Essays on Decision Making in Social Environments

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Dedication

In memory of my father. To my mother for her perseverance. To my grandfather for his inspiration. To my godfather for his guidance. This achievement would never be possible without all of you.

Abstract

Individuals who respond to those that affect them may be influenced primarily by the actions or intentions of those they are responding to or to the outcomes their actions produce. Recent experimental economic studies and predictions adapted from behavioral economic theories of fairness suggest that individuals respond primarily to intentions. In Chapter 1 we present an experiment called the "accountability game". Using the data from the experiment and an alternative framework, we demonstrate and rationalize the opposite conclusion, i.e. outcomes loom larger than actions or intentions in accountability judgments, even when they do not provide information about actions. The experimental design consists of one subject choosing between a lottery and a certain amount on behalf of another subject who responds after the outcome of the choice is revealed. We find that lottery outcomes and not actions or intentions are important in determining the nature of the responses. We investigate a form of fairness termed the *Control Principle* which asserts that we should hold others responsible only for the events which they can control. In our environment first movers cannot control the outcome of the lottery, yet second movers will assign a higher payment to first movers when the lottery outcome is favorable. We find that this "unfair" sensitivity to outcomes is cut in half when second mover responses are elicited contingently before the first mover acts. The pattern of responses in each experimental manipulation can be rationalized by a framework that first considers what is termed as a *salient perturbation* of a decision environment (Myerson 1991) and then applies principal-agent theory to the perturbed environment.

In Chapter 2 we apply the ideas developed in Chapter 1 to data generated in an experimental environment we call the "voting game". In the voting game individuals make a decisions in the context of a group where the payoffs are perfectly correlated. In this context we find that individuals make fewer risky decisions in the context of a group when it is salient that the choice is attributable to them. The data cannot be rationalized by a popular model of social decision theory (Eliaz, Ray, and Razin 2006). The data can be explained by incorporating well known experimental results on social norms and our results on accountability judgments from Chapter 1.

In Chapter 3 we investigate important gender differences in risk-taking and accountability judgments. We find that women are more risk-averse than men and tend to hold others accountable according to an absolute measure of performance. Men on the other hand tend to hold others accountable according to benchmarks less related to performance.

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1 Accountability in Economic Behavior: Decisions vs. Outcomes[1]

1.1 Introduction

Most real-world decisions involve gambles. From the normative point of view, if we are to hold someone accountable for a decision, we should take into account the quality of the inherent gamble and refrain from rewarding or punishing them based on the luck of the ensuing outcome.[2] In order to assess the relevance of this norm, we ask: will people hold others accountable for outcomes that do not depend on their actions? If so, do outcomes matter more than the actions or intentions that lead to them? From the perspective of microeconomic theory the answer to these questions is immediate; it depends on the information available to the individual and the nature of the individual's economic incentives. While these factors are both important and real, a perspective which utilizes them exclusively neglects important behavioral factors.[3] It has been shown that in experimental settings decision makers will—to a significant degree—disregard economic incentives and incur a cost in order to hold the agents whose actions affect them accountable by means of rewards or punishments (Fehr and Gachter 2000). What has not been determined is whether this behavior is predominantly a response to the actions and intentions of other agents or to the outcomes that follow.

A recent study by Falk, Fehr, and Fischbacher (2008) offers a tentative answer using a set of ideas developed from the fairness and reciprocity literature of behavioral economics. In an experimental environment where the actions of other agents are observable and their role in the outcomes that follow are known, Falk et al. show that voluntary actions receive substantial weight and outcomes relatively lit-

[1] Analysis of an experiment conducted jointly with Mehmet Y. Gurdal and Aldo Rustichini

[2] This assumes that the good and bad luck outcomes do not differ with respect to the available rewards and punishments.

[3] Behavioral factors such as social norms, social preferences, cognitive limitations and emotions may be triggered introducing non-economic incentives into a decision environment, which run counter to the prevailing economic incentives.

tle when individuals hold others accountable via costly reciprocation. This result and the behavioral economic theory of fairness which rationalizes it have implications for how decision makers account for actions and outcomes. They predict that for individuals, the impulse to reciprocate either positively or negatively is governed by fairness concerns and is triggered only if they experience outcomes that are determined by the voluntary actions of another agent.

We examine a notion of fairness termed the Control Principle, which supports the view of considering actions rather than outcomes. We develop an alternative viewpoint informed by Roger Myerson's notion of a salient perturbation of a decision problem, principal-agent theory, and a literature from experimental psychology, which challenges the behavioral relevance of the Control Principle. We present an experimental design, detail representative results, and reconcile the predictions that each viewpoint has for subject behavior in our experiment. Finally we discuss economic implications for the behavior observed from subjects.

The Control Principle: a Norm of Fairness

The idea that we should hold others accountable for outcomes only to the extent that those outcomes are under their control we term here the *Control Principle*.[4] Many would consider it unfair and ethically questionable to judge two individuals differently for taking gambles commonly known to be identical but whose realized outcomes differ due only to luck. Although the Control Principle differs from the normative concept of equality in distribution most commonly referred to as fairness in the experimental economics literature, it neatly satisfies the general definition fairness from welfare economics, namely the *equal treatment of equals* principle. The *equal treatment of equals* states "if two persons have identical characteristics in all dimensions relevant to the allocation problem at hand, they should receive the same treatment–the same share of goods, of decision power, or of whatever is being distributed"(Moulin 2003). In order to apply the principle one must first define the domain over which individuals may be assessed to be equals or unequals. In our

[4] The idea as stated has a long history in western philosophical thought dating at least to Immanuel Kant. The term is taken from Nelkin (2004), which contains a discussion on the philosophical problem of moral luck. See Nagel (1979); Williams and Nagel (1976)

case this the domain consists of the quality or desirability of actions. The principle asserts that equals in this domain must receive equal treatment. Therefore, if two agents with identical actions produce different outcomes, they are equals and deserve equal treatment; if one agent whose act may lead to different outcomes in states of world determined by chance, his selves in each state of the world are equals and merit equal treatment. Therefore, fairness when holding someone accountable is equivalent to following the Control Principle.

The Control Principle captures more than the abstract notion of fairness, it appears to be a widely accepted norm of conduct as evidenced by its intuitive appeal, support by prominent moral philosophers, and enshrinement in legal statutes. Adam Smith wrote: "Everybody agrees to the general maxim, that as the event does not depend on the agent, it ought to have no influence upon our sentiments with regard to the merit or propriety of his conduct."[5] This norm is incorporated into legal institutions by restricting individual culpability to those events that can be expected to follow from actions that were taken.[6] In the law of torts, the reasonable person rule dictates, for example, that a physician assessed to have provided an appropriate medical standard of care cannot be held liable for adverse outcomes arising from that care. In the law of trusts, the prudent investor rule dictates, for example, that if the fiduciary of a pension fund is determined to have followed the standard of prudent investing, then the beneficiaries of the fund have no legal recourse in the event of losses.[7] It appears that upon deliberation, people will conclude that when evaluating the decisions of others, it is the quality of the

[5]Adam Smith,"The Theory of Moral Sentiments" (1759). David Hume remarked "After the same manner, when we require any action, or blame a person for not performing it, we always suppose, that one in that situation shou'd be influenc'd by the proper motive of that action, and we esteem it vicious in him to be regardless of it. If we find, upon enquiry, that the virtuous motive was still powerful over his breast, tho' check'd in its operation by some circumstances unknown to us, we retract our blame, and have the same esteem for him, as if he had actually perform'd the action, which we require of him." ()Hume 1898 (1740)

[6]The decision in *United States v. Carroll Towing Co.* 159 F.2d 169 (2d. Cir. 1947) establishes this very norm as the principle in assessing negligence. The decision, known as the Hand rule, in its essence declares that individuals should be judged for the ex-ante expected value of their decision and not ex-post realized value.

[7]See Schanzenbach and Sitkoff (2007) for a discussion.

actions or intentions that is important and not the outcomes which follow them.

The Difficulty of Being Fair

Although the fair treatment demanded by Control Principle may be readily approved of and codified in law, there are significant reasons to doubt that it is descriptive of actual behavior or prescriptive for optimal behavior in the most common of circumstances. Although the legal statutes cited earlier (all relating to the reasonable man rule) indicate that society values the Control Principle, the form the statutes take suggests that they may exist to limit a human tendency to violate it. Adam Smith, though recognizing the Control Principle as a maxim of universal assent, acknowledged this tendency with the observation that "when we come to particulars, we find that our sentiments are scarce in any one instance exactly conformable to what this equitable maxim would direct."[8] This perception of actual behavior is readily confirmed from observational data. Empirical studies suggest that U.S. presidents are evaluated by voters based on economic performance that cannot be credibly attributed to their decisions (Converse 1964). An American CEO whose company performs worse than the industry average is more likely (all else equal) to be removed during industry downturns than booms (Jenter and Kanaan 2008). A study of how anesthesiologists judge the appropriateness of care found that knowledge of the severity of an adverse outcome can influence a reviewer's assessment(Caplan, Posner, and Cheney 1991). These naturally-occurring observations indicate that an individual's lack of control over the realized outcomes which follow his actions may be commonly ignored when holding him accountable for his actions.

Economic theory provides a rational basis for violating the Control Principle in practice. The Control Principle as stated does not provide guidance in the most common circumstance of incomplete information. In the context of the principal-agent problem with adverse selection and moral hazard, the principal is uncertain about an agent's type and actions; therefore, a necessary feature of the principal's optimal behavior is to reward positive outcomes and punish negative outcomes (e.g. see Myerson (1982)). Any additional information that is informative with regard

[8] Adam Smith, ibid.

to the agent's actions will be incorporated into the principal's optimal payment scheme, but paying based on outcomes is never eliminated as long as the principal cannot completely observe the actions of her agent (Hölmstrom 1979). Although there exist practical situations when agent types and actions are considered observable for all parties involved, the notion one is observing all relevant information is illusory. The causal link between an agent's observed type, action and the ultimate likelihood of outcomes is never certain and therefore it is imperative to select and retain agents that understand their environment and to give them incentive to learn from it. With this in mind, types and actions must be defined more broadly to include the internal cognitive states of an agent such as understanding and mental effort, which cannot be completely observed or contracted upon. Having this deeper recognition of uncertainty, we may therefore conclude that we live in a world where adverse selection and moral hazard are ubiquitous and therefore people should account for outcomes, regardless of the actions or intentions.

While Principal-Agent theory may explain behavior in common situations, it cannot on its own explain outcome-based pay in situations where principals *believe* that all the relevant actions of their agent are observable and they are able to contract on those actions.[9] A behavioral link is needed to explain why principals may continue to be inclined to consider outcomes in observable action situations. We conjecture that norms of behavior and emotional responses to outcomes may be adapted to the more common environment of moral hazard. This conjecture captures Myerson's concept of a *salient perturbation* of a decision problem (Myerson 1991). A salient perturbation of a given decision problem is a similar decision problem that people are more likely to frame as the current problem because the perturbation is more like the kind of situations that they commonly experience. If a situation is unusual and there exists a salient perturbation then we should expect individuals to behave as if they are operating in the salient perturbation without strong and clear incentives to do otherwise.[10] It is our contention that the

[9]This assumes: 1) the principal's ability to pay is not affected by the outcome, and 2) the principal does not have any risk sharing motivations, i.e. the principal is risk-neutral.

[10]Notice that this is different from a framing effect or an appeal to subject misunderstanding. The subjects may understand and believe the situation as it really is, but nevertheless behave as if

generic environment of hidden action is the salient perturbation of the principal-agent problem with observable actions.[11] The prominent feature of the optimal contract in the hidden action environment is that principals must force the agent to share more risk by making lower payments to the agent for low outcomes and higher payments for high outcomes in order to give the agent incentive to choose appropriately.[12] As this feature is the norm in the salient perturbation, we expect that individuals may carry with them a behavioral rule that the reward has to depend on the outcome, with the actions or intentions as a secondary matter.[13] We should not expect the consideration of outcomes to disappear entirely unless there are clear and strong incentives to do so.

There is some indication from the psychology literature that individuals will over-emphasize outcomes when evaluating the decisions of others. A behavioral phenomenon termed *outcome bias* (Baron and Hershey 1988), occurs when individuals take into account outcome information in a way that is irrelevant for the particular judgment they are making regarding another's decision.[14] The experimental demonstration of its existence typically involves research subjects reading a hypothetical vignette describing a decision maker choosing between two actions, where one action leads deterministically to an outcome and the other action is followed by two possible outcomes that are random and independent of the given action. The subjects judge the decision maker after reading the vignette. Outcome bias occurs when the judgement is more favorable for a good outcome than a bad

they are in the salient perturbation. Frames and perturbations may interact, the way a situation is framed may call attention to a different perturbation.

[11] By generic we mean of full measure if the Lebesgue measure is applied to the parameter space that governs each instantiation of the problem. For example, in the linear technology framework of Mirrlees 1974, the observable action case happens when the normal error term has zero variance, a measure zero event.

[12] Assuming one-shot and no-recontracting

[13] The rule may be an emotional bias governed by reactions to pleasure and pain which trigger the impulse to blame or praise. See Alicke (2000) for a psychological theory of blame that relates to our current context.

[14] Outcome bias is distinct from the related concepts of the curse of knowledge and hindsight bias as it does not require a revision of the decision problem context.

outcome. In psychology experiments, outcome bias has been observed in studies involving decision quality evaluation (Baron and Hershey 1988; Mowen and Stone 1992; Tan and Lipe 1997), responsibility attributions (Walster 1966), ethicality judgments (Gino, Moore, and Bazerman 2008), blame (Gino, Moore, and Bazerman 2008; Mazzocco, Alicke, and Davis 2004), and punishment recommendations (Mazzocco, Alicke, and Davis 2004). Outcome bias may be moderated somewhat by perceived controllability (Tan and Lipe 1997), but its existence appears significant, substantial and robust to changes in the experimental environment. It has not been demonstrated that outcome bias is robust to an experimental economic environment with real subject interactions and real monetary consequences which affect both parties.

The Experiment

In order to test our conjecture that outcomes will be more prominent than actions, we designed a one-shot two-player experimental game with a first mover, whom we shall refer to as the agent and a second mover whom we shall refer to as the principal. The agent chooses on behalf of the principal and the principal assigns a payment to the agent. The constraints and incentives differ from the canonical principal-agent problem. Each subject is assigned to a unique role of either principal or agent, which does not change. Subjects participate in 10 periods. Principals and agents are matched anonymously each period by the experimenter.[15] The agent decides between a risky and risk-free option on behalf of his principal and the principal observes the agent's action. The risky option consists of a lottery with a low and a high payoff. The risk-free option consists of a certain payoff which is strictly between the low and high payoff of the lottery. In 7 out of the 10 periods one of the options would be strictly preferred by anyone with a reasonable level of risk-aversion. After the agent chooses for the principal, both subjects observe a random device that determines the outcome of the lottery. Next the principal assigns a payment to the agent. It is costless for the principal to assign payment as she transfers money from a "pay it or lose it" account budgeted only for that purpose. This payment system induces risk-neutral preferences for the principal and rules

[15]This removes the participation constraint and reputation effects.

out risk-sharing motivations that could give an incentive for outcome-based pay.[16] The game is implemented under three distinct experimental treatments which differ in the way principals make payments to their agents. In the *ex-post (EP) treatment* the principal assigns payment to the agent immediately after the outcome of the random device is realized. In the *hidden contract (HC) treatment*, before the agent chooses, the principal privately commits to make payments to the agent that are contingent on their own payoffs. In the *revealed contract (RC) treatment* the principal commits to contingent payments and reveals them to the agent before the agent chooses. In both contract treatments principals assign payments based on the payoffs they receive. As the high, low, and certain payoffs all differ, the principal may pay based on the action of the agent by simply assigning uniformly higher or lower payments for the two payoff outcomes of the lottery. In this context, knowledge of the lottery outcome, though hedonically meaningful, is strategically irrelevant for the purposes of designing a contract to implement the principal's preferences.

