Experiencing Unfair Chances Reduces Investments in Ambiguous Assets

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Discrimination leads to disadvantageous financial, labor market, academic, and social outcomes for those affected, driven by the decisions of those who discriminate. Inherent in the experience of discrimination is unfair treatment but little is known about whether experiencing unfair treatment subsequently alters own decision-making in unrelated contexts. Using a novel experiment, we examine how experiencing unfair treatment affects investment decisions. We introduced unfair treatment by varying the probability of receiving a small performance bonus (5% vs 95% chance) that determines participants' relative earning ranking. We found that after experiencing unfair treatment (without explicit discrimination), participants invested less in ambiguous assets but their investments in risky assets remained unchanged. We further explored whether wealth, gender, and previous experience of discrimination mitigate this reduction in invetment. Our findings highlight the broader economic consequences of unfair treatment, emphasising its potential to shape financial decision-making.

Keywords: ambiguity; risk; decision-making, unfair treatment, discrimination

JEL codes: C91, D80, D91

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Acknowledgments:

This work was supported by the Australian Research Council's Centre of Excellence for Children and Families over the Life Course (Project ID CE140100027 and CE200100025) and the University of Sydney Faculty Research Support Scheme. For helpful comments, we thank participants in the 2021 ESA Global Virtual World Meeting, 2022 ESA Asia Pacific Meeting, and 2024 Pre-FUR Workshop on Neuroeconomics of Disadvantage. We thank Sura Majeed for excellent research assistance. The paper was previously circulated as "The Effect of Disadvantage on Decision Making Under Uncertainty".

1. Introduction

It is a sobering reality that people are not treated equally nor given the same opportunities. For instance, in certain U.S. cities, individuals with white-sounding names receive up to 50% more call-backs for job interviews compared to those with Black-sounding names (Bertrand & Mullainathan, 2004). Similarly, women in academia are less likely to receive tenure or hold faculty positions than their male counterparts, despite having comparable annual publication rates and career impacts for equivalent bodies of work (Huang et al., 2020; Ortega-Liston & Soto, 2014). In the US, it is estimated that racial discrimination will cost the US economy between $1 trillion and $1.5 trillion between 2019 and 2028 (Noel et al., 2019). The direct health cost of racial discrimination in the Australian economy is estimated to be roughly 3% of the gross domestic product annually (Elias & Paradies, 2016).

It is straightforward to predict that if two otherwise identical individuals experience differing levels of discrimination, the economic situation of the one that is discriminated against will eventually deteriorate. This decline can occur either directly due to the discrimination or indirectly, through poorer mental health and loss of confidence. The extensive research on the cumulative negative effects of discrimination on economic outcomes (Becker, 2010; Cain, 1986; Cavalcanti & Tavares, 2016) has generally overlooked the impact of being treated unfairly on individual decision-making. This is a crucial omission because if the experience of discrimination alters one's own economic decisions, it could lead to a "disadvantaged" feedback loop (Haushofer & Fehr, 2014), further perpetuating the disadvantaged status. This raises the question: does experiencing unfair economic opportunities influence an individual's decision-making in unrelated economic contexts where discrimination is absent?

There is surprisingly little evidence on the effects of being treated unfairly on individual decision making. Majority of the experimental literature on discrimination focuses on interactions between an "in group" and "out group" (see the meta-analysis by Lane (2016)) or how individuals with differing status interact (Ball et al., 2001). Of the few papers on individual decision making, they are either field based (Parsons et al., 2011) or do not establish a causal relationship (Pascoe & Richman, 2009). The very few lab-based papers focus on labor outcomes. For example Bracha, Gneezy, & Loewenstein (2015) find that salary discrimination reduces labor supply. The lack of clear causal evidence on the effects of discrimination on decision making is apparent and needs to be addressed.

In this paper, we focus on a feature that is always present in discrimination—*unfair treatment*—and ask how the experience of unfair treatment impacts subsequent investments in ambiguous and risky assets. We focus on investment decisions because our goal is to quantify the financial burden of unfair treatment inflicted by the change in ones' own decision-making. We purposefully study investments in the ambiguous assets in addition to the investments in risky assets because most economic decisions in real life are under conditions of ambiguity, that is with unknown outcome probabilities. For instance, the decisions of whether and how much to invest in the stock market, whether and how much insurance to buy, and whether to continue education are all ambiguous. We hypothesize that these decisions, where there is some subjectivity in the assessment of the success probability, are more likely to be affected by unfair treatment than decisions in the situations where all risks are known.

To study our research question, we developed a novel experimental design that induces unfair treatment through unfair chances in a competitive environment and allows for both within-subject and between-subject analyses. Our goal was to design an experiment that resembles a workplace situation and assesses the impact on investments that occur outside the job-related tasks. We were inspired by the unfair chance framework of Gagnon, Bosmans, & Riedl (2021), who investigated the effect of unfair chances and gender discrimination on labor supply. In addition, we integrated the comparative environment framework from Haushofer, Schunk, & Fehr (2019), which creates wealth distortions through performance rankings. At the start of the experiment, we randomized the amount of endowment that participants get with the advantaged ones receiving five to seven times more than the disadvantaged subjects. This allowed us to study whether the additional financial buffer mitigates the effects of unfair treatment. One could expect that better financial status mitigates the effect of unfair treatment because of a simple diminishing utility argument—a fixed reduction in income that results from unfair treatment, would have a lower economic impact on somebody who is wealthier.

Gender and race of our participants, the attributes which often lead to unfair treatment, are not emphasized to participants in our research design. We made this choice because we are not interested in the behaviour of those who discriminate but rather those who are treated unfairly. In real life unfair treatment is often subtle and the victim needs to infer why they receive unfair treatment. Nevertheless, we explicitly study whether the prior experience of discrimination outside laboratory (assessed in a post experimental survey) makes people more or less resilient to the unfair treatment in our experiment. The effect of prior discrimination could manifest in two distinct ways. First, it is conceivable that individuals who have previously experienced

discrimination, may find even minor unfiar treatment events intolerable, exacerbating its effects on behavior. On the other hand, prior exposure to discrimination might result in habituation, whereby individuals become desensitized to being ttreated unfairly in the future.

We found that experiencing unfair treatment reduces investments in ambiguous (but not risky) assets in an independent subsequent task. This reduction in investiment is driven by male participants. Our findings are indicative that prior experience of discrimination may reduce the effect of the unfair chances. Receiving a higher show up fee and reporting higher wealth does not eliminate this effect, even though the difference between high and low show up fee was 50 to 60 times bigger than the monetary consequences of unfair treatment. Therefore, our findings cannot be explained through wealth effects.

In addition to the above contributions, we conduct a small replication exercise. Cavatorta & Schröeder (2019) developed a survey module to elicit ambiguous attitudes efficiently. Currently our empirical knowledge regarding ambiguity preferences comes almost exclusively from laboratory studies with restricted subject pools and other constraints imposed by the laboratory setting. Unlike costly and time-consuming incentivized laboratory experiments, this survey module is a quick and accessible method which could in principle be integrated into representative sample surveys to understand the distribution of and role of ambiguity preferences on life outcomes in large representative samples. We find that the complete survey module score correlated with the amount invested in the ambiguous but not risky asset, which suggests it is a good survey-based measure of investment in ambiguous assets, measured distinctly from risk attitudes.

2 Experimental design and methodology

2.1 General data collection details

217 participants completed the experiment. We conducted sessions at the University of Sydney (124 participants, 8 sessions) and at the Royal Melbourne Institute of Technology (RMIT) (93 participants, 10 sessions). Our target sample was approximately 200 which, assuming a power of 0.9 and alpha equal to 5%, would allow us to detect small differences in means in within-subject comparisons and medium differences in between-subject comparisons. Participants were recruited from the student research volunteer databases using ORSEE (Greiner, 2015).

Each session lasted approximately 90 minutes and involved on average 15 participants (ranging between 3 and 26).

