%	11.45%	0.00%	0.00%
4	100.00%	53.99%	56.69%	23.72%	32.28%	0.00%
5	100.00%	53.99%	70.08%	23.72%	71.34%	20.86%
6	100.00%	53.99%	70.08%	23.72%	71.34%	20.86%
7	100.00%	53.99%	70.08%	23.72%	71.34%	20.86%
8	100.00%	53.99%	70.08%	23.72%	71.34%	20.86%

Table B.4: Pre-college Rankings and Decision-making

Dep. Var.	(1) MPL1 Risk Neutral	(2) MPL2 Risk Neutral	(3) Law of Large Numbers	(4) Two Indifferent Choices	(5) Exact Representiveness	(6) Dictator's Sharing	(7) Bystander's Belief
Non-Economics× Top Ranking	-0.133***	-0.087**	-1.213**	0.521	-1.031*	-7.000	27.165*
	(0.040)	(0.040)	(0.532)	(0.436)	(0.546)	(15.917)	(16.001)
Non-Economics× Middle Ranking	-0.118**	-0.030	-1.412**	-0.350	-1.343**	11.401	18.771
	(0.047)	(0.048)	(0.637)	(0.521)	(0.653)	(18.870)	(19.697)
Non-Economics× Bottom Ranking	-0.103**	0.006	-1.009	0.689	-1.124	0.859	17.671
	(0.050)	(0.051)	(0.677)	(0.554)	(0.694)	(20.366)	(21.678)
Common Support of Major Preference	X	X	X	X	X	X	X
Constant	0.353***	0.312***	18.314***	15.092***	25.443***	187.892***	165.262***
	(0.026)	(0.027)	(0.354)	(0.290)	(0.363)	(10.279)	(10.624)
Observations	802	802	802	802	802	274	275
R-squared	0.043	0.019	0.022	0.013	0.023	0.053	0.019

In this table, we introduce three interaction terms between pre-college rankings and a dummy of non-economics education. The first row reports the differences in the treatment effects between top-ranking (pre-college) students who are assigned to a non-economics majors, compared to the average outcomes of economics students, while the second and third show the differences in treatments of middle-ranking and bottom-ranking students, relative to the average outcomes of economics students.

The dependent variables in columns (1) and (2) are the share of risk neutral students in MPL 1 and MPL 2, in which we pool WTA and WTP together. Columns (3), (4), and (5) report results on probabilistic belief questions on the law of large numbers (LLN), two identical choices, and Exact Representativeness (ER), respectively. The outcome variables in columns (6) and (7) are the Dictators' actual sharing and Bystander's beliefs regarding the Dictators' sharing in the Dictator Game.

All columns control for a vector of dummies that denotes whether students' majors in their rank-order list belong to the economics major category (*Major-Preference FX*), and limit the regression sample to students who put both economics and non-economics majors in their rank-order list (*Common Support of Major Preference*).

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table B.5: The Rank of the Admitted Major and Decision-making

Dep. Var.	(1) MPL1 Risk Neutral	(2) MPL2 Risk Neutral	(3) Law of Large Numbers	(4) Two Indifferent Choices	(5) Exact Representiveness	(6) Dictator's Sharing	(7) Bystander's Belief
Admitted in 3rd	0.031	-0.077	0.407	-0.786	0.214	10.476	18.890
	(0.055)	(0.050)	(0.732)	(0.601)	(0.750)	(20.406)	(23.570)
Admitted in 4th	0.010	-0.036	0.659	-1.188*	0.589	-24.469	49.618*
	(0.063)	(0.056)	(0.839)	(0.689)	(0.860)	(23.028)	(27.543)
Admitted in 5th	0.040	0.009	0.541	-1.194	-1.828*	7.373	13.042
	(0.072)	(0.064)	(0.964)	(0.790)	(0.987)	(32.910)	(28.412)
Admitted in 6th	-0.017	-0.044	2.410**	-0.935	0.102	1.485	10.245
	(0.083)	(0.077)	(1.115)	(0.914)	(1.142)	(29.531)	(42.952)
Admitted in 6th+	X	X	X	X	X	X	X
F-Joint Test	0.17	1.21	0.95	0.99	1.32	0.49	0.78
P-value	(0.97)	(0.30)	(0.45)	(0.42)	(0.25)	(0.78)	(0.56)
Econ-related Majors FX	X	X	X	X	X	X	X
Major-Preference FX	X	X	X	X	X	X	X
Common Support of Major Preference	X	X	X	X	X	X	X
Constant	0.269***	0.314***	17.150***	15.881***	24.844***	188.856***	164.705***
	(0.030)	(0.025)	(0.401)	(0.329)	(0.411)	(11.933)	(12.100)
Observations	802	802	802	802	802	274	275
R-squared	0.044	0.019	0.028	0.015	0.031	0.059	0.033

This table reports the results using regression equation (4). Variables *Admitted in 3rd - Admitted in 6th* denote that students were admitted to the third,...,sixth rank of their rank-order list. *Admitted in 6th+* indicates that students didn't meet the cutoffs of all the six majo-preferences and were assigned to a major by CUFE.

The dependent variables in columns (1) and (2) are the share of risk neutral students in MPL 1 and MPL 2, in which we pool WTA and WTP together. Columns (3), (4), and (5) report results on probabilistic belief questions on the law of large numbers (LLN), two identical choices, and Exact Representativeness (ER), respectively. The outcome variables in columns (6) and (7) are the Dictators' actual sharing and Bystander's beliefs regarding the Dictators' sharing in the Dictator Game.

All columns control for a vector of dummies that denotes whether students' majors in their rank-order list belong to the economics major category (*Major-Preference FX*), and limit the regression sample to students who put both economics and non-economics majors in their rank-order list (*Common Support of Major Preference*).

F-test for the joint significance of the dummies for the position of admitted major in the rank-order list and p-values are provided.

