UNIVERSITY OF CALIFORNIA,
IRVINE

Three Essays in Experimental Economics with a focus on Psychology

DISSERTATION

submitted in partial satisfaction of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

in Economics

by

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2023

DEDICATION

To

my beloved parents

in recognition of their endless support

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

ACKNOWLEDGEMENTS

I would like to express the deepest gratitude and appreciation to my committee chair, Professor Michael McBride, for his invaluable guidance, expertise, support, and encouragements. His mentorship and dedication have helped me navigate through the complexities of academia. Without his insightful feedback and constant motivation, this dissertation would not have been possible.

I would also like to thank my three other committee members. They have played crucial roles in my academic journey. I am grateful to Professor John Duffy for his advice on experimental design. I thank Professor Igor Kopylov for his insightful suggestions on my research process. I extend my gratitude to Professor Michael Choi for giving me important feedback throughout my academic journey.

Lastly, I would like to acknowledge the financial support, including the summer fellowship, I received from University of California, Irvine Department of Economics. The financial support is vital for my experiments and ensures the successful completion of my study.

ABSTRACT OF THE DISSERTATION

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University of California, Irvine, 2023,

Professor Michael McBride, Chair

This dissertation exams how psychology theory can be implemented into experimental economics and studies the effects of human cognitive activities on their economics behaviors. It also studies how people react to job applicants with misdemeanor records and how economics outcomes can affect lying behaviors. The data used for this dissertation include self-collected data from laboratory experiments and publicly available data from U.S. government agencies. In the first chapter, I show that people with anxious attachment style tend to make economics decisions appealing to others yet people with avoidant attachment style care more about their own economics well-beings. In the second chapter, I show that marijuana possession-related misdemeanor significantly hinders one's employment outcomes and expungement could help the situation. Although the legalization of marijuana is still an on-going process, without expunging the previous misdemeanor records related to marijuana, the legalization itself is not enough to improve the employment outcomes. In the third chapter, I show that constant moral reminders decrease dishonest behaviors whereas higher rewards lead to more deception. Moral reminders can only work to certain extent and as the rewards go higher, the effect of moral reminders gradually diminish.

CHAPTER 1. IMPACT OF DIFFERENT ATTACHMENT STYLES ON ECONOMICS BEHAVIORS

Introduction

Attachment, defined as "a strong emotional bond that an infant forms with a caregiver; affectionate regard," can be seen as a measure of closeness between one agent and another. Attachment theory firstly studied the relationship between infants and their primary caregivers, usually mothers, in the early stage of its development and gradually expanded to study the adult relationship such as romantic relationship and friendship. The importance of close relationship in economic activities is obvious. For example, a household decides when and where to buy a house, a couple of friends decide which concert they want to go and etc. Thus, it is important to examine the impact of agents' closeness to each others, on economics behaviors. In this paper, we take advantage of the rich literature in attachment theory from psychology, and examine not just agents' closeness, but their attachment styles specifically to see its impact on their economics behaviors. However, attachment theory is still an under-studied literature in economics before Almakias and Weiss (2012) introduced it into our field.

This paper experimentally studies the role of attachment in economic decisions. We first look at why using attachment theory can be advantageous, particularly in games such as prisoner's dilemma. Most of the researches focusing on the impact of personality on cooperation still look at the direct personality types and their effects on human behaviors. It works well when we study them at the individual level, and categorize similar types of people into a few personality types. However, when dealing with multiple agents who have close relationship, attachment becomes an important factor that may impact their decisions including economic ones. In this paper, we focus mainly on the cooperation since all experiments we use in this study involve some level of competition or cooperation. We attribute cooperation into the following types: 1) cooperation between pure strangers, which

means they have very little to no attachment with each other (But we do need to consider the attachment among human being as a whole. For example, human beings have sympathy to tragedies happened to strangers, which is essentially a form of attachment); 2) cooperation between somewhat closer ties, such as roommates, colleagues, and friends; 3) cooperation between extremely close ties, such as ties between parents and children, siblings, and cousins. We will approach cooperation from an economic perspective under attachment theory. By the nature of anonymity of our experimental design, our paper focus mainly on cooperation between pure strangers and leave the other two types for the future research.

We now do a brief literature review to look at the steps that lead economists to use attachment theory in our field. Firstly, cooperative and (non-)cooperative behaviors have already been studied in economics for many years due to its importance in public economics and political economics. The omnipresence of studies in cooperation in social sciences literature is mostly due to its importance to our society. Without cooperation, our society cannot operate smoothly. It is important to point out that different cultures may view cooperation in slightly different ways. For example, the impact of culture and education on cooperative behaviors had been studied many times such as Cox, Lobel, and McLeod (1991) and Boone and Van Witteloostuijn (1999). Notice that when we shift our views to culture, we are already implicitly considering the impact of personality because culture shapes an individual's personality and culture is the personality of a society (Markus & Kitayama, 1998), which we can consider as a large-scale personality impact on cooperation.

Other literature considers individual level of personality (Swope, Cadigan, Schmitt, & Shupp, 2008). Ferguson, Heckman and Corr summarized how personality theory and economics "have independently tackled several common questions" such as "what can explain heterogeneity in behaviors both within and across tasks" (Ferguson, Heckman, & Corr, 2011). Perhaps the most famous game in economics that proves the heterogeneity in human being's behaviors is prisoner's dilemma. Indeed, it is easy to be implemented and does not require a lot of instructions to the subjects in the lab, and subjects over-cooperate which violates

the theoretical prediction. In addition, Boone, Brabander, and Witteloostuijn claimed that personality matters in cooperative behaviors and they conducted a lab experiment of prisoners' dilemma to test "the impact of personality on cooperative versus competitive choices in a Prisoner's Dilemma context" by giving up the assumption of *homoanonymous*, which means "all economic agents behave alike if confronted with the same circumstances" (Boone, De Brabander, & Van Witteloostuijn, 1999). Brosig also focused on the cooperative behaviors in prisoner's dilemma and talked about the impact of one's emotional system. It is safe to attribute one's emotion as a product of one's personality, and emotion is surely a product of one's attachment to her closed ones as well (Brosig, 2002). People may have various emotions such as happiness, anger, resentment due to the actions of their closed ones, and those emotional reactions can be deeply tied with their different attachment style. Various other research also studied identification in different game settings such as public goods game, and they concluded a group attachment impact (Christens, Dannenberg, & Sachs, 2019). Ben-Ner and Kramer further expand the context into dictator game experiment and test how lab subjects react when "presented with different persons who can be classified as kin, collaborator, competitor and neutral based on their similarity/relationship to the subject" (Ben-Ner & Kramer, 2011). Albeit Ben-Ner and Kramer did not use the term attachment, they clearly classified these relationships based on how close these people are to the subject, which has the flavor of attachment.

Finally, Almakias and Weiss (2012) introduced attachment theory into economics and conducted laboratory experiments to study the impact of different attachment styles on ultimatum game. In their study, they tested "the implications of [attachment theory] for economic behavior specifically in the anonymous setting of the ultimatum game, a setting in which it is least likely to succeed." They did an "Experience in Close Relationship Scale (ECR)" (Brennan, Clark, & Shaver, 1998; Mikulincer & Florian, 2000) to determine the subjects' attachment style, and looked at their behavior and monetary payoff in a repeated laboratory ultimatum game. In addition, all subjects had the same role as their first round

and were re-matched with different subjects randomly for different rounds. Their data supported that proposer will offer more with higher level of anxiety and lower level of avoidance at 5% significance level. It is important to keep in mind that their lab setting is anonymous, and with random re-matching at the beginning of each round, which they themselves claimed as a setting that is "(the theory) least likely to succeed," yet the result looks fairly successful. One possible explanation is that there may be learning effect after many rounds of playing the same game, especially with the same roles. What Almakias and Weiss did, created a lower bound of the impact of attachment theory due to the all-stranger lab environment. Instead of focusing on the all-stranger situation and ultimatum game only, we shift our view to explore a less strange setting and check if the theory survives in more games.

This paper builds on the work begun by Almakias and Weiss (2012). We still preserve the anonymity to avoid strategic cooperation. Instead of only using ultimatum game and check the reject/accept offer for each attachment style, this paper adds various different simple games such as simple trust game and prisoner's dilemma to test attachment theory. We start off with a relatively low intimate level to test the theory in an all-stranger situation. We then build up the intimacy level of the lab environment by creating chat rooms in the treatment session, before we run the experiment. The paper contributes to further expand the attachment theory into economics field in the behavioral aspects and could be expanded further in the future. By conducting the following lab experiment, the paper enriches the current literature in both economics and psychology. Economics literature still lacks data to test the impact of attachment on cooperative behaviors. Psychology has a rich literature on attachment theory, but they did not take advantage of explaining the impact in economics perspective yet. Our paper also puts attachment theory in a broader context by testing it in various games instead of limiting it in one game.

The rest of the paper is structured as follows. Section 2 elaborates more details of attachment theory. Section 3 looks at attachment theory in different games and summarizes

the experiment. Section 4 discusses the implications of the results and possible future study of attachment theory in economics. Section 5 closes with a conclusion.

Attachment Theory

Attachment theory was first introduced by psychiatrist John Bowlby in the 1950s to explain the infants' relationship with their mother and gradually developed to study attachment (Bowlby, 1958, 1973). It was tested by psychologist Mary Ainsworth. The theory initially focused on the attachment between infants and their primary caregivers. Ainsworth used "the Baltimore Project", or more broadly referred as Strange Situation, a procedure to test how infants react when their primary caregivers are away for a period of time and then come back to them, to observe the attachment style of these infants and further develop the attachment theory (M. D. Ainsworth, Blehar, Waters, Wall, et al., 1978; M. D. S. Ainsworth, Blehar, Waters, & Wall, 2015; Main & Solomon, 1986).

The brief version of the strange situation is described as follows: infant and primary caregiver (usually their mothers so we use mother as a term to refer primary caregivers for the rest of the paper) are introduced in a strange room and left alone. Experimenters observe how the infant reacts when her mother is in the room with her. Then experimenters ask the mother to leave the room when the infant's attention is away and observe how the infant reacts after she realizes her mother is gone. After a while, the experimenters re-introduce the mother into the room and observe how the infant will react to the return of the mother. The experiment initially concludes three types of attachment styles and associating behaviors. Secure babies are explorative in the strange situation when their mothers are around, which means they feel safe in a strange situation as long as they are accompanied by their mothers. They express clear negative emotions such as upset and sadness when they realize their mothers are missing. Upon reunion with their mothers, they are happy and welcoming and can quickly go back to explore the strange room. In a word, their mothers are their secure bases. Anxious-ambivalent type babies are clingy and demanding in a strange situation even

when the mothers are with them. They do not explore the strange room that much and they tend to stay with their mothers as close as possible. They are usually angry and resentful when they realize their mothers are gone. They show mixed signs such as welcoming and resisting when they reunite with their mothers. Anxious-avoidant type babies show very little interest in exploring the room, act aloofly when their mothers are away, and have no sign of welcoming when their mothers are coming back to them. A group of babies who have tense movements that were initially hard for Ainsworth to categorize, are later classified as disoriented by Main and Hesse and further approved by Ainsworth (Main & Solomon, 1986; M. D. S. Ainsworth, 1990; Brennan et al., 1998).

The theory quickly extends to explain adult attachment which can apply to various forms of intimacy such as friendships, romantic relationships, and kinships (Hazan & Shaver, 1987). Although later psychology studies in adult attachment conceptualize attachment styles into a continuous two-dimensional space, it still has four regions and for the purpose of this paper, we use categorized name to describe the attachment styles (Bartholomew & Horowitz, 1991). Let us have a quick look at the personality traits of these four different styles in adults, which can help us to develop the hypothesis in the following section. Hazen and Shaver argued that adult love styles are closely following infant attachment style Ainsworth concluded. Thus, we present the following four adult attachment style: (1) secure (2) anxious, (3) avoidant (4) anxious-avoidant (which is a close match to disoriented babies), and each of four types associates with different behaviors. Secure adults usually have almost no or very little problems to become close with other people and form social bonds. They look at both themselves and other people with positive opinions. Anxious adults are preoccupied by relationships (not only romantic but could be parental relationship, friendship, etc.) and need constant reassurance from others to affirm their relationship. They tend to have negative opinions about themselves and positive opinions of others, which also amplifies their anxiety. They care about how they are looked at by their closed social bonds. Avoidant adults, however, tend to have positive views of themselves and negative views of others, and

often avoid building close interaction with other people. They usually use rejection as a defense mechanism.

Bakermans-Kranenburg and IJzendoorn conducted research to study the distribution of different attachment style in population. Albeit there's more or less differences across the age, language, culture, sex, clinical data or non-clinical data, and other factors, they studied parental relationship and concluded that "58% of the mothers in norm group (non-clinical) are secure, with less than a quarter being classified as insecure-dismissing, and almost one-fifth as insecure-preoccupied." They also mentioned 18% of non-clinical mother has unresolved attachment style. In addition, to break the stereotypes, the distribution doesn't vary too much across different sex, which means male and female have very similar distribution of the attachment style. In addition, the distribution is independent of culture and language (Bakermans-Kranenburg & van IJzendoorn, 2009). Many studies tried to decipher the general distribution of attachment style across the population and have similar conclusion. Thus, we safely assume that 50% of the population have secure style and the other half of the population have insecure style, which breaks down to 20-25% anxious and 20-25% avoidant, with 5% unresolved, or we call it anxious-avoidant style. Our focus will be on anxious adults and avoidant adults since they tend to have the most different personalities (Levine & Heller, 2012).

Experimental Design

Our experimental design addresses the following objectives: to determine the impacts of different attachment style on (non-)cooperative behaviors, and to evaluate how attachment can influence subjects' behaviors in different level of intimate environment by implementing the ultimatum game along with several simple games. We test attachment theory starting from a complete stranger situation and build up the intimacy level by introducing a chat room. The experiment was conducted on oTree (D. L. Chen, Schonger, & Wickens, 2016).

We re-do the stranger case to see if we can replicate Almakias and Weiss (2012) did and if the results align with their hypothesis in a somewhat different setting. The reason we hope to build up the intimacy level is that attachment theory is studying people's behaviors in close relationship, so that an all-stranger setting is not the most ideal situation to learn the impact of attachment styles, though the easiest to achieve in a lab setting.

The experiment is done by oTree coding. We start off with the general case and an all-stranger situation, then gradually build up the intimacy level by introducing ice-breaker technology before the experiment starts. The experiment contains 1 control and 1 treatment. During the experiment, we first conducted ECR questionnaire to decide subjects' anxious and avoidance level. We then randomly pair two subjects together to run three phases of simple games including prisoner's dilemma, simple trust game, and ultimatum game , naturally, under anonymity and the pair will last until the end of the experiment. The simple trust game and prisoner's dilemma are testing attachment theory in different games other than ultimatum game alone to see if different attachment styles will have different behaviors in a game needs more trust, for ultimatum game may not be the most cooperative game that requires trust. We add ultimatum game in the experiment to replicate the result of Almakias and Weiss but also check if the result could be improved after subjects already had interactions before they enter into the stage of ultimatum game, which may generate some attachment between two stranger subjects.

In the control session, the experiment will start with the ECR questionnaire. They then enter into a one-shot prisoner's dilemma game, once the subjects are done with matching. Two players will encounter a payoff matrix showed below, and they need to decide whether to cooperate with each other or to defect on each other.

As figure 1.2 indicates, if both players choose to cooperate[1], they will each get \$5 payoff, and \$2 if both choose to defect. If one defects on the other who chooses to cooperate, then the defect will get \$8 and the defected will get \$0. The dominant strategy is clearly to

[1]All languages in the experiment will be neutral. For example, instead of seeing *cooperate* and *defect* as their strategies, experimental subjects will see "*OptionA*" and "*OptionB*"

defect if both of them are monetary payoff maximizing players, although cooperation can bring both players to a better stage. It requires trust to operate cooperative behaviors in the experiment. Once everyone made their choice, their choices will be revealed.

Once all players are done with the prisoner's dilemma, the game then enters into the second phase in which we run a standard experimental trust game with perfect information (Kreps, 1990), where the pair remains the same. There is no strategy method involved in this phase. We start the game with an initial endowment of \$2 to the first mover, who is decided by the computer with equal chance to both subjects. The first mover is then $Player1$ and she can choose to either *keep* all the money by sending \$0 to $Plyaer2$, or *pass* part or all of it to her partner. Once the money is passed, $Player1$ has no control over the money passed to her partner anymore. If $Player1$ passes certain amount of money to the second mover, $Player2$, then the money will be tripled. $Player2$ in turn, faces the same problem of either *pass* some money back to $Player1$ or *keep* all of it. The phase ends after $Player2$ makes her choice and regardless of what her choice is. It is not hard to see that the sub-game perfect equilibrium for both players is not to pass the money or keep all the money whenever having the chance if they are maximizing their monetary payoffs, though both players will benefit each other by cooperating and cooperation requires trust. The figure below is an example of possible scenario if $Player1$ chooses to pass her entire endowment to her partner, where the number on the left is $Player1$'s payoff and the right $Player2$'s. After both players make their moves, their actions will be revealed.

For example, in figure 1.3, if $Player1$ passes x amount of money to $Player2$, and $Player2$ passes y amount of money back to $Player1$, then $Player1$ receives $2 - x + y$ and $Player2$ receives $3x - y$.

Lastly, the game enters into the final phase that consists with a one shot ultimatum game. The pair remains the same and the computer randomly chooses the first mover again. The first mover will be the proposer and the second mover will be the respondent. The proposer is endowed with \$8 and is asked to make an allocation of the money between herself and

*Player*2. After *Player*1 makes the proposal, *Player*2 can choose either to *Take* it where both players receive the money as allocation suggested, or *Leave* it where both players receive nothing. Below is an example that *Player*1 makes a split even \$4 and \$4 offer and the subsequent monetary payoff to the players giving *Player*2's choice, where the payoff on the left is *Player*1's payoff and right *Player*2′s.

We use strategy method in our design. Subjects will be asked to make decisions as both proposers and respondents before they see the allocation. They will encounter the following two questions:

1. If you are chosen as the proposer, you need to make the decision of how you want to allocate the \$8.00 between you and your partner and wait until your partner to make a decision of either to accept the offer or reject it. Please enter "How much money do you want to offer your partner?"