Figure 1 presents the timeline of the experiment. First, the instructions (available in the Appendix A2) were read aloud by the experimenter and displayed on the participants' computer screens. After the instructions, participants had the opportunity to practice the tasks and to answer comprehension questions (Appendix A3) to ensure they understood the tasks before starting. Participants then completed experimental tasks designed to answer our research questions. After these tasks, participants completed a demographic and socioeconomic questionnaire and an ambiguity survey (Appendix A4). They were then informed about their final payment. The study was approved by the Human Research Ethics Committee at the University of Sydney and RMIT.

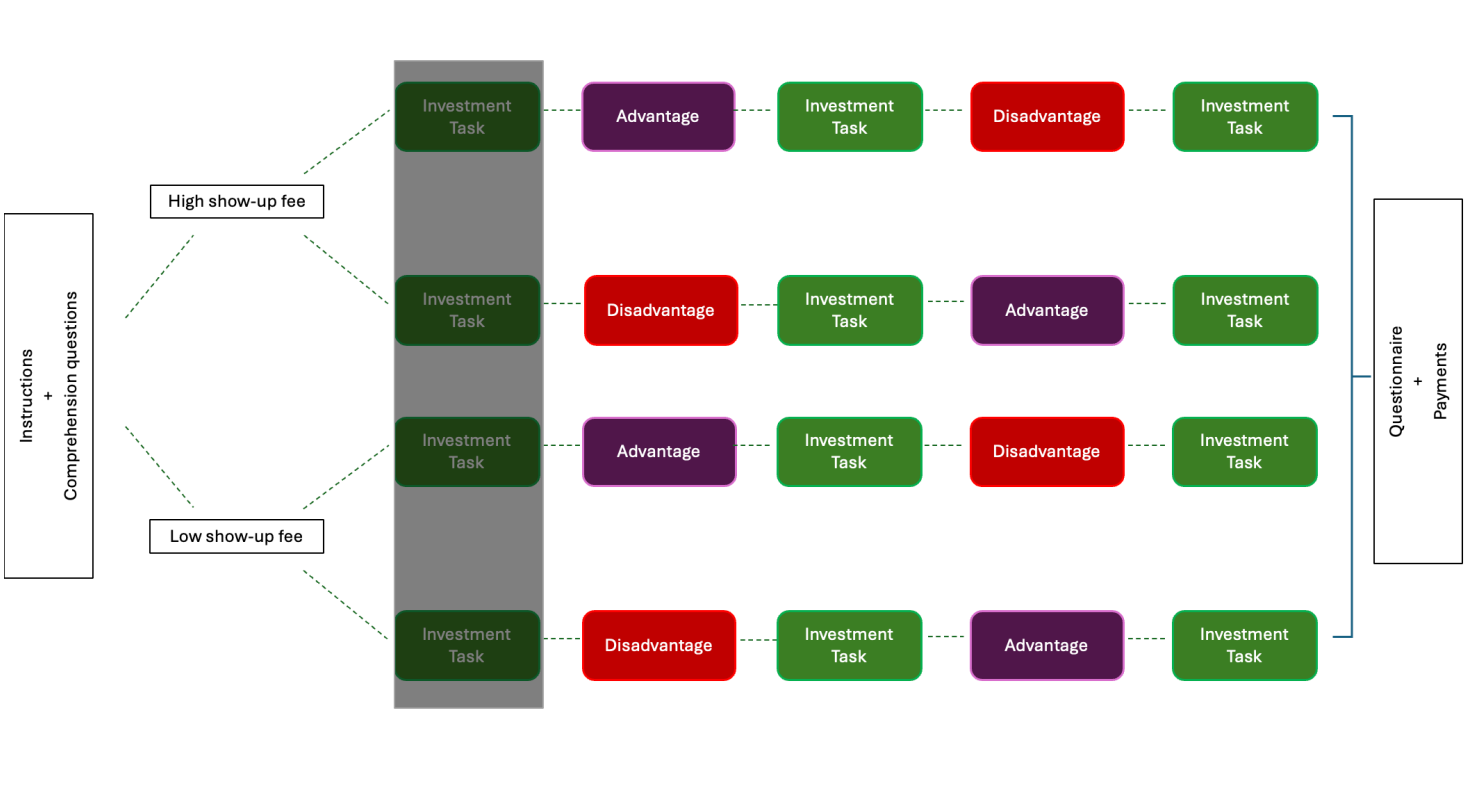


Figure 1. Timeline of the experiment.

2.2 Experimental tasks

Participants completed two tasks: *an unfair chances treatment* (labelled "Advantage" and "Disadvantage" in Figure 1) that involved a real effort task and *an investment task* (labelled "Investment" in Figure 1) that included investments in risky and ambiguous assets.

2.2.1 Unfair chances treatment

Our goal was to simulate, in the laboratory, a situation where people provide equally good work output, but some of them are treated unfairly for reasons not related to their performance. To make the work task meaningful we decided to use a real effort slider task developed by Gill & Prowse (2012). There were two blocks of the slider task, each comprised of ten trials. To successfully complete a trial, participants had to set six sliders to a value between 49 and 51 (Figure 2). We chose this range rather than an exact number because pretesting revealed that this range allowed for a high success rate on attempt but wasn't too trivial. Participants had 50 seconds to adjust all six sliders. After 50 seconds, if all sliders were set correctly (as in Figure 2), participants received a guaranteed lottery ticket and had some chance of receiving an additional bonus lottery ticket. If after 50 seconds, all sliders were not set correctly, participants received neither the guaranteed lottery ticket nor the bonus for this trial.

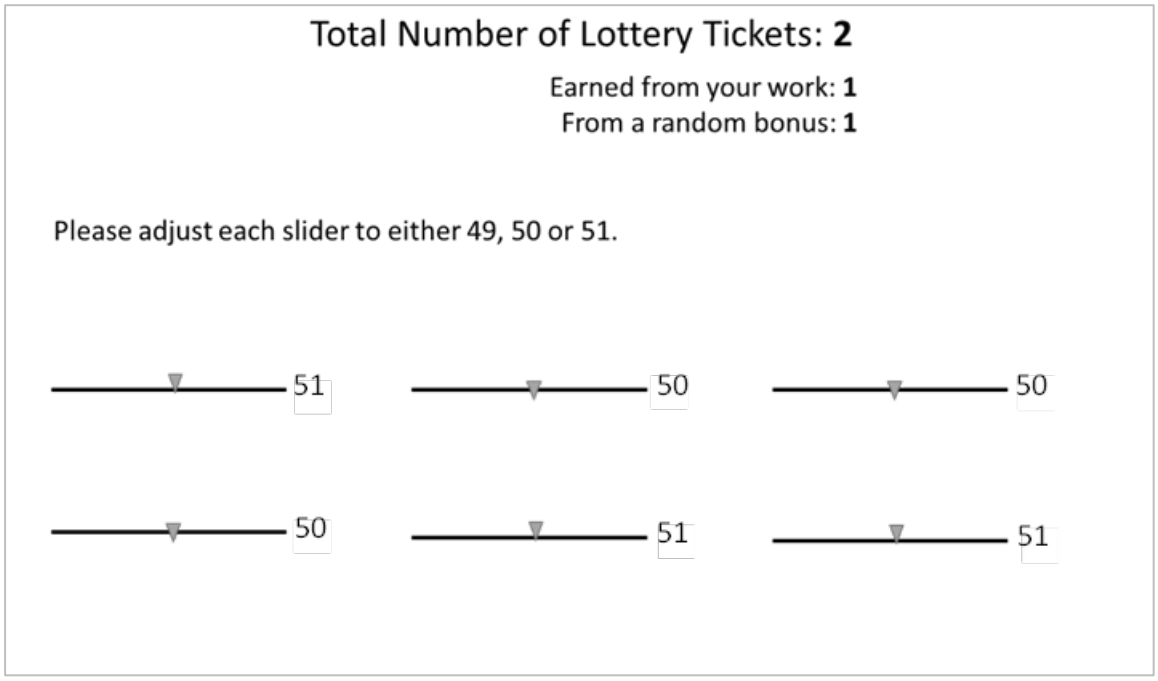


Figure 2. Slider task. In this example, all sliders are set correctly.