Table B.6: Out of Pocket, Preferences and Beliefs

Dep. Var.	(1) MPL1 Risk Neutral	(2) MPL2 Risk Neutral	(3) Law of Large Numbers	(4) Two Indifferent Choices	(5) Exact Representiveness	(6) Dictator's Sharing	(7) Bystander's Belief
Econ	0.154***	0.076**	1.519***	-0.496	1.411***	-10.276	-17.631
	(0.032)	(0.033)	(0.430)	(0.353)	(0.444)	(12.575)	(13.075)
In Deficit	0.002	0.002	-0.013	0.002	-0.003	0.180	6.583
	(0.001)	(0.001)	(0.015)	(0.013)	(0.016)	(0.268)	(5.178)
Common Support of Major Preference	X	X	X	X	X	X	X
Constant	0.218***	0.250***	16.946***	15.460***	24.173***	192.755***	186.403***
	(0.020)	(0.020)	(0.266)	(0.218)	(0.274)	(8.047)	(8.199)
Observations	796	796	796	796	796	270	275
R-squared	0.031	0.009	0.016	0.002	0.013	0.004	0.012

In this table, we introduce a variable of financial status last month, *In Deficit*, as a control variable, which equals the difference between money from family and spending last month.

The dependent variables in columns (1) and (2) are the share of risk neutral students in MPL 1 and MPL 2, in which we pool WTA and WTP together. Columns (3), (4), and (5) report results on probabilistic belief questions on the law of large numbers (LLN), two identical choices, and Exact Representativeness (ER), respectively. The outcome variables in columns (6) and (7) are the Dictators' actual sharing and Bystander's beliefs regarding the Dictators' sharing in the Dictator Game.

All columns control for a vector of dummies that denotes whether students' majors in their rank-order list belong to the economics major category (*Major-Preference FX*), and limit the regression sample to students who put both economics and non-economics majors in their rank-order list (*Common Support of Major Preference*).

Standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table B.7: Heterogeneous Effects by Gender

Dep. Var.	(1) MPL1 Risk Neutral	(2) MPL2 Risk Neutral	(3) Law of Large Numbers	(4) Identical Choices	(5) Exact Representiveness	(6) Dictator's Sharing	(7) Bystander's Belief
Econ* Male	0.111*	0.015	1.850**	-0.620	-1.237	-15.266	-36.177
	(0.066)	(0.068)	(0.885)	(0.735)	(0.914)	(26.811)	(27.031)
Econ	0.084**	0.047	0.579	-0.072	1.667***	4.217	-7.702
	(0.043)	(0.044)	(0.571)	(0.474)	(0.590)	(16.437)	(18.144)
Male	0.084**	0.063	1.130**	0.693	1.804***	-10.453	8.045
	(0.042)	(0.043)	(0.563)	(0.468)	(0.581)	(17.834)	(17.292)
Major-Preference FX	X	X	X	X	X	X	X
Common Support of Major Preference	X	X	X	X	X	X	X
Constant	0.199***	0.237***	16.653***	15.153***	23.628***	191.426***	184.770***
	(0.026)	(0.026)	(0.342)	(0.284)	(0.354)	(10.040)	(11.302)
Observations	802	802	802	802	802	274	275
R-squared	0.065	0.020	0.049	0.011	0.035	0.056	0.026

This table presents the average difference in survey responses between economics students and non-economics students by gender by introducing an interaction term *Male* × *Economics* using regression equation (3).

The dependent variables in columns (1) and (2) are the share of risk neutral students in MPL 1 and MPL 2, in which we pool WTA and WTP together. Columns (3), (4), and (5) report results on probabilistic belief questions on the law of large numbers (LLN), two identical choices, and Exact Representativeness (ER), respectively. The outcome variables in columns (6) and (7) are the Dictators' actual sharing and Bystander's beliefs regarding the Dictators' sharing in the Dictator Game.

All columns control for a vector of dummies that denotes whether students' majors in their rank-order list belong to the economics major category (*Major-Preference FX*), and limit the regression sample to students who put both economics and non-economics majors in their rank-order list (*Common Support of Major Preference*).

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table B.8: Robust Check Using [-0.15, 0.15] Times the Standard Deviation

	Risk Preferences		Probabilistic Beliefs (3)-(5)			Dictator Game		Trust Game (8)-(11)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	MPL1	MPL2	Law of Large	Two Indifferent	Exact	Dictator's	Bystander's	Proposer's	Bystander's	Reciprocity of	Bystander's Belief
Dep. Var.	Risk Neutral	Risk Neutral	Numbers	Choices	Representiveness	Sharing	Belief	Sharing	Belief	Player 2	about Reciprocity
Econ=1	0.147***	0.070**	1.439***	-0.402	1.409***	-8.551	-15.888	-11.689**	9.788	-3.370	0.721
	(0.031)	(0.032)	(0.410)	(0.339)	(0.422)	(12.038)	(12.763)	(5.768)	(6.917)	(3.530)	(5.965)
M′=(-50, 0, 50)										1.195***	
										(0.055)	
Econ*M′										-0.035	
										(0.087)	
M′=(-50, 0, 50)											1.147***
											(0.094)
Econ*M′											-0.403***
											(0.145)
Common Support	X	X	X	X	X	X	X	X	X	X	X
Constant	0.223***	0.257***	16.978***	15.522***	24.192***	192.252***	186.535***	127.711***	100.156***	100.186***	98.064***
	(0.020)	(0.020)	(0.261)	(0.216)	(0.268)	(7.912)	(8.190)	(3.742)	(4.301)	(2.241)	(3.790)
Observations	849	849	849	849	849	294	289	430	419	848	848
R-squared	0.026	0.006	0.014	0.002	0.013	0.002	0.005	0.010	0.005	0.477	0.186

In this table, we limit the regression sample using a new criteria: students lying in the 0.15 times the standard deviation within the distribution of the CEE score.
The dependent variables in columns (1) and (2) are the share of risk neutral students in MPL 1 and MPL 2, in which we pool WTA and WTP together. Columns (3), (4), and (5) report results on probabilistic belief questions on the law of large numbers (LLN), two identical choices, and Exact Representativeness (ER), respectively. The outcome variables in columns (6) and (7) are the Dictators' actual sharing and Bystander's beliefs regarding the Dictators' sharing in the Dictator Game. Column (8) analyzes how an economics education affects students' sharing behavior as the Proposer in the Trust Game, which could be interpreted as students' beliefs regarding the amount that the other players would like to reciprocate. The dependent variable is Bystanders' belief regarding the mean amount of Player A's sharing in the Trust Game in column (9). Columns (10) and (11) ask Player B the amount she would like to give back if Player A gives 50, 100, 150 Yuan and Bystander's belief regarding the mean amount of Player B's giving back.
All columns control for a vector of dummies that denotes whether students' majors in their rank-order list belong to the economics major category (*Major-Preference FX*), and limit the regression sample to students who put both economics and non-economics majors in their rank-order list (*Common Support of Major Preference*).
Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table B.9: Economics &Business Majors before Non-economics Majors