In each treatment we find that the outcome of the lottery, which possesses zero information value and is governed entirely by luck, significantly and substantially influences payments: that is, principals assign a higher payment if the lottery outcome indicates that the forgone payoff is worse. This *outcome bias* is prominent in the ex-post treatment with outcomes mattering more than actions. Outcome bias is halved (though not eliminated) in the hidden contract treatment. In the revealed contract treatment, outcome bias is also halved and, as expected given the incentives, there is an increase in the frequency with which principals design contracts that provide clear incentives for their agent to choose what the principals want. Finally, we find that the treatments with more outcome biased contracts lead to a higher variance in payoffs to the agent which induces more risk-averse decisions on the part of agents.

[16]The Principal divides assigned money between their agent and a randomly selected agent in the room, this assures that the principal will not simply assign all the money to the agent in order to avoid leaving unassigned money to the experimenter.

Rationalizing the Data

The salient perturbation arguments developed earlier may be used to explain the differences between experimental treatments. We propose that the salient perturbation of the ex-post treatment is a principal-agent problem with hidden actions, and the optimal contract in this environment (or the implicit contract from the repeated game) explains the qualitative properties of principal behavior. The ex-post treatment is like a hidden action environment because principals are not queried to evaluate and respond until the payoff is realized, well after the agent acts and the die is thrown. In this situation the action of the agent is not salient and thus principals may respond spontaneously to their payoffs treating them as indicators of good or poor performance from their agent. Principals may exhibit decreased outcome bias in the ex-ante contract treatments because the task of constructing the contingent payment scheme forces them to consider the agent's decision problem before they act and feels more like a situation where the principal is paying for an agent to perform a task, not deliver an outcome. The salient perturbation of the contract treatments may therefore be closer to the principal-agent problem with observed actions, which would explain, as an incidental result, the increased level of fairness in the contract treatments. Extrapolating the results, we suggest that increasing the salience of incentives by, for example, having the principals compete for agents who may reject contract offers would create an environment identical to the observable action principal-agent problem with participation constraints and may lead to complete fairness on the part of principals. Thus, eliciting fair behavior from individuals may require institutions which either give incentive to do so or enforce it by statute.

Reconciling our Results with the Fairness Intentions Literature

The predictions inspired by theories of fairness in the behavioral economics literature and suggested by recent experimental results do not hold in our experimental environment. In the experiments of Falk, Fehr, and Fischbacher (2008) and Charness (2004) subjects will incur a cost to respond to the voluntary actions of other subjects, but are unwilling or only marginally willing to do so when the actions of the other subjects are determined by a random device.[17] We propose that it is the passive nature of agents whose actions are determined by a random device that explains why the fair behavior exhibited in their environment does not carry over into our environment. The passiveness of the first mover creates a context that is similar to a dictator game for the second mover who is the only active subject that can determine the payoffs of each player. In Falk, Fehr, and Fischbacher (2008) the role of the second mover in the random device treatment is that of a dictator with a choice set that allows for taking and giving, a context which has been shown to reduce giving in a dictator game (List 2007). The lack of reciprocation for consequential outcomes that are not voluntarily determined by an agent can therefore be adequately explained by appealing to the contextual role of the choice set without appealing to the role of fairness. As these environments do not address situations when actions are voluntary but may generate several possible outcomes, they do not provide a basis for appealing to fairness when explaining behavior of principals in our environment. In a recent study, Charness and Levine (2007) provide an experimental environment closer to ours which allows the consequences which follow the voluntary action of a first mover to be determined by a random device.[18] In this context first movers can engage in a high sacrifice or low sacrifice action that are each costly and observable. The random device determines a good luck or bad luck scenario. The second mover receives the same payoff from high sacrifice/bad luck and low sacrifice/good luck. The second movers sacrifice significantly and substantially more of their payoff in the high sacrifice/bad luck condition even though the payoffs are the same. This indicates that intentions and actions matter more than outcomes. Although we find that outcomes matter more than actions, we don't believe that our results contradict this result. Our study

[17]Charness (2004) considers a wage/effort gift-exchange game. If the first mover employer delivers a low-wage, employee responders exhibit costly effort less often if the low wage was deliberately chosen than if it was determined by a random device. Within the random wage treatment, effort level did increase with wage, but the basic structure of the game could induce a form of gains sharing norm and subject cohort versus experimenter effects due to the low cost of effort relative to the gains received by the employer and the potentially wasted surplus left to the experimenter if effort is not given (efficiency motive).

[18]This study was published after half of the experimental sessions were complete

adds an important dimension to the interpretation of the fair intentions results and their applicability more generally. In our study the principal can evaluate if the agent's action was desirable or of high quality since the action and decision problem are perfectly observable. Although we find that outcomes matter more than actions, the difference comes from the fact that in our experiment the principal cannot observe the intentions, effort or understanding the agent has of the decision problem because the agent's choice does not involve a costly and observable sacrifice as in Charness and Levine (2007). Although intentions, effort and understanding are not in principle relevant for evaluation in our context, we contend that they are relevant more generally so that the optimal responses in these environments carry over into ours. Conversely, as irrelevant outcome information was considered in our environment and was weighted more than action information, we expect outcomes to be over-emphasized in more common environments, revealing a difficulty of being fair.

Contributions and Economic Implications

It is claimed that actions and intentions matter more than outcomes in governing the impulse to respond to others whose actions affect us (Charness and Levine 2007; Falk, Fehr, and Fischbacher 2008). We test the robustness of this claim to changes in the role that agency plays in determining outcomes. By manipulating the link between actions and outcomes so that the outcomes which follow actions are probabilistic rather than deterministic, we find that individuals will be sensitive to outcomes under circumstances when only actions are relevant for the purposes of determining a fair response. In this context, individuals will fail to be fair even when it is costless to do so. These results extend the results of the outcome bias literature in psychology to experimental economic settings. With real subject interaction and real monetary consequences for their partners there is a possibility that subjects will focus more on the task at hand and exhibit more concern for the real feelings of fairness their partners experience and the payoffs their partners receive. Instead we find that outcome bias persists at a level that is qualitatively equivalent to earlier results in the psychology literature. In addition, the existence of outcome bias in the contract treatments and their contingent nature provides the first true demonstration (to our knowledge) of the distinction between outcome bias and the related phenomenon of hindsight bias and the curse of knowledge.

The fact that outcome bias appears difficult to resist and leads to more cautious decision making on the part of the recipients of the bias has important economic implications. A study by Del Guercio (1996) shows that fiduciaries who manage trust funds and pension funds make more risk-averse decisions than managers of mutual funds with identical objectives. The difference arises from prudent man laws, which apply to trust fund and pension fund managers but not to mutual fund managers. A beneficiary's decision to litigate is not simply a rational cost-benefit calculation; it involves the feeling of being wronged and the impulse to blame, which are both subject to outcome bias. The evidence presented to a judge in a litigation case includes both the action of the manager and the performance outcomes of the fund; therefore, the judge's decision is sensitive to outcome bias when evaluating the managers' actions with the necessarily imprecise standards of prudent investment prescribed by the law. It is the fear of successful litigation, an event subject to outcome bias, which leads some fiduciaries to be excessively risk-averse and to an inefficient allocation of financial capital in the economy. These inefficiencies may be present more generally. Therefore, in organizations or environments where a standard of best practices exists it may be important to get people to hold others accountable for how well their actions adhere to best practices, rather than the consequential outcomes that follow. In light of our results, this will be difficult without the appropriate enforcement mechanisms or incentives to guide behavior.

Overview

The paper is organized as follows. The next section presents the experimental design and procedures. Section 3 discusses the predictions and the results. Section 4 provides a discussion and conclusion.

1.2 Experimental treatments and procedures

Our experimental design for each two-player treatment is a restricted version of the principal agent problem with observable agent actions, costless decisions on

Table 1: The Binary decision problem for each period

	Risky Option (R)			Risk-Free Option (Rf)
Period	high	probability high	low	certain
1	\$30	0.25	\$0	\$9
2	\$20	0.50	\$0	\$10
3	\$20	0.25	\$0	\$4
4	\$10	0.75	\$0	\$7
5	\$30	0.50	\$0	\$15
6	\$20	0.25	\$0	\$8
7	\$10	0.25	\$0	\$5
8	\$30	0.25	\$0	\$12
9	\$20	0.50	\$0	\$6
10	\$10	0.25	\$0	\$3

the part of the agent, costless contracting on the part of the principal, and no participation constraint[19]. Each treatment consists of 10 periods with each period involving a choice stage where a subject makes a decision $D \in \{R, Rf\}$, where R is a risky option and Rf) is a risk-free option. The risky option consists of a lottery $L \in \{h, \ell\}$, where h is the high outcome with probability p and $\ell = 0$ is the low outcome with probability $1-p$.[20] The risk-free option consists of a certain payment c. The probability and high outcome of the lottery and the certain payment vary across periods, with $h > c > \ell = 0$ and $p \in \{.25, .5, .75\}$. The outcome of the lottery is determined by a 4-sided physical die. Each period's choice stage is fixed and identical across all subjects, all treatments, and all sessions and they are listed in Table 1.

The outcome of the risky option in a given period is determined by a single physical 4-sided die for all subjects. In order to increase payoff diversity for risky options, the mapping between die numbers and payoffs was randomly permuted across subjects so that realized outcomes vary across subjects[21]. The lotteries were

[19]It must be emphasized that this is not the canonical principal-agent problem from contract theory. The terms principal and agent are used here as they most appropriately describe the subjects' constituent roles in the experiment

[20]In the experiment the risk-free option was named the safe option

[21]For example, in period 1 all subjects have the choice between \$9 and (\$30, .25; \$0, .75), but a given four subjects may each see a unique representation of the lottery: for the first subject die

designed so that nearly risk-neutral subjects would predominately pick the risk-free option in Periods $1, 6, 7, 8$ and the risky option in Periods $3, 4, 9$ and would be nearly split between risk-free and risky for Period $2, 5, 10$.[22].

1.2.1 Ex-post (EP) Treatment

At the beginning of each period, subjects are matched with a new partner randomly and anonymously without repetition for 10 periods.[23] In the first stage of a period the agent chooses between a risky option and a risk-free option for the principal, who is idle at that stage. In the second stage the principal observes the decision of the agent $D \in \{R, Rf\}$ and a die is thrown so all subjects may view the die and infer the outcome of their lottery $L \in \{h, \ell\}$. The principal wins \$$c$ if the agent chooses $D = Rf$, if the agent chooses $D = R$ then the principal wins \$$h$ if the lottery is high and \$0 if the lottery outcome is low. In the third stage of the period the winnings are presented on the screen of the principal and the principal, having seen the decision and outcome (D, L), is prompted to assign a payment \$$\pi_a(D, L) \geq 0$ to the agent and \$$\pi_{ro}(D, L) \geq 0$ to a random other agent in the room, where $\pi_a + \pi_{ro} \leq 15$ with any unassigned money kept by the experimenter.[24] Next the agent views the payment for the principal. Finally, both

face 1 may yield \$30 with the other die faces yielding \$0, for the second subject die face 2 may yield \$30 with the other die faces yielding \$0, etc.

[22]Research subjects tend to be risk-neutral for these amounts

[23]Anonymity and preventing subjects from being paired again with the same partner was important so as to rule out reputation effects.

[24]The Principal essentially divides \$15 between her agent, a random different agent and the experimenter. The randomly selected agents find out about their payment at the end of the experimental session. If we did not include the random agent in the design we could have induced a subject versus experimenter effect– principals may become inclined to assign all \$15 in each period to their agent and fellow undergraduate rather than let the experimenter keep any money. If we allowed principals to assign their own money or keep any unassigned payments, wealth effects from variable winnings would make the results difficult to interpret, decisions motivated by risk-sharing could cloud the role of incentive motivations and other factors, and, of course, principals would have even less incentive to assign non-zero payments. Although there are no financial incentives in the EP-treatment for principals to act in any particular way, we believe that the existence of real monetary consequences for both principal and agent adds motivation that a vignette based approach cannot provide.

agent and principal rate how they feel about their partner's decision on a $1-10$ scale from very bad to very good.

1.2.2 Hidden Contract (HC) Treatment

Subjects are paired randomly and anonymously without replacement for 10 periods. In the first stage the agent chooses between the risky option and the risk-free option for the principal while the principal simultaneously completes a hidden contract, which specifies how to assign the $15 to the agent and the random other for each *payoff contingency* of high, low and certain, i.e. the principal chooses $\pi_a(D, L), \pi_{ro}(D, L)$ for each $(D, L) \in \{R, Rf\} \times \{h, \ell\}$ such that $\pi_a(D, L) + \pi_{ro}(D, L) \leq 15$ and payments are contingent on payoffs only constraining the payments when safe is chosen to satisfy: $\pi_a(Rf, h) = \pi_a(Rf, \ell)$ and $\pi_{ro}(Rf, h) = \pi_{ro}(Rf, \ell)$.[25] In the second stage the principal observes the choice of the agent and rates how she feels about it on a scale of 1 to 10 while the agent observes the portion of the principal's contract still relevant after his choice and rates how he feels about it on a scale of 1 to 10. Finally, the die is thrown in front of all subjects, the lottery outcome is revealed, and the payoffs are displayed.

1.2.3 Revealed Contract (RC) Treatment

Subjects are paired randomly and anonymously without replacement for 10 periods. In the first stage the principal completes the payoff contingent contract just as in the HC-treatment while the agent waits. In the second stage the contract is revealed to the agent before the agent chooses between the risky option and the risk-free option for the principal. Next both agent and principal rate how they feel about their partner's decision on a scale from 1 to 10. Finally, the die is thrown so all subjects may view the outcome and the payoffs are displayed.

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[25] Having subjects pay contingent on the lottery outcome when safe is chosen seemed unnatural with a potential to lead to demand effects.

1.2.4 Alone (A) Treatment

In every experimental session, each subject participates in this treatment as the third and final treatment of the session. Subjects are not paired, there are no roles, and they choose independently only for themselves. Over the course of 10 periods subjects choose between the same risky and risk-free options viewed in the previous treatments (with the same order). The payoffs are not determined while the subjects choose. In addition to the payoff selected from the other treatments, a single period is chosen to count for payment from this treatment with a physical die thrown at the end of the experimental session.