2. If you are chosen as the respondent, you will need to check the offer your partner sends you. What is the minimum amount of money you will need to accept this offer (that means, if the provider offers less than this money, you will reject the offer)? Please enter the minimum acceptable offer, that is, the minimum amount of money that has to be offered for you to accept it (for example, 0 means you will accept any offers more than or equal to 0 that is offered to you)

The system then decides who will be the proposer and respondent and exercise their decision on their behalf respective to their answers. After this phase, the experiment ends.

In the treatment session, all settings will be the same as the control session, but we introduce an ice-breaker phase after all subjects are paired up. The ice-breaker phase contains a chat room. It is a strategy that has been done in many studies. The innovation in our experiment design is that we measure subject's attachment style immediately after the chat room. Subjects need to stay in the room for at least 10 minutes before they proceed to the next stage and they can chat with their partner to get familiar with each other. Subjects

cannot skip this phase unless they chose not to chat at all and only wait for the time to pass. This phase is aiming to increase the intimacy level in the lab experiment to see if the attachment theory can survive in the environment other than all strangers with no interactions. However, it is still important to notice that our paper, similar to Almakias and Weiss, is still working around the lower bound of the attachment theory, for the experimental environment may still need further enhancement on the intimacy level. After this phase, the experiment enters into the ECR questionnaire, and the rest of the experiment is the same as the control session.

Hypothesis

From section 2, we know that secure attachment style is cooperative and trusting, and anxious attachment style is preoccupied with close relationships and needs constant reassurance to trust other people. They sometimes even need to test other people to see if they can be trusted. Thus, secure type are willing to trust, and anxious type are willing to trust given they are comfortable and their demands for closeness can be met. Avoidant attachment style, however, is afraid of intimacy and usually try to avoid being too attached or close to other people, thus, tend to have mixed feelings in trust. They are longing for closeness but escaping from closeness as well.

Therefore, we can safely assume that attachment anxious level will increases ones willingness to appeal others, whereas attachment avoidance level can form personalities that care less about others and focus more on their own benefits. Thus, I make the following hypothesis.

(i) Hypothesis 1:

   (a) Attachment anxiety level correlates positively with the cooperating rate and attachment avoidance level correlates negatively with the cooperating rate in prisoner's dilemma;

(b) Attachment anxiety level correlates positively with the sending rate and attachment avoidance level correlates negatively with the sending rate in simple trust game;

(c) Attachment anxiety correlates positively with offers on the proposer's side and attachment avoidance correlates negatively with offers on the proposer's side in ultimatum game;

(d) Attachment anxiety correlates negatively with minimum amount of money required to accept an offer on the respondent's side and attachment avoidance correlates positively with minimum amount of money required to accept an offer on the respondent's side in ultimatum game;

(ii) Hypothesis 2:

(a) Having chat increases the attachment intimacy and therefore increases the cooperating rate in prisoner's dilemma;

(b) Having chat increases the intimacy, which increases chance of sending money when being the first mover in simple trust game;

(c) Having chat increases the intimacy, which increases the offered money and decreases the minimum accepted money in ultimatum game

General Results

The subjects who participated in the experiments are undergraduate students from UC Irvine's ESSL subjects pool. They self-selected into the experiment. There were total 64 subjects with 32 in the control session and 32 in the treatment session. The average earnings for subjects in the control session is \$7.70 excluding the \$7 show-up fee, and \$10.57 for subjects in the treatment session excluding the show-up fee.

In the control session, 50% of the subjects chose to cooperate in the prisoner's dilemma game. About 76.92% of the subjects chose to send or send back money when giving the

chance, where 6 subjects cannot make any choice since their partners did not send them any money for them to send back. In the ultimatum game, the average offered amount is approximately $3.53, with 18 subjects chose to offer $4 which is the mode of the offered amount. For the minimum acceptable offer, however, the average is only $2.86, with 14 subjects required at least $4 to accept the offer, which is the mode of the minimum acceptable offer.

In the treatment session, subjects need to spend at least 10 minutes in the chat room. They could not skip this part for the next button to proceed to the following page will not show up if they don't stay long enough. As a result, this feature pushed everyone in the treatment session to use the chat function, though some talked more and some less. The average time they spent in the chat room is about 10 minutes, which means almost all subjects choose to click next button when it appeared with a few exceptions due to internet connection issues or late show-up. It is understandable since most subjects probably did not come to the experiment with the intention of making friends. As indicated from figure 5, 81.25% of the subjects chose to cooperate in the prisoner's dilemma game. Approximately 96.77% (30 out of 31) of subjects chose to send or send back money when given the chance, with 1 subject had no choice because her partner did not send her any money. It is interesting to point out that both players in this pair chose to cooperate in the prisoner's dilemma game, so that the first mover in the simple trust game did not choose to send $0 for reciprocity reason. Based on these results, figure 1.5 suggests hypothesis 2a and 2b. In the ultimatum game, the average offered amount is approximately $3.76, which is slightly higher than the control session and the average minimum acceptable amount is $2.31 which is lower than the control session. Figure 1.6 shows the comparison visually and suggests Hypothesis 2c. Thus, it is within our expectation to see this trend because we predicted that the ice-breaker phase should amplify the attachment styles' behaviors. However, it turns out that the effect is not statistically significant. The regression results are shown below.

Regression Analysis

Both anxiety and avoidance level are continuous variables between 1 and 7. We use regression analysis to test the effects of these two variables on cooperation rate in prisoner's dilemma, offered monetary amount and accepted monetary amount in ultimatum game. Notice that it will be better if we can categorize subjects perfectly into the four attachment style. However, there is no clear standard based on the ECR questionnaire because it is harder to set a hard limit on anxiety and avoidance scores to classify someone's attachment style. From Figure 1.1, we can see that it is easy to tell one's attachment style once he or she has an extreme score on one or both scales, but much harder to classify them if their scores are around 4 points. For example, one subject has anxiety score 3.96 points and avoidance score 4.12 points, which is pretty close to secure type or at least around that origin area in the 2-dimensional attachment scale. However, if we choose (4,4) as a strict cut-off point, then the subject needs to be placed as avoidant style, which may not be the case.

Therefore, instead of categorizing them into specific attachment style, we use the anxiety and avoidance level only to see the effects. One possible risk we are taking now is that, the secure type also has certain level of anxiety and avoidance, but they may not affect them as much. Nevertheless, until we have a perfect cut-off point for categorization, the anxiety and avoidance level are the better option at this point.

We thus present the regression tables below, where *avganxiety* and *avgavoid* stand for average anxiety level and average avoidance level respectively. *dilemmachoice* is a dummy variable with 1 indicating subject chooses to cooperate in the prisoner's dilemma game and 0 otherwise. $defected$ is a dummy variable indicating that the subject is betrayed by her partner in either prisoner's dilemma or simple trust game, or both. Being betrayed is defined as the subject chose to cooperate yet her partner chose to defect, and her partner did not send her any money in simple trust game. We also include $defectedpd$ as an additional term to capture reciprocity in the prisoner's dilemma game only to see its effect on subject's behaviors in simple trust game. In addition, *sendorno* stands for subjects' behavior in simple

trust game. They either send the money when given the chance where *sendorno* equals to 1, and 0 otherwise. In the ultimatum game, $offer$ stands for how much money subject chooses to allocate to her partner when being asked as a proposer and *minaccept* is the minimum amount of money required for subject to accept the offer when being asked as a respondent. Lastly, chat is a dummy variable distinguishing control session and treatment session when we combined the data to capture the treatment effect.

Table 1.1 presents the result for the control session. We first run the Probit analysis to examine the effect of subject's anxiety level and avoidance level to their behavior in prisoner's dilemma and simple trust game. As can be seen that the higher the anxious level, the more likely one wants to cooperate and the higher the avoidance level, the less likely one wants to cooperate. The trend is correct and follows hypothesis 1a, but it is not statistically significant at even 10% significance level. One possible explanation is that prisoner's dilemma game may be well-known to subjects, and subjects are making decisions based on the dominant strategy regardless their attachment style's impact. In the simple trust game phase, we have the similar finding which follows hypothesis 1b, but it is not statistically significant at 10% significance level. However, it is important to point out that being defected in the prisoner's dilemma phase significantly lowers the chance one sends money to her partner, so we can safely assume that the reciprocity effect is so strong that attachment style could not affect subject's behavior as much.

We then run the linear regression analysis to look at the effect of anxiety and avoidance level on the monetary offer subjects are willing to make in the ultimatum game. We introduce defected variable to indicates whether they are defected in the previous two games to capture the reciprocity. Although we did not present the monetary payoff at the end of each game, subjects can still see their partners' choices. Thus, it is important for us to include this variable or else it may disturb the effect of the anxious and avoidant level. We also include the *minaccept* variable because people usually make offer based on how much they will accept minimally, and it should be reflected on their offers. The result is consistent with Hypothesis

1c at 5% significance level. For avoidance level, the effect is even stronger that the result is consistent with hypothesis 1c at 1% significant level. Lastly, we regress minimum amount of money subject is willing to take to accept offer on their anxious and avoidance level. We include $offer$ variable here for the same reason we include *minaccept* variable when we examine $offer$. The result is consistent with Hypothesis 1d at 5% significance level. For avoidance level, we correctly predicted the sign, which is the higher your avoidance level, the more you require in the ultimatum game when being a respondent, but the result is not statistically significant.

Table 1.2 presents the average treatment effect of adding the treatment of chat for prisoner's dilemma. We first combined data from control and treatment session, with a new variable *chat* to distinguish them. When *chat* equals 1, it means the subject received the treatment which is the ice-breaker phase, and 0 otherwise. We see that chat has significant impact on the cooperating rate in prisoner's dilemma. The variable names are still the same with 1 at the end stands for receiving treatment. Equation 1 shows that the average treatment effect of chat is 0.307 in the prisoner's dilemma, which is consistent with 2a at 1% significance level. Table 3 presents the average treatment effect of chat for simple trust game. Similar to the details in Table 1, the average treatment effect on the sending rate is 0.145 in the simple trust game, which is also consistent with our expectation, and it follows Hypothesis 2b at 1% significance level. Note that we only have 57 subjects here because we dropped 7 subjects from both sessions who did not have any choice but to send \$0 back to their partners because their partner did not send them anything in the first move.

However, we need to notice that once subjects receive the treatment, the effect of their anxiety and avoidance level can have mixed signs comparing with the baseline session, which goes against with our expectation because we predicted that the impact of attachment styles should be amplified. For example, from regression equation of subjects who received the treatment (equation 2) in Table 1.3, we can see that average anxiety level will decrease the chance of sending money to one's partner and avoidance level increases it, which is

inconsistent with Alternative 3 and 4. We may be able to explain it through attachment theory. In details, although anxious and avoidant level have their distinctive behaviors, they also have some similarities. Anxious style can be withdrawing and dismissing when they feel insecure and need to test their friends and family by being a bit dismissing at certain time, which is called protest/defensive behaviors. In addition, avoidant style does not mean they always want to avoid intimacy. They enjoy it in the short run but avoid the long run commitment to it.(Levine & Heller, 2012) Therefore, it is possible to see such behaviors especially the subjects just established their intimacy/friendship at the bare minimum level.

Table 1.4 and 1.5 presents the average treatment effect for offer and minimum required money in ultimatum game and suggests hypothesis 2c. From the tables, we can see that the average treatment effect is consistent with what we expected in hypothesis 2c. Receiving treatment does increase subjects' offer amount and lower their requirement amount. Yet the treatment effect is not statistically significant for these two variables. We also realize that from the comparison between control group and treatment group, the effect of their attachment style decreases in the treatment group for ultimatum game specifically. For example, equation 2 from table 1.5 indicates that one unit increase in the average avoidance level will increase subject's required offer by only \$0.082 in the treatment group, which is less than the \$0.399 required in the control group showed in Table 1.1. There are also mixed signs once subjects received treatment such as the $offer$ variable. We think that these could be attribute to the same reason that explained the mixed signs in table 1.3.

Discussion

The baseline regression results proved that attachment anxious and avoidance level have impacts on economics behaviors, specifically in the games we tested above. However, the treatment effect is not statistically significant for ultimatum game when we introduced the ice-breaker phase, although we found the expected trends. We first tried to replicate

Almakias and Weiss's result in a somewhat different setting to see if the results are still valid. We successfully did so in the control session. We then hoped to increase the intimacy level and see whether the attachment style has amplifying impact, but we found out mixed effects, and some are statistically significant and some are not. It is possible that the ice-breaker phase kind of created the bare minimal level of intimacy and instead of triggering their attachment style's typical reaction, subjects may behave in defensive behaviors or enjoying the intimacy since they just started knowing each other.

In addition, some improvements could be made by introducing friends subjects into the experiment. Attachment theory, after all, is examining the close relationship. However, due to the current global pandemic situation, it is difficult to communicate with subjects and not very easy to recruit friends subjects especially when everything is online. Although we tried to increase the intimacy level, it is not an easy task to force people to become close with each other within a short period of time, let alone in a virtual lab where they basically had zero human interaction. Secondly, the experiment can be improved by introducing more simple tasks into it, and without showing their partners' choices. For example, we had to introduce $defected$ variable because the subjects in our experiment could see each other's actions and choices, which could cause reciprocity problem. If we minimize the spillover effects by hiding subjects' actions until the end, we can isolate the effects of attachment anxious and avoidance level as much as possible. In addition, we can also keep the same pair to do the same task for multiple rounds to increase the intimacy. Note that Almakias and Weiss did the similar settings but the pairs in their experiment will re-match in each new round. Lastly, we think that dividing the subjects into the designated attachment style type may explain more of their behaviors in the experiment, but we have not yet found out a good cut-off point to categorize them, which could be done by better experimental designs in the future.

Conclusion

In conclusion, unlike Almakias and Weiss, we achieve to move beyond lower bond of the impact of attachment theory in simple games, little by little. Although we know the ideal situation is to randomly select human subjects who have deep bonds already, and we can directly observe their behaviors in coordination games to test the attachment theory with the closest setting the theory proposed, we believe that our paper still sheds light based on the current experimental limitations.

Our paper examines the impact of attachment style on individuals' economics behavior in experimental games. What we found in the baseline group proves that attachment anxious level tends to have more friendly in the prisoner's dilemma, simple trust game, and particularly ultimatum game. Higher anxious level correlates positively with the money one is willing to offer to their partners and negatively with the minimum money required for them to accept the offer. Higher avoidance level, however, correlates negatively with the money offering to one's partner and positively with the minimum required amount. It resonates with the psychology literature since attachment anxious type tend to be more insecure of their relationship, thus can be more eager to appeal their partners to leave a good image. Attachment avoidant type, however, tends to be a bit more self-focused and avoiding intimacy, which causes their behaviors to be more prone to selfishness. We also found out that ice-breaker phase does increase the intimacy which is what we wanted and predicted to see, yet the effect can be mixed. Furthermore, our paper tested the attachment theory in more economics games, which expanded Almakias and Weiss' work. Our paper proves that attachment theory can survive in a more complicated economics situation instead of a repeated single game.

We also think the paper can be extended to multiple other directions. First we can increase the intimacy level by introducing friends into the experiment to see how attachment styles react in a more intimate environment.In addition, we can cut down the intimacy level and test how attachment theory work in a group identity setting. For example, Chen and

Li tested the envy and charity feelings towards in-group and out-group members (Y. Chen & Li, 2009). Future research can incorporate attachment theory into their design to see how different attachment style can affect group identification and group attachment. In addition, we can build up the intimacy level to set up an experimental environment that allow attachment theory to work, where we need more close subjects such as friends. Further, we can keep cutting down the intimacy level to test the implication of attachment theory in social context to see how they contribute in public goods game, which of course, needs careful experimental design to keep certain level of intimacy but not too much. We also hope to introduce attachment theory into other economics fields such as political economics to see whether different attachment style can affect voting behaviors. In fact, attachment theory should not be limited in coordination games alone, due to the important social meanings of trust and cooperation. Lastly, we propose to incorporate attachment theory into Neuroeconomics. Neuroscience that focuses on human attachment tries to explain emotions and behaviors through human attachment by studying brain functions, and economics choice is also subject to brain functions (Coan, 2008; Fehr & Rangel, 2011). Van Rooij and Van Orden showed an example of using brain scanning to explain how subjects' brains react to unfairness in ultimatum games, which we think is a huge step to study economics, and can be introduced to study attachments' impact in economics choice (Van Rooij & Van Orden, 2011). We believe all of these explorations are worth studying and they're left to the future research.