To incentivize performance, at the end of the session, one of the two slider task blocks was randomly selected. All lottery tickets they and other participants in their session accumulated in this block were placed in a lottery which randomly picked one ticket as the winner of an extra $5 cash prize for this session. Participants accumulated lottery tickets over the ten trials of each slider task block. Lottery tickets were reset to zero at the start of each block to ensure no carry-over effects.

We implemented the unfair chances treatment by varying the chance of receiving the additional bonus lottery ticket and splitting people into "advantage" and "disadvantage" groups (see Figure 1). All participants had a chance to receive the additional bonus lottery ticket for each trial in which they adjusted all sliders correctly. For participants in the disadvantage group, the chances of receiving the bonus ticket were only 5% and for participants in the advantage group, the chances were 95%. Given that we expected most subjects to be able to complete each slider screen within the time limit, the difference in the chances of receiving the bonus lottery ticket was aimed to create a substantial difference between participants in their total lottery tickets earned. In each block, half of the participants were allocated to each treatment and stayed in that treatment for the whole block. Each participant experienced both treatments, doing the slider task once in the disadvantage and once in the advantage. The order of the treatments was randomly selected for each participant. In the instructions, we emphasized that all participants were doing the same task but the probabilities of getting random bonus lottery tickets may be different for each participant. To mimick unfair treatment in real life, we did not say explicitly what these differences were.

The experience of unfair chances (disadvantage veresus advantage) was made salient to participants through feedback displayed after each trial. Figure 3 shows an example. The vertical axis represented the total number of lottery tickets earned so far. Each bar represented one participant in the session. Participants could easily identify themselves in this graph because their own score was marked by a red bar. To further reinforce the importance of treatment for participants' relative position, we reminded them of how many tickets they earned through work and how many through a random bonus. Participants saw this feedback for 10 seconds after each screen. This allowed participants to saliently perceive their own ranking relative to other participants in the same session and made it possible for those in the disadvantage group to observe the results of the unfair treatment. We designed the slider task so that it was not too challenging which means that we expected most participants to complete all or almost all screens correctly and earn all possible lottery tickets from work. This means that we expected the differences between subjects to be mainly due to the different chances of getting a bonus.

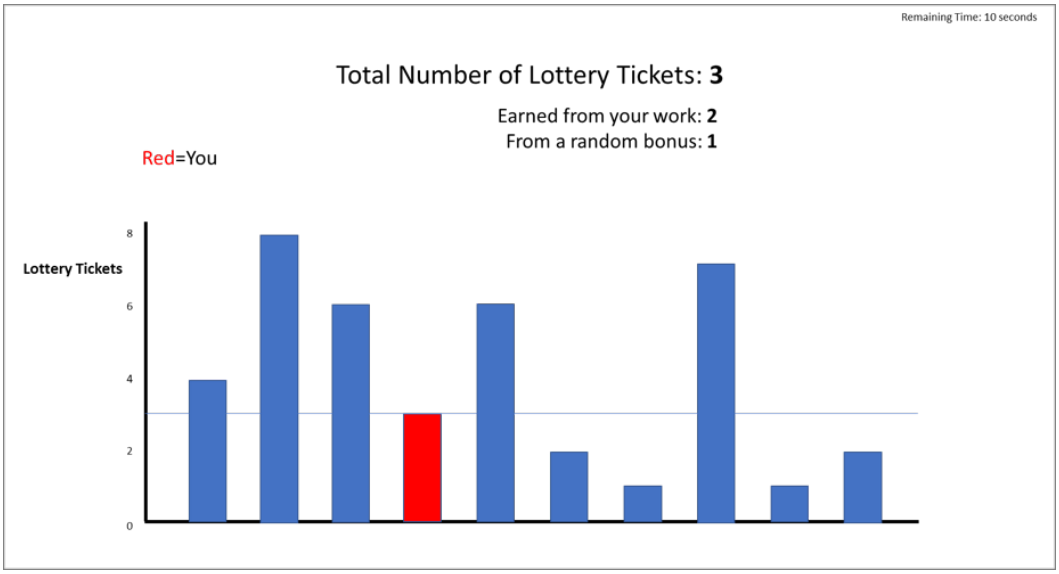


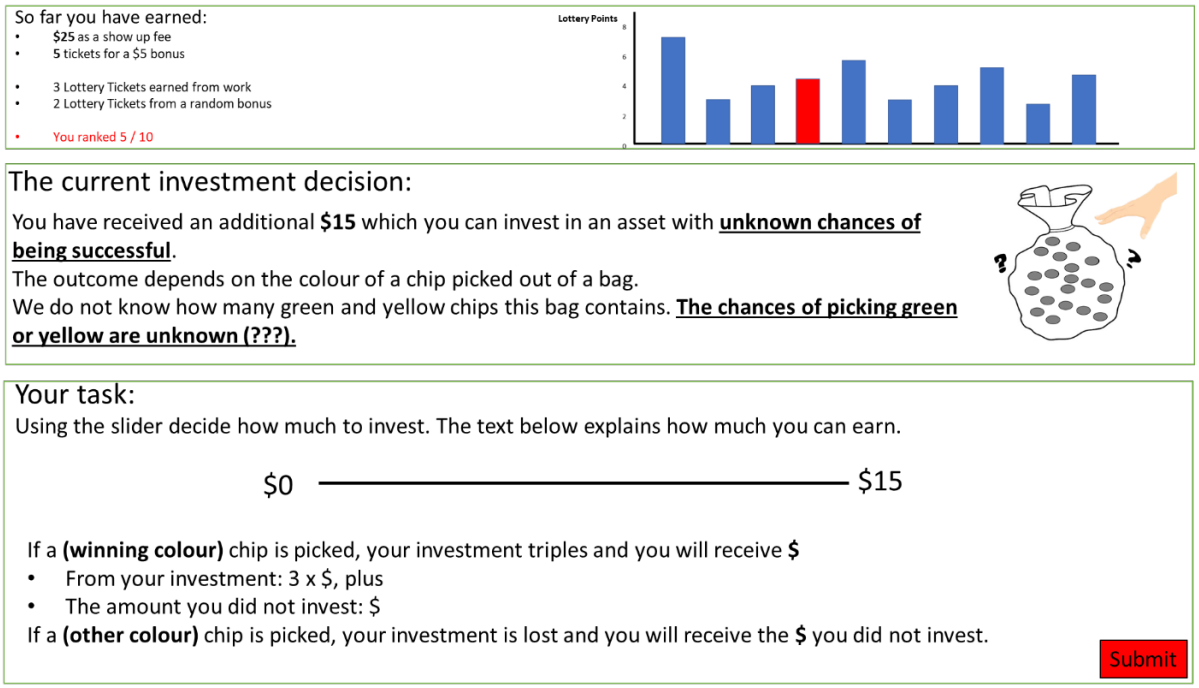
Figure 3. Slider task feedback screen.

The lottery prize was $5, which we chose to be large enough to incentivize effort and amplify the competitive nature of the slider task and make the treatment more effective. However, at the same time, the lottery prize was not large enough to meaningfully affect expectations about earnings in the task and create substantial wealth effects across the treatments. Assuming 16 participants in the session, the expected difference in earnings between the disadvantaged and advantaged group was approximately only $0.55.

2.2.2 Investment task

The ideal task to measure the effect of unfair treatment on investments provides precise estimates but is also short. The short duration is particularly important in this experiment for two reasons – first, we want to measure people's uncertainty preferences before the treatment effect wears off, and second, as the slider task is already taking some time, we need to ensure that the whole duration of the experiment is not too long. Having these concerns in mind, we used an investment decision task based on Gneezy & Potters (1997). At the start of the task, each participant received $15. To elicit risk attitude, we asked participants to decide how much of this $15 they would like to invest in an asset that with 50% chance yielded a return equal to three times their investment and with 50% had no return (meaning that the invested amount was lost). Additionally, participants made the same decision but now the specific probability of successful investment was ambiguous. Figure 4A shows the investment task screen for the ambiguous asset, and Figure 4B shows an example for the risky asset.