1.2.5 Procedure and Methods

The experiment was programmed and conducted with z-Tree experimental software (Fischbacher 2007). For each experimental session we visited the last 5 minutes of a 200-500 student principles of economics class and informed the students that they may participate in a 2 hour economic choice study beginning immediately. They were also told that they may receive between $8 and $70 for their participation, with more than half of the subjects receiving between $20 and $40. Typically, 30-40 subjects were recruited and walked over with the experimenter to the Social and Behavioral Sciences Laboratory (SBSL) at the University of Minnesota. Upon arrival, students were seated and given consent forms and payment receipts[26]. The subjects were seated at individual computer carrel booths that isolated their screens from the view of other subjects. Subjects were informed only that they would participate in three 10-period treatments, after which they would be requested to respond to feedback questions and finally paid. Subjects were told that we would be randomly selecting (with physical dies) one decision period from the first two treatments and one decision period from the final treatment to count for payment. The session began by privately and permanently assigning students to the role of either principal or agent and then commencing the first

[26]see supplementals for details

treatment.[27] Each treatment was preceded by flash video instructions presented individually with headphones streaming via a web browser and distribution of a sheet presenting a summary of instructions. Next, using z-Tree experimental software we ran $1-2$ periods of practice, and questions (with individualized feedback) to check for understanding of instructions — no details regarding later treatments were given. After these preliminaries we launched the $15-25$ minute treatment in z-Tree. A total of 210 subjects (105 principals and 105 agents) participated, with each subject participating in one of the following three experimental sessions: 1) The EP/HC-session, where the order of treatments were the EP, HC, followed with the A-treament (70 subjects, 2 days); 2) HC/EP-session where the order of treatments were the HC, EP, followed with the A-treament (82 subjects, 4 days)[28]; 3) The RC/HC-session where the order of treatments were the RC, HC, followed with the A-treatment (58 subjects, 2 days). When the treatments were completed the subjects typed open-ended feedback to questions and then were paid.

1.3 Results

1.3.1 Estimating Preferences

In each experimental session subjects chose for themselves in the third and final experimental treatment. In the HC-treatment (which is present in all experimental sessions) in each period the principal rates how they feel about their agent's decision after the agent chooses but before the outcome is revealed. The behavior of principals and agents in the A-treatment, and each principal's rating of their agent's choice in the HC-treatment will serve as a benchmark for measuring the desirability of the risky option relative to the risk-free option in each period. This measure of desirability will play a central role in our analysis presented in the ensuing sections.

We measure the index of desirability for the risky option on the $(0, 1)$ scale,

[27]The subjects' roles were named rule chooser (principal) and option chooser(agent) in the actual experiment

[28]One day of the HC/EP-session had only 6 subjects. We can guarantee anonymity in this case but we cannot prevent repeated pairing. Results were significant with or without this data.

term it the *risky index* and treat it as a ratio scale variable. *Risky index* is the strength of preference for the risky option.[29] We measure the *risky index* using four approaches, one is individualized and the other three are population-wide measures. Each approach yields an estimate of *risky index* for each period, which we may interpret as the probability of choosing the risky option in that period. Each approach yields a comparable estimate and these are reported in Table 2.[30]

Individualized Measure

For each decision period in an experimental session the principal rates how she feels about her agent's choice in the HC-treatment before the outcome is revealed on a scale from 1 to 10, in addition she makes a separate decision between the risky and risk-free for herself in each period of the A-treatment. The individualized measure of *risky index* for a given period t combines the principal's ratings of how they felt about the agent's choice in the HC-treatment for that period with what the principal actually chose in that period in the A-treatment. Let the variable *risky choice* be defined to be 1 if the risky option is chosen and 0 if the risk-free option is chosen. Let the variable *feel choice* be defined to be the principal's rating of their agent's choice in the HC-treatment on a scale from 1 to 10. An individual principals i's *risky index* variable in period t is defined as followed:

$$\textit{risky index}_{i,t} := \begin{cases} \frac{1}{2}\textit{risky choice}^{\text{A}}_{i,t} + \frac{1}{2}\frac{\textit{feel choice}^{\text{HC}}_{i,t}}{10} & \text{if } D^{\text{HC}}_{a(i),t} = R \\ \frac{1}{2}\textit{risky choice}^{\text{A}}_{i,t} + \frac{1}{2}\frac{10-\textit{feel choice}^{\text{HC}}_{i,t}}{10} & \text{if } D^{\text{HC}}_{a(i),t} = Rf \end{cases}$$

[29] We do not include the end points because, for technical reasons, in later sections for models of principal payments we will be creating an interval scale variable *risky desirability* which is defined as the log-odds of the *risky index*

[30] Note these estimates are unlikely to be precise measures of the risk aversion of individual agents. The lotteries are not paired with certain payments on a fine enough grid to elicit certainty equivalents precisely and therefore the most likely risk aversion parameter in the MLE procedure described in the current section will be sensitive to the probability structure imposed on the decision.

where $D^{\text{HC}}_{a(i),t}$ is the choice of principal i's agent, $a(i)$, in period t of the HC-treatment.[31] Notice that *risky index* may take on values zero and one, therefore to avoid taking log(1/0) when we perform log-odds transformations in later sections we record and report $0.5 + (0.0495/0.05) * (\textit{risky index} - 0.5)$ here instead. The average values of *risky index* for each period are reported in Table 2 in the first row under "individual risky index".[32]

Population Measure

An alternate approach to measuring the desirability of the risky option is to measure the average tendency of choosing the risky option. The most obvious measure is to record the frequency with which subjects chose the risky option in each period, this is reported in the second column of Table 2. The remaining measures we estimated the probability of choosing the risky option based on a Bernoulli probability model of choice that parameterizes the choice probability according to specific structure, that is:

$$\textit{risky choice}_t := \begin{cases} 1 & \text{with probability} \quad F(\phi(\boldsymbol{\beta}, \boldsymbol{x}_t)) \\ 0 & \text{with probability} \quad 1 - F(\phi(\boldsymbol{\beta}, \boldsymbol{x}_t)) \end{cases}$$

Where $\boldsymbol{x}_t$ is a column vector consisting of the scalar 1 and the values of the independent variables in period t. The independent variables are the high payoff from the risky option (h), it's probability (p) and the payoff from the risk free option (c). The vector $\boldsymbol{\beta}$ consists of the parameters of the model, and both ϕ and F are a functions. The parameters $\boldsymbol{\beta}$ are estimated using the maximum likelihood procedure.

One specification is to have a linear ϕ with $\boldsymbol{\beta}$ as a column vector of coefficients

[31] Many subjects have a tendency to assign around 5 to decisions they don't like and around 10 to decisions they like, which means that the choice of their partner can affect their rating as measured by our index. For example if they like the risk-free option in a particular period, then if their partner chooses risky and they rate the choice as a 4 then we would record 4 as part of our risky index measure, but if their partner chooses the risk-free option they would rate the choice a 9 and we would record $10 - 9 = 1$ as part of our risky index.

[32] notice:the risky option is desirable in periods 2,4,9 and the risk-free option is desirable in periods 1,6,7,8, while in the remaining periods the subjects are divided in their preferences.

Table 2: Different measures of *risky index*

	Period									
	1	2	3	4	5	6	7	8	9	10
indiv. risky index	0.142	0.663	0.476	0.859	0.429	0.0998	0.107	0.101	0.851	0.353
risky frequency	0.129	0.719	0.538	0.919	0.405	0.0429	0.0667	0.0476	0.952	0.414
linear-logistic (re)	0.164	0.680	0.489	0.997	0.304	0.0670	0.0874	0.0274	0.966	0.259
linear-logistic (pa)	0.244	0.694	0.541	0.992	0.384	0.126	0.156	0.0628	0.949	0.345
crra-logistic (cls.)	0.158	0.459	0.689	0.604	0.439	0.0460	0.0755	0.0110	0.974	0.361

and a logistic (sigmoid) F, so $\phi(\boldsymbol{\beta}, \boldsymbol{x}_t) := \boldsymbol{\beta}'\boldsymbol{x}_t$ and $F(\phi) := 1/(1 + e^{-\phi})$. This is a logistic choice model. We may use the estimates of $\boldsymbol{\beta}$ to compute the probability of choosing risky in each period. As we have repeated measures we can do this in two ways, (1) add random individual effects in estimating $\boldsymbol{\beta}$ and predict the probability of choosing the risky option in each period under the assumption that the random effects are zero, , these predictions are reported in the third row of Table 2, (2) add fixed individual effects with population-averaged coefficients in estimating $\boldsymbol{\beta}$ and predict the probability of choosing the risky option in each decision period using these parameters, these predictions are reported in the fourth row of Table 2.

A second class of specifications involves modeling the Bernoulli probability of choosing the risky option as a function of the expected utility of each option. In this model the utility function has parameters $\boldsymbol{\beta}$ and the explanatory variables $\boldsymbol{x}_t$ are as before. We employ a constant relative risk-aversion (CRRA) utility function $u(x) := x^{1-\sigma}/(1-\sigma)$, so that $\boldsymbol{\beta}$ consists of the single parameter σ. In the Logistic and Probit version of this model ϕ is the expected utility premium of choosing risky $\phi(\boldsymbol{\beta}, \boldsymbol{x}_t) := Eu(L) - u(c)$, and F is equal to the the logistic function and cumulative normal distribution respectively. In the Luce Ratio version of this model $\phi(\boldsymbol{\beta}, \boldsymbol{x}_t) = Eu(L)/(Eu(L) + u(c))$ and F is the identity function. The predictions from the Logistic CRRA model are reported in the fifth row of Table 2. In all models we cluster for repeated observations.

1.3.2 Principal behavior: ex-post treatment

We expected outcome bias to manifest itself in the ex-post (EP) treatment with the following pattern of payments by the principal to the agent:

$$\pi_a(R, h) > \pi_a(Rf, \ell) > \pi_a(Rf, h) > \pi_a(R, \ell) \tag{1.1}$$

The middle inequality indicates outcome bias if the risk-free option is chosen, while the indirect inequality between the far left term and far right term indicate outcome bias if the risky option is chosen. The two types of outcome biases we expected are evidenced in the summary statistics of Table 3. As predicted the average payment for each decision and lottery outcome pair (D, L) satisfy the predicted ordering in expression (1.1) with the highest average payment of \$12.22 going to the agents who chose a winning risky option, the second highest average payment of \$10.50 going to the agents who chose the risk-free option forgoing a losing risky option, the third highest average payment of \$8.59 going to the agents who chose the risk-free option forgoing a winning risky option, and the lowest average payment of \$4.31 going to the agents who chose the losing risky option. Using a two-tailed Mann-Whitney U-test we find that all six pairwise tests on the payments in the four possible decision and lottery conditions indicate the strict ordering observed in the summary statistics with max{p-value} < 0.0004. Although important, these summary statistics as well the Mann-Whitney test-statistic confound the role of the amount won and the desirability of the risky option versus the risk-free option, as these factors vary between periods. In addition, independence is violated as these statistics include data from subjects with observations in multiple periods.

Restricting analysis to a single decision period we do not have to worry about other factors such as the payoff and decision quality variation between periods influencing the mean payments as the decision problem is the same for all subjects in a given period. Moreover we should not expect any dependence between the four decision and lottery outcome groups as subjects only have one observation per period and they are placed in the groups by the decision of their randomly matched agent and the random outcome of a lottery. Conducting the same Mann-

Table 3: Summary Statistics for payments to partner by decision and lottery outcome

		Payment to Partner		Payment to other		Total Payment	
		High	Low	High	Low	High	Low
Risky	Mean	12.22	4.31	2.18	7.22	14.40	11.53
	S.D.	3.30	4.86	2.89	5.71	1.83	5.58
Risk free	Mean	8.59	10.50	5.16	3.02	13.76	13.52
	S.D.	3.80	3.95	3.65	3.11	2.92	3.33

	Number of Observations	
	High	Low
Risky	149	176
Risk free	116	319

Whitney U-test on individual periods to obtain a more accurate test of outcome bias. Although we have 76 principal observations for each period, only periods 3 and 5 have more than 9 observations for each (D, L) pair, therefore we restrict our tests to these periods only and we see that the ordering of expression (1.1) is preserved.[33] In period 3 the tests indicate that the first two inequalities of expression (1.1) are strict and the last one is weak– five out of six pairwise tests are significant at the 0.025 level, where on the sixth test we cannot reject the hypothesis that $\pi_a(Rf, h) = \pi_a(R, \ell)$. In period 5 the tests indicate that the first inequality of expression (1.1) is weak and the last two are strict– five out of six pairwise tests are significant at the 0.025 level, where on the first test we cannot reject the hypothesis that $\pi_a(R, h) = \pi_a(Rf, \ell)$.[34]

[33]This should not be a surprise. We do not have enough observations to test for significance in the majority of periods because, as experimenters, in a given period we cannot control the decisions of agents and the outcomes of lotteries. We have enough observations for periods 3 and 5 because we should expect subjects to be indifferent on average between the two options in these periods and thus choose them equally often in the EP-treatment (as can be seen in Table 1 the frequency with which subjects choose either option is close to even in the A-treatment) For comparison, in period 1 there are no observations for (R, h) and in period 9 there are no observations of (Rf, ℓ). **See Appendix for separate tests conditional on risky or risk-free**

[34]The asymmetry between periods 3 and 5 for which the inequality in expression (1.1) is significant with the pairwise test points to a factor that will be accounted for in the regressions later. In period 3, the winnings for the principal in the four (D, L) conditions are $\pi_a(R, h) = \$20, \pi_a(Rf, \ell) = \$4, \pi_a(Rf, h) = \$4$ and $\pi_a(R, \ell) = \$0$ respectively. Perhaps the payments to the agent in period 3 do not differ significantly between conditions (Rf, h) and

These results largely replicate the Baron and Hershey (1988) results, but this time in a situation where the subjects are actual participants who are paid and assign real money based on the outcomes and thus the consequences for their bias are real.[35]

EP-treatment: Regression analysis of the outcome effect

In this section we measure more precisely the role of outcome bias in the principals' assignment of payment to their agents and compensate for the limitations of the non-parametric tests of the previous section. We analyze principal behavior from all four decision and lottery outcome conditions in a single regression model that controls for the relative attractiveness of the options and the winnings of the principals. The regression model below indicates that outcome bias is significant, substantial, and in the predicted directions for both the risky and risk-free decisions.

As we have repeated observations from subjects, we employ a panel data regression model. Let $\pi_{i,t}$ be the payment assigned by the ith principal to her agent in period t. We estimate the following model:

$$\pi_{i,t} = \boldsymbol{\beta}'\boldsymbol{x}_{i,t} + \eta_i + \varepsilon_{i,t}$$

where $\boldsymbol{x}_{i,t}$ is a column vector consisting of 1 and the independent variables, η_i represents the latent individual characteristics of subject i, and $\varepsilon_{i,t}$ is the error term

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(R, ℓ) because in both conditions the counterfactual decision is better and the winnings are both low, while the payments to the agent do differ significantly between conditions (R, h) and (Rf, ℓ) because the winnings are high in the first condition and low in the second, which overwhelms the fact that the decision in each condition is better than the counterfactual decision. The same reasoning applies to period 5, indicating that principals may pay attention to their payoffs relative to that of their agents.