	Risk Preferences		Probabilistic Beliefs (3)-(5)			Dictator Game		Trust Game (8)-(11)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	MPL1	MPL2	Law of Large	Two Indifferent	Exact	Dictator's	Bystander's	Proposer's	Bystander's	Reciprocity of	Bystander's Belief
Dep. Var.	Risk Neutral	Risk Neutral	Numbers	Choices	Representiveness	Sharing	Belief	Sharing	Belief	Player 2	about Reciprocity
Econ*M'										-0.062	-0.238*
										(0.123)	(0.127)
Econ=1	0.147***	-0.017	1.581***	0.464	1.727***	-11.908	-32.883*	-8.611	9.105	-1.666	-5.206
	(0.043)	(0.045)	(0.610)	(0.505)	(0.593)	(17.252)	(18.525)	(8.467)	(10.317)	(5.024)	(5.188)
M'=(-50, 0, 50)										1.171***	1.221***
										(0.071)	(0.073)
Common Support	X	X	X	X	X	X	X	X	X	X	X
Constant	0.186***	0.261***	16.634***	15.092***	24.088***	206.486***	184.966***	123.103***	94.943***	97.684***	97.356***
	(0.024)	(0.025)	(0.342)	(0.283)	(0.332)	(9.387)	(11.005)	(4.808)	(5.703)	(2.817)	(2.907)
Observations	430	430	430	430	430	152	136	214	216	430	430
R-squared	0.026	0.000	0.015	0.002	0.019	0.003	0.023	0.005	0.004	0.481	0.467

In this table, we limit the regression sample to students who always put economics &business majors before all the non-economics majors.
The dependent variables in columns (1) and (2) are the share of risk neutral students in MPL 1 and MPL 2, in which we pool WTA and WTP together. Columns (3), (4), and (5) report results on probabilistic belief questions on the law of large numbers (LLN), two identical choices, and Exact Representativeness (ER), respectively. The outcome variables in columns (6) and (7) are the Dictators' actual sharing and Bystander's beliefs regarding the Dictators' sharing in the Dictator Game. Column (8) analyzes how an economics education affects students' sharing behavior as the Proposer in the Trust Game, which could be interpreted as students' beliefs regarding the amount that the other players would like to reciprocate. The dependent variable is Bystanders' belief regarding the mean amount of Player A's sharing in the Trust Game in column (9). Columns (10) and (11) ask Player B the amount she would like to give back if Player A gives 50, 100, 150 Yuan and Bystander's belief regarding the mean amount of Player B's giving back.
All columns control for a vector of dummies that denotes whether students' majors in their rank-order list belong to the economics major category (*Major-Preference FX*), and limit the regression sample to students who put both economics and non-economics majors in their rank-order list (*Common Support of Major Preference*).
Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

B.2 Details of Survey Design

Risk Preferences Elicitation

There are three sets of questions to elicit students' risk preferences.

The first set of questions elicit students' either Willingness to Pay (WTP) or Willingness to Accept (WTA) between a given lottery and a series of monotonically increasing certain payoffs, {25 RMB, 30, 35,..., 55, 60}. The lottery pays 30 RMB with probability 0.25 and 60 RMB with probability 0.75, and is the same for all students. The students are, however, randomized into two groups where one group is asked about their WTP for this lottery and the other group is asked about their WTA for this lottery. Both WTP and WTA are elicited using a multiple price-list style table.

In the case of WTA, for each row, subjects are presented the money they would get had they sold out the lottery. Then, subjects are given two options, "sell" and "not sell", which are presented on the right side of the "selling price". The table below shows the structure of WTA mode:

Table B.10: WTA

# of question	Price	Options	
1	25	Sell	Keep
2	30	Sell	Keep
3	35	Sell	Keep
4	40	Sell	Keep
5	45	Sell	Keep
6	50	Sell	Keep
7	55	Sell	Keep
8	60	Sell	Keep

In the case of WTP, each subject is endowed with 60 RMB. For each row, subjects are presented the money they would have to pay had they bought the lottery. Then, subjects are given two options, "buy" and "not buy", which are presented on the right side of the "buying price". The table below shows the structure of WTP mode:

Table B.11: WTP

# of question	Price	Options	
1	25	Buy	Not Buy
2	30	Buy	Not Buy
3	35	Buy	Not Buy
4	40	Buy	Not Buy
5	45	Buy	Not Buy
6	50	Buy	Not Buy
7	55	Buy	Not Buy
8	60	buy	Not buy

As we can see from the preceding table, for the WTP mode, as the price increases from top to bottom, the deal becomes less and less appealing. Therefore, we expect a student who pays enough attention to such questions to select "Buy" first and then, at some point opt into "Not Buy" (of course, she could just choose "Buy" all the way from question 1 to 8). Our data suggests that this is the case: the vast majority of students answer in a consistent way with at most one switching point. The question where the answer differs from the previous question is called the "switching point".

For the second set of questions, we follow the price-list methodology developed by Holt and Laury (2002). Each decision row is a choice between Option A and B. Option A receives 30 RMB with probability $Pr = 0.25$, and 60 with $Pr = 0.75$. The series of Option B receives 400 with increasing probabilities $Pr(400) = \{0.01, 0.03, 0.05, 0.07, ..., 0.23, 0.25\}$, or receives nothing, with probability $1 - Pr(400)$. The structure of the elicitation is as follows:

Table B.12: The Second Set of Questions

# of question	Option A	Option B
9	Pay 30 w.p.0.25, pay 60 w.p.0.75	Pay 400 w.p.0.01, pay 0 w.p.0.99
10	Pay 30 w.p.0.25, pay 60 w.p.0.75	Pay 400 w.p.0.03, pay 0 w.p.0.97
11	Pay 30 w.p.0.25, pay 60 w.p.0.75	Pay 400 w.p.0.05, pay 0 w.p.0.95
12	Pay 30 w.p.0.25, pay 60 w.p.0.75	Pay 400 w.p.0.07, pay 0 w.p.0.93
13	Pay 30 w.p.0.25, pay 60 w.p.0.75	Pay 400 w.p.0.09, pay 0 w.p.0.91
14	Pay 30 w.p.0.25, pay 60 w.p.0.75	Pay 400 w.p.0.11, pay 0 w.p.0.89
15	Pay 30 w.p.0.25, pay 60 w.p.0.75	Pay 400 w.p.0.13, pay 0 w.p.0.87
16	Pay 30 w.p.0.25, pay 60 w.p.0.75	Pay 400 w.p.0.15, pay 0 w.p.0.85
17	Pay 30 w.p.0.25, pay 60 w.p.0.75	Pay 400 w.p.0.17, pay 0 w.p.0.83
18	Pay 30 w.p.0.25, pay 60 w.p.0.75	Pay 400 w.p.0.19, pay 0 w.p.0.81
19	Pay 30 w.p.0.25, pay 60 w.p.0.75	Pay 400 w.p.0.21, pay 0 w.p.0.79
20	Pay 30 w.p.0.25, pay 60 w.p.0.75	Pay 400 w.p.0.23, pay 0 w.p.0.77
21	Pay 30 w.p.0.25, pay 60 w.p.0.75	Pay 400 w.p.0.25, pay 0 w.p.0.75

This set of questions is similar to the previous. The left column presents a fixed option and the right column becomes better and better as the # of the question increases. We would expect same pattern (switching between options once at most), and, indeed, what we find verifies our expectation.

As Holt and Laury (2002) notes, such a manner of elicitation can characterize subjects' preferences because of the monotonicity of the column. Since one of the two options is fixed while the other becomes better and better (or worse and worse) over time, the presence of the switching point indicates that subjects' preferences change between the two particular questions around the switching point. Assume that there is an indifferent point such that the lottery in the column with varying lotteries brings about equivalent utility to the other column with a fixed lottery, then the switching point effectively bounds the indifferent point. This indifferent point is indicative of subjects' risk attitudes in this particular context, and we have discussed in Section 2.4 about how to relate the risk preference parameters to the switching point.

Probabilistic Beliefs Elicitation

There are, in total, three questions for this part. All the questions are taking the form of an asset allocation problem: Students are asked to allocate their resources (30 virtual coins for each question) between two Arrow-Debreu assets, A and B. Asset A pays off if and only if event A is realized. By the same token, asset B pays off if and only if event B is realized. Each unit of asset A or B pays 1 lottery if event A or B is realized.

Question 1

Flip a fair coin 1,000 times.
Event A: the coin's head appears at least 530 times.
Event B (complements): the coin's head appears less than 530 times.
Allocate your resources on asset A and asset B such that asset A + asset B = 30.

Question 2

Flip a fair coin 10 times.
Event A: the coin's head appears in the ninth and tenth round. Event B: the coin's tail appears in the ninth and tenth round.
Allocate your resources on asset A and asset B such that asset A + asset B = 30.

Question 3

Flip a fair coin 100 times. Event A: the coin's head appears for exactly 50 rounds. Event B: the coin's head appears for either more or less than 50 rounds.
Allocate your resources on asset A and asset B such that asset A + asset B = 30.

Social Preferences Elicitation

In the module of social preferences, students were asked to play a series of real-stakes games, wherein they received the payoff promised if their responses were randomly selected for reward.

Dictator Game: there are two players, A and B, in the game. In the first step, Player A receives 500 Yuan. She is then told to split the money between herself and the Player B. She can choose any amount she likes (from 0 to 500 Yuan) to keep, and give the rest to Player B. Player B can only accept what he gets from Player A. In terms of monetary incentive, Player A will get 500 minus the amount she/he sends out, and Player B will get the money that Player A is willing to transfer. In the Dictator Game, each participant is randomly assigned to one of the three scenarios: (a) if you are the Dictator (Player A), how much money out of 500 Yuan are you willing to share with Player B? (b) as a Bystander, what's your belief regarding the median value of the Dictators' sharing value in the Dictator Game? (c) you are the Receiver (Player B), no action is needed.

Ultimatum Bargaining Game: there are two players, A and B, in the game. In the first step, Player A receives 500 Yuan. She is then told to split the money between herself and Player B. She can choose any amount she likes (from 0 to 500 Yuan) to keep, and give the rest to Player B. In the second step, Player B can choose to accept, which results in the same outcome as the Dictator Game, or choose to decline, in which case both players get zero.

Trust Game: there are two players, A and B, in the game. In the first step, Player A could choose to send X amount of 500 Yuan to Player B. Player A is also informed that what she sends would be tripled when Player B receives the money. In the second step, Player B gets three times the money from Player A (A is like an investor and B is like a manager). Player B is told to split the money he has between A and B: he can also choose whichever amount he likes (from 0 to all the money he has) to keep, and give the rest to Player A.

In the Trust Game, there are two anonymously paired players. Player A chooses to send amount X out of 500 Yuan to Player B (including zero). Player A is also informed that whatever she sends will be tripled by our website. Consequently, when the Player A shares a value X with Player B, the website will triple it, and give $3X$ to Player B. Player B then makes a similar decision —- gives some amount out of $3X$ back to Player A, including possibly zero. We constructed the question as "If you are Player B and know that Player A sends you X, the you will get $3X$. How much money are you willing to give back to Player A, when X takes the values of {50, 100, 150}, respectively?" Additionally, we ask their beliefs as a Bystander regarding the median value of the amount given back, where X takes the values of {50, 100, 150}.

Belief elicitation: We ask Bystanders to predict the median action of Player A in the Dictator Game and the Trust Game, and the action of Player B in the Trust Game following Krupka and Weber (2013).

The logistics are as follows. There are three roles in all games: Player A, Player B, and a Bystander. Every student is randomly assigned to one of the roles (so each of them will play one (potentially different) role in each of the games). Player A and B play what we describe previously; the Bystander elicits her beliefs regarding Player A and B's actions.

B.3 Additional Results in Robustness Check and Heterogeneity of Treatment Effects

Financial Status

Economics students in their fourth year may participate in more part-time internships and accordingly have differential financial situations. Luckily, the survey also includes questions about students' financial status, for example, the difference between income and spending in the survey month. We add variables on financial status together with all the other control variables to our main analysis. The results are summarized in Table B.6 (Appendix B1). In all columns, the coefficients for economics majors are highly consistent with our main results, implying that financial status is unlikely to drive our main results. We also test the robustness of our results by: (i) adding a variety of additional controls in the regression; (ii) altering the classification of treatment and control group. Results are reported in Table B.9 (Appendix B1).