A.



B.

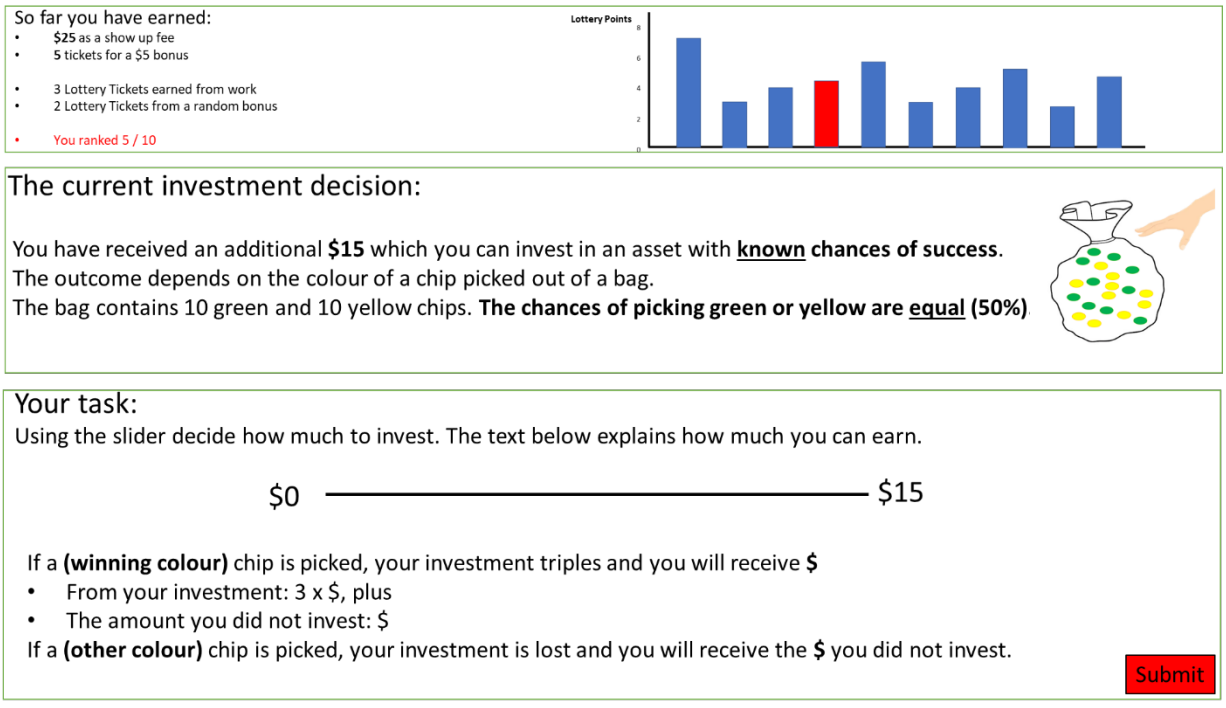


Figure 4. Task screens for investment task. A. ambiguous investment, B. risky investment.

We communicated a lot of information on the screen thus the contents of each investment task screen (Figures 4A and 4B) appeared sequentially. Initially, only the top panel appeared. The purpose of this first panel was to remind participants of their performance and rank in the slider task using both text and visual (bar graph on the right) presentation. After pressing continue, participants would additionally see the middle panel that explains the investment task, including the odds of the investment being successful. After another continue press, the bottom panel would appear, and participants would indicate their desired investment level. To facilitate decision-making, the possible outcomes if the investment succeeded or failed were updated according to the position of the slider in real time and were displayed underneath the slider. The starting position of the slider and earning fields were empty at the start of each investment decision.

At the end of the experiment, one investment decision was randomly selected for payment. The success of the investment was determined by the experimenter drawing a colored chip from a bag without looking. The risky bag had an equal amount of green and yellow chips while the ambiguous bag had an unknown proportion of green and yellow chips. At the beginning of the experiment, participants selected which color they would like to be associated with investment being successful. If the chip drawn from the bag was the same color, the investment was "successful", and vice versa. The chip bags were real physical bags and stayed in participants' view for the duration of the session. Because, ex-ante, the experimenter cannot foresee which colors participants will choose as their winning color, the participants have no reason to believe that the experimenter manipulated the content of the bags to their disadvantage.

Each participant made both investment decisions after each treatment in an order randomized individually for each participant.

2.3 Show-up fee treatment

We were interested in whether improving participants' financial situation by giving them a higher show-up fee would affect their investments or mitigate the effects of the unfair treatment. At the beginning of each session, participants were randomly assigned to receive a high show-up fee or a low show-up fee. To make the show-up fee prominent, it was displayed in the instructions and on the screen during the investment task (see Figure 4 top left corner). This variation allowed for a between-subject investigation of the effect of the show-up fee on participants' investments and their reaction to the unfair treatment. Participants were not told that there were different show-up fees and only knew how much they received. We chose show-

up fee amounts that are substantially higher and lower than the endowment in the investment task. The difference between the low and high fees was also substantially larger than any reasonable wealth differences in earning expectations created by the unfair chances treatment. The low show-up fee was $5. The high show-up fee was $25 at the University of Sydney and $35 at RMIT.

Our design (Figure 1) allowed us to conduct within- and between-subject analysis of the effect of the unfair treatment on investments, between-subject analysis of the effect of the variation in the show-up fee on the investments; and investigate the interaction effect between unfair treatment and show-up fee treatments on participants' investment in risky and ambiguous assets.

2.4 Questionnaires

After completing the experimental tasks, participants filled out a standard questionnaire asking for demographic and socioeconomic information, as well as their private experience of discrimination. In addition, participants completed an ambiguity attitudinal survey module (Cavatorta & Schröeder, 2019), which was designed to elicit ambiguity preferences that correlate with the investment in ambiguous assets elicited from incentivized tasks in a lab. The questions include an Ellsberg urn question, a dynamic hypothetical Ellsberg urn experiment as well as five attitudinal questions that have been taken from psychology literature surrounding ambiguity surveys. We independently test the validity of this survey module.

2.5 Payment

Participants' final compensation consisted of three parts: show-up fee, the outcome from one randomly selected investment decision, and $5 cash prize if the participant was the lottery winner in the slider task.

To determine participants' payment from the Investment Decision Task, the experimenter randomly picked a colored chip from the ambiguous bag and risky bag without looking. Participants were shown and told the result of these draws which were used to determine the success of their randomly selected investment decision.

To determine the winner of the $5 cash prize from the slider task, the experimenter showed their screen to run the lottery from the results of the Slider Task. This was a simple excel spreadsheet that randomly chose one of the two blocks of the Slider Task, and then randomly

chose a single participant from the aggregated lottery tickets earnt from that specific block. The winner of this lottery was shown on the screen and announced verbally.

All payments were made via Internet transfers. Participants were given the option to be paid via PayPal in Australian dollars or paid via Alipay in Chinese Yuan at the exchange rate of the day the payment was made[1]. Payments were sent within 2 business days from the experimental session.

2.6 Other details

We conducted the experiment at two sites: at the University of Sydney in October of 2021, and at RMIT in March of 2024. The sessions at the University of Sydney took place on Zoom while the sessions at RMIT took place in the lab. The high show-up fee at RMIT was higher ($35) than at Sydney ($25). The last difference is that during the sessions at RMIT we removed the first block of investment task that took place before the treatments (circled in grey in Figure 1). This does not affect the analysis as we are mostly interested in the differences in investments after the treatments. In all our analysis, we use site dummy to control for potential differences across the sites caused by these procedural differences.

2.6.1 Online experiment procedure

Our online procedure followed the University of California, Santa Cruz LEEPS economics laboratory procedure (Zhao et al., 2020). We aimed to create an environment that mimicked the controlled environment of an in-lab session, ensuring participants were focused and engaged with all tasks.