[35] At this point people may object that it is inequity aversion or envy that is driving this. There is some support for this since no matter how you slice it or dice it, between 25% and 50% of subjects (depending on context) will not give more to their partner or random other than they win (if we condition on periods where they can actually give more than they won). But note that we have eliminated the role of risk-sharing, and we have seen 50%+ give more than they won when they don't have to. Moreover, and most importantly, the outcome bias (blame) presents itself when safe is chosen, which is a pure measure of this effect.

with the appropriate gaussian distribution assumptions. Unless stated otherwise we assume that the latent effects η are random, with mean zero, and uncorrelated with $\boldsymbol{x}$ and ε, i.e. a random-effects model.[36]

Our independent variables are the following: *risky desirability*, the log-odds transformation of the unit interval variable *risk index* measured in various ways (described in Section 3.1) in order to capture numerically the relative attractiveness of the risky option versus the risk-free option in a given period[37]; *lottery high*, a dummy variable equal to one if the die face indicates a high outcome for the lottery; *payoff displayed*, the winnings of the principal displayed on the screen before the principal assigns the payment to the agent; *risky choice*, a dummy variable equal to one if the principal's partner decides to choose the risky option; the three interaction terms of *risky choice* multiplied by the other variables.

In Table 4 we present the results from our model of principal payments to their partner for two difference measures of *risky desirability*. In the left three columns of the table we present a random effects model where *risky desirability* is a population measure (and thus independent of the random individual effects) that is computed as the log-odds of the frequency with which risky was chosen in the A-treatment by principals across all experimental sessions. The regression model with the individualized measure of *risky desirability* as an independent variable is presented in the second three columns of Table 4, note that we have used fixed effects as we expect there to be an interaction between the latent individual characteristics η and the individualized measure *risky desirability*.[38] Notice that

[36]There is every reason to believe that latent individual effects are independent of the explanatory variables in most of our estimated models as the individuals have no control over explanatory variables in these cases.

[37]It is arbitrary whether we measure *risky desirability* or *risk-free desirability*. Although the unit interval representation of *risky desirability* with the *risky index* variable is the most natural and easy to interpret, we must use the log-odds transformation of the unit measures. The unit measure representation of *risky index* or *risk-free index* sum to 1, therefore in that representation *risk-free index*$= 1-$*risky index*. As their distributions on $[0, 1]$ differ, if we used the unit measures we may get erroneous significance or insignificance in a regression model with interaction effects depending on whether we decide to use *risk-free index* or *risky index*, as zero values on indicator interaction variables can zero out different sides of the distribution. If we perform a log-odds transformation we get identical results from both measures.

[38]The random effects model did not differ here for this measure of *risky desirability*

Table 4: Regressions with pooled data; $N = 760$, $I = 76$

	Random Effects (pop.)			Fixed Effects (indiv.)		
	R^2 .40	χ^2 676.29		R^2 .48	F 110.45	
Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value
constant	6.601 0.491	13.451	0.000	6.584 0.391	16.822	0.000
risky choice	-2.605 0.502	-5.194	0.000	-2.604 0.467	-5.580	0.000
risky desirability*	-0.437 0.099	-4.394	0.000	-0.386 0.071	-5.456	0.000
risky desirability* × risky choice	1.130 0.158	7.138	0.000	1.042 0.100	10.448	0.000
lottery high	-1.559 0.363	-4.296	0.000	-1.275 0.353	-3.616	0.000
lottery high × risky choice	6.050 0.947	6.391	0.000	5.467 0.894	6.112	0.000
payoff displayed	0.357 0.042	8.501	0.000	0.364 0.040	9.030	0.000
payoff displayed × risky choice	-0.206 0.061	-3.383	0.001	-0.200 0.058	-3.432	0.001

*Measurement depends on the regression:

- For the random effects regression: the population measure, log-odds frequency of risky choice for all principals in the A-treatment
- For the fixed effects regression: the individualized measure, log-odds of the weighted average of the individual principal's risky choice in the A-treatment and the normalized "how do you feel?" rating by principals in the HC-treatment

Table 4 presents coefficients from models that measure *risky desirability* in entirely unrelated ways yet the magnitude and significance of the coefficients barely differ.[39,40] We shall restrict our comments below to the random effects model with the log-odds frequency measure of *risky desirability* in the left three columns of 4

The non-parametric tests of the previous section indicate differences in behavior across agent *risky choice* decisions that should be reflected in the independent variable and interaction term coefficients. In particular according with our earlier

[39]It must be noted that the correlation of the two *risky desirability* measures is 74%

[40]The models with the other measures of *risk index* produced coefficients same level of significance and nearly identical magnitudes as the models reported here

predictions, table 4 suggests that principals account for the desirability of the options chosen for them: the coefficient on *risky desirability* is negative (-0.437) indicating that principals pay less in periods with more desirable risky options if the risk-free option is chosen, moreover the sum of this coefficient with the coefficient on the interaction term *risky desirability* $\times$ *risky choice* is a positive number ($0.693 = -0.437 + 1.130$) indicating principals pay more in periods with more desirable risky options if the risky option is chosen. The regression results also suggest that principals are outcome biased: the coefficient on *lottery high* is negative (-1.559) indicating principals pay less in periods where the lottery outcome is high if the risk-free option is chosen, because the forgone risky option is better, also, this coefficient and the coefficient on the interaction term with *risky choice* sum to a positive number ($4.491 = -1.559+6.050$) indicating principals pay more in periods where the lottery outcome is high if the risky option is chosen, because the forgone risk-free option is worse. Finally, the model suggests that principals account for the magnitude of their own winnings: the coefficient on *payoff displayed* is positive (0.357) and its sum with the coefficient on the interaction term with *risky choice* is positive ($0.151 = 0.357 - 0.206$) indicating that principals assign more to their partners when their winnings are higher.[41]

EP-Treatment: further bias, decision quality

The independent variable *risky desirability* is a measure of the appropriateness of choosing the risky option, so we may treat this variable as proxy for the perceived decision "quality" of choosing the risky option. With this interpretation we can test our prediction that if the agent chooses the risky option for the principal, the quality of decision making will play a lesser role for the principal in determining payment to the agent if the principal receives the high outcome of the lottery than if the principal receives the low outcome. To test this we perform the regression from the previous section on the subset of the data where the agents chose the risky option. We use the same independent variables as before leaving out the *risky choice* variable as it doesn't vary on this subset of the data and adding an interaction with *lottery high* to test the prediction. In Table 5 we present both the

[41] All the coefficients were significant under robust standard errors

Table 5: Regressions with pooled *risky choice* data; $N = 325$, $I = 76$

	Random Effects (pop.)			Fixed Effects (indiv.)		
	R^2 .50	χ^2 498.1		R^2 .55	F 117.5	
Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value
constant	3.914 0.421	9.288	0.000	3.945 0.248	15.885	0.000
risky desirability*	0.821 0.139	5.925	0.000	0.641 0.102	6.280	0.000
risky desirability*× lottery high	-0.597 0.293	-2.040	0.041	-0.508 0.173	-2.938	0.004
lottery high	5.715 1.116	5.123	0.000	5.716 0.987	5.789	0.000
payoff displayed	0.134 0.047	2.840	0.005	0.136 0.045	2.998	0.003

*Measure depends on the regression:

- For the random effects regression: the population measure, log-odds frequency of risky choice for all principals in the A-treatment
- For the fixed effects regression: the individualized measure, log-odds of the weighted average of the individual principal's risky choice in the A-treatment and the normalized "how do you feel?" rating by principals in the HC-treatment

random effects and fixed effects model as earlier, we can see immediately that the coefficient for *risky desirability*×*lottery high* is negative and significant for both models, indicating that principals put less weight on the "quality" of the agent's decision when they win the lottery.[42] This indicates that the outcome can bias how relevant factors such as decision quality are incorporated into evaluation.

The study of Tan and Lipe (1997) found a similar pattern for the degree of control the agent possessed over the outcomes. In their study, principals accounted for control when they were evaluating a bad outcome but not when they were evaluating a good outcome. In contrast with the present results, the subjects in their experiment did not exhibit the same asymmetry in their consideration of decision quality.

[42]If we condition the data further to the subset that contains only observations where the risky option is chosen and the lottery outcome is high and then run the same regressions we see that *risky desirability* is not significant while *lottery high* is. If we condition on the subset of the data where the risky option is chosen and the lottery outcome is low and then run the same regressions we see that *risky desirability* is significant in both models

1.3.3 Treatment Effects: HC/RC vis-á-vis the EP-treatment

Between Subjects

In the HC-treatment and RC-treatment we can measure directly the outcome bias of principal i in a single period t for the case of the the risky option being chosen by taking the difference between the payoff contingent payments $\pi^{\text{HC}}_{i,t}(R,h) - \pi^{\text{HC}}_{i,t}(R,\ell)$, while we cannot measure outcome bias if the risk-free option is chosen as the contract is constrained to depend on payoffs only and not on the agent's decision and lottery outcome pairs.[43] There is no comparable measure of outcome bias for a single period in the EP-treatment for a between subjects test because each period has one possible principal observation corresponding to a unique agent decision and realized lottery outcome. In Table 6 we present summary statistics of the principals' payment to their agents when the risky option is chosen and the lottery is either high or low under each treatment when that treatment is the first in an experimental session.[44] There appears to be a difference in $\pi(R,h)$ across treatments for all observations, but as can be seen the difference in payments between the high and low outcome is still positive in each treatment, albeit diminished in the HC and RC-treatments. The seeming persistence of outcome bias of such a large magnitude within the HC and RC-treatments was surprising and will be discussed in detail in the next section. Nevertheless, Table 6 contains suggestive evidence of treatment effects.[45] In the right set of columns summary statistics for the restricted data containing observations where principals indicated a weak preference for the risky option are listed.[46] When risky is chosen and the lottery

[43]Recall that the ex-ante treatment payments are payoff contingent and a they are constrained so that $\pi^{\text{HC}}_{i,t}(Rf,h) = \pi^{\text{HC}}_{i,t}(Rf,\ell) =: \pi^{\text{HC}}_{i,t}(Rf)$

[44]This avoids confounding the significant order effects of the HC-treatment, which are discussed later

[45]We can resample from the ex-ante treatments to match the period level observation count and the subject level repeated observation count of the EP-treatment and collapse to get subject mean payments in order to perform a between treatment ranksum test on the resampled data, but we find significant differences in payments in only 10% of simulations

[46]**THESE ARE NOT COMPARABLE** In the EP-treatment we say that a principal indicates a weak preference for the risky option in period t if they choose the risky option in the A-treatment for that period. In the HC/RC-treatments we say that principals indicate a preference for the

Table 6: Summary Statistics for payments to partner between treatments

		All Observations						Principals with preference for the risky option					
		Lottery High			Lottery Low			Lottery High			Lottery Low		
		EP	HC	RC	EP	HC	RC	EP	HC	RC	EP	HC	RC
Risky	Mean	12.13	9.93	7.88	4.93	5.04	4.11	12.02	11.44	10.43	5.69	9.63	8.68
	S.D.	3.20	4.97	5.62	4.47	5.17	5.00	3.30	3.96	5.02	4.84	4.57	5.61
	N	72	410	290	68	410	290	58	148	76	48	148	76

*In the EP-treatment we say that principals indicate a preference for the risky option in period t if they choose the risky option in the A-treatment for that period. In the HC/RC-treatments we say that principals indicate a preference for the risky option in period t if their contract assigns payments to the high and low outcome that are both higher than their payment if the risk-free option is chosen. **THESE ARE NOT COMPARABLE**

outcome is low, principals assign higher mean payments in both the HC and RC-treatments, which are much closer to their corresponding mean payments when the lottery outcome is high than the EP-treatment. This suggests that the principals in the HC and RC-treatments who consider their preferences ex-ante may attenuate their bias when assigning contingent payments to their agent. For each treatment, the number of observations for this restricted data are distributed unevenly across periods and there are not enough observations in most periods to perform a statistical test.[47]

In order to utilize all the data in uncovering differences between treatments we must control for between period variability and therefore we must use the regression model of the previous section (Table 5) on the ex-ante treatments. To make the data comparable so that we can combine data from all treatments into a single regression model, we first incorporate only the EP-treatment data restricted to the periods where the agent chose the risky-option for the principal. Secondly, we expand each HC and RC-treatment observation into two observations, one for the payment when the risky option is chosen and the lottery outcome is high and the other for when the risky option is chosen and the lottery outcome is low, and then combine these observations with the EP-treatment data.[48]

risky option in period t if their contract assigns payments to the high and low outcome that are both at least as high as their payment if the risk-free option is chosen.

[47] Only period 9 has more than 10 observations for each treatment in the restricted data, and the Mann-Whitney U-test is significant at the 5% level for both treatment effects vis-á-vis the EP-treatment

[48] the repeated observations will be handled with random and fixed effects as before

Table 7: Random effects regressions with pooled *treatment* data and robust se; restricted to risky choice data($N = 960$, $I = 76$) ($N = 720$, $I = 64$)

	EP/HC-treatment			EP/RC-treatment		
	R^2 .30	χ^2 732.39		R^2 .28	χ^2 517.12	
Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value
constant	5.520 0.484	11.413	0.000	4.611 0.568	8.110	0.000
risky desirability*	0.583 0.087	6.721	0.000	0.609 0.106	5.732	0.000
risky desirability* × lottery high	0.153 0.124	1.231	0.218	0.337 0.156	2.166	0.030
risky desirability* × lottery high× EP	-0.729 0.298	-2.443	0.015	-1.206 0.318	-3.787	0.000
lottery high	2.187 0.558	3.921	0.000	2.422 0.715	3.390	0.001
lottery high×EP†	2.963 0.735	4.034	0.000	4.198 0.809	5.192	0.000
EP	-1.138 0.751	-1.517	0.129	-0.243 0.815	-0.299	0.765
payoff displayed	0.141 0.024	5.862	0.000	0.082 0.032	2.581	0.010

*Measured as the log-odds of the frequency of principal risky choice in the A-treatment.
† note: the correlation between *lottery high×EP* and *payoff displayed×EP* is 93%.