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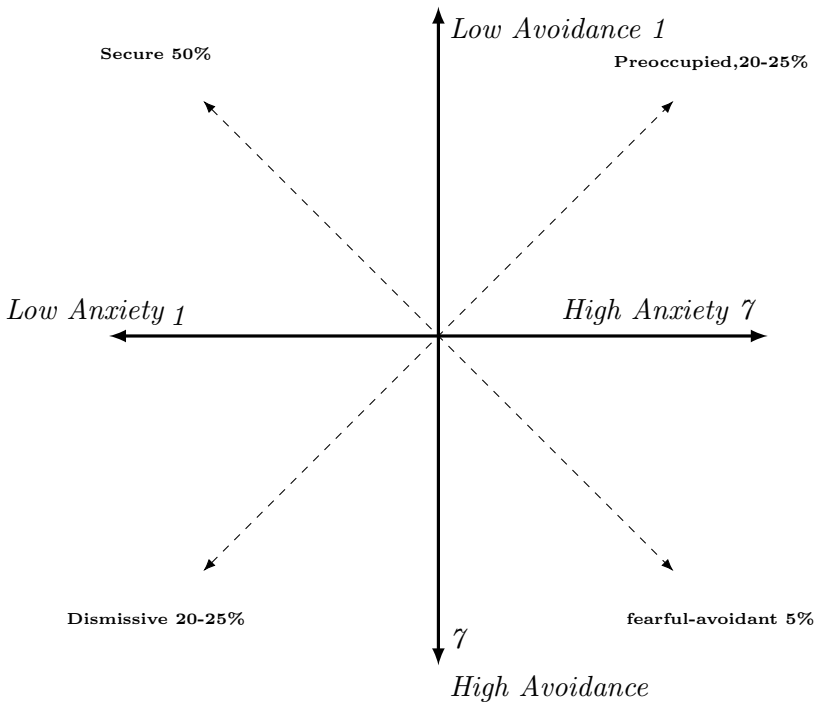

Figure 1.1. Four Attachment Styles (Note: The origin is (4,4))

		Player 2	
		$Cooperate$	$Defect$
Player 1	$Cooperate$	$5, 5$	$0, 8$
	$Defect$	$8, 0$	$2, 2$

Figure 1.2. Modified Version of Prisoner's Dilemma

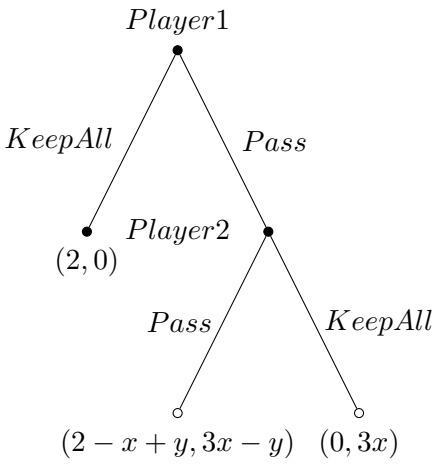

Figure 1.3. Trust Game

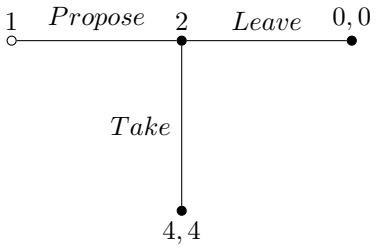

Figure 1.4. Ultimatum Game

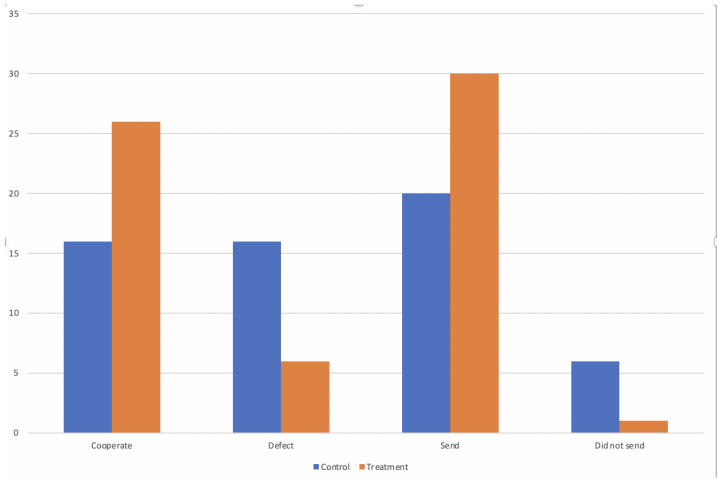

Figure 1.5. Comparison of Cooperation Rate in Prisoner's Dilemma and Sending Rate in Simple Trust Game

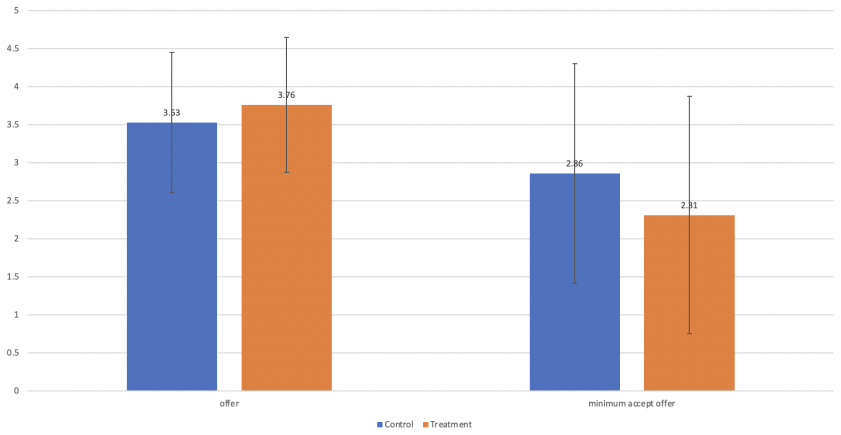

Figure 1.6. Comparison of Total Monetary Offers/Minimum Accepted Offer in Ultimatum Game

VARIABLES	(1) dilemmachoice	(2) sendorno	(3) offer	(4) minaccept
avganxiety	0.309	0.296	0.307**	-0.461**
	(0.240)	(0.340)	(0.116)	(0.213)
avgavoid	-0.0586	-0.0560	-0.426***	0.399
	(0.272)	(0.433)	(0.141)	(0.279)
minaccept			0.379***	
			(0.0803)	
defected			-0.263	0.285
			(0.243)	(0.437)
defectedpd		-1.360**		
		(0.637)		
offer				1.192***
				(0.253)
Constant	-0.861	0.472	3.107***	-1.377
	(0.977)	(1.469)	(0.597)	(1.476)
Observations	32	26	32	32
R-squared			0.599	0.482

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

Table 1.1. Result for Control Session

VARIABLES	(1) ATE	(2) dilemmachoice1
avganxiety		-0.461*
		(0.263)
avgavoid		-0.156
		(0.348)
chat	0.307***	
	(0.112)	
Constant		3.344**
		(1.354)
Observations	64	64

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.2. Average Treatment Effect(ATE) of Chat on Prisoner's Dilemma

VARIABLES	(1) ATE	(2) sendorno1
avganxiety		-0.482***
		(0.171)
avgavoid		0.396**
		(0.184)
defectedpd		2.247***
		(0.651)
chat	0.145**	
	(0.0724)	
Constant		2.376***
		(0.617)
Observations	57	57

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.3. Average Treatment Effect(ATE) of Chat on Simple Trust Game

VARIABLES	(1) ATE	(2) offer1
avganxiety		-0.202
		(0.146)
avgavoid		-0.0231
		(0.140)
minaccept		0.0507
		(0.0870)
defected		0.132
		(0.593)
chat	0.265	
	(0.214)	
Constant		4.464***
		(0.474)
Observations	64	64

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.4. Average Treatment Effect(ATE) of Chat on Offers in Ultimatum Game

VARIABLES	(1) ATE	(2) minaccept 1
avganxiety		-0.0414
		(0.280)
avgavoid		0.0816
		(0.398)
offer		0.170
		(0.294)
defected		-0.354
		(0.797)
chat	-0.599	
	(0.386)	
Constant		1.580
		(1.744)
Observations	64	64

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.5. Average Treatment Effect(ATE) of Chat on Minimum Required Money in Ultimatum Game

CHAPTER 2. IT IS TIME TO EXPUNGE MARIJUANA-RELATED RECORDS: A LABORATORY EXPERIMENTAL STUDY OF THE IMPACTS OF MARIJUANA POSSESSION-RELATED MISDEMEANOR RECORD ON EMPLOYMENT OUTCOMES

Introduction

In early 2021, New York, along with three other U.S. states, enacted the legalization of marijuana and the automatic expungement of a broad array of past marijuana convictions. The main goal in expunging these criminal records is to offer ex-offenders a fresh start in the labor market to reduce criminal recidivism (Schlussel, 2021). Employment is consistently cited as one of the most critical components for desistance by ex-offenders and deserves more attention (Bahr, Harris, Fisher, & Harker Armstrong, 2010; Denver, Siwach, & Bushway, 2017; Leasure, 2019). To date, only eighteen states have legalized the use of marijuana, and many still require a petition be filed to expunge marijuana-related records. Some studies suggest justice reform and advocate for the en-masse expunging of marijuana-related criminal records. However, there is a lack of sufficient supportive data demonstrating the need to automatically expunge low-level marijuana-related criminal records (A. E. Rosen, 2019). Thus, this paper addresses several questions: (1) whether employers discriminate against job applicants with marijuana-related misdemeanor records; (2) whether expungement helps job applicants improve their employment outcomes; and (3) whether race and gender play a role in the hiring processes when applicants have marijuana-related records.

The present study focuses on the expungement of marijuana-related misdemeanor records for two reasons. First, there have been increasing concerns expressed by the general public about the punitive treatment of less serious criminal offenders, given the trend of the legalization of recreational use of marijuana. These concerns have led officials in nearly every state to discuss new laws related to marijuana (Berman, 2018). It is thus important to study how

these changes, particularly those related to expungement, impact ex-offenders' employment outcomes. I focus on misdemeanors because most studies that find discrimination against job applicants with criminal records during the job search and hiring processes do not differentiate between a felony or misdemeanor record (Denver, Pickett, & Bushway, 2018; Agan & Starr, 2018). Despite the obvious importance of these findings of discrimination, it is crucial for researchers to recognize that most criminal cases concern misdemeanors rather than felonies. In addition, although drug-possession-related crimes are usually considered low-level crimes, arrests for these have increased every year since 2015 (data from the FBI website).

Despite the legalization of the recreational use of marijuana in eighteen states over the last decade, it is still illegal to possess an excessive amount. For example, California legalized recreational use in 2018 but possessing more than 28.5 grams of marijuana is still illegal and considered a misdemeanor. Once arrested for possession that crosses this threshold, the penalty is up to six months of jail time and a $500 fine. Similar laws vary by state, but most states consider possession of marijuana beyond a certain limit to be a misdemeanor. Nationwide, many drug arrests are still related to marijuana possession. Based on data disclosed by the FBI's Uniform Crime Reporting (UCR) program, 40% of drug arrests are marijuana-related, and in 2018, 92% of these arrests were for possession-related offenses. Thus, it is crucial to pay attention to misdemeanor records. Second, expunging felony records is more complicated, and automatic expungement may not be feasible for marijuana-related felonies.

There are limited studies exploring the impact on employment access of having a misdemeanor record. Uggen et al. (2014) and Leasure (2019), in particular, touch on the economic perspective, focusing on labor-market outcomes. Uggen et al. (2014) find that low-level misdemeanor records have statistically non-significant negative effects on hiring outcomes and Leasure (2019) find that a misdemeanor conviction significantly hinders hiring outcomes,

regardless of the job applicant's racial background. These two studies enriched the literature and are the foundation for this paper. Both studies answer several essential questions but with mixed results. First, Uggen et al.'s (2014) findings suggest that job applicants with misdemeanors should not be too concerned because the negative effect on employment outcomes of having a misdemeanor is statistically small.

However, the findings of Leasure (2019) suggest that the negative effect of a misdemeanor on job applicants' employment outcomes can be quite significant. The present paper is motivated by this disparity because if having a misdemeanor record has no negative impacts on employment, as Uggen et al. (2014) suggest, then it is unnecessary to expunge the records from an economics perspective. Second, both studies are field experiments with fictitious job applicants. Uggen et al. conduct an audit study, whereas Leasure (2019) use fictitious resumes for online job applications. Both methods are suboptimal for the following reasons: (1) Currently, most advertisements for entry-level jobs are posted online and do not allow in-person application, so audit studies limit the data collection pools; (2) the Covid-19 pandemic has resulted in job-search and hiring processes being more complex and time-consuming, and it may take longer than usual to collect data through field experiments; and (3) there is insufficient data to motivate additional states to enact legislation automatically expunging marijuana-related records and data collection through field experiments usually takes longer compared to laboratory experiments. To avoid these problems, I collected data through laboratory experiment, which allow the collection of large quantities of data in a short period and the easy modification of the independent variable.

Literature Review

Employment is a key factor preventing recidivism (Heinrich, 2000); it is also the primary source of legitimate income for ex-offenders and directly associated with housing and food security(Bushway & Apel, 2012; LeBel, Burnett, Maruna, & Bushway, 2008; Bucklen &

Zajac, 2009). In addition, public support for expungement is high for persons convicted of substance-related offenses, who "signal" their reform through stable employment (Burton, Cullen, Pickett, Burton Jr, & Thielo, 2021). However, many studies find that having a criminal record significantly lowers the chances of getting a job (Denver et al., 2018; Agan & Starr, 2017; Pager, Western, & Sugie, 2009; Solomon, 2012; Western, 2008). Previous research studies show that employers are reluctant to hire ex-offenders because they are worried they will revert to criminal behaviors (Flake, 2015). The dilemma is that the general public supports expungement when reform is signaled through employment, but ex-offenders face difficulties finding jobs without expungement of their records. How to leave their criminal background in the past and start a new life with a job becomes one of the most important challenges for ex-offenders. Those with misdemeanor convictions are no exception. The punishment for their crimes may be light, but the effect on employment outcomes of having a misdemeanor record can be as severe as those for having a felony record. As Jacobs points out in *The Eternal Criminal Record*, it has become increasingly difficult to escape the mark of a criminal record (Jacobs, 2015).

In most states in the U.S., misdemeanor records follow ex-offenders their entire lives and rarely get expunged. In California, background-check agencies are permitted to disclose crimes up to seven years after conviction. For example, if a college undergraduate is arrested and convicted at the age of 22 for excessive possession of marijuana in California (i.e., over one ounce), their record will follow them for the remainder of their lives, and a background check will disclose this record until they are 29 years old. If they are only arrested and not yet convicted, the background check can disclose their arrest record for as long as the trial is ongoing. The trial can go on for months or, in some cases, a year or two. If they plead not guilty, this will prolong the process, and the arrest record will show up until the case is resolved. To expunge this misdemeanor record, the individual would need to file a petition and go through numerous other processes. However, with automatic expungement, this college undergraduate can pay the necessary fines and live their life without further

concern about the record. However, it is worth asking whether there exists discrimination in relation to marijuana-related misdemeanors. The automatic expungement of marijuana-related misdemeanors is only meaningful if it can mitigate any negative effects. Without negative impacts on employment, marijuana-related misdemeanor records may not need to be automatically expunged.

Researchers who focus on misdemeanors have mixed results. Uggen et al. (2014) focus on drug-related arrests using an audit experiment. Subjects were paired by race, and one subject per pair was assigned to the treatment condition each week. The treatment condition was a sole arrest for disorderly conduct, with no resulting charge or conviction. Each week, the subjects applied for jobs in person and tried to have conversations with hiring authorities or other employers. If they could not meet with the employers directly, they maximized their in-person contact by asking to speak to a manager. Most of the communication on the subject's end was scripted, such as asking questions about the pay and expressing their interest in the job. If and when treatment subjects were asked for more information by employers, they also divulged their criminal history and indicated that they had never been convicted. As is typical of such studies, the focus is the callback rate. The results suggest that although misdemeanor arrests lower applicants' callback rates, the effect is minimal. For both white and black Americans, the difference between the callback rate of applicants with clean records and applicants who have been arrested for misdemeanors is only 4%. In this case, automatic expungement seems unnecessary.

However, Leasure (2019) argues that the audit method limits the pool of employers considered because job openings are now increasingly posted online. In-person applications are rare. Uggen et al. (2014) also focus on individuals who had been arrested but not convicted; also, being convicted could have a different effect on employers' decisions. Leasure (2019) pays attention to those convicted of drug-related misdemeanors and conducts a correspondence experiment, sending 1,000 fictitious resumes to employers in Columbus, Ohio, through

online job application sites such as Craigslist. The data in the project are a sub-sample of the 1,000 resumes sent. The control group comprised applicants with clean records, and the treatment group included fictitious job applicants with drug-related convictions; other characteristics were controlled. The treatment groups were further divided into two sub-groups, one with applicants with felonies and the other with misdemeanor drug convictions. All details were included on the resume so that employers did not have to run a background check. Similar to Uggen et al. (2014), Leasure (2019) further study the racial disparity in results. The correspondence study focuses on callback rates, and the results suggest that a misdemeanor record significantly hinders an applicant's chances of receiving a callback; indeed, applicants with misdemeanor records had a callback rate 13% lower than that of applicants with clean records. This finding supports automatic expungement. Interestingly, Leasure (2019) does not find any significant difference between white and black applicants with or without criminal records, contrary to the findings of racial disparity in many previous studies.

Uggen et al. (2014) and Leasure (2019) may be the only studies that primarily focus on misdemeanors from an economic perspective. The difference between their results is thus intriguing and requires further investigation. Furthermore, neither study focuses on expunging misdemeanor records. Thus, the present paper contributes the following: First, I focus on marijuana-possession-related misdemeanors to offer a new perspective on the automatic expungement of marijuana-related misdemeanor records from the perspective of employment outcomes. Second, this is a study of both misdemeanor-conviction and arrest records rather than only one or the other in order to check whether there are differences in discriminatory attitudes towards them. Third, I incorporate both racial and gender differences when studying the effects of a misdemeanor record on job applicants' employment outcomes. In 1980, women comprised only 14% of misdemeanor arrests; this number jumped to 25% in 2016 (Public Policy Institute of California Data). More women are arrested for misdemeanors, and the employment access of these female ex-offenders deserves more attention. Finally,

the present study takes advantage of previous studies and uses a 10-point-scale evaluation that was designed in response to Heckman's (1993; 1998) critique, discussed below.

Experimental Methodology

Vignettes have long been used in laboratory experiments to study gender discrimination in the labor market. They are also used in other studies, such as those focusing on ethnic and age discrimination and discrimination on the basis of physical appearance (B. Rosen & Jerdee, 1974, 1977). When studying discrimination in hiring processes using vignettes, researchers usually change the variable of interest – for example, gender is used in gender discrimination studies – and use the correspondence method to check whether employers or laboratory subjects decide to hire or promote based on the characteristics reflected in fictitious resumes.

Heckman (1998) and Heckman and Siegelman (1993) challenge the validity of such correspondence studies and offer several criticisms. They argue that discrimination can be identified through experiments, but these cannot differentiate between taste-based or statistical discrimination. They are also concerned about variance in the unobserved productivity variables, which can result in biased estimates of discrimination (Heckman, 1998; Heckman & Siegelman, 1993). Neumark (2012) further elaborate this second point and propose using a heteroskedastic probit model. In light of Heckman's critique, Kübler, Schmid, and Stüber (2018) offer a 10-point-scale experimental design in their gender-discrimination study. They argue that instead of using a binary invitation decision (for example, hire or not hire, callback or no callback), using a 10-point evaluation scale allows for robust results, as long as the scale covers all the evaluations the employers might wish to make (Kübler et al., 2018). Rather than measuring callback rates, they asked employers to express, on a scale of 1 to 10, how likely they were to select the fictitious resumes for the next round of interviews.