Participants were emailed a Zoom link twice, 24 hours and 2 hours, before their scheduled session commenced. Participants were required to have their cameras on throughout the entire session, allowing for the experimenters to observe and monitor them, as well as create a more engaged environment. Participants were encouraged to ask questions if tasks were not clear. Participants were also told that if they appeared distracted or absent, they would forfeit their payment for the session.

Participants joined the meeting and were placed in a waiting room, from where we brought one participant into the main room at a time, checked their ID, renamed them to an identifying number in Zoom, and then placed them in a breakout room to wait for the experiment to

[1] We provided different payment options for participants who were in China because PayPal is not available in China.

commence. One experimenter would check IDs in the main room and the second would monitor participants in the waiting room. This approach ensured anonymity for all participants. Once everyone had been checked in, participants received unique URLs through a direct message feature on Zoom, taking them to the online experiment window of zTree Unleashed (Duch, Grossmann, & Lauer, 2020). There, they viewed a participant consent form which they had to sign before the experimental session commenced. Participants could not unmute or rename themselves once the experiment began. They could message the experimenters but not other participants. At the end of the session, the experimenter waited until all participants completed the study and then announced the end of the experiment over the Zoom call. Participants could simply close their experiment windows and leave Zoom.

2.6.2 In-person experiment procedure

The optimal environment for our experiment is in a laboratory. Conducting experiments in a lab ensures the sensitivity of the mouse remains consistent across all participants, thereby eliminating any potential disadvantage in completing the Slider Task arising from equipment differences. Consequently, we have decided to complement our data with in-person experiments in the RMIT Behavioral Business Lab.

We adhered to the standard procedures for lab experiments. Upon arrival, participants signed consent forms, read the instructions, and completed comprehension questions. They then proceeded with the experimental task, followed by a post-experiment questionnaire. To maintain consistency and avoid any additional variability that could arise from in-person experiment, we continued to use PayPal to compensate our participants for participation.

3. Results

3.1 Preliminary results

3.1.1 Slider Task and treatment implementation

We were successful in creating a real effort task that participants can complete. As shown in Figure 5A, participants on average completed 9.15 out of 10 screens correctly and did equally well in the advantage and disadvantage groups (9.16 in the advantage and 9.14 in the disadvantage group, $p = 0.86$). As intended, in the advantage treatment they were allocated significantly more bonus lottery tickets, 8.67 versus 0.5 in the disadvantage group ($p < 0.001$) (Figure 5B).

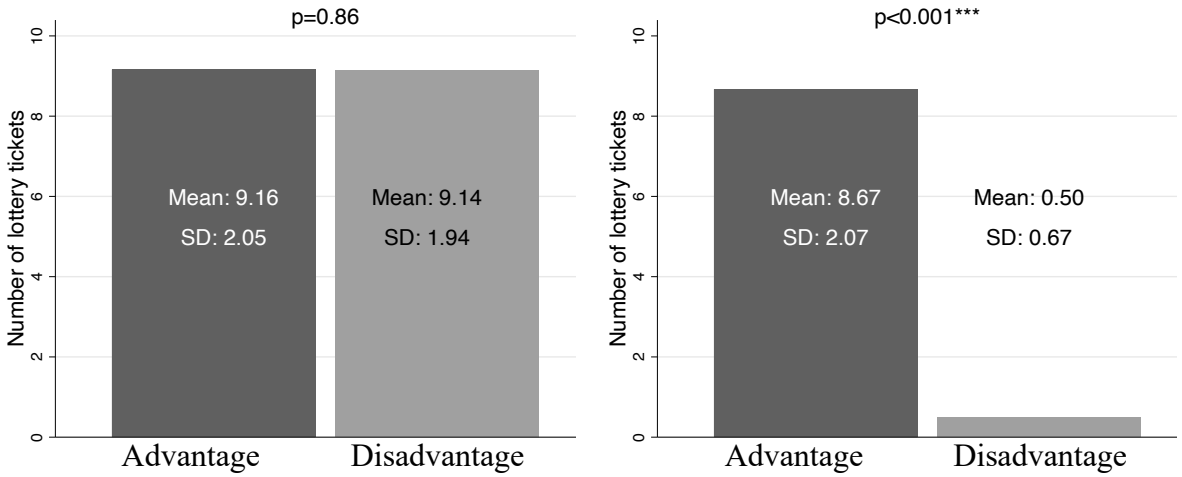


Figure 5. Lottery tickets earned A. through work B. as bonus lottery tickets by treatment.

The treatment was successful on the aggregate level, but we still need to confirm that the unfair chances treatment was correctly applied on the participant level. Participants in the advantage condition, to really feel advantaged, should always rank in the top half in each trial, and those in the disadvantage condition should rank in the bottom half. Due to chance in the early trials or due to advantaged participants not completing the task correctly, participants could have received ranking feedback that was not aligned with their treatment. In Figure 6 we illustrate the number of participants as a function of treatment accuracy, from 0% treatment accuracy meaning that the participant was never ranked correctly to 100% accuracy meaning that the participant was always ranked correctly. The spike at 100% treatment accuracy indicates that most participants were accurately ranked throughout the block. 21 participants were ranked accurately less than half of the time in the first block and 9 in the second block (grey bars in Figure 6). 127 participants (out of 217) were correctly ranked all the time in both blocks. On average, participants were correctly ranked in 87.42% of the trials in block 1 and in 92.95% of trials in block 2. We exclude thirty participants who had a treatment accuracy below 50% in either block from our analysis as they did not experience the treatment as we intended. This leaves us with data from 187 participants for further analysis.

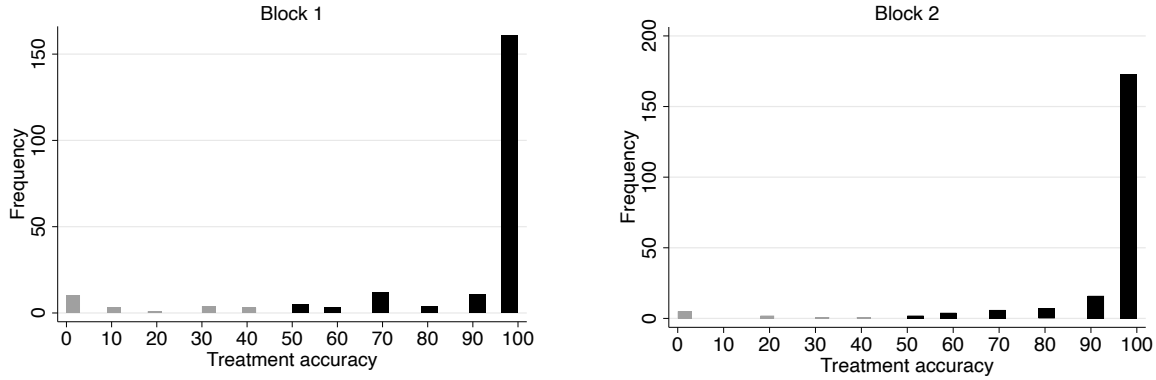


Figure 6. Accuracy of treatment. A. Block 1, B. Block 2.

3.1.2 Sample characteristics

Table 1 indicates that our sample is well balanced on gender at both sites. Participants from RMIT are significantly older than those from the University of Sydney. Importantly, all variables of interest that relate to the behavior and treatment in the experiment – the number of sliders adjusted correctly, random bonus lottery tickets allocated, and treatment accuracy – do not differ significantly across sites.

Table 1. Sample Characteristics

	Total	The University of Sydney	RMIT University	p-value
N	187	102	85	
Female	0.524	0.529	0.518	0.873
Age	23.476	21.990	25.259	0.001
Relative wealth perception	46.797	49.627	43.400	0.050
Discrimination Index	20.818	20.235	21.518	0.155
Sliders adjusted correctly	118.599	118.353	118.894	0.231
Random tickets - advantage	9.166	9.078	9.271	0.189
Random tickets - disadvantage	0.460	0.461	0.459	0.984
Treatment accuracy block 1	95.241	95.000	95.529	0.760
Treatment accuracy block 2	96.257	95.784	96.823	0.473

3.2 Within-subject analysis of the effect of unfair chances (disadvantage treatment) on investments

3.2.1 The effect of unfair chances on ambiguous investment decisions

We now come to our main question: did the experience of unfair chances in a real effort task affect participant's subsequent investments in an unrelated task? We found that experiencing unfair chances significantly decreased participants' investments in the ambiguous asset. After experiencing advantage, participants on average invested \$7.23 and after disadvantage \$6.80. This is a significant ($p = 0.014$) 5.95% drop in investment (Figure 7).