In Table 7 we report the results of each pooled regression. As with the regression performed only on EP-data, *risky desirability* here has a smaller coefficient in the EP-treatment when *lottery high* is equal to 1 and in this case *risky desirability* also matters significantly less than the HC-treatment as we can see from the coefficient double interaction term *risky desirability×lottery high×EP*. Note the the significance of the coefficient on *risky desirability×lottery high* in the RC/EP comparison but not on the HC/EP comparison, we will investigate the greater role of risky-desirability in the RC-treatment compared to the EP-treatment in a later section. Most importantly we see the treatment effect with the interaction *lottery high×EP*, this indicates that the magnitude of outcome bias is significantly higher in the EP-treatment than it is in the HC-treatment or the the RC treatment. Note however that these regressions yield the same significance if we replace *lottery high*×*EP* with *payoff displayed*×*EP*, but not if we include both. This is due to the fact that *lottery high*×*EP* and *payoff displayed*×*EP* are 93% correlated due to the relationship between *lottery high* and *payoff displayed*.[49] This means that we are unable to firmly conclude that the treatment effect is a pure difference in outcome bias due to a state of the world which calls attention to the forgone risk-free option and the unattained high winnings. It may be that this is merely a magnitude bias arising from the norm the more I get the more I give or the emotion the better I feel the more you get. We can conclude, however, that there is a significant difference between treatments in how principals react to their payoffs.[50]

Within Subjects

Although we cannot quantify outcome bias in the EP-treatment, we can test whether payment rules in the HC-treatment are systematically different within subject. We measure the treatment effect on the amount of money the principal assigns to the partner directly with a matched pairs Wilcoxon sign rank test. We may perform two tests: one on the payment to partner when risky is chosen and the lottery outcome is high, denoted, $\pi_a(R,h)$, and the other when risky is chosen and the lottery outcome is low, denoted $\pi_a(R,\ell)$. The matched between treatment difference terms Z_i in the Wilcoxon sign rank test statistic for $\pi_a(R,h)$, are computed for principal i according to the following formula

$$Z_i := \begin{cases} \frac{1}{\#H_i} \sum_{\substack{t=1 \\ t \in H_i}}^{10} \left[\pi_a^{\text{EP}}(R,h)_{i,t} - \pi_a^{\text{HC}}(R,h)_{i,t}\right] & \text{if} \quad H_i \neq \emptyset \\ \text{not recorded for agent } i & \text{if} \quad H_i = \emptyset \end{cases}$$

where H_i is the set of periods in the EP treatment for principal i where the agent chose risky in the period and the lottery was high. Note that it may be possible for $H_i = \emptyset$ if all agents for princpal i chose the risk free option or if all periods in which the risky option was chosen for principal i the lottery was low. In this case

[49] recall that $\ell = \$0$ in all periods, therefore when the variable *payoff show* is zero, the variable *lottery high* is zero and when *payoff show* is positive *lottery high* is 1

[50] **rephrase this**

principal i's behavior will not be included in the test statistic. The differences term in the test statistic for $\pi_a(R, \ell)$ were computed in an analogous way. Performing the test in the session with the HC-treatment followed by EP-treatment yields significant results for both tests with the Wilcoxon test statistic equal to 2.669 and a corresponding p-value of 0.0059 with 38 subjects for the $\pi_a(R, h)$ test and with a test statistic equal to 2.669 and a corresponding p-value of 0.0076 with 39 subjects for the $\pi_a(R, \ell)$ test.[51] In the sessions that involved the HC-treatment following the EP-treatment we find no significant difference in the contracts regardless of the day of the experimental session. The order effect demands explanation as it is robust to the day of the experimental session. A possible explanation for absence of significantly different within subject principal behavior between treatments in the EP/HC-session is that winnings are "experienced" in the EP-treatment and thus the connection between winnings and contracts are established by the principals in the EP-treatment and the behavior by principals in the following HC-treatment coheres to the behavior in first treatment. The explanation for significantly different between treatment behavior in the HC/EP-session may be due to the contingent nature of the HC-treatment payoffs before winnings can be experienced and the salient contrast of the ensuing EP-treatment realized payoffs provide before payments are assigned by the principal.[52]

1.3.4 Principal behavior: Hidden Contract Treatment

Hidden Contract: Outcome bias persists, pseudo-signalling

In this section we will discuss the HC-treatment when it is the first treatment of the experimental session.[53] Although, as indicated in the previous section the magnitude of the outcome bias diminishes in the HC-treatment, it is striking the degree of outcome bias that persists in the HC treatment. Unlike in the EP-treatment, in the HC-treatment we may measure outcome bias for the case when

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[51] If we perform the signrank tests for first and second set of experimental sessions days of the HC/EP-session the contract differences remain significant at the 5% level.

[52] **rephrase**

[53] Recall the payments $\pi(R, h)$ and $\pi(R, \ell)$ are not significantly different between the EP and HC-treatment when the HC-treatment is the second treatment of the experimental session

risky is chosen directly in a given period k for a particular principal i by computing the difference between contingent payment to the agent when the lottery outcome is high and when the lottery outcome is low[54]:

$$\Delta(\pi)_{i,t} := \pi_a^{\text{HC}}(R,h)_{i,t} - \pi_a^{\text{HC}}(R,\ell)_{i,t}$$

A histogram of the differences for each period and each subject can be seen in figure 1 below. The right skewness indicates the large number of observations with positive outcome bias. A t-test that the average difference for each partner is equal to zero yields a test statistic $t_{40} = 6.49$, which implies a p-value less than 0.0001.[55]

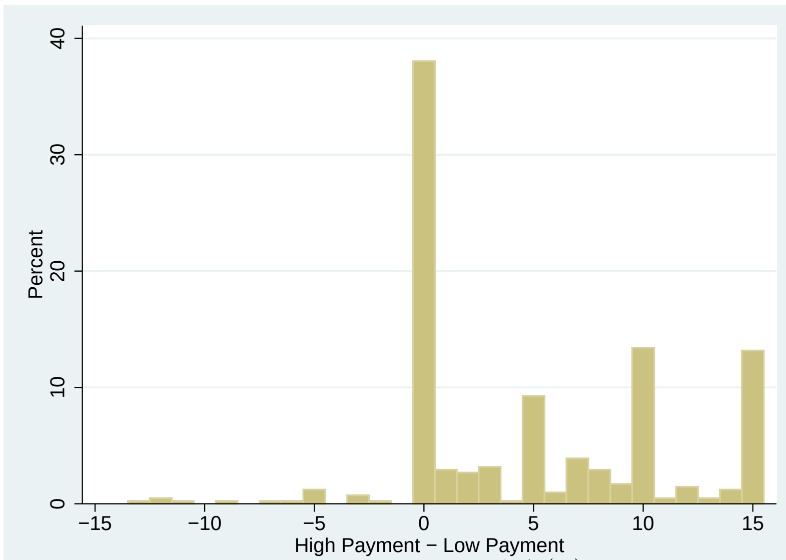


Figure 1: Histogram of $\Delta(\pi)_{i,t}$

The nature of the HC-treatment contingent payment system allows us to investigate aspects of data that are not available in the EP-treatment. We define six indicator variables that are 1 when their condition is true in Table 8. As an illustration, for the first indicator variable C_R we say that principal i's contract in period t reveals a strict preference for risky if $C_{R_{i,t}} = 1$, which happens when

[54]note that we do not have a measure of outcome bias when safe is chosen as the contract must assign the same amount to the agent regardless of the outcome of the lottery

[55]A signrank test on the null hypothesis that the average of the high and low payments are equal for each subject yields $z = 5.069$ and a p-value less than 0.001

Table 8: Indicator Variables

Variable	Event/formula	Event Description
C_{NB}	$\pi(R,h) = \pi(R,\ell)$	Contract has no outcome bias
C_R	$\pi(R,h), \pi(R,\ell) \geq \pi(Rf)$ with one strict	Contract indicates strict preference for risky
C_{Rf}	$\pi(R,h), \pi(R,\ell) \leq \pi(Rf)$ with one strict	Contract indicates strict preference for risk-free
C_I	$\pi(R,h) = \pi(R,\ell) = \pi(Rf)$	Contract indicates indifference
C_M	$C_M := 1 - C_R - C_{Rf} - C_I$	Contract is mixed*
C_S	$C_S := C_R + C_{Rf}$	Contract indicates strict preference

*These contracts are nonetheless orderly in the sense that $\pi_a^{\text{HC}}(R,h)_{i,t} > \pi_a^{\text{HC}}(Rf)_{i,t} > \pi_a^{\text{HC}}(R,\ell)_{i,t}$ for 95% of observations

$\pi_a^{\text{HC}}(R,h)_{i,t} \geq \pi_a^{\text{HC}}(Rf)_{i,t}$ and $\pi_a^{\text{HC}}(R,h)_{i,t} \geq \pi_a^{\text{HC}}(Rf)_{i,t}$ with at least one strict.

Although outcome bias emerged in the majority periods, we conjectured that the magnitude of outcome bias $\Delta(\pi)_{i,t}$ would be less for outcome-biased principals who indicated a strict preference ($C_S = 1$ and $C_{NB} = 0$) than for outcome-biased principals who had a mixed contract ($C_M = 1$ and $C_{NB} = 0$), since subjects that that indicate a strict preference between options (without incentive to do so) show forethought and the willingness to separate actions from consequences for at least the first degree (decision between options phase). Our supposition was confirmed with the Mann Whitney U-test indicating that subjects had significantly higher outcome bias when their contracts were mixed with a test statistic $z = 2.15$ and a corresponding p-value of .0316.

Hidden Contract between session comparisons: Order Effects

In the earlier regressions and sign-rank tests we did not find a treatment effect in the EP/HC-session, while we found one in the HC/EP-session. Since the EP-treatments did not differ significantly between sessions we may expect to find a difference between the HC-treatments in each session. A Mann-Whitney U-Test on the average $\Delta(\pi)$ in the HC-treatment for each subject between sessions yields a test statistic $z = 2.189$ with a p-value of 0.0286, indicating that the outcome bias was significantly higher for the HC-treatment in the EP/HC-session.[56] We

[56]The similar test comparing $\Delta(\pi)$ in the HC-treatment between HC/EP-session and the RC/HC-session is not significant. Only total pay is significantly different between these two sessions (if

conjecture that in the EP/HC-session the principals' experience of and attitudes towards winnings in the EP-treatment influences their behavior in the subsequent HC-treatment so that it coheres with the first treatment. The idea that norms are established when a task is performed for the first time in such a way that it influences future behavior has been noted in Ariely, Loewenstein, and Prelec (2003). The first treatment may establish norms within the experiment and the behavior in the second treatment will differ (fail to cohere) only so far as its environment saliently contrasts with the first treatment. We contend that the EP/HC-session does not lead to a strong contrast between each treatment and thus behavior may be similar, while the same is not true with respect to the HC/EP-session.[57] If this is the case then we may treat the HC-treatment in the EP/HC-session as a proxy for the EP-treatment and thus have a direct proxy measure of outcome bias $\Delta(\pi)$ and the indicator variables presented in Table 8 for the EP-treatment. Between session comparisons on these measures in the HC-treatment can be viewed as an approximation of treatment effect between the EP and HC-treatments (instead of order effects on the HC-treatment). In figure 2 we present the frequency across sessions of the indicator variables C_I and C_{NB}, notice that the fair rule (C_{NB}) differences are the most prominent.

Besides the outcome bias, we may also investigate how many periods each subject submitted a mixed contract and look at the frequency distribution by session. As we can see in Figure 3, subjects were no more likely to submit 0 mixed contracts or 10 in the EP/HC-session, whereas they are in the other treatments.

1.3.5 Principal behavior: Revealed Contract Treatment

Unsurprisingly, with the ability to deliver the contract to the agent ex-ante in the RC-treatment, principals more frequently make contracts that deliver strict incentives to the agent to choose either the risky option or the risk-free option than in the HC-treatment. The surprising feature is that subjects continue to construct mixed contracts at the same rate in the HC-treatment, i.e. the increase in frequency

we take the payment to the partner as a percentage of total pay we get the same results though)

[57]**flesh out**

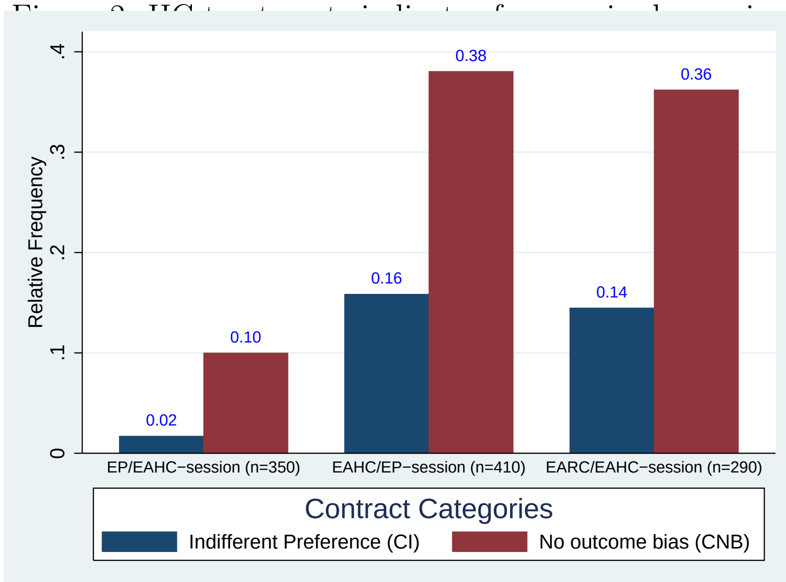
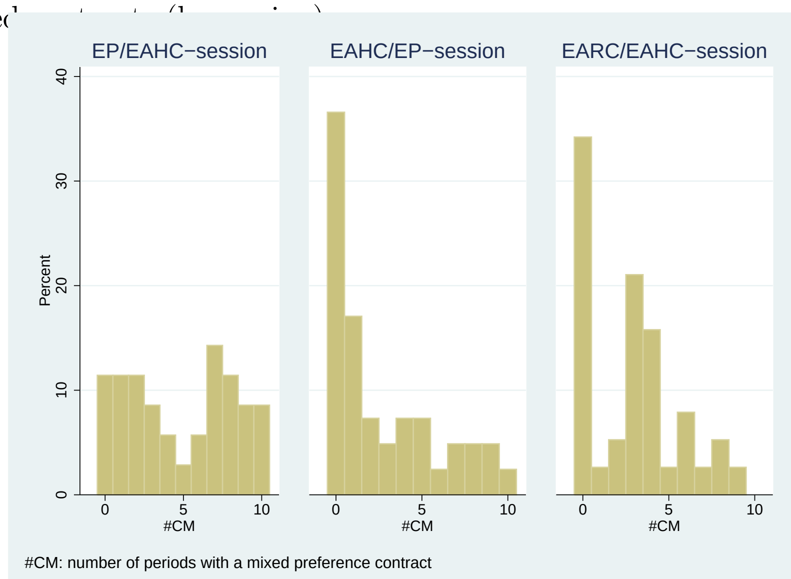


Figure 3: HC-treatment: relative frequency distribution over subjects of the number of mixed

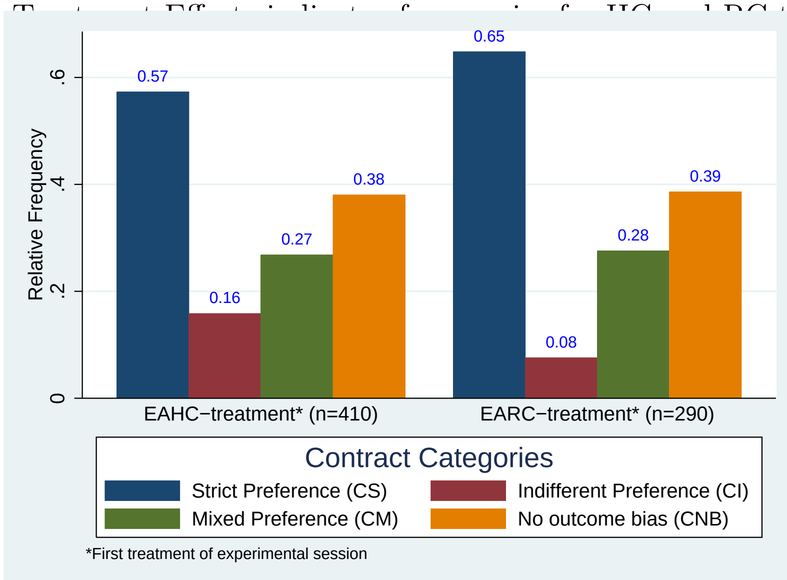


of strict incentive contracts comes entirely from a decrease in frequency of contracts with indifferent incentives. This relationship is evidenced in the Figure 4 below for the HC and RC treatments when they are first in the experimental session.[58]

[58]These differences are significant by observation but not when collapsed by subject. We found no evidence in either treatment that principals constructed strict incentive contracts more often in

Note that the principals who chose the risky option in the A-treatment and who construct a mixed contract instead of a contract that indicates a preference for the risky option suffer a welfare loss as agents choose the risky option less frequently , the welfare loss applies to principals that chose the safe option in the A-treatment.