A brief summary of their argument follows. The notations are changed to criminal background because of this study's focus. Criminal background is denoted by $c \in \{1, 0\}$ where $c = 1$ for applicants with criminal backgrounds and $c = 0$ otherwise, the firm is denoted by f, and individual productivity X of an applicant consists of X_O, the observable characteristics, and X_U, the unobservable productivity characteristics, such that $X = (X_O, X_U)$, where X_U is normally distributed and $E(X_U^{c=1}|X_O^S) = E(X_U^{c=0}|X_O^S) = 0$ by assumption. By assumption, the unobservable productivity characteristics have different standard deviations, that is $\sigma_U^{c=1} \neq \sigma_U^{c=0}$. The productivity of an applicant is defined as $P = P(X, f, c) = P(X, f) = X_O\beta + X_U + f$, assuming criminal background does not influence productivity and its linearity in the different productivity components. Define the treatment T of a person with criminal background c and productivity P applying at firm f as $T(P(X, f), c)$. The discrimination is defined as $T(P(X, f), c = 1) \neq T(P(X, f), c = 0)$.

In correspondence studies, researchers are interested in $T = 1$, applicants receive a callback, or $T = 0$, they do not. Assume that the 10-point scale captures all evaluations the employers might want to make. The treatment is linear in productivity and $T(P(X, f), c = j) = X_O^S\beta + X_U^{c=j} + f$ for $j \in \{0, 1\}$, assuming there is no discrimination against applicants with criminal records. The conditional mean is $E(T(P(X, f), c = 1|X_O^S) = X_O^S\beta + f$, and $E(T(P(X, f), c = 0|X_O^S) = X_O^S\beta + f$. Thus, $E(T(P(X, f), c = 1|X_O^S) = E(T(P(X, f), c = 0|X_O^S)$ even if $\sigma_U^{c=1} \neq \sigma_U^{c=0}$. For details, see Kübler et al.(2018). Changing the characteristics of applicants should fulfill the assumption. However, it is not feasible to use this method in a field experiment because researchers cannot easily send out numerous different resumes and ask employers to rate these on a 10-point scale. I take advantage of the design of Kübler et al. (2018) and bring it into a laboratory setting, where it is easy to simultaneously present applicants with different characteristics and ask subjects to rate them.

Experimental Design

The experiment uses oTree software (Chen, Schonger, & Wickens, 2016). The study's subjects were UC Irvine undergraduate students from the UCI Experimental Social Science Laboratory. The experiment's show-up payment was $7, and subjects earned additional money during the experiment. The experiment proceeded as follows: first, subjects reviewed 21 fictitious resumes for an entry-level position as a customer service representative. They knew the resume format was standardized and that the applicants had already passed initial screening. This was done to guide the subjects to not focus on the job experience reflected on the resumes. The detailed instructions to subjects are set out in the Appendix. After reviewing the resumes, subjects rated the likelihood of each applicant being hired on a 10-point-scale (1 = not likely to hire and 10 = very likely to hire). The subjects were also asked to leave brief comments on each resume to explain their rating decision.

Resumes are divided into seven groups based on their characteristics. In the control group (Control), the job applicants are white males. I chose white-sounding names to indicate race. Names were randomly chosen from the 2010 U.S. census data. This method has been used in many studies, including Bertrand and Mullainathan's study (2004). The resumes contained similar elements with minor modifications. For example, all applicants were high school graduates from different cities in Orange County, California, and all had entry-level, customer-service-related job experience, but in different fields or at different companies. The applicants were in their early 20s. All criminal records were recent (within than two years) because, in California, criminal convictions can only be reported for seven years unless another law requires employers to look deeper into an applicants' background. These minor changes were necessary because a 10-point evaluation assumes that the characteristics reflected in the resumes cover all aspects employers care about. The resume template follows Leasure (2019). The resumes in the first treatment group (Treatment 1) have all the features of the control group, but with an added misdemeanor record and contain the following

statement to signal this criminal history: "the job applicant has a misdemeanor record and allows us to run a background check on him/her." This conveys a message that the job applicants chose to self-disclose their criminal history, and this was noted by human resources staff (note that the experiment is scripted and there is no real human resource involvement).

It is important to note that employers cannot ask about applicants' criminal backgrounds according to California's 2018 "Ban the Box" policy. However, they can run background checks. for Human Resource Management (2012) finds that 90% of surveyed employers conducted background checks on some candidates and 70% conducted background checks on all candidates. Furthermore, using self-disclosed criminal history in the experimental design is justified for the following reasons. First, according to correspondence studies, it is not uncommon to self-disclose criminal history (Ahmed & Lång, 2017; Leasure, 2019). Previous studies show that self-disclosing criminal history is recommended to ex-offenders applying for jobs because they can control disclosure rather than waiting for their history to be disclosed in a background check(Harding, 2003; Myrick, 2013; Winnick & Bodkin, 2008). Thus, self-disclosure of criminal history is a realistic experimental setting.

Treatment 2 is nearly identical to Treatment 1, the only difference being the applicants' conviction status. The resumes, rather than stating the applicant has been convicted of a misdemeanor, state that they have been arrested but not convicted. Thus, Treatment 2 includes those with misdemeanor arrest records but not convictions. Treatment 3 has the same characteristic feature as Control but includes applicants with a different racial background. Treatment 3 contains resumes for black job applicants with clean records. Race is indicated by using black-sounding names. Names are borrowed from Bertrand and Mullainathan (2004). Treatment 4 is identical to Treatment 1, except that resumes are for black applicants who have been convicted of misdemeanors. Treatment 5 is identical to Control, except that resumes are for white female applicants. Treatment 6 includes the same characteristics as Treatment 1 but with white female applicants. Table 2.1 presents the main

characteristics of each group.

After rating all the resumes, subjects were asked to rank them from the best (1) to worst (21). Subjects did not have to explain their decisions in this phase. This different approach enforces the idea of there being competition between resumes. In the field experiments studying employment such as Leasure's study, applicants received callbacks if they met the requirements of the employers, and there is a cut-off point: if applicants are above the points required by the companies' hiring standard. Ranking does not provide such a clear cut-off point. However, the better the ranking, the more likely it is that applicants will move to the next round of interviews, which means they are closer to the cut-off point. Thus, ranking serves a similar purpose. In the rating task, the competition among applicants is not obvious, and the rating is based on individual resumes.

Additionally, I held a treatment session for the same resumes but with the expungement treatment included. In each of the groups of those with records – "convicted white male," "convicted white female," "convicted black male" – and the "arrest-only white male" group, one resume is randomly selected to have a clean record. I simply removed the self-disclosure of their criminal background to demonstrate the effect of expungement. In the treatment session, subjects performed the same tasks in relation to these modified resumes as with the other resumes. The goal was to study whether these modified resumes received better ratings after any criminal background was expunged. Thus, the experiment is divided into a control session and a treatment session.

One concern in this experiment is whether the quality of data is as good as that collected by field experiments. Thus, to safeguard the quality of the data, I implemented numerous steps to ensure that subjects understood their task to rate and rank these applicants as if it were part of a real hiring process. Another concern regarding the rating task was that, on average, some subjects might consistently award low scores and others consistently award high scores; for example, if subject A's average rating for all resumes is 5.8 and subject

B's average rating is 7.6. Ranking avoids this problem and provides a way to double-check the consistency of the rating results. After ranking all resumes, the subjects completed a short survey to disclose their own race and gender. Their demographic information made it possible to check whether they might have racial or gender preferences in relation to applicants that could compound or affect the impact of the applicant having a misdemeanor record.

There are some drawbacks and limitations to using lab experiments to study hiring decisions. As mentioned above, the quality of lab data and how realistic they are compared to field experiments are still under investigation. The experimental design lacks a connection between subjects' earnings and their ratings and rankings. However, laboratory experiments require less time to collect data and allow easy modification of the research design to study race, gender, and expungement simultaneously.

Hypothesis

Based on the experimental design, I propose the following hypothesis:

- H1(main): Misdemeanor records have no impact on one's resume rating/ranking
- H2(main): Being arrested only has no impact on one's resume rating/ranking
- H3(main): Expungement has no impact on one's resume rating
- H4: Applicant's race has no impact on resume rating/ranking
- H5: Applicant's gender has no impact on resume rating/ranking

Data

The control session included 99 subjects, and 30 subjects participated in the treatment session. An obvious outlier was defined as a subject who rated all resumes with the same score or left meaningless comments. After the exclusion of seven obvious outliers, the remaining 92 subjects in the control session generated 1,932 observations. Among the 92 subjects, 62 identified as Asian, 21 identified as Latino or Hispanic, and the remainder identified as white. Of the total, 64 subjects identified as female and the rest male. The average rating for all observations is 6.749. The average rating for observations with convicted status is 6.116. The scores represent how likely the applicants were to be hired based on the subjects' decisions.

Regression Analysis

The following tables present the results of the experiment. Table 2.2 presents the linear regression results when comparing resumes with different conviction statuses. Column 1 shows that, when pooling all the resumes, as long as a resume indicates that the applicant has a record (regardless of the conviction status), the rating is negatively affected at a 1% significance level. The result allows Hypothesis H1 to be rejected and demonstrates that having a misdemeanor record has a strong negative impact on employment outcomes. Thus, it is worth studying whether expungement also affects the ratings. The results also suggest that black applicants are discriminated against, and this result is significant at the 1% level. Female applicants receive slightly lower ratings, but this result is not statistically significant.

All the resume data for female and black applicants are then dropped to compare the impact of having a misdemeanor conviction on record compared to having only an arrest record. White male applicants with clean, arrest-only, or convicted status were kept in

the sample to control for race and gender. Column 2 suggests that being arrested without conviction lowers an applicant's ratings by 1.971 points, and being convicted lowers the rating by 1.652 points. Both results are significant at the 1% level. This shows that subjects do not differentiate between a conviction or an arrest when dealing with applicants with records. Although the results suggest that applicants with arrest-only records receive lower points than those with misdemeanors, the difference is small. Subjects tend to pay some attention to job experience and score accordingly, but the dominant impact still comes from the criminal record. Hypothesis H2 can thus be rejected, contradicting the results in Uggen et al. (2014). Uggen et al. (2014) find that being arrested has almost no effect on employment access. However, Uggen et al. (2014) use an audit study and increase contact with employers. For example, applicants are always asked to have a conversation with the hiring manager when they fill out the in-person application. Uggen et al. (2014) conclude that contact with hiring managers is significantly related to increased callbacks. However, applicants applying for jobs online do not have such close contact. Thus, the present experimental design is closer to an online job application process, which does not facilitate direct contact between employers and applicants. The results demonstrate that although marijuana is legalized in California, subjects still consider marijuana-related misdemeanors as a negative component in the job hiring processes. Thus, both Columns 1 and 2 suggest that marijuana-related misdemeanors have a negative impact on employment access, implying that automatic expungement should improve employment outcomes.

Table 2.3 presents the linear regression results with the inclusion of expungement. All resumes with clean records were dropped other than those for which records were expunged. The expungement process involves removing the record from an applicant's background. I removed the self-disclosure of applicants' criminal backgrounds to mimic this result. I then divided the control session into three groups consisting of around 30 participants and calculated the average rating for all resumes that included a criminal background. Because of limited funding, I only conducted one treatment session consisting of 30 participants.

I calculated the average ratings for all resumes with criminal backgrounds and those with expungement. Column 1 of Table 2.3 shows that the difference in the average scores between the control and treatment sessions are significantly affected by the expungement process. Without expungement, the difference between the two sessions for the resumes with criminal records is almost neglectable. With expungement of records, however, the ratings for the resumes increased by 1.173 points, and this result was statistically significant at the 1% level. Hypothesis H3 can thus be rejected, demonstrating that the same resumes perform much better in the ratings after expungement, which further supports the idea that automatic expungement should improve ex-offenders' employment outcomes.

Table 2.4 presents the linear regression results when comparing the resumes of applicants of different races. Black and white applicants with clean records or a conviction were kept in order to control for gender and conviction status. Both Columns 1 and 2 suggest that black applicants receive lower scores. The results show that the discriminatory attitudes are statistically significant at the 1% level. The interaction term of black applicants and a misdemeanor record in Column 2 suggests that black applicants receive ratings that are 0.246 points lower than those of white applicants with misdemeanor records. Thus, Hypothesis H4 can be rejected, which is in line with many previous research findings. It is important to pay attention to those with marijuana-related misdemeanors who are black because of the racial disparity in ratings (Vitiello, 2019). The results here also demonstrate that black applicants are double-penalized for having a marijuana-related record, and automatic expungement will improve their employment outcomes.

Table 2.5 presents the linear regression results for the comparison of resumes for applicants of different genders. Applicants' racial background was controlled, and only the resumes of white male and white female applicants with either clean or misdemeanor records were kept. Column 1 shows that female applicants received slightly lower ratings, with this result being significant at the 5% level. Thus, Hypothesis H5 can be rejected. Column 2

includes the interaction term of female applicants and misdemeanor records. As the results suggest, female applicants receive lower scores than male applicants with similar misdemeanor records, but the effect is not statistically significant at the 5% level. However, the results still suggest that female applicants face a similar double penalty as black applicants in the job market. Both gender and race play important roles when applicants have marijuana-related misdemeanor records. Thus, it is safe to conclude that automatic expungement will significantly improve ex-offenders' employment outcomes, particularly for racial minorities and women.

To summarize the results of the resume-rating process, contrary to Uggen et al.'s (2014) findings, having a record for a misdemeanor, regardless of conviction status, has a significant negative effect on employment access (expressed through the rating score). The results also suggest that race significantly impacts employment access, and gender has some impact. The results are largely consistent with Leasure (2019). Leasure (2019) finds that employers did not distinguish between applicants with records on the basis of the crime's severity. A misdemeanor record had the same negative impact on employment access as a felony record. These results resonate with my findings here that arrest and conviction have an almost equal negative effect on employment access. Furthermore, black and female applicants receive worse ratings than other applicants with misdemeanor records and are double penalized. The following tables present the regression results for the rankings.

Table 2.6 presents the linear regression results comparing the rankings of resumes with different conviction statuses. Column 1 shows that, as predicted, having a record, regardless of conviction status, significantly increases the ranking. Note that the higher the ranking, the worse subjects think the applicants are in terms of employability. Race and gender are then controlled to compare only the effects of a conviction and of an arrest only. Column 2 shows results consistent with the rating. Resumes with arrest records are ranked much worse than resumes with no records. The impact of only being arrested is almost the same

as having a misdemeanor conviction.

Table 2.7 presents the linear regression results when comparing the resumes of applicants of different races. Black and white applicants with clean records or a conviction are kept to control all other variables. All columns show that black applicants receive a slightly worse ranking than white applicants. Including the interaction term of black applicants and a misdemeanor record, the results in Column 2 suggest that among applicants with misdemeanor records, black applicants receive a ranking of 1.12 points worse than that of white applicants. The results correspond with the ratings, but having a criminal background still has a larger impact on a resume's ranking than race.

Table 2.8 presents the linear regression results comparing the resumes of applicants of different genders. Race was controlled, and only the resumes of white males and white females with either a clean or misdemeanor record were kept. The results in Columns 1 and 2 suggest that female applicants receive slightly better rankings, but the result is not statistically significant. Column 2 includes interaction terms of female applicants and misdemeanor. It suggests that among applicants with a misdemeanor record, female applicants receive a ranking of approximately 0.74 points higher than male applicants, but the effect is not statistically significant.

In summary, the rating and ranking results are evidence that misdemeanor records have a strong negative impact on employment access, which aligns with the findings of Leasure (2019) and contradicts those of Uggen et al. (2014). Moreover, being arrested without conviction has almost the same negative effect as being convicted for a misdemeanor. This is further evidence that the employment outcomes for ex-offenders with marijuana-related misdemeanors deserve more attention from researchers. In addition, race plays an important role. In general, black applicants receive worse ratings and rankings than applicants of other races, which is consistent with previous research findings, and black applicants with a misdemeanor record are double penalized. Finally, while there are clear gender differences in

the results, female applicants do not receive better ratings, although they do receive better rankings. On average, those applicants with a marijuana-possession-related record receive a score 1.6 points lower than other applicants in the resume ratings. Although there is tolerance of marijuana use, even after legalization for recreational use, related misdemeanors still produce negative effects. The results here provide support for the automatic expungement of marijuana-misdemeanor records. With expungement, resumes receive much better ratings, which demonstrates that expungement can improve ex-offenders' employment outcomes.

Conclusion

This results of this study reiterate the need to expunge marijuana-related misdemeanor records, particularly in regard to employment outcomes. They suggest that marijuana-related misdemeanors significantly hinder employment access, including of arrestees who have not been convicted. This shows that while the physical punishment for marijuana-related misdemeanors can be minor following legalization and decriminalization, the long-term negative effects of being arrested or convicted on such charges could potentially be huge if records are not expunged. As Leasure (2019) suggests, the findings "should be used to better inform prosecutorial charging decisions and judicial sentencing decisions." Moreover, policymakers must be informed of these negative outcomes and craft policies accordingly. There are several questions worthy of further discussion: is it possible to implement automatic expungement of marijuana-related misdemeanors in all states that have already legalized marijuana use; is it possible to not disclose marijuana-related misdemeanor records on background checks in the states that have not yet legalized marijuana use; should the government invest in career-path programs to better help those with marijuana-related misdemeanors combat the stigma they will face when looking for jobs in states that have not legalized marijuana use?

By conducting laboratory experiments, I had easy access to subjects and could easily

incorporate gender and race as factors in the experiment. For the most part, my findings here are consistent with those in previous studies. Race does have a statistically significant negative impact on employment access, and black applicants with marijuana-related misdemeanor records receive worse ratings and rankings, implying that they are double penalized. As some studies point out, in some states, racial disparity in arrest cases increased after the legalization of marijuana (Firth, Maher, Dilley, Darnell, & Lovrich, 2019). Thus, automatic expungement would significantly benefit black applicants with misdemeanor records entering the job market. The results pertaining to gender, with few other studies in the area for comparison, show that female applicants have an advantage in rankings but not in ratings. Both groups would benefit from the automatic expungement of marijuana-related records. Further investigation and different approaches are required to validate the results. It is worth noting that women with misdemeanor records do not hold many advantages compared to their male counterparts. As mentioned in the literature review, the number of women arrested and convicted for misdemeanors has been rising for the past three decades. Consequently, policymakers should develop gender-specific social reentry programs to better assist women with misdemeanor records in improving their employment outcomes.