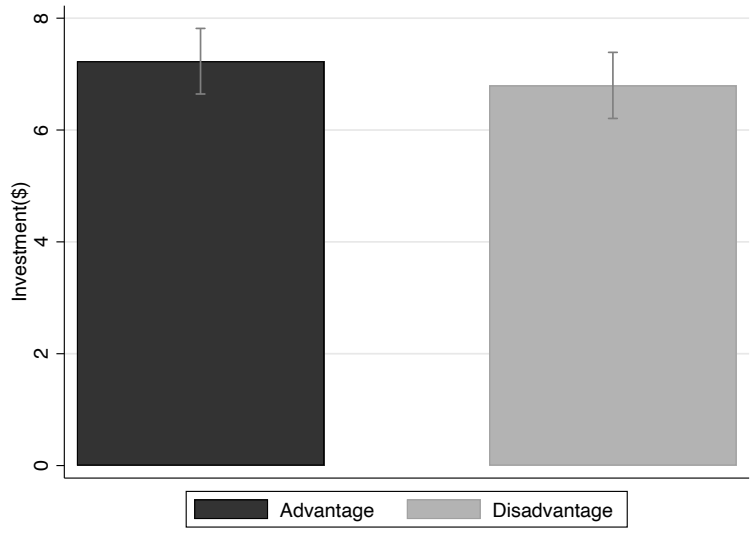


Figure 7. Invested amount in ambiguous assets after advantage and disadvantage.
Within-subject comparison. Bars are 95% confidence interval.

Using regression analysis, we investigated the factors that moderate the effect of unfair chances on the willingness to invest in ambiguous assets (Table 2). First note, that the result remains significant in a regression analysis and is robust to including a range of control variables. Without any controls, we estimate that participants invested \$0.43 less after experiencing disadvantage. As we include the controls the effect gets stronger, with the experience of unfair treatment reducing investments by \$2.86 (model (7) in Table 2). In all models, we used a site dummy (Sydney) to control for potential differences across the sites caused by the procedural differences. Since neither the site dummy nor its interaction with disadvantage dummy is significant (model (6)), our result is consistent across sites.

We hypothesized that several factors could moderate the effect of unfair chances on investments. First, we designed our experiment to test whether providing people with more financial resources via a higher show up fee will make them more immune to our unfair

treatment. While the regression analysis reveals that the effect of unfair treatment was reduced by $0.33 (approximately half) for participants who received the higher show up fee (model (2)), this effect is far from being statistically significant. Since it is possible that the difference in the show-up fee is too small to have an effect, we have also checked whether differences in self-reported relative wealth reduce sensitivity to unfair treatment. Again, the sign of the coefficient indicates a mitigating effect, but it is highly insignificant. Moreover, even moving from the lowest self-reported wealth of 0 to the highest self-reported wealth of 100, would not be enough to fully eliminate the negative effect of unfair treatment on the amount invested.

Table 2. The effect of unfair chances treatment on investment in ambiguous assets (within-subject). Dependent variable is the invested amount. Disadvantage is a treatment dummy (= 1 in disadvantage, 0 in advantage). High fee = 1 if participant received a high show up fee. Relative wealth is self-reported wealth relative to peers (0 = least wealthy; 100 = most wealthy). Discrimination is a survey score of the real-life discrimination experienced. Female = 1 for females and 0 otherwise. Sydney = 1 for USyd and 0 otherwise. Age is in years. X denotes interaction variables.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Disadvantage	-0.4326*	-0.6034*	-0.7484*	-1.4208*	-0.8775**	-0.5329*	-2.8558***
	(0.1759)	(0.2383)	(0.3440)	(0.6591)	(0.2916)	(0.2529)	(0.8500)
High fee		0.0507					0.3418
		(0.5947)					(0.5820)
High fee X Disadvantage		0.3258					0.2846
		(0.3503)					(0.3644)
Relative wealth			0.0241				0.0212
			(0.0163)				(0.0157)
Relative wealth X Disadvantage			0.0067				0.0100
			(0.0068)				(0.0064)
Discrimination				0.0017			-0.0001
				(0.0536)			(0.0524)
Discrimination X Disadvantage				0.0475			0.0591+
				(0.0310)			(0.0307)
Female					-2.2202***		-2.1830***
					(0.6087)		(0.6108)
Female X Disadvantage					0.8490*		0.8836*
					(0.3530)		(0.3640)
Sydney X Disadvantage						0.1839	0.2019
						(0.3517)	(0.3581)
Sydney	0.8120	0.8184	0.6833	0.8467	0.7195	0.7200	0.5534
	(0.6055)	(0.6072)	(0.6004)	(0.6181)	(0.5909)	(0.6295)	(0.6181)
Age	0.0323	0.0308	0.0453	0.0329	-0.0025	0.0323	0.0083
	(0.0409)	(0.0409)	(0.0432)	(0.0409)	(0.0416)	(0.0410)	(0.0432)
Constant	6.0296***	6.0347***	4.6641**	5.9596***	8.0597***	6.0798***	6.7073**
	(1.1454)	(1.1673)	(1.4643)	(1.7589)	(1.3129)	(1.1511)	(2.1490)
N	374	374	374	374	374	374	374

Standard errors clustered on individual in parentheses. +p<0.1, *p<0.05, **p<0.01, ***p<0.001

Second, we hypothesized that people who experience more discrimination in their everyday life may react differently to our treatment. We asked participants nine questions about how often they experienced being treated with less courtesy, respect, receiving poorer service, being treated as less smart, more dangerous, more dishonest, and being insulted, and threatened more often than others using a survey by Williams et al. (1997). Participants responded on a scale from 1 (never) to 6 (almost every day) (see Question 10 in Appendix B for exact wording). The coefficient is positive, and we estimate that each additional point increase in the reported everyday experience of discrimination, reduces the impact of our unfair chances treatment by \$0.05-0.06. In model (7) that includes all controls, the effect is significant at the 10% level. We conclude that this evidence is suggestive that previous experience of everyday discrimination may reduce the impact of our laboratory unfair chances treatment on investment.

Finally, because many papers found that men and women have different attitudes to risk, we investigated whether the effect differs by gender. Our data indicates that women invested less (by \$2.22 on average) than men. However, they were also much less affected by the unfair chances treatment than men. The coefficients on Disadvantage and the interaction of Female and Disadvantage have opposite signs and are close to equal in magnitude, meaning that the effect vanishes for women. To investigate this further, in Figure 8 we compare the average investments after disadvantage and after advantage separately for men and for women. After experiencing an advantage, males invested \$8.39 on average, whereas after experiencing disadvantage, they invested \$7.51. This represents a significant 10.49% decrease in investment after disadvantage ($p = 0.003$) (Figure 8A). Females, on the other hand, invested less overall compared to males, but did not significantly alter their investment following the disadvantage. Specifically, females invested \$6.18 after experiencing advantage and \$6.15 after disadvantage. The difference in investment for females is economically small and statistically insignificant ($p = 0.886$).