Major Preferences

As we have discussed in Section 2.2 and Section 2.3, conceptually we identify the causal effect by restricting our analysis to students who prefer economics major to other majors.

As we demonstrate in Section 2.4, a large majority of students indeed put all economics majors above other types of majors, suggesting that they unambiguously prefer economics majors. There are still, however some students whose ROLs are like what we show in Panel B of Table 2.1, where there are multiple switches between economics and non-economics majors. While these students still prefer an economics major to a non-economics major if we just look at the local major ranking, it becomes less clear whether these students always prefer an economics major unconditionally.

In this section we exclude the students aforementioned, namely, those whose ROLs exhibit multiple switch between economics and non-economics majors. The regressions are running based on equation (1) and (2). Again, the results reported in columns (1) -(11) of Table B.9 are quite robust and similar in significance and magnitudes to those results in Table 2.4, 2.6, 2.8, and 2.9.

Success/failure in Major Application

An alternative explanation could be that a successful experience in major application (which leads to enhanced self-confidence), career validation, or a shock to future expected income, undermines our interpretation. This implies that the effect we observe should be strong even for the freshman and is homogeneous regardless of which compulsory courses taken, as these courses only teach students concepts common in academia, but not guidance of career development. Another possibility is that the impact on preferences is most salient in the first year and wanes as time goes by. However, we have demonstrated in previous sections that the magnitude of effect does depend on length of enrollment and curriculum for a variety of the outcome variables, and that the effect is fostered, rather than diminished, over time. Therefore, our results cannot be easily explained by immediate impacts brought about by success/failure in major application.

We also conduct additional analyses that test the effect of disappointment. If the disappointment alone explains the pattern in our data, we would expect students with higher pre-college rankings to be more disappointed than those with lower pre-college rankings, given that both are assigned to non-preferred majors. The results are presented in Table B.4. The first column reports the performance of top-ranking students who are assigned to non-economics majors, compared to the average of economics students. The second and third show the performance of middle-ranking and bottom-ranking students. It appears that the effects of pre-college ranking on all outcomes do not significantly differ, implying that disappointment is not the main driving force, otherwise the coefficients of the top-ranking non-economics students would be significantly larger than that of the middle ranking in absolute value.

Another piece of evidence that contradicts the disappointment effects is that among students who are not assigned to their most preferred majors, the position of students' admitted major in their rank-order list does not have a significant effect on decision-making variables. If the disappointment effect drives our main result, we would expect students with a lower admitted position to feel more unsatisfactory than those with a higher admitted position. Thus we estimate the following equation:

$$Y = \lambda_0 + \sum_{i=2}^{7} \lambda_i position_i + \omega econ + MajorFE + \epsilon. \tag{B.1}$$

Where Y is subjects' response. $position_i$ is a vector of dummies that indicate the place of students' admitted major in their rank-order list. Therefore, λ_i represents the effect of students' position of admitted major in their rank-order list on risk, social preferences. We also add an indicator of the economics & business major and students' major-preference fixed effects. If the confounding of admission position is at play, we would expect that λ_i should be jointly significant. In the regression, we drop students who were admitted in the first position because only students who declared economics & business majors as their firstly preferred majors are included in our sample. We summarize the results of our regression analysis in Table B.5 (Appendix B1). In this table, Admitted in 3rd - Admitted in 6th denote that students were admitted in the third,...,sixth position of their rank-order list. And Admitted in 6th plus indicates that students didn't meet the cutoffs of the six major preferences, but were assigned to a major by CUFE. We conduct the F-test for the joint significance of the dummies for the position of admitted major in the rank-order list. The test results are reported at the middle of each column. The p-values of the test indicate that disappointment from an undesirable major is unlikely to affect our results.

Heterogeneous Treatment Effects by Gender

Many studies have highlighted that higher education and academic economics have unequal treatment effects on students by gender. For example, women are less likely to receive credits from a co-authorship and get a promotion (Sarsons, 2017; Card et al., 2020); professor gender has a larger impact on female students' performance in science classes and their future development Carrell, Page, and West (2010). In this paper, we exploit a similar procedure to examine the relationship between economics education and gender difference. Table B.7 demonstrates the unequal (equal) treatment effects of economics education on the same outcomes as shown in Table B.4 using the following equation.

$$Y = \kappa' + \beta_1' econ + \beta_2' econ^{*} male + \theta' X + \epsilon'''. \tag{B.2}$$

Where Y is the subjects' response. We introduce the interaction term $econ*male$, which implies whether a participant is a male economics student or not. The coefficients on the interaction term $econ * male$ show the difference of outcomes between male economics students and female economics students. Only column (1) and (3) show week evidence on the unequal treatment effects of economics education: the treatment effects mainly concentrate on male students comparing to female students. Columns (4)-(7) indicate that, on average, male economics students show no difference relative to non-economics students in the indifferent-choice question, the probabilistic belief questions on exact representiveness and social preferences.

Appendix C

Chapter 3 Supplementary Materials

C.1 Appendix Tables

Table C.1: 2*2 Experimental Design

Religion \ Information	Read the gap in CEE score between Muslim and Non-Muslim students	Read the Sino-Japanese Gap in GDP pc
Exemption to delay fast for the CEE	Exemp*Info	Exemp*No Info
No Exemption	No Exemp*Info	No Exemp*No Info

Note: This table summarizes the 2*2 design of our survey experiment. Randomly, half of the Muslim students get pro-exemption reading materials explaining that it would be permissible to postpone the fast until after the CEE (*"Exemption"*), while the other half of students read a placebo reading material unrelated to religion (*"No Exemption"*). Then we cross-randomize between these two groups, such that half of them are required to read a graph on "Hui-Han CEE gap between 2011 and 2016" (*"Info"*) while the other half of them required to read a placebo graph on "Sino-Japanese income gap between 2011 and 2016" (*"No Info"*).