Moreover, these results, which largely align with the findings of Leasure's field experiment, show that the quality of data collected through laboratory experiments is secured. Currently, those studying marijuana-related misdemeanors and their expungement do not have sufficient data, and there are not many studies available for comparison. The advantage of conducting laboratory experiments is that researchers can easily and quickly collect data and can modify the features of fictitious resumes relatively easily. Laboratory experiments allow the verification of the results of field experiments across different settings.

Finally, this study has several limitations that must be considered. First, the subjects are undergraduate students with no human-resources experience. Second, black and white are the only racial groups included. Third, I did not consider different types of crimes or jobs

but focused on entry-level customer service positions. Fourth, a stronger connection between subjects' risk attitudes and the task of rating and ranking resumes needs to be built into the experimental design. Future studies could extend the design by, for example, incorporating implicit association tests, including different racial groups such as Asians and Latinos in the resume pool, and examining whether the effects of having a misdemeanor record change when applying to different types of jobs. Future studies can address these limitations and extensions.

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Group	Race	Misdemeanor Record	Gender
Control	White	Clean Record	Male
Treatment 1	White	Convicted	Male
Treatment 2	White	Arrested yet not Convicted	Male
Treatment 3	Black	Clean Record	Male
Treatment 4	Black	Convicted	Male
Treatment 5	White	Clean Record	Female
Treatment 6	White	Convicted	Female

Table 2.1. Characteristics of Resumes

VARIABLES	(1) Rating	(2) Rating
Criminal Record	-1.720***	
	(0.0780)	
Female Applicant	-0.140	
	(0.0887)	
Black Applicant	-0.307***	
	(0.0956)	
Misdemeanor		-1.652***
		(0.132)
Arrest Only		-1.971***
		(0.128)
Constant	6.492***	6.652***
	(0.191)	(0.284)
Observations	1,932	828
R-squared	0.495	0.592

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.2. Comparison of Conviction and Arrest Only Ratings

VARIABLES	(1) Difference
Expungement	1.173***
	(0.150)
Black	-0.298**
	(0.145)
Female	-0.307*
	(0.161)
Constant	0.135
	(0.0885)
Observations	36
R-squared	0.723

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.3. Effect of Expungement

VARIABLES	(1) Rating	(2) Rating
Misdemeanor	-1.496***	-1.652***
	(0.104)	(0.133)
Black Applicant	-0.402***	-0.558***
	(0.104)	(0.149)
Black Applicant x Misdemeanor		0.312
		(0.207)
Constant	6.533***	6.611***
	(0.302)	(0.303)
Observations	1,104	1,104
R-squared	0.471	0.472

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.4. Ratings of Black and White Applicants

VARIABLES	(1) Rating	(2) Rating
Misdemeanor	-1.812***	-1.652***
	(0.0963)	(0.130)
Female Applicant	-0.232**	-0.0725
	(0.0963)	(0.134)
Female x Misdemeanor		-0.319*
		(0.193)
Constant	6.438***	6.359***
	(0.249)	(0.261)
Observations	1,104	1,104
R-squared	0.537	0.538

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.5. Ratings of Female and Male Applicants

VARIABLES	(1) Ranking	(2) Ranking
Criminal Records	5.489***	
	(0.258)	
Female Applicant	-0.429	
	(0.298)	
Black Applicant	0.998***	
	(0.311)	
Misdemeanor		5.322***
		(0.447)
Arrest Only		6.112***
		(0.440)
Constant	7.701***	8.300***
	(0.969)	(1.525)
Observations	1,932	828
R-squared	0.214	0.303

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.6. Comparison of Conviction and Arrest Only Rankings

VARIABLES	(1) Ranking	(2) Ranking
Misdemeanor	5.208***	5.322***
	(0.340)	(0.455)
Black Applicant	1.234***	1.348***
	(0.340)	(0.499)
Black Applicant		-0.228
x Misdemeanor		(0.681)
Constant	6.029***	5.972***
	(1.242)	(1.239)
Observations	1,104	1,104
R-squared	0.219	0.219

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.7. Rankings of Black and White Applicants

VARIABLES	(1) Ranking	(2) Ranking
Misdemeanor	5.446***	5.322***
	(0.322)	(0.442)
Female Applicant	-0.199	-0.322
	(0.322)	(0.450)
Female Applicant x Misdemeanor		0.248
		(0.644)
Constant	8.459***	8.521***
	(1.229)	(1.251)
Observations	1,104	1,104
R-squared	0.251	0.251

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.8. Rankings of Female and Male Applicants

CHAPTER 3. EVERYONE HAS THEIR PRICE: A LABORATORY EXPERIMENT STUDY ON ETHICAL REMINDERS AND DECEPTION

Introduction

Although deception is largely derided by the general public, it occurs all the time. It is nearly impossible to read the news without encountering a report of dishonest behavior. Deception and dishonesty penetrate all aspects of our daily lives, including business deals, politics, diplomatic matters, laws, scholarship, admissions, and many other fields—there are even reports of experimental subjects cheating in lab experiments.

Recent media coverage from the film *Tinder Swindler* and the television show *Bad Vegan* have returned public attention to deception and lying behaviors. Dishonesty often results in economic losses and emotional scars. From the $27 million fraudulent Fyre music festival to over $1.7 million in money theft by the Pure Food and Wine vegan restaurant, the general public has witnessed a range of tragedies caused by deception, and its trust has, to some extent, been seriously undermined. Many parents and teens were let down by the 2019 college admissions bribery scandal—detailed in the documentary *Operation Varsity Blues*—because they felt they could no longer trust the prestigious schools involved, which included Stanford and Yale. Economists have measured the magnitude of fraud in the U.S. properties and casualty insurance industry as reaching $24 billion annually, with other deceptions—including tax fraud and employee theft— potentially causing considerably more economic damage (Mazar, Amir, & Ariely, 2008). Naturally, this raises the following question: Why do people lie, and can we stop them from doing so?

From a philosophical perspective, Kant argued that deception is unacceptable regardless of the circumstances. Hence, it should be forbidden in all circumstances. However, it is

impossible to eliminate deception because it is also a vital part of daily life. Deception ranges from practical jokes and white lies to serious scams (Hyman, 1989). If we accept the existence of deception and admit that we cannot fully eliminate this behavior, then researchers must at least identify the motivations for lying and the methods that can be used to decrease its frequency or lower its negative impacts.

The standard economic model of the behavior of rational economic agents suggests that people will lie when they can achieve better economic outcomes from doing so. Economic researchers have agreed that people are honest or dishonest based on an external cost-benefit analysis in favor of one action or another (Hechter, 1990; Lewicki, 1983). Psychologically speaking, the internal values people hold against lying to some extent also weigh on their decision-making processes. Dishonest behaviors bring bad feelings such as guilt, while honest behaviors can be self-rewarding (Garrett, Lazzaro, Ariely, & Sharot, 2016). Studies in neuroscience find evidence of the same primary reward centers in the brain (i.e., nucleus accumbens) activating when people internally reward themselves for being honest as when they receive external benefits, such as monetary payoffs (O'Doherty, Deichmann, Critchley, & Dolan, 2002). Researchers have, based on findings like these, proposed the theory of self-concept maintenance (Mazar et al., 2008). Briefly, in this context, although people can cheat to gain economic rewards, they may suffer a loss in their sense of themselves as honest. Other studies have returned similar results and suggested that self-reputation is one of the key factors preventing people from lying.

If self-reputation and self-concept maintenance work, then ethical training should develop and strengthen one's honest behavior, and we should expect fewer deceptions after such training. However, researchers have found that the ethical training and business ethics programs in corporations and business schools are often useless (Badaracco Jr & Webb, 1995). Furthermore, human brains are known to adapt to deception very quickly. Thus, the more lies one tells, the more one gets used to deception, and the less guilty one feels

about telling lies (Garrett et al., 2016). Tenbrunsel and Messick (2004) argue that people will deceive themselves and fail to acknowledge that they are dishonest when they tell lies; they do this to make themselves feel better while pretending that their self-reputation and self-concept are not hurt.

Tenbrunsel and Messick (2004) also refers to the phenomenon of ethical fading: dishonest decisions made by an agent will appear less unethical if the self-assuring lies that the agent tells themselves are sufficient. This can be particularly serious, and the chance of ethical fading increases because people often make decisions on their own, and the process is not typically monitored. Thus, the question of how to improve honesty remains difficult to answer.

Since dishonesty is often accompanied by other feelings, it should be investigated in an intersectional way. For example, experiment-based research shows that the feeling of being treated unfairly can significantly increase one's likelihood of cheating in subsequent activities (Houser, Vetter, & Winter, 2012). Conversely, honesty can be increased when participants care about the well-being of other participants. This only increases the difficulty of studying deception since researchers cannot perfectly comprehend when people lie, why people lie, or how to increase honesty and decrease dishonesty.

Deception can be studied with minimal social interaction and checks to determine whether people lie. Research of this type has been performed in a lab experiment with mixed results. The assumption is that with minimal social interaction and a guarantee that there is no way to verify their answers, participants will maximize their lying to increase their monetary payoffs. The results suggest, however, that most participants are only partial liars; that is, they might lie a little bit to increase their monetary payoff, but not to the maximum level even when they know that their deception would not be detected (Fischbacher & Föllmi-Heusi, 2013; Pruckner & Sausgruber, 2013; Nagin & Pogarsky, 2003). Extensions of these findings suggest that incomplete deception exists in other settings and further suggest that

moral reminders would, contrary to the common expectation, increase the lying behavior (Zhao, Dong, & Yu, 2019). Others argue that ethical reminders do improve honesty (Grym, Liljander, et al., 2016).

No perfect reasoning can explain these puzzling results. Nevertheless, I propose that a potential reason for these results can be found in the variation of the payoffs. In (Zhao et al., 2019), participants receive cash rewards, but in (Grym et al., 2016), the reward is that the "top 25% participants can enter into a movie ticket lottery," which, when compared with a cash reward, significantly lowers the incentive to lie. At least one study has already shown that higher incentives can increase cheating as long as participants think that it is difficult to detect their lies (Kajackaite & Gneezy, 2017).

The present paper reviews the findings in the literature and extends these to include ethical reminders with a variation in payoffs. The experiment allows me to go beyond the mixed results in the literature and determine whether methods such as ethical reminders can improve honesty. It is hoped that this study will deepen the understanding of deception in different situations and provide clarity regarding whether payoff variation and external ethical reminders could improve honesty.

Related Literature

Economists have studied deception for decades. The field has transformed from its early "homo economicus" assumption—that rational and selfish agents who are not concerned about others' well-being will and should lie to gain economic benefits—to a common understanding that, depending on the situations and consequences, all people lie to some extent (Akerlof, 1978; Gneezy, 2005). The study of deception requires differentiating the many types of deception. Gneezy (2005) categorize lies into four categories: (1) white lies that do not harm anyone and might increase one's mood; (2) altruism or lying for the "greater

good", even if it means harming oneself to some degree; (3) lies that have no particular motive and may even harm the liar; and (4) lies that help the liar and could potentially harm the other party. This categorization is based on a cost-benefit analysis, which raises the following question: What can one gain from cheating, and what can one lose? If the gain from being honest is greater than that from cheating, then a person should not lie since it is costly to do so. This theory offers a simple solution to decreasing deception: increase its cost. This suggests that as long as the cost is sufficiently high, the benefits of cheating remain unattractive.

By contrast, Ariely and Jones (2012) argue that this theory is ineffective because people do not engage in the same cost-benefit assessment for lying as they would regarding other activities. The author clarifies this using the death penalty as an example. In short, murder comes with a massive cost, which is the death penalty. People see that the cost of murdering others involves giving up their own lives and would seem simply too high to motivate anyone to commit such crimes. Nevertheless, homicide still exists. While this is not to say people do not think about the cost of lying, the cost is only one of numerous factors affecting their decision.

Fischbacher and Föllmi-Heusi's (2013) finding also echoes Ariely and Jones's (2012) argument. They designed a lab experiment without a monitor. Each participant had a die that they tossed, reporting the number on the die to earn monetary payoffs. The researcher emphasized that participants' activities would not be monitored and that no one would verify the results, creating a perfect environment for cheating. Participants could simply report the number that gave them the highest payoffs, regardless of the number they obtained or whether they actually rolled the dice. Fischbacher and Föllmi-Heusi (2013) find that although some participants did not lie to gain the maximum reward, most did. They referred to these participants as "partial liars". Some participants even reported numbers that returned rewards of $0, suggesting that they were being completely honest. Similar

results were also obtained in the field experiment.

Other studies return similar findings. Ariely and Jones (2012) designs a field experiment with a vending machine that gives extra bags of snacks without taking money. A label on the vending machine provided a number for buyers to call if there was anything wrong with the machine. Unsurprisingly, no one called the number. However, no one took more than five bags of the snacks either. This suggests that while people do not lie to the maximum level, they are also not completely honest.

It is intriguing to see that some participants in the laboratory experiments behave with complete honesty, but this was not the case in the field experiment. A completely honest person would have called the number provided on the vending machine. If complete honesty does not show up in the field experiment, it becomes a bit surprising to see it in the lab experiment. I argue that the lab experiment design in Fischbacher and Föllmi-Heusi's (2013) study can be improved in several ways. First, the authors argue that since the experiment was short and simple, it could be attached at the end of other studies. I argue that this approach may have had unknown effects on the subjects. Subjects may not realize the individuality of their experiment, no matter how much this is emphasized by experimenters.

Although subjects were told that they were not being monitored, the main experiment they had attended was mostly monitored, and participants might not transition well from a monitored to an unmonitored condition. Additionally, the experiment was a one-shot game. Participants tossed the dice once and recorded the number. Fischbacher and Föllmi-Heusi (2013) introduce repetition by having participants join in multiple different experiments, ending with another short experiment. I do not believe that the data collected using this method is accurate, and not all participants had the chance to repeat the experiment. Ethical fading does not happen immediately, and brains adapt to deception gradually, suggesting that deception occurs over time with sufficient repetition (Garrett et al., 2016; Tenbrunsel & Messick, 2004). Thus, a one-shot game may not be the best experimental design to study

deception. Nevertheless, the results offer valuable insight supporting Ariely and Jones's (2012) argument that a cost-benefit analysis is insufficient.

Ariely and Jones (2012) proposes increasing honest behavior by reinforcing personal moral standards. This is similar to the ethical training offered in business programs, the effects of which can be short-lived (Richards, 1999). Badaracco Jr and Webb (1995) even argue that such training has no specific effect in terms of changing the behavior of trainees. Tenbrunsel and Messick (2004) supports this idea and proposes the theory of ethical fading, which explains how a person's moral standards can drop over time, even after receiving ethical training. I argue, in line with Ariely and Jones, that this is more likely to be avoided with constant reminders of ethical training. Instead of providing one-time ethical training and hoping that its effects will be long-lasting, people should receive constant reminders.

Zhao et al. use a dice-rolling setting identical to Fischbacher and Föllmi-Heusi (2013) and employ moral reminders. Their treatment variable is explicit moral reminders and implicit moral reminders versus the control variable with no moral reminders. Instead of a normal distribution, the authors find unusually high frequencies of high numbers being reported. Their results are intriguing and suggest moral reminders trigger more lies regardless of whether the reminders are implicit or explicit. This goes against common expectations and Dan Ariely's suggestion.

Yet, Grym et al.'s (2016) research offers supportive evidence that moral reminders can improve honesty. For their experimental design, rather than dice rolling, they give participants a mathematical quiz and ask them to grade and report their own scores on finishing. The quiz involves simple calculations, such as addition and subtraction, so that participants' mathematical skills do not overly affect the results, and the test is not a determination of intelligence or ability. The authors compare the grades from classes in which participants are enrolled, finding that there is no significant difference among participants in terms of mathematical ability. The result is an aggregate, so it cannot be used to identify who is and

is not cheating; however, the abnormally high scores suggest more cheating activities. Their result suggests that participants with moral reminders report much lower scores than those without, and the difference is statistically significant. It is worth noting that the reward in Grym et al.'s (2016) experiment may be less attractive than the cash reward in Zhao et al. (2019).

Researchers have also discussed other factors that could potentially affect lying. Some suggest that women demonstrate a greater aversion to lying. In fact, Grym et al. finds that female participants lie less (have lower mean math scores) than male participants when there is no moral reminder, yet the mean score for female and male participants is similar when moral reminders are implemented, suggesting that women are more averse to lies. However, recent research offers no clear evidence supporting this. Childs (2012) examine whether women and men have different attitudes toward lying when facing small monetary benefits and conclude that there is no obvious difference in lying by gender. Age is another factor that has been investigated. Unfortunately, very few studies have assessed the frequency of lying among different age groups. Notably, Ruffman, Murray, Halberstadt, and Vater (2012) find that young adults are better at lying than older adults; however, it is unclear whether age affects the frequency of dishonest behavior. Neuroscience also provides a framework for studying deception and emphasizes that the prefrontal cortex plays a key role; however, the results are ambiguous (Abe, 2011). Thus, demographic traits could have potential effects on deceptive behavior.

In summary, despite the frequency with which humans hear or tell lies, deception remains a puzzling activity requiring additional research. The present study contributes to the literature through a lab experiment. The benefit of lab experiments is that they make it possible to easily collect demographic data, vary monetary rewards, and implement similar dice-rolling experiments with moral reminders. Such an experiment could answer several questions: whether moral reminders decrease or increase honest behavior; whether the pay-

off is the definitive factor motivating deception; and, lastly, whether gender plays a key role in dishonesty.

Experimental Design

This experimental design isolates the internal motives affecting deceptions and honesty by utilizing and slightly modifying Fischbacher and Föllmi-Heusi's design and offering no monetary incentive for honest behavior. The subjects were UCI undergraduate students recruited from the ESSL lab. The experiment was a two-by-two design with one control and three treatments.

The experiment was conducted online, and subjects used virtual fair dice. In the control session, subjects were asked to roll the dice and record the numbers: a roll of six would return $1, and all other numbers would return $0. Subjects knew that there was no verification, and they did not have to turn on their camera to participate; this removed any doubts that they were being monitored and their behavior verified.