Figure 4: … reatments



Observe in Figure 4 that the frequency of outcome biased contracts do not differ between the HC and RC treatments as there is still no incentive to avoid outcome bias if risky is chosen. we should not expect subjects to avoid it. All non-parametric tests comparing the HC and RC-treatments both within and between treatments do not reveal significant differences for $\pi(R,h), \pi(R,\ell)$ or $\Delta(\pi)$ whether the test is for each period, or collapsed over all subjects.[59] Looking at the risky portion of the contract in RC there is more magnitude bias in HC. In safe section of the contract there is more magnitude bias in HC.[60]

periods where one option is considerably more desirable than the other

[59]Note that the total payments to partners is significantly less in the RC-treatment, but if we measure the payments as a percentage of total payment we find no significant difference

[60]less magnitude bias could also mean less outcome bias, but the collinearity between LH and PS prevents us from being conclusive on this

Table 9: Rate of agent risk-free choices by Treatment and session type

	Frequency of risk-free choice	
	EP/HC-session	HC/EP-session
EP-treatment	60%	55%
HC-treatment	58%	50%

1.3.6 Agent Behavior

Between treatment, within and between session comparisons of agent safe choice behavior follows roughly the same pattern as the comparisons we made for principal payment behavior.

Between Subjects

We predicted that the higher level of outcome bias in the EP-treatment would induce a higher rate of risk-free choices on the part of agents since the contracts would be more risky.[61] Table 9 presents the agent choice behavior for each session and treatment.[62]

As can be seen in table 9, the agents choose the risk-free option more often in the EP-treatment (60%) than in the HC-treatment (50%) when they are the first treatment of the session.[63] In a Mann-Whitney U-test treating each agent's frequency of risk-free choice as an observation we find this difference to be significant with test statistic $z = 2.622$ and a p-value of 0.0087. Furthermore, the frequency of safe choice by agents in the HC-treatment is different across sessions, with agents choosing the safe option more often when the EP-treatment came first (55%) than when it came second (50%), this difference is significant with a test statistic of $z = 2.36$ and a p-value of 0.0183.[64] Just as is the case for principal

[61] We measure the rate the agents' of risk-free choices instead of risk aversion as agents do not bear the consequences of their choices

[62] Table 9 does not include agent choices in the RC-treatment, as agents simply follow the contracts that principals construct

[63] recall that principals construct outcome biased contracts more often in the EP-treatment as well.

[64] recall that principals construct outcome biased contracts more often in the HC-treatment in the session where the EP-treatment came first as well.

contracts there is no significant difference in the frequency of risk-free choice by agents in the EP-treatment between sessions and no difference in the frequency of risk-free choice between the EP and HC-treatments when they are the second treatment of an experimental session.[65] Although examining agents' frequency of choosing the risk-free option over an entire treatment does yield information, we may attain more precise understanding of the relative role of the treatment and experimental session if we control for the properties of the options themselves, which consists of the high outcome winnings on the risky option's lottery (h), the probability of winning the high amount (p), and the winnings from the risk-free option (c).[66] In table 10 we present two random effects logistic regressions with the agent's risk-free choice as the dependent variable that capture the between subjects treatment effect for the HC and EP-treatment agent choice data, the left three columns present the results when the treatments are first in an experimental session and the right three columns present the results when they are second. Just as in the non-parametric tests just presented and the principal contract results of the earlier sections, we see a treatment effect when they are first in an experimental session but not when second.

In table 11 we present a random effects logistic regression similar the regressions in table 10 that capture the between subjects session differences for the agent choice data in the HC ,EP and A-treatments with the indicator variable *EP/HC-session* which is 1 when the session type is the EP-treatment preceding the HC-treatment. As can be seen in the table (and Table 34), the agents choose differently between sessions only for the HC-treatment mirroring again the results we have for principals.

Within Subjects

Just as principals become more outcome biased moving from the HC-treatment to the EP-treatment but not vice-versa, individual agents choose the risk-free option more frequently in the EP-treatment than the HC-treatment in the HC/EP-session,

[65] Note there is no significant difference in agent choices across session in the A-treatment, though agents choose the risk-free option more frequently in the EP/HC-session

[66] We need not control for the contracts themselves as agents do not view them

Table 10: Random effects logistic regressions for pooled agent choice data; $N = 760$, $I = 76$

	HC vs. EP when first treatment			HC vs. EP when second treatment		
		$\chi^2_{(4)}$ 149.13			$\chi^2_{(4)}$ 146.48	
Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value
constant	4.932 0.512	9.628	0.000	6.661 0.635	10.490	0.000
h	-0.178 0.026	-6.735	0.000	-0.155 0.029	-5.288	0.000
p	-17.230 1.418	-12.151	0.000	-22.061 1.862	-11.849	0.000
c	0.594 0.064	9.272	0.000	0.607 0.074	8.240	0.000
EP-treatment	0.797 0.275	2.893	0.004	-0.292 0.304	-0.959	0.338

while there is no significant difference for the EP/HC-session.[67] A random effects logistic regression for the within subjects data is presented in table 12, as can be seen the results of the non-parametric within subjects test is confirmed, the treatment effect is present in the HC/EP-session but not in the EP/HC-session.[68]

1.4 Discussion

The implication of the Control Principle that equally responsible agents should be treated equally, although appealing as a norm of fairness, has been shown to be questionable as a descriptive feature of human decision making. We have discussed several reasons to doubt the behavioral relevance of this norm by appealing to observations of naturally-occurring data, the conjecture that the hidden action environment is the salient perturbation of most decision problems that involve evaluating another agent, and results from the outcome bias literature in psychology.

[67] The Wilcoxon sign-rank test with each observation consisting of the frequency of agent choice revealed significant within subject differences for the HC/EP-session with a test statistic $z = 2.237$ and a p-value of 0.0253

[68] recall that agents do not choose for themselves significantly different between sessions, and also note that agents choice for themselves lies somewhere between their HC and EP-treatment choice, without any significant difference

Table 11: Random effects logistic regressions for pooled agent choice data; $N = 760$, $I = 76$

	HC-treatment		
		$\chi^2_{(4)}$ 151.57	
Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value
constant	-0.706 0.473	-1.491	0.136
h	-0.145 0.026	-5.577	0.000
p	-17.916 1.476	-12.140	0.000
c	0.520 0.063	8.224	0.000
EP/HC-session	0.669 0.263	2.548	0.011

Table 12: Random effects logistic regressions for pooled agent choice data; $N = 760$, $I = 76$

	EP/HC-session			HC/EP-session		
		$\chi^2_{(4)}$ 153.48			$\chi^2_{(4)}$ 161.46	
Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value
constant	-1.283 0.691	-1.856	0.063	0.151 0.336	0.451	0.652
h	-0.143 0.031	-4.686	0.000	-0.192 0.026	-7.313	0.000
p	-19.943 1.675	-11.904	0.000	-19.070 1.505	-12.674	0.000
c	0.523 0.071	7.358	0.000	0.670 0.066	10.135	0.000
EP-treatment	0.156 0.228	0.683	0.495	0.435 0.200	2.172	0.030

Our results provide evidence that people place a significant and substantial weight on consequences that are irrelevant for assessing the merit of the actions and intentions which led to them. We have shown that the concern for fair outcomes, fair actions or fair intentions highlighted in Falk, Fehr, and Fischbacher (2008) and Charness (2004) can be easily subordinated in favor of other more salient and non-pecuniary motivations, which in this case consists of the emotions and norms that underlie the outcome bias. In our context, though incidental, the inclusion of pecuniary incentives leads to an increased level of fair behavior. Unfair behavior never entirely disappears in our experiment as there is only incentive for the principal to assign uniformly higher or lower payments for each outcome of the lottery—there is no incentive to pay the same for each outcome.

Perhaps it is the inability of agents to reject contracts that they deem unfair, which leads to the persistence of unfair principal behavior. As in the ultimatum game where responders may reject unfair offers, it may not be the concern for fairness which leads to fair behavior but the concern for being treated fairly and having a credible mechanism to punish unfair treatment. We suggest that further studies which a employ a participation constraint for the agent may lead to increased fairness. An environment that allows agents to reject contracts they deem unfair may increase fairness through purely behavioral factors. An environment where agents may choose between contracts offered by principals may increase fair behavior as a by-product of the incentives of competition between principals for the services of agents.

2 Norms or Preferences[69]

2.1 Introduction

Why do people generally drive more safely when accompanied by passengers in their cars?[70] A natural explanation is that social norms may impose a duty of care on individuals whose decisions can affect the well-being of others as well as themselves. In this paper, we study experimentally, individual behavior observed during decisions made alone and in the context of small groups. Consequences of risky decisions made in the group environment, are certain to fall upon all members including the decision maker. Our main hypotheses are the following: **(i)** in group decision environments subjects will exhibit behavior that can be characterized as more risk averse than when choosing alone. **(ii)** the magnitude and direction of the change in risk behavior will be determined by individual subject's perception of social norms.

We are motivated by the following empirical phenomena where adherence to the social norm of caution when choosing for others is a heuristic that decreases the likelihood of blame, retribution and legal sanctions.[71] In developing countries where borrowers can not meet the minimal standards for obtaining regular loans, utilization of this social norm induces members of micro-credit groups to pay back their loans.[72] Although no collateral is demanded, the ability to borrow subsequent loans by each member depends on the successful repayment of the loan by other members of the same group.[73] This group structure, where individual decisions

[69]Joint work with Mehmet Y. Gurdal.

[70]As an example, Evans and Wasielewski (1982) finds that unaccompanied drivers drove closer to the vehicles in front of them than did drivers with passengers.

[71]The similarity between our design and the motivating examples is expected to induce subjects to refer to this heuristic while making decisions in groups.

[72]Highest number of microfinance institutions operate in India & Bangladesh, the most well-known one being the Grameen Bank founded by Nobel peace prize laurate Muhammed Yunus.

[73]Besley and Coate (1995) claim that a high degree of social connectedness among group members may constitute a powerful incentive device, since the costs of upsetting other members in the community may be high. We interpret social connectedness as a factor increasing the power of social norms.

have a direct effect on future financial prospects of others, generates record payback amounts (Hossain 1988). Social norm of caution also manifests itself in legal requirements regulating a wide range of fiduciary relations in a developed economy. In the United States, the so called Prudent Man Rule (1830 Massachusetts court decision - Harvard College v. Armory) dictates trustees to observe the probable safety of capital to be invested and allows beneficiaries to seek damages from a fiduciary in case of a misconduct. While an objective measure of safety is not provided by the rule, the effect of its requirements can be observed in behavioral differences across institutional investors. Del Guercio (1996) examines the differences in portfolio structures of bank managers and managers of mutual assets and finds that the former tilt their portfolios towards high quality assets. Giving examples from past cases in which fiduciaries are held responsible for losses on investments that would ex ante be considered as prudent, she argues that bank managers, exclusively governed by this law, are conservative in exploring investment opportunities. Another study finds that the average return for portfolios of educational institutions were much lower than stock mutual funds during the period (1959-1968).[74] This difference is predicted to arise from prudence standards that places primary emphasis on avoiding losses. A less restrictive version of the Prudent Man Rule, known as Prudent investor Act which allows portfolio diversification, still requires the trustee to exercise reasonable care, skill and caution.[75]

There are two paid treatments in our experiment: Alone and Group. Similar to our motivating examples, decisions made in group treatment involve unilateral risks, the consequences of which fall onto all members of the same group. At each treatment, subjects make 27 decisions between risky lotteries and risk-free options. During alone treatment, subjects' decisions only matter for themselves. Unlike this, the decisions made in group treatment are recorded as votes to be inputs for the group choice function. If all members vote for the risk-free option, everyone in the group is paid this (undivided) amount which carries no uncertainty. Otherwise, if one (or more) members vote for the risky option, all members are rewarded based

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[74]Ford Foundation: Managing Education Endowment 1969.

[75]For the complete text see section 2(a) of the Uniform Prudent Investor Act.

on the outcome of this option. Paid treatments are followed by a questionnaire that collects information about subject characteristics. We believe that social norms in our experimental setting will be determined through two channels: 1) Subjects will be aware of care and caution expected for decisions made in groups (injunctive norms) and 2) Subjects will have beliefs about what is more likely to be done by others in these situations (descriptive norms). We expect both types of norms to play a role in group decisions and we test their effect on individual choices in a group decision making environment.[76] Our findings imply that choices made in groups exhibit a higher degree of caution. Moreover, we observe that this difference in risk attitudes is driven by prevailing social norms, both injunctive and descriptive.

Our approach departs from experimental studies of lottery choices in groups in the way individual preferences are aggregated to form group preferences. Among most recent examples of these studies, Harrison, Lau, Rutstrom, and Tarazona-Gmez (2005) shows that social risk (which is individual's risk attitude inside the group) can be approximated by individual risk attitudes and the findings do not reveal a significant difference between group and individual choices. In Shupp and Williams (2008) groups are more risk averse than individuals on average yet groups tend to be less risk averse in low risk situations (where the probability of loss is relatively low). Masclet, Loheac, Denant-Boemont, and Colombier (2006) has a similar result showing that groups converge to less risky decisions for low winning percentages. In addition to this, subjects who are relatively less risk averse are more likely to conform to group average. Baker, Laury, and Williams (2007) shows that number of safe choices by groups is significantly greater than the average number of safe choices by members (except for low risk). These studies use majority or unanimity rules for decisions made in groups. In reality, many risky decisions yielding correlated payoffs are not made under approval or even knowledge of stake holders other than the decision maker. Motivated by this observation, we are led to selection of the specific voting rule which imposes unilateral risks on every other

[76]These concepts are defined in Cialdini, Reno, and Kallgren (1990) which argues that the impact of social norms can be properly understood when we separate the norms as injunctive norms (what most others approve or disapprove) and descriptive norms (what most others do).

member of the group. We also expect the saliency of group membership to increase interdependence of preferences as in Charness, Rigotti, and Rustichini (2007) and briefly introduce members to each other before they make choices in groups.[77]

The changes in preferences for decisions made alone and in groups are labeled as "choice shifts" by the related studies from social psychology. The terms "cautious shift" and "risky shift" in groups point out the two opposite directions of this change in individual preferences. Prior to 1960's group decisions were believed to be more cautious compared to individual ones (see Davis (1992) for a survey) and Stoner (1968) provides a primary example including actual decisions for hypothetical choice dilemmas where individuals make cautious-shifts while deciding in a group. This study includes items where widely held values (social norms) favor the cautious alternative. In a series of similar studies, Blascovich and Ginsburg (1974) argues that cautious shifts (and also the risky ones) can be induced by appropriate manipulation of emergent norm and Blascovich, Ginsburg, and Howe (1976) supports the hypothesis that individuals will change their risk levels as a function of the emergent normative risk level of the group. Hong (1978) attributes the difference in choice shifts by subjects from different backgrounds to values inherent in the culture they are from. We interpret these studies as confirming our belief in the power of social norms for manipulating individual preferences. In our experimental setting, a cautious shift is defined as an individual's reversal of preference from a risky option during the alone treatment to a risk-free option during the group treatment. Risky shifts on the other hand refer to the opposite case. We find that subject's beliefs about others' attitudes toward risk, and self comparisons with respect to group members in terms of risk preferences have an effect on the choice shifts they make. These beliefs and comparisons are essentially components of the prevailing descriptive norms for a particular subject. As expected, when descriptive norms point out a popularity of risk taking among group members, cautious shifts become less likely. Moreover, individual subjects seem to be aware of injunctive norms demanding cautious shifts for decisions made in the group.