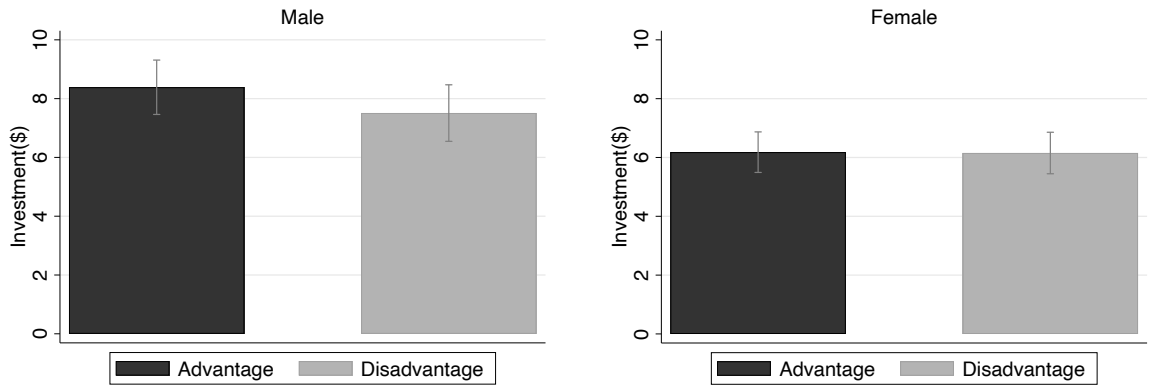


Figure 8. Invested amount in ambiguous assets after advantage and disadvantage by gender. A. Male, B. Female. Bars are 95% confidence interval.

The effect of disadvantage on the investment in ambiguous assets could be in principle driven by a change in both the attitude to ambiguity and risk since both preferences drive the investment in ambiguous assets. Therefore, next we investigated whether the unfair chances treatment affected investments in risky assets as well.

3.2.2 The effect of unfair chances on risky investment decisions

We did not find any significant effect of unfair chances treatment on the amount invested in the risky asset with known probabilities of win and loss. As shown in Figure 9, participants invested on average \$8.63 after experiencing advantage and \$8.58 after experiencing disadvantage. This difference of only 5 cents is not statistically significant ($p = 0.684$). This result is further confirmed by regression analyses (Table 3). Although the coefficient on Disadvantage is negative, it is not significant in any of our models. The only two significant variables are Female, indicating women invested less than men, and Sydney with participants from the University of Sydney investing more (but not being more or less sensitive to treatment). We therefore conclude that our unfair chances treatment did not affect how much participants invested in the risky asset.

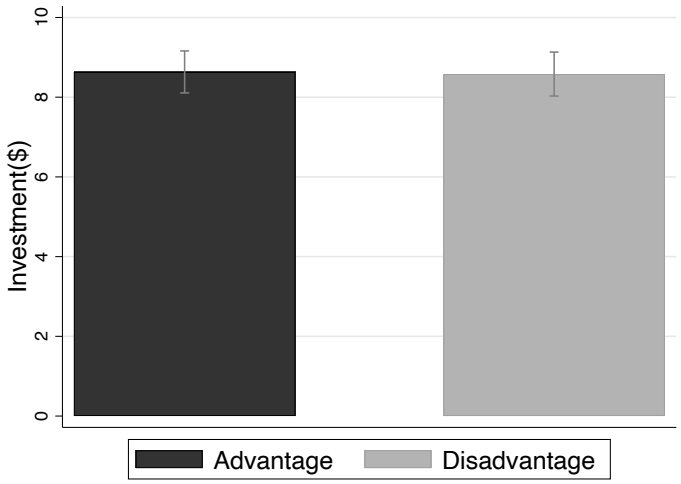


Figure 9. Invested amount in risky assets after advantage and disadvantage. Within-subject comparison. Bars are 95% confidence interval.

Table 3. The effect of unfair chances treatment on investment in risky assets (within-subject). Dependent variable is the invested amount. Disadvantageis a treatment dummy (= 1 in disadvantage, 0 in advantage). High fee = 1 if participant received a high show up fee. Relative wealth is self-reported wealth relative to peers (0 = least wealthy; 100 = most wealthy). Discrimination is a survey score of the real-life discrimination experienced. Female = 1 for females and 0 otherwise. Sydney = 1 for USyd and 0 otherwise. Age is in years. X denotes interaction variables.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Disadvantage	-0.0529	-0.2191	-0.0582	-0.0810	-0.0281	-0.1624	-0.3702
	(0.1305)	(0.1832)	(0.3813)	(0.5541)	(0.1943)	(0.2255)	(0.8835)
High fee		0.4539					0.6778
		(0.5307)					(0.5236)
High fee X Disadvantage		0.3171					0.3371
		(0.2602)					(0.2712)
Relative wealth			0.0083				0.0065
			(0.0151)				(0.0148)
Relative wealth X Disadvantage			0.0001				-0.0000
			(0.0070)				(0.0070)
Discrimination				0.0123			0.0054
				(0.0477)			(0.0482)
Discrimination X Disadvantage				0.0013			0.0030
				(0.0247)			(0.0261)
Female					-1.7543**		-1.8130**
					(0.5422)		(0.5482)
Female X Disadvantage					-0.0474		-0.0814
					(0.2625)		(0.2747)
Sydney X Disadvantage						0.2006	0.2235
						(0.2697)	(0.2666)
Sydney	1.0765+	1.0950*	1.0373+	1.0942+	0.9849+	0.9762+	0.8741
	(0.5505)	(0.5509)	(0.5512)	(0.5670)	(0.5366)	(0.5550)	(0.5573)
Age	0.0996*	0.0953*	0.1036*	0.1000*	0.0652	0.0996*	0.0610
	(0.0408)	(0.0403)	(0.0415)	(0.0412)	(0.0439)	(0.0408)	(0.0446)
constant	5.7076***	5.5602***	5.2462***	5.4353**	7.4849***	5.7623***	6.9046**
	(1.1116)	(1.1215)	(1.4393)	(1.6321)	(1.2949)	(1.1138)	(2.1374)
N	374	374	374	374	374	374	374

Standard errors clustered on individual in parentheses. +p<0.1, *p<0.05, **p<0.01, ***p<0.001

3.3 Between-subject analysis of the effect of unfair chances treatment on investments

So far, we analyzed the effect of unfair chances on investments using a within-subject analysis. We designed our experiment to allow us to also compare participants' investments using a between-subject approach, by comapring the amounts that participants invested after the first treatment only. Ninety participants experienced disadvantage first and 97 experienced advantage first. Participants who experienced disadvantage invested \$6.63 in the ambiguous asset which is significantly ($p = 0.092$) less than those who experienced advantage, \$7.64 (Figure 10A). Regression analysis (Table D1) is consistent with the within-subject analysis presented in Table 2, however, the significance decreases, as anticipated, because using a between-subject approach requires a larger sample size to detect effects of the same size due to reduced statistical power.

Additionally, in line with our within-subject analysis, we found that unfair chances treatment did not affect participants' investments in the risky asset (Figure 10B and Table D2). On average, participants invested \$8.54 after experiencing advantage and \$8.33 after disadvantage. This difference of \$0.22 was not statistically significant ($p = 0.670$).

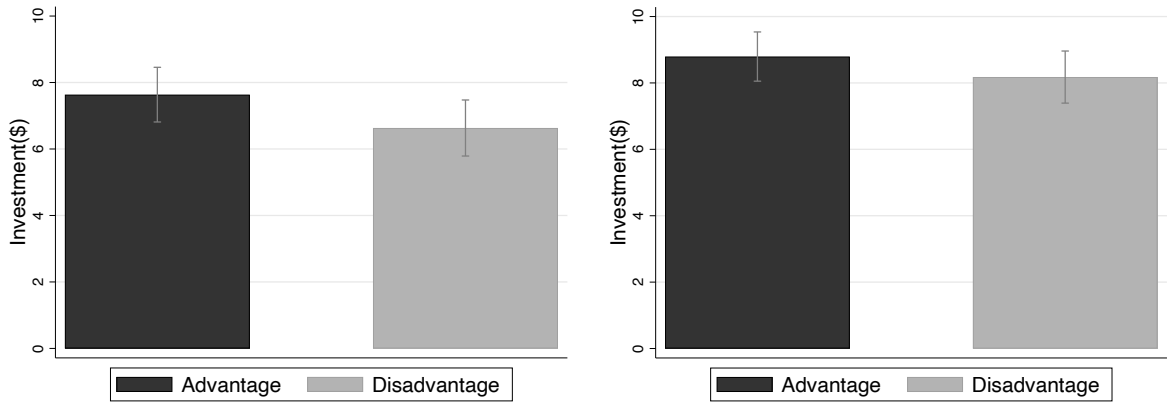


Figure 10. Amount invested in A. ambiguous and B. risky assets after advantage and disavantage. Between-subject comparison. Bars are 95% confidence interval.