Moreover, the experimenter was not present during this phase, and the experiment was conducted on oTree; there was no way for experimenters to check for lying(Chen, Schonger, & Wickens, 2016). This offered a motivation to lie, with the best strategy being to report a six regardless of the number rolled. The design is slightly different from that in Fischbacher and Föllmi-Heusi. Instead of gradually increasing the payoff with an increase in the number rolled, the experiment made very clear to participants that reporting a six was the "money-maximizing" strategy. All other options would return no monetary payoffs, and students reporting these numbers must be honest. During the experiment, neutral notifications such as "If you have any questions, please message the experimenter" would pop up every five rounds. These statements are neutral and do not relate to payments or honesty.

In this case, being honest and reporting a number other than a six was clearly a "money-losing" strategy. However, with a fair die, there is a 1/6 chance of a six being rolled, and an unusually high frequency of reporting a six indicates the subject is lying. This task was repeated 30 times to assess how repetition affected subjects' behaviors; research suggests that ethical fading occurs more easily with repetition (Tenbrunsel & Messick, 2004; Garrett et al., 2016). Small lies often serve as self-signals, making people feel increasingly comfortable with telling lies. Moreover, people do not feel like they are lying after they have told many small lies (Garrett et al., 2016). Thus, such a design can reflect whether subjects lie more with repetitions. If this is the case, I should find a higher frequency of subjects reporting six in the later trials.

In addition, the one-shot design in Fischbacher and Föllmi-Heusi is an assessment at the aggregate level, whereas I am interested in the individual level for the control session. A concern for a one-shot game or smaller number trials is that a uniform distribution cannot be guaranteed. Subjects may get lucky and roll a six twice, or most participants may be lucky that day and roll a six. With 30 trials for each participant, this can be avoided. Moreover, by reporting their own numbers, subjects were not burdened with concern for the well-being of others. Thus, being truthful in this task suggests an extremely strong aversion to lying. Regarding the cash reward, one trial was randomly selected, and if the number reported for that trial was six, then the subject would receive \$1 and \$0 otherwise. In Phase 3, the subject's demographic information was collected. The control session is a session with a low reward (the highest monetary reward is \$1) and no moral reminder.

The treatment sessions were mostly identical to the control session but with moral reminders given throughout and different monetary rewards offered. This design echoes the theory in Ariely and Jones, which explores whether moral reminders can serve to sanitize deception and lower the frequency of dishonest behaviors. In short, Ariely and Jones (2012) argue that small lies could blind a subject, and they may self-signal that they are not lying.

Moral reminders remove these blinders and erase the effect of self-signaling. In the experiment, moral reminders took the form of words that popped up during the experiment, such as "honesty is an important virtue in our daily lives" and "I appreciate your honesty."

Treatment 1 is a session with a low reward and a moral reminder. Participants roll a die 30 times and report the resulting numbers. Their monetary reward is decided by random selection of a trial. If the number reported for that trial is six, subjects receive $1, and $0 otherwise. Moral reminders pop up for every fifth trial throughout the experiment.

Treatment 2 is a session with a high reward but no moral reminder. Participants roll the dice 30 times and report the numbers accordingly. Their monetary reward is decided by a randomly selected trial. If the reported number of that trial is six, subjects receive $5, and $0 otherwise. There is no moral reminder of any kind, and only neutral statements are offered, as in the control session.

Treatment 3 is a session with a high reward with a moral reminder. Participants roll the dice 30 times and report the numbers accordingly. Their monetary reward is decided by a randomly selected trial. If the reported number for that trial is six, subjects receive $5, and $0 otherwise. Throughout the experiment, moral reminders are given for every fifth trial.

Another concern of implementing a large number of trials and then only selecting one as a payment round is that this may impact subjects' behavior. If only one round is selected, the expected payoff is low. But, if a few rounds are selected, and participants can get a higher payoff if a six is recorded for at least one, then they may not lie as much; the best strategy in both scenarios is to report a six in every round regardless of the payment mechanism. In response to this concern, another session was added. Everything is the same as in the control session, but three rounds are randomly selected at the end of the experiment, and if a six is recorded for at least one of those, then subjects receive $1 for the task. If the distributions of reporting a six in both the control and treatment session are similar, this means subjects do

not behave differently; if the distributions are different, subjects do indeed behave differently depending on the number of payment rounds selected.

The one-shot dice-tossing game is a simple yet profound design that has been used in many research studies related to deception since it was proposed by Fischbacher and Föllmi-Heusi (2013). My experiment took advantage of the original design and incorporated priming. Previous studies have tended to only combine priming and ethical reminders with the one-shot dice-tossing game. My design varied the trial numbers allowing the effect of priming to be considered at both the individual and aggregate levels. In addition, no existing studies have used a combination of the high versus low payoff treatment and the treatments with and without ethical reminders; this is an intriguing area in the study of deception.

Hypothesis

Based on the experimental design, the following main hypotheses are proposed, assuming subjects are lying in the experiment:

- H1: Constant ethical reminders have no impact on subjects' lying behaviors (Moral reminders have no effect)
- H2: Higher rewards have no impact on frequencies of subjects reporting six (Higher payoffs do not lead to more dishonesty)

Hypotheses H1 reflect the expectation that subjects will lie and report a six in more rounds to gain higher payoffs. The logic is that with a large number of trials, reported numbers should be uniformly distributed, with each number having a 1/6 chance of being reported. Thus, the unusually high frequency with which a six is reported indicates deception.

H1 is based on the findings of Ariely and Jones (2012) and assesses whether constant moral reminders can decrease the frequency of deception (i.e., an unusually high frequency of reporting a higher number). The current findings on moral reminders (i.e., Zhao et al. (2019) and Grym et al. (2016)) are contradictory and deserve more attention. The comparison is cross-sessional, with monetary rewards as the control variable. Notably, H2 is interested in how rewards motivate deception. Moral reminders may have worked in Grym et al. (2016) because the reward was too low (the top 25% participants could enter into a raffle for a movie ticket). Tests other than a t-test, including Chi-square and Kolmogorov-Smirnov tests, were utilized to check the distribution because of my interest in determining whether a different method of payment-round selection affects subjects' behavior.

Results

Although it is expected that subjects will lie in this type of experiment, it is still necessary to prove this. The frequency of occurrence of each number tossed on a fair die should be uniformly distributed, and the expected frequency of reporting a six in a 30-trial dice-rolling task should be five. The results for the two-tailed t-test suggest that subjects are lying in all sessions at a 5% level of significance, regardless of the reminder condition. Thus, my assumption for the hypothesis stands, and subjects are indeed dishonest when reporting rolled numbers. The Kologorov-Smirnov test further proves that the reported dice-rolling results are not uniformly distributed ($p = 0.012$), which is within my expectations and is consistent with previous results.

I then compare the sessions in detail. I first control the payoff conditions and look at the effect of moral reminders. A total of 27 subjects participated in the control session (low payoff and neutral statement), and this generated 810 observations. I use the mean value of reporting a six as an indicator of dishonesty. As Table 3.2 suggests, in the control session,

subjects reported, on average, ten rounds of six. By contrast, 28 subjects participated in Treatment 2 (low payoff with moral reminder), reporting 6.58 rounds with a six, on average. The moral reminder condition significantly improves honesty and lowers the frequency of reporting a six ($p = 0.021$). Although the recruiting process for the experiment is random, the majority of subjects were women, and most subjects had an Asian racial background, it is not possible to conclude from the data whether there is a clear difference in the effects of moral reminders on different gender and race. However, it is clear that moral reminders decrease dishonesty.

I also compare Treatment 1 (high payoff and neutral statement) and Treatment 3 (high payoff and moral reminder) to see the effect of moral reminders on subjects when the payoff is high. As Table 3.3 suggests, subjects report rolling a six an average of 14.74 times in Treatment 1 but 10.37 times in Treatment 3. This suggests that moral reminders indeed lower the frequency of reporting a six even when the payoff is high, at a 5% significance level; however, the effect is inconsistent across racial and gender groups. For example, White subjects report more rounds with a roll of six (almost five rounds more) with moral reminders, whereas adverse effects are seen in other racial groups. Men also increase the frequency of reporting a six slightly with moral reminders. However, both of these conclusions regarding race and gender are not definitive because of the low numbers of men and White people among the participants.

Overall, the findings in both cases support the argument that moral reminders lower dishonesty, and thus, H1 (moral reminders have no effect) can be rejected. The mean values of both comparisons also suggest that subjects report a six more often in high payoff sessions. I confirm this by controlling the moral reminder conditions and comparing how subjects behave in high and low payoff sessions.

Table 3.4 compares subjects' behaviors in high (Treatment 1) and low payoff sessions (control) when both sessions include only neutral statements. As the table shows, subjects

report 4.72 more rounds with a six in high payoff sessions, which is statistically significant at the 1% level. The effects are consistent across genders and racial groups. Higher payoffs indeed motivate more dishonesty, which rejects H2 (higher payoffs do not lead to more dishonesty).

Table 3.5 compares subjects' behavior in high (Treatment 3) and low payoff sessions (Treatment 2) when both sessions include moral reminders. As the table shows, subjects report 3.59 more rounds of rolling a six in the high payoff sessions than in the low payoff sessions. Although moral reminders have adverse effects on lying in both sessions (as Tables 1 and 2 suggest), a higher payoff still convinces subjects to lie more often, even with moral reminders. The comparisons in Tables 3.4 and 3.5 reject H2 and the supports the conclusion that higher payoffs indeed lead to more dishonesty.

Additionally, in response to the concern that subjects may behave differently depending on the expected payoffs, I run one more session that uses a different payment-round selection mechanism. Instead of randomly selecting one round as the payment round, three rounds are randomly selected, and subjects are paid if a six is recorded for at least one of the three rounds. The results in Table 3.6 suggest that there is no significant difference between the control session and the session using a different payment-round selection mechanism. Pearson's chi-square test also suggests that the two sessions have a relatively similar distribution (p=0.787).

Lastly, Table 3.7 presents the regression analysis. Equation 1 pools all the data to examine the effect of a moral reminder, low payoff, and the combined moral reminder and low payoff. As the results suggest, in general, moral reminders have a strong adverse effect on reporting a six; this result is significant at the 5% level, which means that such reminders can effectively decrease dishonesty. However, my result does not suggest that a one-shot reminder would have the same effect; the result applies to constant moral reminders. Secondly, it suggests that the reminder works better in low payoff settings, which is understandable. As the

rewards grow, there is an increased motivation to cheat, and moral reminders, although they may still work, have less effect on subjects' behaviors. It also suggests that when the reward is sufficiently high, its effect outweighs the effect of moral reminders, yet, the interaction term of moral reminder and the high payoff has no statistical significance.

Discussion and Conclusion

Contrary to Zhao et al.'s (2019) findings, my study suggests that moral reminders have strong adverse effects on dishonesty, which is in line with many previous studies(Grym et al., 2016). However, I find that higher rewards lower the effect of moral reminders and priming, which is a question raised and discussed in previous studies(Kajackaite & Gneezy, 2017). I argue that constant reminders still work effectively to lower incidents of dishonesty, but society cannot rely on these when the reward from cheating is huge. It is also not to suggest that constant moral reminders have the same power as punishment, but constant moral reminders can serve to prevent and thus decrease dishonesty.

My study utilizes an online experiment, which offers subjects more privacy, and they are less suspicious that they are being monitored or that their dice number will be secretly verified. In addition, my study combines moral reminders and a change of payoff to examine simultaneous effects rather than focusing on either moral reminders alone or payoffs alone; these have been studied many times before. It is also important to notice that the moral reminders in my study were constant (not once), which is consistent with Ariely and Jones (2012) idea. In addition, my study also responds to a few puzzling findings from previous studies.

I see several flaws in the unique findings of Zhao et al. (2019) that subjects show more dishonesty when being reminded. The design was in Chinese, and the instruction in Chinese

was to "report the actual number you rolled" in English instead of reminders such as "honesty is greatly appreciated." The essence of a moral reminder was lost, and the instruction functions as more of a command, which may have unexpected and unpredictable effects on subjects in reporting the numbers. In addition, the authors argue that moral reminders did not work in their experiment because of the signaling effect. They argue that when subjects see such a reminder, they are signaled that other subjects in the experiment are lying, which increases their willingness to lie. If this is indeed the case, subjects in my experiment should have lied more often when they received constant moral reminders, but they did not.

In addition, the high versus low payoff design means that my study can look into the effect of moral reminders while also taking account of the reward. Unlike Grym et al.'s (2016) findings that suggest moral reminders work effectively when the reward is low, I find that higher rewards will offset some, but not all, of the effects of moral reminders. It is important to recognize the combined effects of moral reminders and rewards; relying on the effect of moral reminders without considering the extent of the reward for dishonesty can be very dangerous.

Lastly, it is important to note that to have any effect; moral reminders must occur constantly. In an ongoing struggle and grappling with dishonesty, a single moral reminder cannot be expected to reduce dishonesty. To the contrary, and as Ariely and Jones (2012) explain, small lies will gradually conquer people's minds. Thus, moral reminders should be treated as sanitizers instead of lie eliminators. The feature of a sanitizer is that it must be constantly used—using it once and expecting miracles to happen is not realistic.

In conclusion, I will note there are limitations to this study. My subjects are college students, which means they have not encountered as many opportunities to lie for financial gain as working adults. The results may differ for subjects from different demographic groups than college students. Secondly, the show-up payment was $10, and the reward in the dice-rolling task was $1 and $5 in the low and high payoff sessions, respectively. There

are, therefore, some concerns that subjects may not care much about the rewards, limiting their willingness to lie. In addition, subjects may have had a hidden agenda and concerns of which I was unaware. For example, they may be afraid that if they lie in an experiment, they will lose their status as an ESSL subject and lose future opportunities to participate in other experiments. Although researchers know that this is not the case, subjects will have their own concerns. Given the particular scope of my study, I cannot address all the various factors and focus on the effects of constant moral reminders and payoffs. However, future studies could explore these limitations and possibly compare whether the frequency of moral reminders has an effect on dishonesty.

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	Control	Treatment 1	Treatment 2	Treatment 3
Reminders	Neutral	Neutral	Honesty	Honesty
Payoff	Low	High	Low	High

Figure 3.1. Sessions

Sessions	N	Average Rounds of Six Reported	Std
Control	27	10.00	5.02
Treatment 1	27	14.74	5.84
Treatment 2	28	6.78	5.77
Treatment 3	27	10.37	7.50

Table 3.1: Summary of Statistics

Variable	Control			Treatment 2			t-Test *p*-value
	N	Average Rounds of Six Reported	Std	N	Average Rounds of Six Reported	Std	
All samples	27	10.00	5.02	28	6.78	5.77	0.023
Male	4	12.50	5.97	5	5.80	3.03	0.063
Asian	17	11.12	5.37	14	5.43	2.47	0.001
White	7	8.29	4.39	8	7.11	9.47	0.768
Latino	3	7.67	3.06	6	9.67	4.84	0.557

Table 3.2. Effects of Moral Reminders in Low Payoff Sessions

Variable	Treatment 1			Treatment 3			t-Test *p*-value
	N	Average Rounds of Six Reported	Std	N	Average Rounds of Six Reported	Std	
All samples	27	14.74	5.84	27	10.37	7.50	0.020
Male	10	15.10	6.89	6	15.83	10.94	0.871
Asian	17	14.47	7.16	14	7.57	4.96	0.005
White	3	16.33	0.58	4	21.50	9.26	0.389
Latino	7	14.71	3.15	9	9.78	5.89	0.066

Table 3.3. Effects of Moral Reminders in High Payoff Sessions

Variable	Control			Treatment 1			t-Test *p*-value
	N	Average Rounds of Six Reported	Std	N	Average Rounds of Six Reported	Std	
All samples	27	10.00	5.02	27	14.74	5.84	0.002
Male	4	12.50	5.97	10	15.10	6.89	0.523
Asian	17	11.12	5.37	17	14.47	7.16	0.133
White	7	8.29	4.39	3	16.33	0.58	0.016
Latino	3	7.67	3.06	7	14.71	3.15	0.012

Table 3.4. Effects of High Payoff in Sessions without Moral Reminders

Variable	Treatment 2			Treatment 3			t-Test *p*-value
	N	Average Rounds of Six Reported	Std	N	Average Rounds of Six Reported	Std	
All samples	28	6.78	5.77	27	10.37	7.50	0.047
Male	5	5.80	3.03	6	15.83	10.94	0.081
Asian	14	5.43	2.47	14	7.57	4.96	0.160
White	8	7.11	9.47	4	21.50	9.26	0.031
Latino	6	9.67	4.84	9	9.78	5.89	0.970

Table 3.5. Effects of High Payoff in Sessions with Moral Reminders

	Control			3 rounds			t-Test p-value
	N	Average Rounds of Six Reported	Std	N	Average Rounds of Six Reported	Std	
All samples	27	10.00	5.02	28	10.93	5.77	0.535

Table 3.6. Effect of Different Payment Mechanism

VARIABLES	(1) All Samples Rounds Of Six
Reminder	-3.214**
	(1.457)
High Payoff	4.741***
	(1.482)
Reminder x High Payoff	-1.156
	(2.339)
Constant	10.00***
	(0.965)
Observations	109
R-squared	0.184

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.7. Regression Result

Appendices

A Details of Experiment in Chapter 1

A.1 Modified version of ECR Questionnaire

The Experience in close relationship questionnaire used in the experiment is a modified version of the original ECR questionnaire.