Table C.2: Impacts of Ramadan on CEE Score

	(1) Score	(2) Score	(3) Score	(4) Score
Hui*Year_2012	-0.9527	-2.3302		
	(2.7122)	(2.7103)		
Hui*Year_2013	-1.0004	-1.6581		
	(2.6467)	(2.6448)		
Hui*Year_2014	-2.7471	-3.5299		
	(2.6090)	(2.6067)		
Hui*Year_2015	-1.9583	-3.1176		
	(2.5705)	(2.5686)		
Hui*Year_2016	-14.5265***	-15.0378***		
	(2.5613)	(2.5596)		
Hui	-14.6394***	-13.3878***	-15.5981***	-15.5981***
	(1.9194)	(1.9183)	(0.8138)	(0.8138)
Hui*Ramadan			-12.8275***	-12.8275***
			(1.8799)	(1.8799)
Mean of Dep Variable	383.218	383.218	383.218	383.218
Year FE	Yes	No	Yes	No
STEM-Year FE	No	Yes	No	Yes
Number of Observations	124369	124369	124369	124369
R squared	0.022	0.025	0.025	0.025

Note: This table presents the effects of taking the CEE during Ramadan on the relative performance of Muslim students. In columns 1 and 2, we interact Muslim dummy with year dummies, and see an abrupt increase the Hui-Han gap in 2016, the year that Ramadan overlaps with the CEE. In columns 3 and 4, we collapse the pre-treatment years into a larger control group, and get quantitatively similar results. In columns 1 and 3, we control for Year FE; in columns 2 and 4, we control for STEM-by-Year FE. Standard errors in parentheses are clustered at the high school level. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table C.3: Balance Test

	All		No Exp*No Info	Exp*No Info	No Exp*Info	Exp*Info	Anova Test	
Variables	Mean	Std.Dev	Mean	Mean	Mean	Mean	F-stat	p-value
Geneder: male	0.405	0.491	0.445	0.398	0.393	0.387	0.38	0.765
Parents with college education	0.045	0.208	0.016	0.047	0.044	0.070	1.57	0.195
Access to computer at home	0.390	0.488	0.390	0.375	0.400	0.394	0.06	0.980
Access to Internet at home	0.814	0.389	0.859	0.758	0.837	0.803	1.67	0.172
Boarding at school	0.831	0.375	0.852	0.82	0.859	0.796	0.84	0.475
Risk loving	2.461	2.125	2.480	2.438	2.652	2.282	0.71	0.548
Perceived value of college	3.692	1.186	3.543	3.680	3.919	3.620	2.51	0.058*
STEM track	0.610	0.488	0.609	0.625	0.630	0.577	0.32	0.810
Honors class	0.334	0.472	0.320	0.336	0.385	0.296	0.88	0.454
Pray everyday	0.589	0.492	0.641	0.555	0.607	0.556	0.95	0.418
Never broke a fast	0.535	0.499	0.602	0.469	0.504	0.563	1.85	0.137
Mock exam score	365.856	62.899	371.006	368.126	366.081	358.953	0.91	0.435
Observations	533		128	128	135	142		

Note: These two panels present the balance tests across the four different arms in the 2*2 experimental design. As can be seen, most variables are well-balanced, indicating that the randomization was well-implemented. “Risk loving” and “Perceived value of college” are measured using a five-point Likert scale. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table C.4: Motivated Cognition in Graph Reading: Alternative Outcome Variable

	(1)	(2)	(3)	(4)	(5)	(6)
				Deviation		
Exemption	-1.6436***	-1.6625***	-1.8617***	-0.2870	-0.5661	-0.5823
	(0.6295)	(0.6346)	(0.6396)	(0.9092)	(0.9238)	(0.9372)
Fast				2.1833**	2.3905***	2.4848***
				(0.8881)	(0.9052)	(0.9216)
Exemption*Fast				-2.6392**	-2.2253*	-2.4455*
				(1.2453)	(1.2788)	(1.3154)
Constant	5.8576***			4.7579***		
	(0.4507)			(0.6303)		
Mean of Control	5.858	5.858	5.858	5.858	5.858	5.858
Class FE	No	Yes	Yes	No	Yes	Yes
Control Variables	No	No	Yes	No	No	Yes
Number of Observations	277	276	274	277	276	274
R squared	0.024	0.144	0.227	0.046	0.167	0.238

Note: This table presents the effects of receiving exemption to delay fast on the accuracy of reading the 2016 enlarged Hui-Han gap in CEE performance, as well as heterogeneous treatment effects of exemption based on fasting history. We use the “absolute deviation from true value" as outcome variable instead of the gap read by students. As shown, it produces similar results. Robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table C.5: The Effect of Exemption on Prior

	(1)	(2)	(3)	(4)	(5)	(6)
		Gap			Deviation	
Exemption	-0.0699	-0.2167	-0.1481	-0.1610	-0.3070	-0.4357
	(0.9995)	(1.0141)	(1.0586)	(0.7726)	(0.7718)	(0.8188)
Constant	-17.9325***			12.3602***		
	(0.7082)			(0.5474)		
Mean of Control	-17.933	-17.933	-17.933	12.360	12.360	12.360
Class FE	No	Yes	Yes	No	Yes	Yes
Control Variables	No	No	Yes	No	No	Yes
Number of Observations	247	247	246	247	247	246
R squared	0.000	0.116	0.218	0.000	0.143	0.217

Note: This table presents the effects of religious intervention alone on updating prior. As shown in the table, the mean of the elicited 2016 Hui-Han gap is -17.97, close to the -16.4 gap between 2011 and 2015, much smaller than the true value of -29.4, indicating that Muslim students have acute downward bias in their priors. Receiving the exemption does not update this prior in any substantial way. As shown in columns 4-6, using the “absolute deviation from true value" as outcome variable produces similar results. Robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table C.6: Effect of Exemption on Placebo Graph Reading (GDP Per Capita)

	(1)	(2)	(3)	(4)	(5)	(6)
		GDP gap			Deviation	
Exemption	-712.08	-876.29	-1126.32	799.78	1011.39	628.58
	(1088.08)	(1146.52)	(1202.96)	(1371.75)	(1375.89)	(1464.61)
Constant	-28433.92***			6140.19***		
	(760.94)			(959.32)		
Mean of Control	-28433.92			6140.19		
Class FE	No	Yes	Yes	No	Yes	Yes
Control Variables	No	No	Yes	No	No	Yes
# of Observations	229	229	228	229	229	228
R squared	0.002	0.061	0.161	0.001	0.149	0.216

Note: This table presents the placebo effect of receiving an exemption on the accuracy of reading the 2016 Sino-Japanese income gap. As can be seen, the religious intervention has no meaningful impacts on reading the income gap. As shown in columns 4-6, using the “absolute deviation from true value" as outcome variable produces similar results. Robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

C.2 Setup of the Model

There are two periods, period 0 and period 1. Student i derives payoff v_i from fasting in each normal Ramadan period. Denote her vulnerability to hunger and thirsty by $\rho_i \in \{0, 1\}$, which she cannot observe directly in period 1. However, she has a prior about this vulnerability which can be fully characterized by $\hat{\rho} \equiv P\{\rho_i = 1\}$.[1] Denote her fasting behavior in periods 0 and 1 by f_0 and f_1 respectively.