[77] The mentioned study focuses on battle of sexes and prisoner's dilemma games and shows that salience of the group changes member behavior by increasing the aggressive stance of subjects when they are watched by their group members while making decisions.

In search of determinants for particular type of choice shifts, we also focus on standard and rank dependent expected utility representations. Standard expected utility framework predicts no shift in preferences between two treatments. This is because voting for risky option in group treatment is the same as choosing it alone whereas voting for risk-free option generates a convex combination of risky and risk-free options. Thus, under this framework, when no external cost of making risky decisions in groups is imposed, any revealed preference in alone treatment is going to be preserved during the group treatment. Motivated by the fact that voting procedure in group treatment induces a probability transfer over the outcome space, we are led to consider rank dependent expected utility framework to explain choice shifts. Eliaz, Ray, and Razin (2006) shows that a concave probability distortion function for rank dependent expected utility implies risky shifts during group treatment. Since concavity of this function would also imply exhibition of Allais paradox, we test whether subjects have this type of a choice anomaly. Although we don't find that the subjects exhibiting Allais paradox are more likely to make risky shifts than others, there is evidence that they might be less likely to make cautious shifts, offering tentative support to the their theory.[78]

To summarize our contribution, we combine the choice shift literature from social psychology with analysis of decision making in the context of groups from experimental economics. We show the moderating effects of social norms in shifting individual preferences when decisions are made in groups where unilateral risks are imposed on other members. We also test the accuracy of predictions offered by Eliaz, Ray, and Razin (2006). We find that the predictive power of this framework would increase if an external cost for violation of social norms is added to the model. We are also not aware of another theoretical setting which would predict increased risk aversion in a group where a single vote is enough to impose the risky option on all members.

Our findings have several implications. First, responsibility distributions among group members that make everyone liable for losses due to decisions by a single member, can be utilized to promote more cautious decision-making. However, this

[78]Both models would predict cautious shifts in an incomplete information game where a sufficiently high cost for low outcomes is added for risky decisions imposed on other members.

might also lead to excessive peer-pressure among group members, generating negative outcomes (for implications of this in the case of microcredit see Montgomery (1996)). Second, although we made no explicit reference to necessity or appropriateness of more cautious behavior in groups, we observed a significant decline in risk aversion for choices made during the group treatment. As a result, we believe that the effect of any regulation or arrangement emphasizing prudence would be to make this readily available social norm more salient. More specifically, as the previous examples show, prudence demanded from individuals in similar decision making environments, is likely to be manifested as an increase in risk aversion or an increase in aversion to loss. Thus we argue that, to overcome the shortcomings brought by suboptimal risk taking, regulatory laws should be more specific about care and caution demanded in financial decisions.

The rest of the paper is organized as follows: In section 2, we first present the design and procedures used to conduct the experiment and then proceed with predictions about subject behavior. After this, results of the experiment and data analysis are given in detail through section 3 and we conclude in section 4.

2.2 Experimental Design and Procedures

Each experimental session consists of a group treatment and an alone treatment that involve subjects deciding between risky and risk-free options, and a post-experiment questionnaire.[79] Each treatment consists of 27 decisions that are presented in the same order across treatments and across subjects. The risk-free option consists of one of nine certain payments between \$6 and \$14 and the risky option consists of a lottery that pays either \$5 or \$15. There are three distinct risky lotteries, a *low value lottery* with a 25% chance of paying out \$15 and a 75% chance of paying out \$5, a *medium value lottery* with an equal chance for each payoff and a *high value lottery* with a 75% chance of paying out \$15. Each lottery is presented (see Appendix B for a sample choice screen) within a menu consisting

[79] The options were entitled *risky option* and *safe option* so as to call attention to injunctive social norms if they exist. Our interest was in whether or not subjects would be willing to trade off their preferences for a social norm. Therefore, as noted in Cialdini, Reno, and Kallgren (1990), the salience of the norm was important.

of nine decisions between that lottery and each of the 9 certain payments ordered from least to greatest.[80] In each treatment subjects complete three stages, the first stage presents the menu of nine decisions between the medium value lottery and the certain payments, the second stage presents the nine decision menu with the high value lottery, and the final stage presents the nine decision menu with the low value lottery.

In the alone treatment subject's decisions are simply choices between the risky and risk-free option where the payoff of the chosen option, if selected for payment, is paid only to the individual making the choice. In the group treatment subjects are assigned to 3-member groups with no communication among members and guaranteed confidentiality of member decisions. In the group treatment the physical decision is the same, but instead of the decision being recorded as a choice, the decision is recorded as a vote to be integrated into a group choice function that selects one of the options. The group choice function selects the risk-free option if each member votes for it, but if one or more members vote for the risky option the group choice function selects the risky option. Each member receives the identical (undivided) payoff from the selected option.

In the questionnaire which followed the two treatments four sections were designed so as to infer the subject's views of prevailing descriptive social norms, determine the role of injunctive social norms and to assess the degree to which subjects exhibit Allais' Paradox so as the test the predictive power of the theory of Eliaz, Ray, and Razin (2006). Subjects were presented with three decisions that were made in the experiment and asked to guess which decision was the most popular in the alone treatment.[81] We may infer that subjects who more frequently guess that the risky option is more popular for these decisions also view the descriptive social norm with respect to risk attitudes when choosing alone is of higher riskiness than how other subjects view it. Subjects were also asked to rate if they

[80]This presentation is similar to the menus of Holt and Laury (2002). Subjects were not instructed to make consistent menu choices, though 87 out of 96 subjects did for all three menus.

[81]The decisions involved the medium value lottery menu with the certain payments of $8, $10 and $12. By selecting these particular options where the popularity of risk-free option is expected to differ significantly, we aimed to generate a variety in guesses.

believed their risk attitudes were more risky, less risky or of the same risk compared to others so as to get a finer measure on each subjects' view of the descriptive social norm when choosing alone, specifically their relative attitude with respect to the norm. In order to uncover awareness of the injunctive social norm when voting with the group we ask subjects to assess their own behavior by responding if their decisions were more risky, less risky or of the same risk between treatments. Combining their actual decisions with their self-assessments allows us to uncover if subjects who are more risky with the group are more unwilling to admit they are compared to those who are less risky with the group, which would indicate an injunctive norm favoring the safe option when choosing alone.[82] Depending on their responses, subjects were also asked the specify the reason for behaving more risky/less risky/same during the group treatment. The final aspect of the questionnaire used in analysis involved checking if subjects exhibited Allais' Paradox in an environment consistent with the theory of Eliaz, Ray, and Razin (2006).

The full procedure for each experimental session involved recruiting subjects from lower-division undergraduate economics courses at the University Of Minnesota-Twin Cities. Subjects were informed that the study would take less than an hour and the expected winnings per subject would be \$20, a \$10 participation fee and a \$10 average earnings. All seven sessions (96 subjects) were conducted at the Social and Behavioral Sciences Laboratory (SBSL) at the university (see Table 13 for details).

Subjects arrived and were seated randomly in partitioned private carrels each possessing a computer terminal. The experimental subject interface was programmed using zTree experimental software (Fischbacher 2007). Subjects completed consent forms and given blank payment receipts to be signed and submitted when they were paid at the end of the session. Next the experimental session began as either the *A/G-session* with the alone treatment followed by the group treatment finishing with the questionnaire or the *G/A-session* with the group treatment followed by the alone treatment finishing with the questionnaire and then the pay-

[82] Or at least an injunctive norm proscribing the risky option when choosing with the groups. In other feedback questions we see that many subjects feel that there is an objective best choice, i.e. they don't see themselves as selfishly violating a fiduciary duty.

Table 13: Session Data

Session	Date	Participants	Treatment Order
1	04/11/2007	9	A/G-session
2	04/18/2007	15	A/G-session
3	11/27/2007	6	G/A-session
4	11/28/2007	15	G/A-session
5	12/03/2007	27	G/A-session
6	12/03/2007	12	A/G-session
7	12/03/2007	12	A/G-session

ment. Before each treatment commenced, subjects were informed orally that they would participate in two sets of 27 decisions and that the current treatment would involve 27 decisions. Subjects were told orally that only one of the 54 decisions would count for payment and that this decision would be determined at the end of the experimental session by first flipping a coin to determine which set (treatment) would be selected and then generating a true random number between 1 and 27 using the objective random number service www.random.org.[83]. Subjects were also told that uncertainty in the lottery would be resolved using a 4-sided physical die. Subjects weren't informed about the nature of forthcoming treatment before they were done with the initial treatment. In the case of the group treatment subjects were assigned to groups in a way so that their seating arrangements were distant from each other. Also, in the case of the group treatment, subjects were told to stand up and face the members of their group briefly (1-3 seconds) and then immediately seated, they were then informed that if the current treatment was chosen for payment with the coin flip, then the random number would be generated for each group so that each member of the group would receive the same certain payment if the risk-free option was chosen or the same $5 or $15 if the risky option was chosen. Regardless of treatment, after being informed of the payment procedures subjects were instructed to read instructions for the current treatment on the computer screen to themselves. Following the instructions subjects responded to questions on the computer that tested their understanding of

[83]Subjects were shown the website, told that many lotteries use random.org's services and informed that the numbers were generated using atmospheric noise under the control of no one.

the task and provided feedback in the case of incorrect responses. Finally subjects completed the treatment.

2.3 Analysis and Predictions

The Game

The voting task described in the previous section is a 3-player simultaneous move game with a move by nature to determine the outcome of the risky option, $\{h, l\}$.[84] The pure strategy space for each player consists of a vote for the safe or risky option, $\{S, R\}$. The payoff function π_i (not utilities) for player i for a pure strategy profile of nature and all players is given by:

$$\pi_i := \begin{cases} l & \text{if some } j \in \{1,2,3\} \text{ chooses } R \text{ and nature's move is } l \\ c & \text{if all } j \in \{1,2,3\} \text{ choose } S, \text{ independent of nature} \\ h & \text{if some } j \in \{1,2,3\} \text{ chooses } R \text{ and nature's move is } h \end{cases}$$

Note that $l < c < h$. Let nature's mixed strategy be given by $p \in \Delta(\{h, l\})$ where we also refer to p as the probability nature chooses h. Let each player's mixed strategy be given by $\sigma_i \in \Delta(\{S, R\})$. Note that the mixed strategy profile of nature and the all players induces a probability distribution over payoffs for each player in the game given by:

$$\pi_i := \begin{cases} l & \text{w.p.} \quad p \cdot \left(1 - \prod_{i=1}^{3} \sigma_i(S)\right) \\ c & \text{w.p.} \quad \prod_{i=1}^{3} \sigma_i(S) \\ h & \text{w.p.} \quad (1-p) \cdot \left(1 - \prod_{i=1}^{3} \sigma_i(S)\right) \end{cases}$$

In order to find the equilibrium of the game we must characterize the best response function of each player. Player i's pure-strategy vote induces the distribution (p_l, p_c, p_h) over payoffs l, c, h. A vote for the risky option yields a probability distribution that depends only on the move of nature and is given by $(p, 0, 1-p)$,

[84] To simplify notation, h, l will indicate nature's strategy, payoff to a player, or an index depending on the context

whereas if player i votes for the safe option the distribution is given by $(p \cdot (1 - a(\sigma_{-i})), a(\sigma_{-i}), (1-p) \cdot (1 - a(\sigma_{-i}))$, where we define $a(\sigma_{-i}) := \prod_{j \neq i} \sigma_j(S)$, this is player i's probability of being pivotal. Each player's best response function cannot be determined without specifying utility functions, and thus the game is not yet completely defined.

Decision Theory

For each player we may study the best response problem as an individual decision under uncertainty. Each individual has preferences $\succeq$ defined over the set of probability distributions $\boldsymbol{Q}$ on the finite payoff space $Z = \{l, c, h\}$. Each decision, whether alone or in a group corresponds to an element of $\boldsymbol{Q}$. When alone, choosing the risky option produces the element $\boldsymbol{r} = (p, 0, (1-p)) \in \boldsymbol{Q}$ and choosing the safe option produces the element $\boldsymbol{s} = (0, 1, 0) \in \boldsymbol{Q}$. When with the group, letting $a := \prod_{j \neq i} \sigma_j(S)$ stand for the probability of player i being pivotal given the other players' strategies, voting for the risky option produces the element $\boldsymbol{r}_a = (p, 0, 1-p) \in \boldsymbol{Q}$ and voting for the safe option produces the element $\boldsymbol{s}_a = (p(1-a), a, (1-p)(1-a)) \in \boldsymbol{Q}$. Note that $\boldsymbol{r}_a = \boldsymbol{r}$ and $\boldsymbol{s}_a = a\boldsymbol{s} + (1-a)\boldsymbol{r}$. As the alone and group treatment differ according to a compound lottery, we should expect that preferences will depend crucially on how they accord with the independence axiom.[85] We have the following lemma:

Lemma 1. *If $\succeq$ has standard expected utility representation, then for any $a \in (0, 1]$, the following holds*

(i) if $\boldsymbol{r} \sim \boldsymbol{s}$ then $\boldsymbol{r}_a \sim \boldsymbol{s}_a$
(ii) if $\boldsymbol{r} \succ \boldsymbol{s}$ then $\boldsymbol{r}_a \succ \boldsymbol{s}_a$
(iii) if $\boldsymbol{r} \prec \boldsymbol{s}$ then $\boldsymbol{r}_a \prec \boldsymbol{s}_a$

We can see that these relationships are clearly implied by the independence axiom by noting that $\boldsymbol{r}_a = a\boldsymbol{r} + (1-a)\boldsymbol{r}$ and $\boldsymbol{s}_a = a\boldsymbol{s} + (1-a)\boldsymbol{r}$. Thus we can conclude that a standard expected utility agent would vote with the group as they

[85] Recall that the independence axiom asserts that for all $\boldsymbol{p}$,$\boldsymbol{q}$,$\boldsymbol{r} \in \boldsymbol{Q}$ and $a \in [0, 1]$, $\boldsymbol{p} \succeq \boldsymbol{q}$ implies $a\boldsymbol{p} + (1-a)\boldsymbol{r} \succeq a\boldsymbol{q} + (1-a)\boldsymbol{r}$. With the usual assumptions on preferences the statement with the indifference relation and strict preference relation are identical (the strict version has $a > 0$.

choose for themselves, i.e. their best response function in the context of the game will not depend on the actions of the other players.