On a scale of 1 to 7, where 1 indicates 'Strongly Disagree' and 7 indicates 'Strongly Agree', please answer the following the questions:

1. I am afraid that I will lose the love from my friends and family
2. I prefer not to show people how I feel deep down
3. I often worry that people will not want to stay with me
4. I feel comfortable sharing my private thoughts and feelings with my friends and family
5. I often worry that people don't really like me
6. I find it difficult to allow myself to depend on my friends and family
7. I worry my friends and family won't care about me as much as I care about them
8. I am very comfortable being close to other people
9. I often wish that my friends and family's feelings for me were as strong as my feelings for them
10. I do not feel comfortable opening up to my friends and family
11. I worry a lot about my close relationship such as friendship
12. I prefer not to be too close to other people

13. When others are out of sight, I worry that they might become interested in someone else
14. I get uncomfortable when others want to be very close
15. When I show my feelings for others, I am afraid they will not feel the same about me
16. I find it relatively easy to get close to others
17. I rarely worry about others leaving me
18. It is not difficult for me to get close to others
19. Others make me doubt myself
20. I usually discuss my problems and concerns with others
21. I do not often worry about being abandoned
22. It helps to turn to others in times of need
23. I find that others don't want to get as close as I would like
24. I tell others just about everything
25. Sometimes others change their feelings about me for no apparent reason
26. I talk things over with others
27. My desire to be very close sometimes scares other people away
28. I am nervous when others get too close to me
29. I am afraid that once others get to know me, they will not like who I really am
30. I feel comfortable depending on others

31. It makes me mad that I don't get the affection and support I need from others (such as friends and family)

32. I find it easy to depend on others

33. I worry that I won't measure up to other people

34. It is easy for me to be affectionate with others

35. Others only seem to notice me when I am angry

36. My friends and family really understand me and my needs

A.2 Games

Here are some details of the games subjects encountered in the experiment.

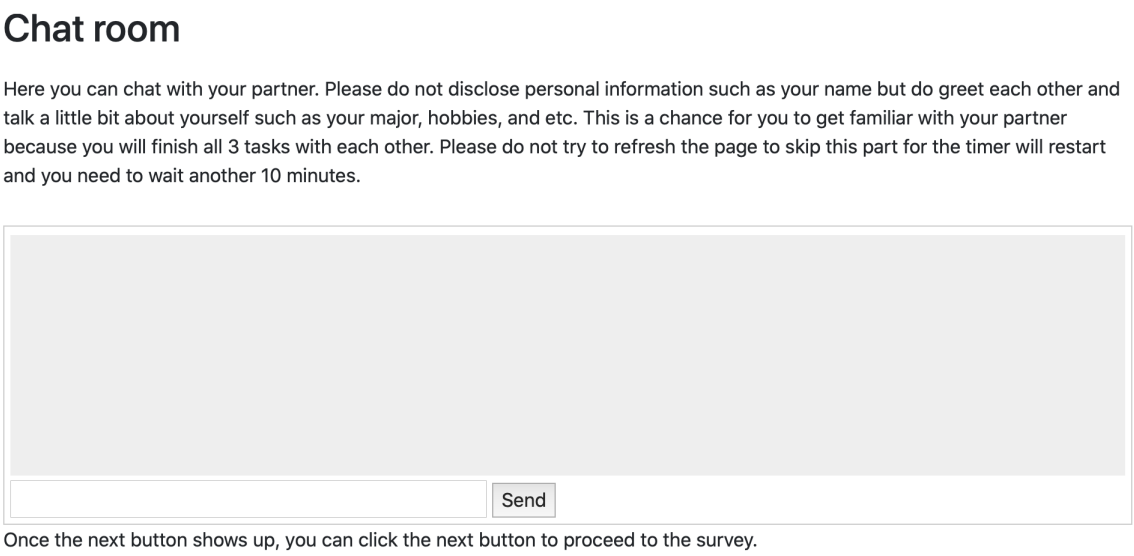

Figure A.2.1. Chat Room

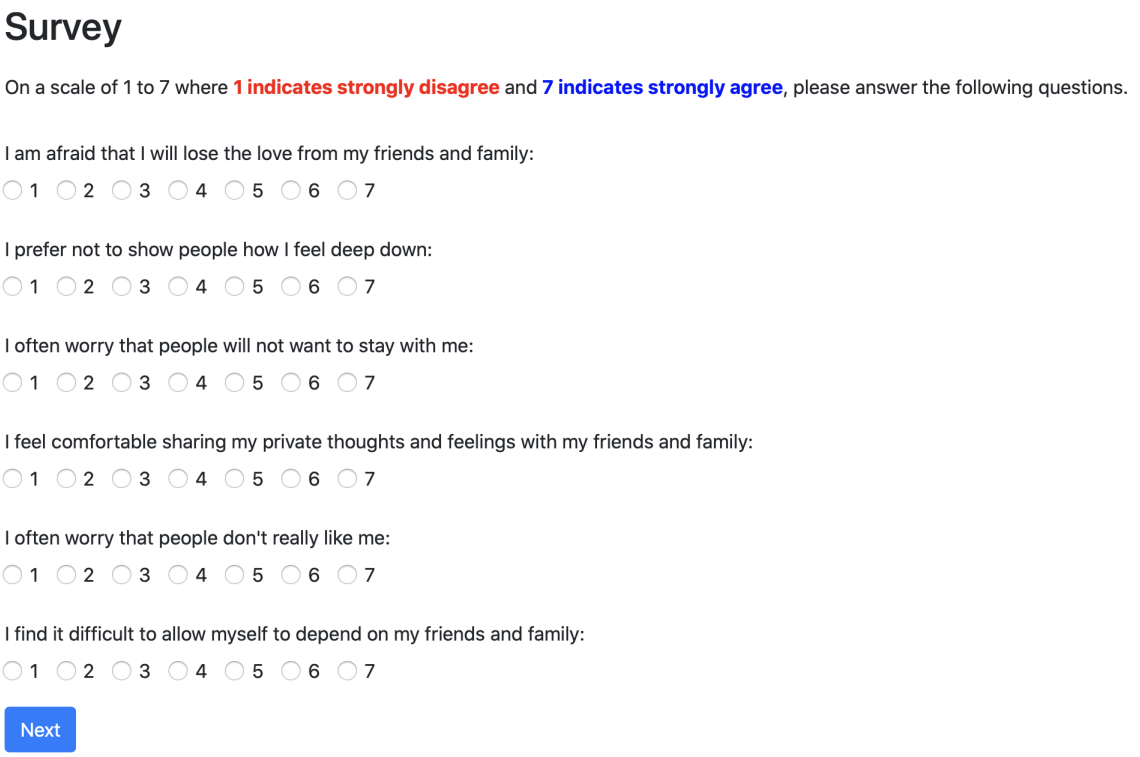

Figure A.2.2. First Part of the ECR Questionnaire

Instructions For Task 1

In this study, you will be randomly and anonymously paired with another participant. Your pair will remain the together throughout the entire experiment.

Part 2 of the experiment consists 3 tasks, and this is ***Task 1***. Below is an example of the matrix you will encounter in Task 1.
Once you proceed, you will have ***1 minute*** to make your decision. If you do not make a decision by the end, the system will choose to exercise ***Option 2*** for you.

		Your partner	
		Option 1	**Option 2**
You	**Option 1**	**$5**, *$5*	**$0**, *$8*
	Option 2	**$8**, *$0*	**$2**, *$2*

How To Read the Table

Each cell stands for a payoff corresponding to the interaction. For example, the cell in the first column of first row stands for the payoff if you choose to exercise Option 1 and your partner chooses to exercise Option 1.

In each cell, the amount to the left (labeled red) is your payoff and the amount to the right (italicized) is your partner's payoff. If you have any questions, please ask the experiment observer, otherwise, you can click the next button below to proceed to part 1.

Next

Figure A.2.3. Prisoner's Dilemma Instruction

Make Your Choice

Time left to complete this page: **0:54**

Please click on the button to make your choice. Once you make your decision, you will not be able to come back to this page.

		Your Partner	
		Option 1	**Option 2**
You	Option 1	**$5**, *$5*	**$0**, *$8*
	Option 2	**$8**, *$0*	**$2**, *$2*

Figure A.2.4. Prisoner's Dilemma Decision Making Page

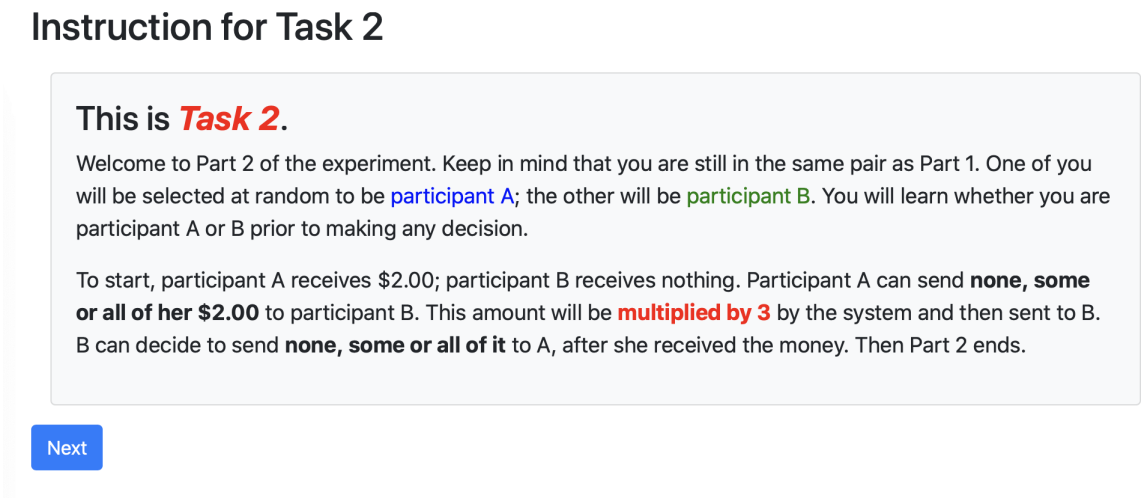

Figure A.2.5. Instruction for Simple Trust Game

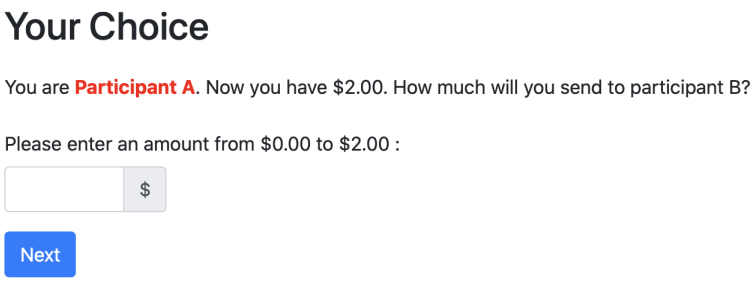

Figure A.2.6. First Mover's Choice Page

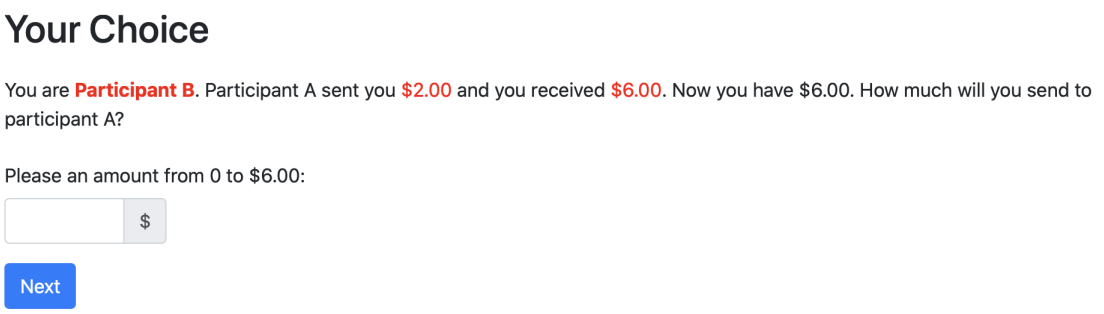

Figure A.2.7. Second Mover's Choice Page

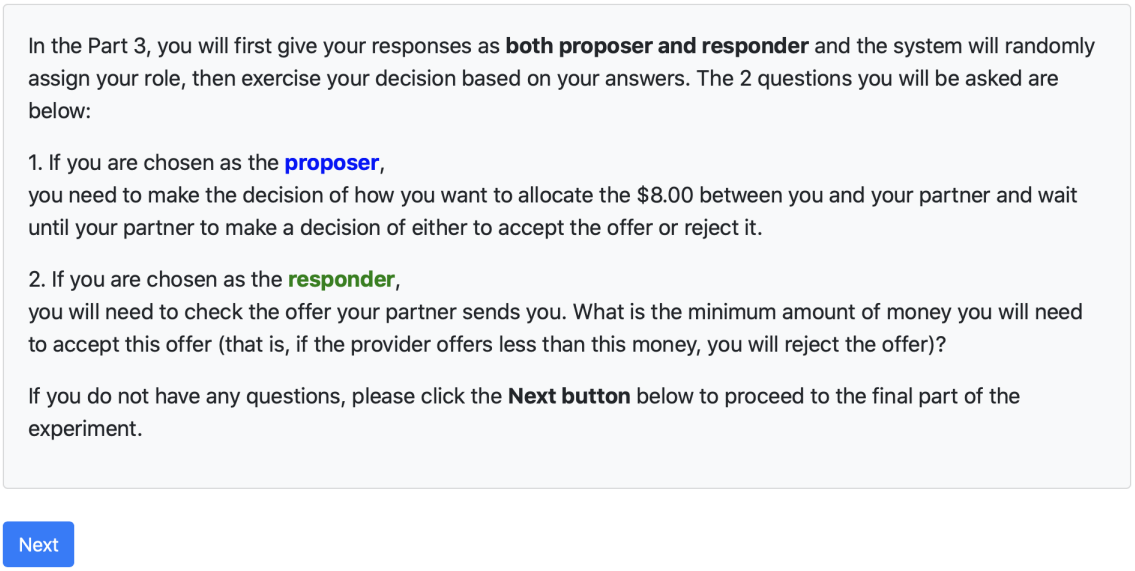

Figure A.2.8. Instruction for Ultimatum Game

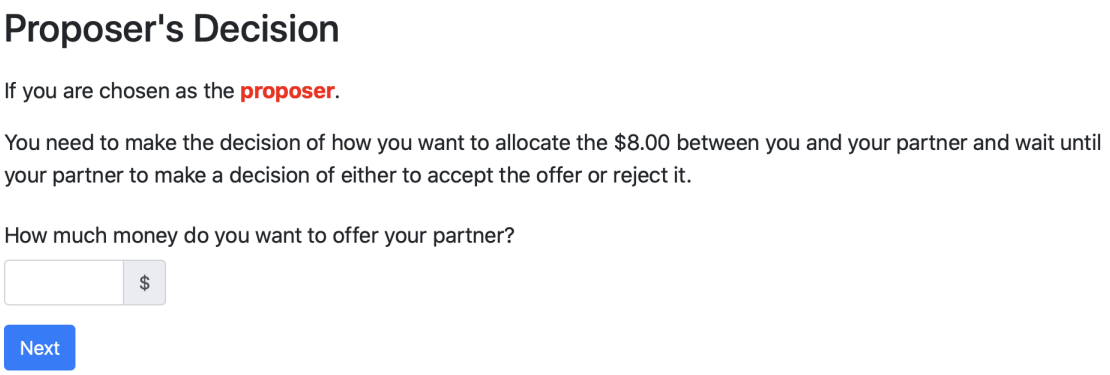

Figure A.2.9. Proposer Decision Making Page

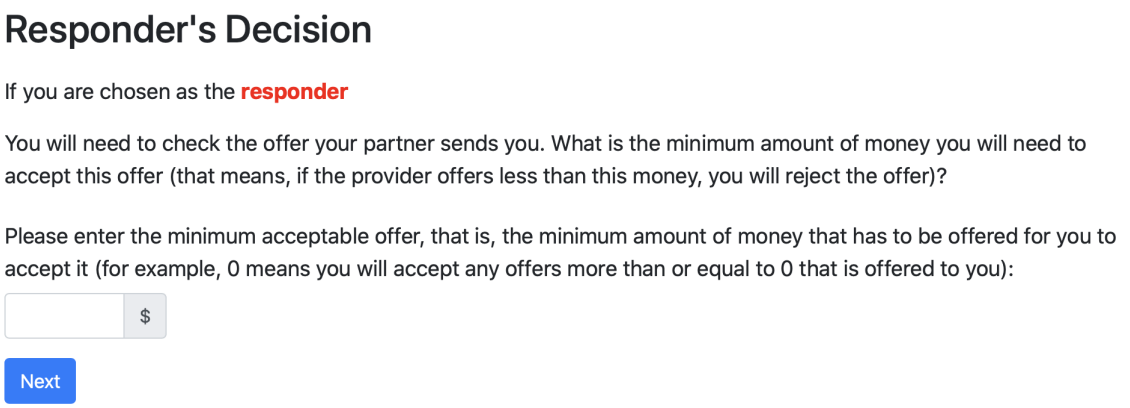

Figure A.2.10. Respondent Decision Making Page

B Details of Experiment in Chapter 2

Instruction for Task 2

Here are 21 job applicants' resumes. For the purpose of this research, we standardized them into the same format, covered the contact information and kept the most relevant job experiences. Your task is as following: consider yourself as an HR, and you're hiring customer service representative. These 21 job applicants fit the position and passed the initial screening, and you need to review them one last time.

Please rate them on a scale from 1-10, where 1 indicates highly unlikely to be hired, and 10 indicates highly likely to be hired. All applicants experiences are highly relevant to the position, and their educational backgrounds are similar. You also need to leave a short comment (within a sentence or two) to explain why you rate each resume as it is. Each tab stands for that job applicant's resume. You can go back and forth during the task to check these job applicants by clicking on the tab. You can also use the Back and Next button at the bottom of each page to go back and forth. However, notice that once you click the next button under the "End of Task" tab, you cannot go back anymore. So make sure that you rated all resumes.

Below are some details of the tasks subjects encountered in the experiment: Figure B.1 and B.2 present what subjects see when they do the rating task; figure B.3 is an example of the resume; figure B.4 shows what subjects do in the ranking task.