Period 0 describes students' fasting behavior during a normal Ramadan period, when Ramadan does not overlap with the CEE. In this period, fasting only affects students' performance in CEE via negatively affecting the effectiveness of learning during Ramadan but not their health status during the exam. The quantity of this effect is expressed as κh, where h is the full effect had she fasted during the CEE and $\kappa < 1$ captures the relatively minor impact on CEE due to inefficient learning during previous Ramadan months. $\omega_i > 0$ represents the importance students i attach to the final outcome of the college entrance exam. For simplicity, we assume that students know their ρ_i due to repeated fasting experience in middle school. They choose f_0 to maximize:

$$f_0 v_i + (1 - f_0)(\kappa \omega_i h \rho_i + \epsilon_{i0}) \tag{C.1}$$

where ϵ_{i0} is a random disturbance governed by distribution $F_0(\epsilon_{i0})$. Put it in another way, students will either fast ($f_0 = 1$), in which case they derive utility v_i by committing to religious practice, or not to fast ($f_0 = 0$), in which case they enjoy enhanced learning effectiveness. Note that we arrange the utility in this form to highlight the tradeoff between fasting ($f_0 = 1$) and not fasting ($f_0 = 0$).

In period 1, students have answered the survey we distributed, and were expecting the CEE in a month. They decide to fast or not in the exam, get anticipatory utility about her performance in the exam and derive utility from fasting behavior, denoted by f_1. In this period, they can no longer remember ρ_i but instead, they form a posterior about ρ_i based on prior $\hat{\rho}$ and previous fasting behavior f_0 as a Bayesian. This is due to either forgetfulness or that they lack knowledge about the impact of fasting on test performance (remember in period 0 that they only experienced fasting when no formal exams like CEE happened). In this period students jointly choose $(\hat{\rho}, f_1)$ to maximize:

$$f_1 \cdot (v_i r - \omega_i E[\rho_i | \hat{\rho}, f_0] h - C(\rho_0 - \hat{\rho})) + (1 - f_1)(-C(\rho_0 - \hat{\rho}) + \epsilon_{i1}) \tag{C.2}$$

where ϵ_{i1} is governed by distribution $F_1(\epsilon_{i1})$. Denote the joint distribution of (ϵ_{i1}, v_i) and the marginal distribution of v_i by $F(\epsilon_{i1}, v_i)$ and $G(v_i)$ respectively. Note that v_i has to be non-negative, which is the only restriction for distribution $F(\epsilon_{i1}, v_i)$ and $G(v_i)$. r is the special return for this special Ramadan period (i.e. fasting during CEE). For simplicity, $r \equiv r_C = 1$ if students regard this fasting period the same and the rest; $r \equiv r_T$ with $0 < r_T < 1$ if students are persuaded by religious leaders, and believe that fasting may

[1] Here we binarize the impact of fasting to be either "negative" or "nonexistent," this is without much loss of generality because no more than 3% of the students in any treatments have beliefs that Ramadan will help boost their performance in the CEE.

not be necessary during the particular exam days. Therefore $v_i r$ captures the payoff from fasting during CEE. $-E[\rho_i|\hat{\rho}, f_0]h$ is the expected cost of fasting during CEE, and $-C(\rho_0 - \hat{\rho})$ is the cognitive cost of manipulating her prior away from her original prior ρ_0 had motivated beliefs been not at play. We assume that $C(.)$ is twice continuously differentiable, minimized at 0. We also assume ρ_0 and $\hat{\rho}$ to be a real number between 0 and 1. Note that we arrange the utility in this form to highlight the utility derived from both fasting ($f_1 = 1$) and not fasting ($f_1 = 0$) respectively.

The major difference between our model and the previous studies is the focus on the manipulable prior $\hat{\rho}$, which merits further discussions. Aside from the mechanical explanation above, another interpretation of ρ_0 is that this prior is subconscious, and the subject's cognition process manipulates her prior away from the subconscious one to maximize her anticipated utility. The modeling of $\hat{\rho}$ is similar in spirit to Augenblick et al. (2016), where students manipulate their beliefs about the probability of dooms day above their original beliefs had a religious concern not been present. Importantly, this subconscious belief need not be accurate. While Augenblick et al. are agnostic about the formation and implications of differential ρ_0 in their paper as this is not their focus, we directly test the additional implication of a wrong ρ_0 and confirms the validity of our model.

Our model is also different from previous studies on motivated beliefs in that the anticipatory utility merely comes from students' expectation about their own performance in the exam. Arguably, as an once-in-lifetime high-stakes exam, for which students have been preparing for years, the effect of anticipatory utility should be particularly strong. We do not specifically model the utility of religious beliefs, such as utility carried by h itself, which may reflect people's belief on how omnipotent their religion is. The primary reason of this omission is that the incorporation of this utility does not qualitatively change our results, and our empirical results do not support this possibility either.

This model has a number of predictions about students' response in beliefs and fasting attitudes. We categorize them into three groups to highlight the relationship between these propositions and the results presented in the next section. Specifically, Proposition 1 predicts response in beliefs under unawareness of fasting impact; Proposition 2, 3, 4 predicts response in beliefs under awareness of fasting impact; Proposition 5 discusses the relationship between the beliefs and the perceived importance of the College Entrance Exam; Proposition 6 and 7 presents our model's prediction on fasting attitudes.

Proposition 1 When $\rho_0 = 0$, $\hat{\rho} = 0$ irrespective of the value of f_0, f_1, r and v_i.