3.4 Additional results - survey measure of ambiguity aversion

To promote research on attitudes towards ambiguous prospects, we included a short survey-based measure of ambiguity tolerance developed by Cavatorta & Schröeder (2019) in our questionnaire. If this survey measure correlates with experimentally observed decisions under ambiguity, it would be of high interest to researchers who want to quickly and without incentives measure people's attitudes towards unknown risks. Cavatorta & Schröeder (2019)

demonstrated that their questionnaire produces a score for an individual's ambiguity tolerance to reliably measure ambiguity attitudes. Here we replicate this finding using the investments in ambiguous assets to investigate the correlation. To ensure our measure is not affected by the unfair chances treatment, we only include investment data from 124 participants for whom we collected investment data before treatments (circled in grey in Figure 1). On average, they invested $7.05 in ambiguous assets. The average of the ambiguity module score was 195.71.

The ambiguity survey module is positively and (statistically) significantly correlated with the investments in ambiguous assets. The Pearson's correlation coefficient is 0.214 ($p = 0.017$). This is similar to what Cavatorta and Schröder (2019) reported (0.26 – 0.33). Moreover, the survey module is significantly correlated with investments in ambiguous assets but not with investments in risky assets. This suggests that the survey is an effective measurement for ambiguity tolerance without being confounded by attitudes towards risk. Next, we break down how each individual question in the survey module correlates with our participants' investments under ambiguity. Attitudinal question 3 preforms particularly well in predicting the ambiguous (but not risky) investments of participants in this study, with a correlation coefficient of 0.208 ($p = 0.021$) for the investment in ambiguous assets. This suggests that more ambiguity tolerant people tend to think solutions are always available. The Dynamic Ellsberg Urn experiment is also somewhat validated, with a correlation coefficient of 0.291 ($p = 0.001$) for the investments in ambiguous assets but it also correlates with invetments in risky assets.

Table 4. Pearson's correlation coefficients of the ambiguity survey module and ambiguity tolerance measures, with question by question breakdown. p-values in brackets.

Ambiguity Measure	Investments in ambiguous assets	Investments in risky assets
Complete Survey Module	0.214**	0.126
	(0.017)	(0.163)
Survey Question Breakdown		
Dynamic Ellsberg Urn	0.291***	0.263***
	(0.001)	(0.003)
Classic Ellsberg Urn	-0.098	-0.111
	(0.277)	(0.220)
1. I would like to live in a foreign country that is new to me.	-0.067	0.144
	(0.460)	(0.111)
2. There is a right way and a wrong way to do almost everything.	0.065	0.099
	(0.474)	(0.276)
3. Practically every problem has a solution.	0.208**	0.075
	(0.021)	(0.406)
4. I feel relieved when an ambiguous situation suddenly becomes clear.	-0.041	-0.028
	(0.650)	(0.760)
5. I find it hard to make a choice when the outcome is uncertain.	0.142	0.122
	(0.115)	(0.178)

+p<0.1, *p<0.05, **p<0.01, ***p<0.001

4. Discussion

Unfair treatment in the workplace is a prevalent issue that affects various groups, leading to negative consequences such as reduced productivity, mental health disparities, and economic instability (Gagnon, Bosmans, & Riedl, 2021). It remains unclear whether experiencing unfair treatment influences one's own subsequent financial decisions. Some previous studies suggest that the experience of discrimination has the potential to alter risk preferences. For instance, a laboratory study by Jamieson et al. (2013), in the context of racial discrimination, found that after being rejected by somebody of a different race participants increased risk-taking in a hypothetical Columbia Card Task (Figner et al., 2009). Yang et al. (2019), using the National Longitudinal Survey of Youth, found that discriminatory experiences in adolescence are

associated with risky health behaviors in early adulthood and compromised health later in life. These two studies did not investigate the impact of unfair treatment on financial investments, nor did they distinguish between known and unknown risks. Using a novel and incentivized experiment that induces unfair treatment in lab, we evaluate the effect of unfair treatment on investments in risky and ambiguous assets and we fill this gap in knowledge.

We found that experiencing unfair chances reduced individuals' subsequent investments in ambiguous assets by 6%. This effect was robust in various regression analyses. Our finding suggests that people who experience unfair chances are worse off economically not only because of the direct consequences of unfair chances, such as reduced access to promotion or bonuses, but also through a reduction in their own profitable investments. This is important as it reveals that even brief experiences of unfair chances can change individuals' decisions in an unrelated task when the outcome probabilities of a decision are unknown. To our best knowledge, our study is the first to study the effect of unfair chances on the investments in ambiguous assets.

We explored several potential factors that could intensify or reduce the effect of unfair chances on behavior. We hypothesized that providing participants with more financial resources via a higher show-up fee may mitigate the impact of the experience of unfair chances. Although the effect is in the expected direction, it is not statistically significant. This suggests that a higher monetary endowment alone may not be sufficient to counteract the negative effects of unfair chances on investment decisions, even if that endowment is much higher than the financial disadvantage directly caused by unfair chances. Similarly, we found no significant mitigating effect of self-reported relative wealth on the impact of unfair chances.

Participants' everyday experiences of discrimination measured using a post-experimental survey module appeared to slightly reduce the impact of our unfair chances experimental treatment. This finding is suggestive of a habituation effect, where individuals accustomed to discrimination may become desensitized to being treated unfairly in specific contexts.

Interestingly, we found that the reduction in investments in ambiguous assets was present only in male participants. After disadvantage, males reduced their investments in ambiguous assets by 10% but females did not. This gender difference in response to disadvantage provides important insights. On one hand, males seem to be more vulnerable to the behavioral consequences of unfair treatment. On the other hand, although females did not reduce their investments after experiencing unfair chances, they generally invested less which limited their

earnings in the experiment. Further research into what drives these gender differences may yield valuable insights into tailored interventions to mitigate the negative effects of unfair chances.

In contrast to ambiguous investments, experiencing unfair chances did not significantly affect participants' investments in risky assets. Participants invested nearly the same amount after experiencing both advantage and disadvantage. This suggests that the emotional and cognitive processes stimulated by experiencing unfair chances may be more closely tied to ambiguity aversion than to risk aversion. It is possible that the experience of unfair chances triggers more pessimistic outlook and while it does not affect the investment in risky asset because the likelihood of it being successful is exactly known and fixed, it may influence people's beliefs about the probability of success of an ambiguous investment which success probability is not precisely known. In other words, the uncertainty about the probability of success of the ambiguous asset means that people have flexibility in judging how likely it is to be successful.

Our findings are in contrast to Jamieson et al. (2013) and Yang et al. (2019) who found more risk-taking after discrimination. There are several potential reasons. First, Yang et al. (2019) studied risk taking in health behaviors and the results may be domain specific. Similarly, Jamieson et al. (2013), used a Columbia Card Task which resembles a game played for points, rather than a real investment decision. It is also possible that the effects depend on how unfair treatment is framed. In our study, it is quite intuitive that experiencing unfair chances to earn extra money may lead participants to underestimate the odds of success in financial investments. Non-economic forms of discrimination may involve a different mechanism. Overall, the lack of significant impact on investments in risky assets and the opposite effect for ambiguous assets relative to previous studies underscores the importance of using incentivized tasks and exploring effects across different domains.

Finally, our study provides a protocol to study experimentally induced unfair chances in the laboratory. Our design is inspired by Haushofer & Fehr (2019) who designed an experiment to study the impact of negative income shocks on behavior by randomly assigning participants into different endowment levels. We created a workplace unfair chances treatment by introducing randomly assigned unfair chances in a design that allows for the identification of both within- and between-subject effects on behavior. The design is easy to implement and influenced participants' behavior. In the future, it can be used to investigate the effects of unfair chances at workplace on other preferences.

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