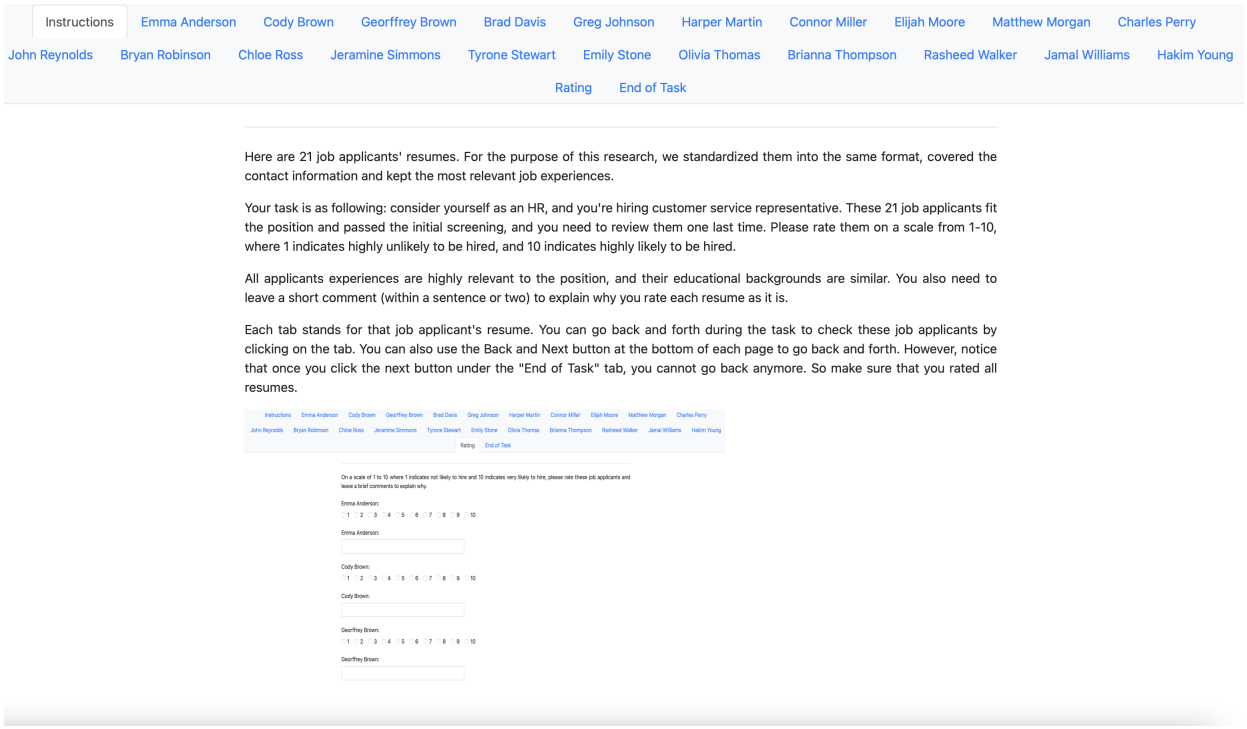

Figure B.1. Rating Instruction

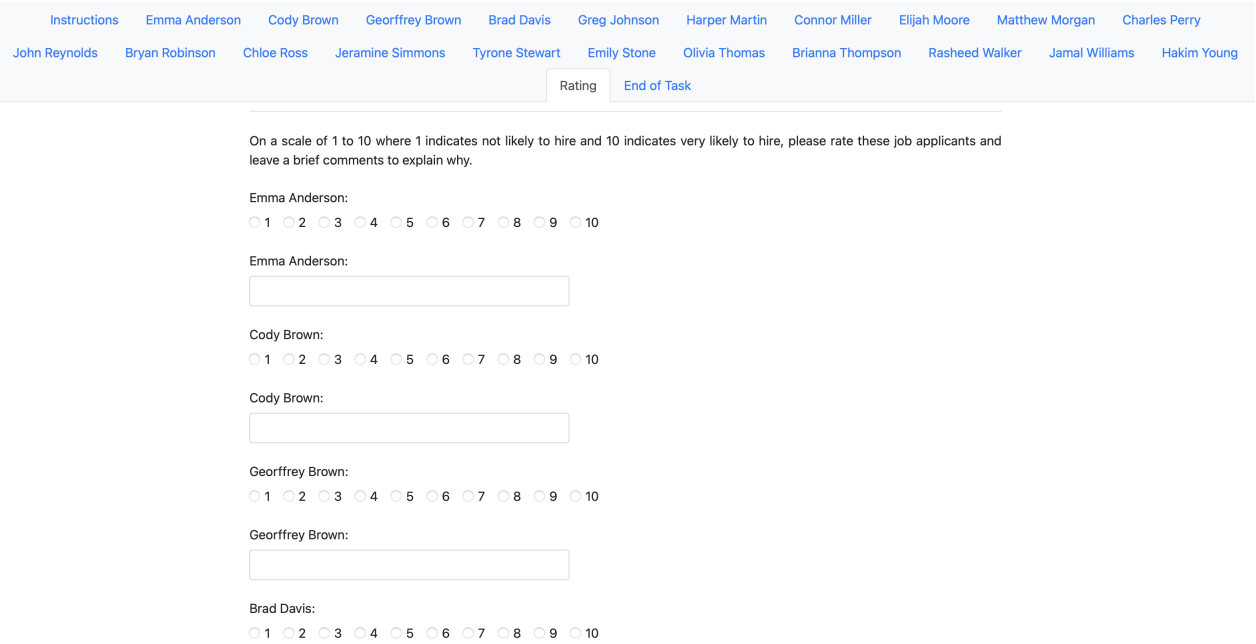

Figure B.2. Rating Task

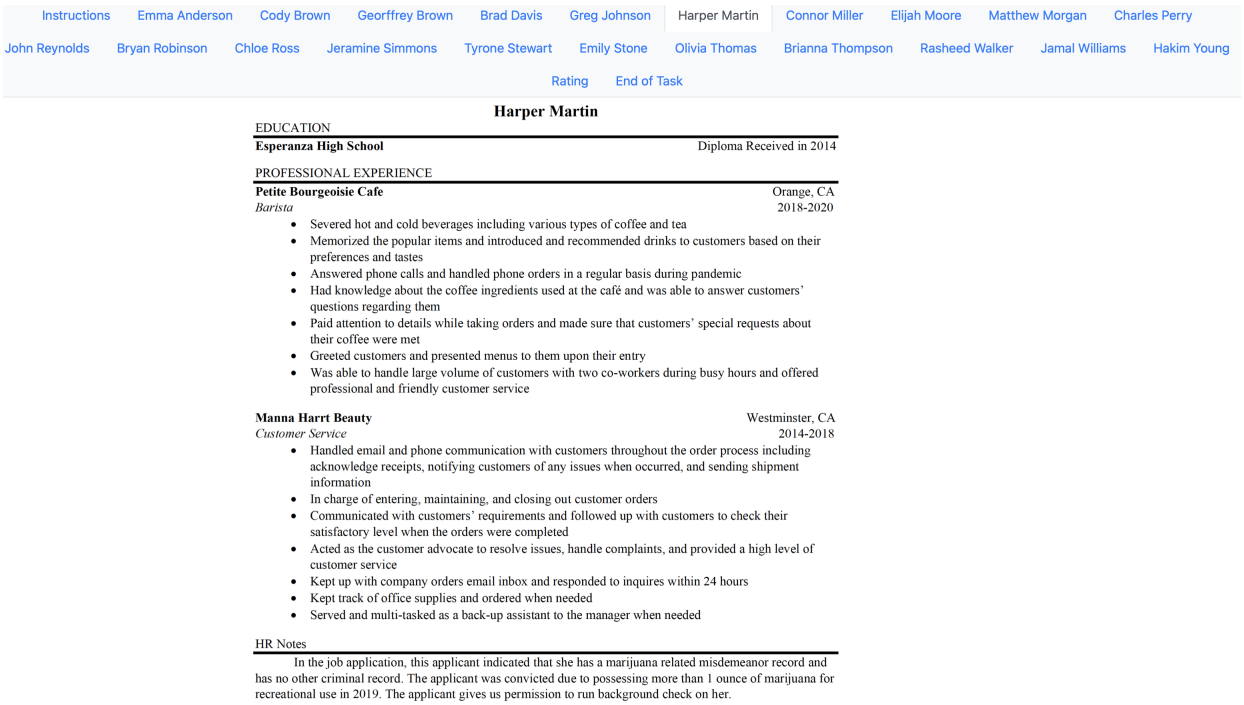

Figure B.3. Sample Resume

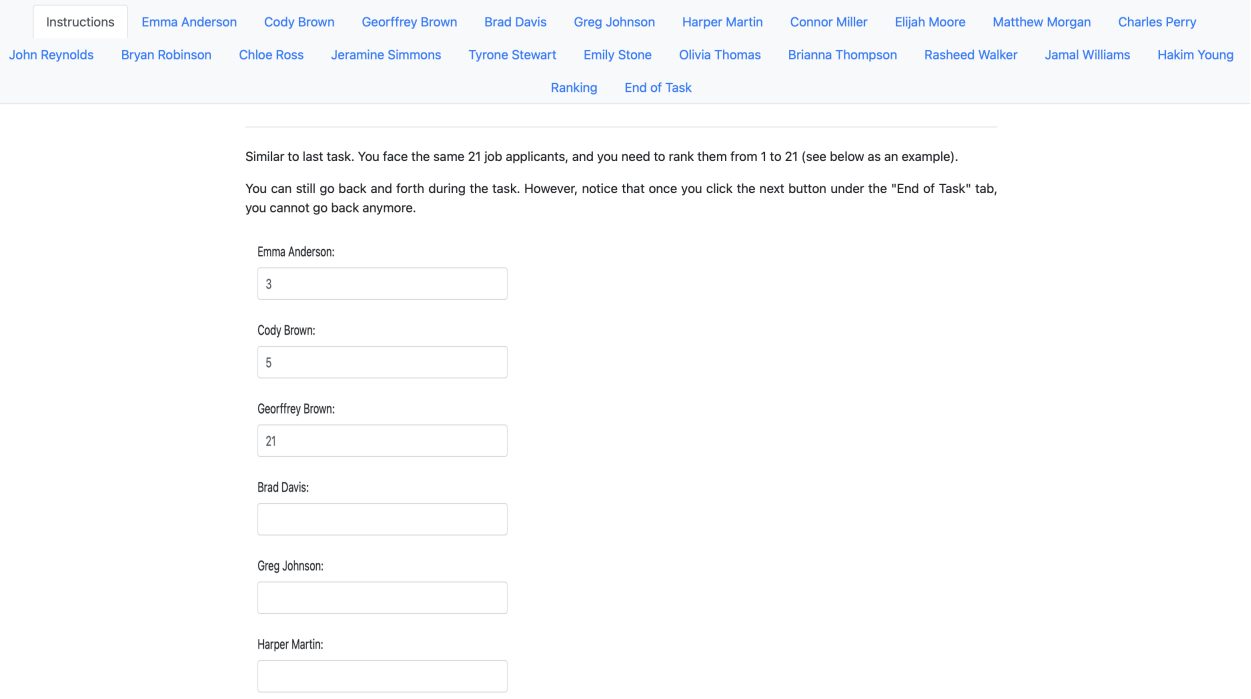

Figure B.4. Ranking Task

C Details of Experiment in Chapter 3

Instruction

Your task is to throw the die and record the number facing up. No one will check or verify the number you record. Given that this is an online experiment, you will throw virtual dice.

Please either use the free virtual die on https://rolladie.net or https://freeonlinedice.com. Please note that I am not the owner, nor do I have any association with these two websites. Therefore, any number you roll on these websites is completely private, and no one will check or verify it.

The experiment consists of 30 rounds, which means you need to repeat the task 30 times. Your payoff will be determined by the experimental software. The experimental software will randomly select 1 of the 30 rounds. If you reported a 6 in the selected round, you will earn $1. If you reported another number (1,2,3,4,5), you will earn $0.

For example, if the experimental software randomly selects round 19, and you recorded a rolled dice number of 6 in round 19, your payoff for the task will be $1. Your total payoff for the entire experiment will be the $10 show-up fee plus a $1 payoff from your task, for a total of $11.

If the experimental software randomly selects round 21 and you recorded a 3, your payoff for the task will be $0. Your total payoff for the entire experiment will be the $10 show-up fee plus a $0 payoff from your task, for a total of $10.

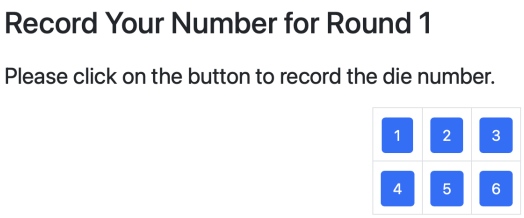

Figure C.1. What subjects see in the experiment

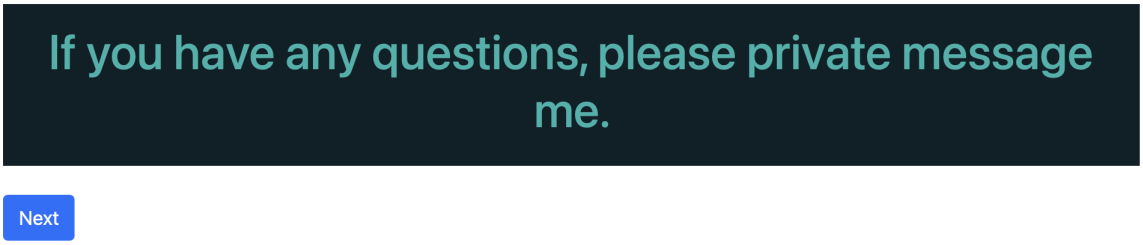

Figure C.2. Example of neutral statement

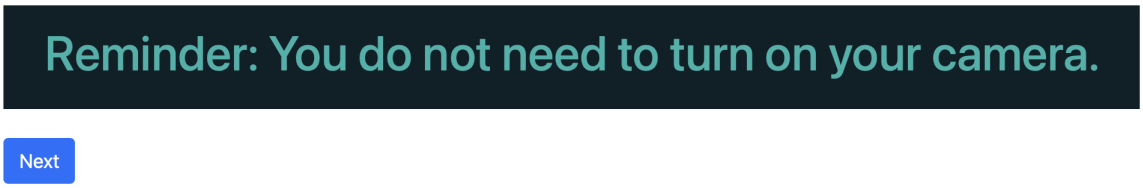

Figure C.3. Example of neutral statement

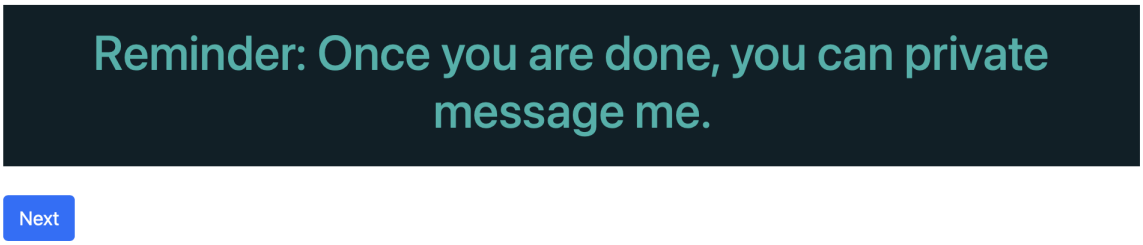

Figure C.4. Example of neutral statement

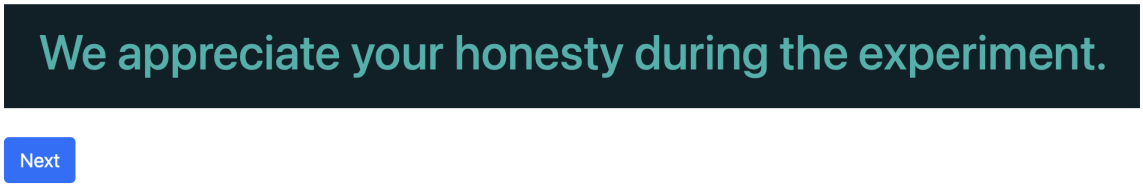

Figure C.5. Example of moral reminder of honesty

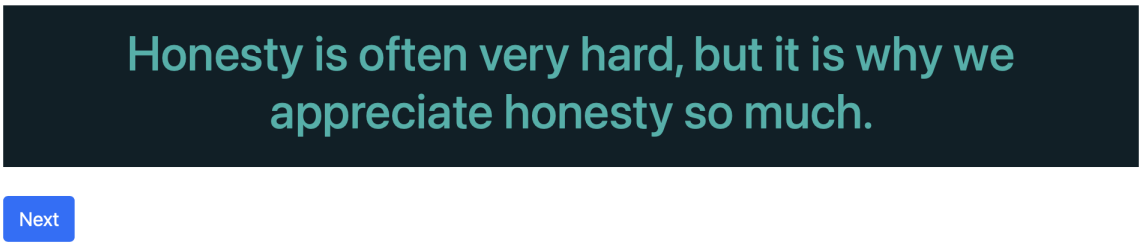

Figure C.6. Example of moral reminder of honesty

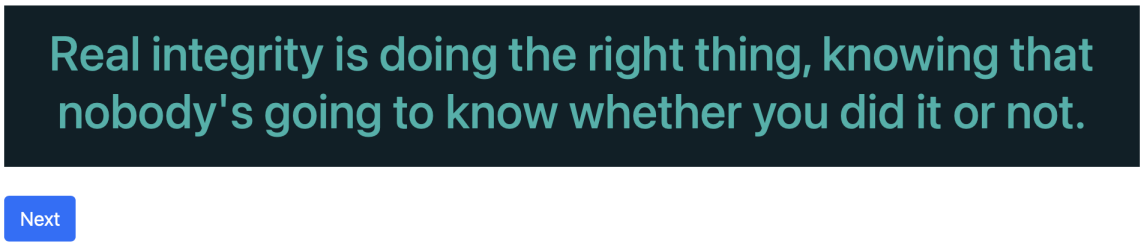

Figure C.7. Example of moral reminder of honesty

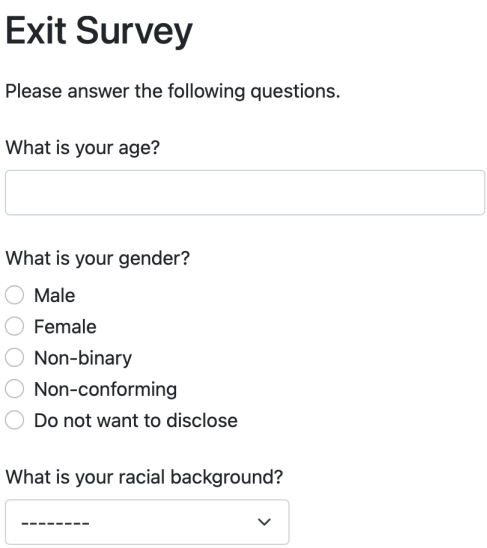

Figure C.8. Exit Survey