This proposition discusses how students might react when their subconscious beliefs are wrong. Since anecdotal evidence suggests that students may not be aware of the negative impact of fasting at all, our proposition focus on the prediction in this case. The framework predicts that the students do not have to incur any cost to create illusion, but just happily take the view that fasting does not do even cause the slightest harm. As a result they sincerely do not believe that on average, fasting is significantly detrimental to their cognitive function regardless of whether religious leader try to persuade them to fast or not during CEE. This prediction of this proposition, in our context, is elaborated by Hypothesis 1 in the main text.

Proposition 2 In case of $\rho_0 > 0$ and for almost any given (ϵ_{i1},f_0), $\hat{\rho} < \rho_0$ if $f_1 = 1$ for any positive r and v_i.

As one of the most basic results of this model, this proposition says that for people who choose to fast, they have the incentive to distort their prior as long as they become partially aware of the fact that fasting is harmful to their exam performance, irrespective of its magnitude. In our experiment, we use "belief about the average impact of taking the CEE during Ramadan" as a proxy for the parameter ρ_0. This prediction of this proposition, in our context, is elaborated by Hypothesis 2 in the main text.

For simplicity, we additionally assume that κ is small in the discussion of last proposition. This assumption says that the impact of Ramadan fasting during pre-exam period (say fasting one year or two years ahead of the CEE) is minor to fasting on CEE exam. We argue that this is a reasonable assumption for the following two reasons: first, the length of Ramadan fasting is merely one month for every year in Islamic calendar, which is relatively short compared to years of exam preparation; second, even if students' learning activity are affected during fasting, they can still make up for it by studying harder before/after the fasting month.

This proposition concerns the heterogeneity of the treatment effects with respective to past fasting behavior f_0. While the prediction of model in general may not be entirely clear given different κ, f_0, $C(.)$ and the joint distribution of v_i and ρ_i, with assumption on κ, we can derive the following results.

Proposition 4 When κ is sufficiently small, the distribution of v_i given $f_0 = 1$ stochastically dominates that given $f_0 = 0$. Hence given the same ρ_0, $E[\hat{\rho}|f_0 = 1] < E[\hat{\rho}|f_0 = 0]$

The proposition discuss the case where fasting in the past can barely affect the CEE outcome. In this case, students can only extract information about v_i from f_0. For those who did not fast in the past, then have lower v_i, hence less incentive to manipulate their beliefs. We view this assumption as plausible because as we have discussed in institutional details, past fasting rarely affects the exam outcome because students have three years to prepare for the exam, hence they can have plenty time and opportunities to make up had they, by any chance, fallen behind during the fasting period. Moreover, the results are fairly robust even when κ is large [2]

The last two propositions concerns treatment effects on fasting attitudes.

Proposition 6 In case of $\rho_0 > 0$, for almost any given (ϵ_{i1},f_0,v_i,r), as long as $h > 0$, $f_1 = 0$ if and only if $\hat{\rho} = \rho_0$

This proposition provides us with a tight link between the elicited beliefs $\hat{\rho}$ and fasting behavior f_1 during CEE: when students are aware of the harm of Ramadan fasting (i.e. their subconscious belief ρ_0 is positive), those who hold the right beliefs will not fast and vice versa. While the implication that we can precisely identify those who do not fast must express the right belief is not robust to alteration such as incorporating people's utility from the omnipotence of their religion (i.e. utility as a function of h), it is indeed

[2]When κ is large, f_0 is affected by both ρ_i and v_i. We need to consider the joint distribution of these two variables. However, even in this case, with moderate assumptions on cognitive cost, we will be able to get the result that the optimal probabilistic beliefs for those who choose f_1 to be 1 is smaller for students who fast in the past.

robust that given a correct ρ_0, as beliefs become more accurate, students are less likely to fast during the CEE across different treatment groups. This proposition provides a way to proxy fasting behavior: if we want to focus on the group of people who fast (say, examine the impact of perceived stake on biases conditional on fasting), we can restrict our attention to subsample where people don't read the graph accurately.

While people will not adjust their beliefs given the initial unawareness of the harm of fasting, the persuasion from religious leaders do decrease r, which decreases the gap of utility between fasting and not fasting in period 1. If there are any independent disturbance of fasting preferences as illustrated by ϵ_{i1} in the model, the rate of fasting will also be decreased by authorization from religious leaders.

The next proposition discusses the effectiveness of information treatments in terms of changing fasting attitudes. We can easily deduce from Equation C.2 that religious leader persuasion alone is sufficient to shift the fasting decisions of some people. In addition to that direct channel, there is also an additional role of information dissemination on changing fasting attitudes:

Proposition 7 For any given ϵ_{i1}, Denote the minimum level of v_i needed to choose fast for treatments *"No Exemp*No Info,"* *"Exemp*No Info,"* *"No Exemp*Info,"* *"Exemp*Info"* by $\bar{v}_1$, $\bar{v}_2$, $\bar{v}_3$, $\bar{v}_4$, respectively. If, say, any non-negative v_i is enough for fast in treatment *"No Exemp*No Info,"* then $\bar{v}_1 = 0$. We have: (i) $\bar{v}_1 < \bar{v}_2$, $\bar{v}_1 < \bar{v}_3$;(ii) $\bar{v}_4 - \bar{v}_2 > \bar{v}_3 - \bar{v}_1$.

This proposition use a specific set measures, $\bar{v}_1$, $\bar{v}_2$, $\bar{v}_3$, $\bar{v}_4$, to measure people's preference to choose fasting in the end. The higher the threshold is, to the less extent people would prefer fasting. (i) says that the threshold for merely providing information $\bar{v}_3$ and threshold for merely providing religious exemption $\bar{v}_2$ both move up relative to control threshold $\bar{v}_1$, indicating that both treatment works in the same direction, whereas the relative effectiveness of them is an empirical question. (ii) says that the information treatment and religious exemption may serve as compliments: when religious exemption is granted, the effectiveness of providing information in terms of the movement of the threshold, $\bar{v}_4 - \bar{v}_2$, is larger than $\bar{v}_3 - \bar{v}_1$, in which case no exemption is granted. Of course, the results still hold when we regard these threshold as a function of ϵ_{i1}, and integrate over it to compare the expected level of thresholds. This prediction of this proposition, in our context, is elaborated by Hypothesis 8 in the main text.