As can be expected, the preference between $\boldsymbol{r}_a$ and $\boldsymbol{s}_a$ may not accord with the preference between $\boldsymbol{r}$ and $\boldsymbol{s}$ if we do not assume expected utility. Since the difference between the distributions arises from a compounding of lotteries (or a probability transfer between outcomes) it is natural to investigate preferences that exhibit Allais-type behavior, namely rank-dependent expected utility (RDEU) with a strictly concave probability transformation function. Recall the typical Allais prospect, for a set of outcomes $Z = \{\$0, \$1 \text{ million}, \$5 \text{ million}\}$ and probability distributions over these outcomes $\boldsymbol{p} = (.89, .11, 0)$ vs. $\boldsymbol{q} = (.90, 0, .10)$ and $\boldsymbol{p'} = (0, 1, 0)$ vs. $\boldsymbol{q'} = (.01, .89, .10)$, Allais found that for most subjects $\boldsymbol{q} \succeq \boldsymbol{p}$ but $\boldsymbol{p'} \sim \boldsymbol{q'}$. Note that the weight .89 is moved from the low to the middle outcome when transforming to the prime versions. A general definition of this phenomenon for three outcomes can be defined as follows:

Definition 1. *Given a* 3*-outcome payoff space* Z*, let* $\succeq$*, defined over the probability distributions on that space, satisfy* $(1-\alpha, \alpha, 0) \sim (1-\beta, 0, \beta)$ *for* $\alpha > \beta > 0$ *, then* $\succeq$ *exhibits Allais' paradox if for all* $\gamma \in (0, 1-\alpha)$*,* $\succeq$ *also satisfies* $(1-\alpha-\gamma, \alpha+\gamma, 0) \succ (1-\beta-\gamma, \gamma, \beta)$

We consider the possibility that instead having preferences representable by EU, individuals have preferences that can be represented by a RDEU function given as follows:

Definition 2. *Suppose* Z *is a* 3*-outcome payoff space with elements* z_i $i = 1, \ldots, 3$ *ordered so that* $z_1 < z_2 < z_3$*. The preference relation* $\succeq$ *over probability distributions* $\boldsymbol{Q}$ *on* Z *has a rank dependent expected utility representation if for every* $\boldsymbol{q}_1, \boldsymbol{q}_2 \in \boldsymbol{Q}$*,* $p \succeq q$ *if and only if*

$$\sum_{k=1}^{3}[f(w_{k+1}(\boldsymbol{q}_1)) - f(w_k(\boldsymbol{q}_1))]v(k) \geq \sum_{k=1}^{3}[f(w_{k+1}(\boldsymbol{q}_2)) - f(w_k(\boldsymbol{q}_2))]v(k) \qquad (2.1)$$

where $v(\,)$ is the nondegenerate increasing utility function defined over the outcomes and $f(\,) : [0,1] \rightarrow [0,1]$ is a continuous and strictly increasing probability transformation function with values $f(0) = 0$ and $f(1) = 1$. The weights $w_k(\boldsymbol{q})$ used as the arguments of $f(\cdot)$ represent the probability that the outcome is worse than z_k under probability distribution $\boldsymbol{q}$, i.e.

$$w_k(\boldsymbol{q}) = \sum_{i<k} p_i(\boldsymbol{q}) \tag{2.2}$$

where $p_i(\boldsymbol{q})$ is the probability that distribution $\boldsymbol{q}$ assigns to outcome i.

Exhibition of Allais paradox and the shape of probability transformation function turns out to be related as summarized in the following lemma.

Lemma 2. *Suppose $\succeq$ has a rank dependent expected utility representation. Then $f(\cdot)$ is strictly concave if and only if $\succeq$ exhibits Allais paradox.*[86]

The following theorem (proof in appendix) is a special case of a more general theorem provided in Eliaz, Ray, and Razin (2006) which states the conditions for cautious and risky shifts in groups. This theorem predicts that subjects who exhibit Allais' paradox (or equivalently have strictly concave probability transformation functions) will vote for the risky option whenever they either prefer it or are indifferent between it and the safe option when choosing for themselves.

Theorem 1. *Suppose $\succeq$ has a rank dependent expected utility representation and exhibits Allais paradox. Then $\boldsymbol{r}_a \succ \boldsymbol{s}_a$ whenever $\boldsymbol{r} \succeq \boldsymbol{s}$ and $a \in (0, 1)$.*

The strict concavity of probability transformation function $f(\cdot)$ is the main factor generating choice shifts here. Voting for a risk-free option is different than choosing it alone, since the subject now faces the fact that someone else in the group can vote for risky, which would be binding for all members of the group. This generates a probability transfer to low and high outcomes from the safe outcome, as can be seen from probabilities that characterize the lotteries $\boldsymbol{s}$ and $\boldsymbol{s}$ (see above). But since $f(\cdot)$ is strictly concave and positive through all its domain, the weight transfer to low outcome is disproportionately high, generating a loss in rank dependent expected utility of the subject. As a result subject preferences would

[86]This relation is first mentioned by Quiggin (1985) and is also proved in the appendix of Eliaz, Ray, and Razin (2006).

imply $\boldsymbol{r}_a \succ \boldsymbol{s}_a$ and $\boldsymbol{r} \sim \boldsymbol{s}$ at the same time. In other words, rank-dependent expected utility framework predicts that risky shifts would be more likely for subjects who are observed to exhibit Allais'paradox.

Equilibrium in the Game

It is clear from our decision theoretic analysis of choices and votes that in the voting game with EU agents who are indifferent between the risky and safe option when alone, all (mixed) strategy profiles are consistent with Nash Equilibrium (NE) and the perfect and proper refinements. On the other hand, if agents exhibit RDEU with a strictly concave probability transformation function the only NE strategy profiles with some players having totally mixed strategies must have at least one player with the pure strategy R. In the RDEU context the only strategy profile surviving the refinement to perfect or proper equilibrium is (R, R, R). This can be readily seen by noting that any ϵ-perfect and δ-proper equilibrium is totally mixed, so by Theorem 1 since $\boldsymbol{r}_a \succ \boldsymbol{s}_a$ for all $a \in (0, 1)$ (i.e. the best response under any ϵ or δ perturbation will be R) all available weight will be placed on R for the ϵ or δ constrained strategies.[87]

Predictions

As we can see from the previous analysis, if preferences are consistent with EU or RDEU we should expect to see either no systematic differences between the alone and group treatment, or substantially more risky shifts. We do not expect to see either of these phenomena.

Compared to alone treatment, we expected that subjects would have lower number of risky choices in group treatment. Due to ordering in the menus of decisions, our design allows each consistent subject to reveal a certainty equivalent for the given risky option. For each one of the three menus, these values are predicted to be lower during the group treatment, meaning that subjects would switch to risk-free options at relatively lower values. As a result, the number of cautious shifts are expected to exceed the number of risky ones. Moreover, subjects would be aware of the injunctive social norms calling for increased caution for

[87] See Myerson (1991) for a definition of perfect, ϵ-perfect, proper, and δ-proper equilibria

decisions made in groups in this particular setting. The extent to which caution is observed in group treatment is predicted to depend on the order of treatments. When group decisions follow individual ones, salience of injunctive norms would be higher, whereas when group decisions come first, they would act as arbitrary ques for decisions to be made alone later on. Both of these effects would make choices in two treatments more similar for G/A sessions. Subject's beliefs about choices made by other subjects in alone treatment and assessment of their own relative risk attitudes determine the prevailing descriptive norms for a particular subject. A subject believing in the popularity of risky options, is expected to be less likely to make cautious shifts in group treatment. Instead, when a subject thinks he is more risk loving than other members of the group, cautious shifts become more likely.

In the next section, we present the results and show the extent to which our predictions and predictions from theory are confirmed.

2.4 Results

We find, as expected, that subjects behave more cautious in group treatment. There is a decline in total frequency with which risky option is chosen when decisions are made in groups versus when they are made individually. This effect is nearly completely diminished when observations are restricted to G/A sessions only, which shows order effect can be stronger than we previously thought. The consistency imposed by the Holt-Laury menu allows us to use the frequency with which risky is chosen as our measure of riskiness, an approach also followed in Baker, Laury, and Williams (2007). We see evidence for an awareness of injunctive social norm calling for increased caution during group treatment, and subjects who adhere to this norm are more likely to be correct when stated and actual risk preferences are compared. This, we believe, is based on difficulty of openly stating violation of a social norm. As implied by the lower frequency of risky choices, cautious shifts in group treatment is much more common than risky ones. In addition to this, the tendency to make a cautious shift declines when the prevailing descriptive norms point out a popularity of risk-loving behavior among group members.

We are not able to find a variable that has a significant effect on the occurrence of risky shifts. This can either be because number of observations for risky shifts is small or these type of shifts are merely random errors observed in choices when same menus are replicated in group and alone treatments. Compared to rest of our subject group, those who exhibit Allais' paradox do not appear to make risky shifts more often, but instead they are less likely to make cautious shifts. We present our result in 3 different parts. First, we present the evidence of cautious shifts, then focus on order effects and provide an explanation for this phenomenon observed in the experiment and then test the impact of social norms and Allais' paradox on choice shifts.

2.4.1 The Cautious Shift

As predicted, subjects choose the risk-free option more often in the group treatment. Table 14 presents the frequency that a risky decision was made (out of 96 subjects) for each of the 27 decisions in each treatment. As expected, the group voting mechanism does not lead subjects to behave erratically in decisions where the risky option is obviously inferior to the risk-free payment so there is little difference between treatments in this case as can be seen in Table 14 for the decisions where the certain payment is higher than the expected value. We should therefore concentrate our attention on the lower neighborhood of the expected value for each lottery, i.e. for $c \in [6, EV)$. We anticipated that the highest disagreement in preferences to be concentrated in the small lower neighborhood of the expected value and therefore predicted the disparity between treatments to manifest itself most prominently for decisions whose risk-free payment is in close proximity below the expected value. Table 15 confirms this, overall subjects choose the risky option 43.3% of the time in the alone treatment and voted for it 40.4% of the time in the group treatment. For decisions where the certain payment is below the expected value of the lottery, the difference is greater with subjects deciding upon the risky option 84.1% of the time and 78.7% respectively.

Looking at subject level data for the frequency of risky decisions (out of 27) we may construct a cumulative distribution function for each treatment (Figure 5). Note that the cdf for the alone treatment is uniformly below the cdf for the group

Table 14: Number of subjects deciding on the lottery (N=96 subjects in each cell)

		Value of Safe Option								
		\$6	\$7	\$8	\$9	\$10	\$11	\$12	\$13	\$14
\$5 if die =1,2 \$15 if die=3,4	Alone	94	93	84	64	16	4	1	2	3
	Group	88	86	75	56	16	3	1	1	1
\$5 if die =1 \$15 if die=2,3,4	Alone	95	95	93	93	83	72	38	9	5
	Group	94	91	90	87	78	61	29	9	6
\$5 if die =1,2,3 \$15 if die=4	Alone	81	65	24	6	1	0	1	1	1
	Group	82	66	26	4	0	0	0	0	0

Table 15: Percentage of Risky Decisions by Treatment and Session

	A/G-sessions	G/A-sessions	All Sessions
Alone Treatment	83.0%† (43.8%‡)	85.2% (42.8%)	84.1% (43.3%)
Group Treatment	76.1% (39.2%)	81.4% (41.7%)	78.7% (40.4%)

† For the 13 decisions with certain payments $c \in [6, EV)$.
‡ All 27 decisions.

treatment indicating the increase caution in the group treatment. On this subject level data we may perform a within subjects between treatments Wilcoxon sign-rank test on the frequency of risky decisions (out of 27) for the 96 independently observed subjects. The test indicates a significant difference yielding a test-statistic $z = 2.813$ and p-value=0.0049.[88]

Using all options subjects went through in the experiment, we analyze a logit model where decision is our dependent binary variable (1=risky 0=safe). We use a random effects model to account nonindependence of observations coming from the same subject (each subject has 54 decisions, 27 for each treatment). Our regressors here are the following: *Certain payment* shows the value of risk-free

[88]When we disregard subject identities and conduct a between subject between treatment test, we fail to find a significant difference in frequencies of risky decisions. This is mainly due to our small sample size, since doubling our sample by creating an artificial observation identical to each existing subject, would bring significant differences at 0.005 level.

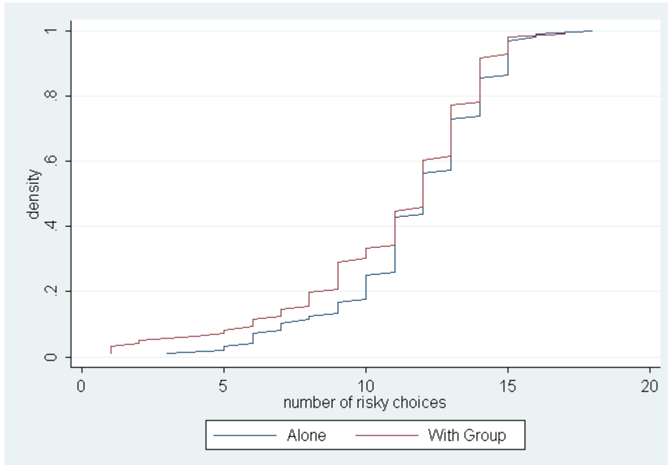

Figure 5: Frequency of risky decision (Cummulative Distribution over subjects by Treatment)

option; *Probability of high outcome* shows the menu dependent probability (0.25, 0.5 or 0.75) of outcome \$15 in the lottery; *Group Treatment* is a binary variable which is equal to one for choices made in group treatment; the interaction term *Group Treatment* × *G/A-session* is a binary variable equal to 1 when decision is made in the group treatment which comes before alone treatment. The results are presented in Table 16. Naturally, the likelihood of a risky decision increases as risk-free option gets smaller or the probability of high outcome in the lottery gets larger. In addition to this, the significant negative coefficient for *Group Treatment* confirms our cautious shift hypothesis. The positive coefficient for the interaction term points out to the existence of an order effect (diminishing the negative effect of *Group Treatment* on risky decisions) which we describe in more detail in the next section.

When each decision made for a particular menu and safe option is compared across treatments, we see that subjects change nearly 15% of their choices where safe option value is in the lower neighborhood of expected value.[89] These changes constitute 179 choice shifts in total, 132 of which were cautious shifts. Moreover,

[89]Expected values are\$10, \$12.5 and \$7.5 for three different menus, respectively. In total 1248 decisions included a safe option value lower than the expected value of the lottery.

Table 16: Random effects logistic regressions with pooled data; $N = 5184$, $I = 96$

	Wald $\chi^2 = 938.68$	Prob $> \chi^2 = 0.0000$	
Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value
constant	8.559 0.374	22.909	0.000
Certain payment	-1.635 0.054	-30.537	0.000
Probability lottery yields \$15	13.527 0.501	26.979	0.000
Group Treatment	-0.705 0.153	-4.603	0.000
Group Treatment × G/A-session	0.524 0.210	2.497	0.013

Note: 27 decisions by each subject in both treatments

when all decisions are compared, it turns out that 35 subjects had more than one cautious shift, whereas only 15 subjects had more than one risky shift. The change in revealed certainty equivalents[90] point out to a cautious shift for the medium value and high value lotteries (where probability of high outcome was 0.5 and 0.75 respectively). In a within subject between treatment Wilcoxon sign rank test, hypothesis of equality for certainty equivalents is rejected for these two lotteries (p-values 0.009 and 0.005 respectively)

2.4.2 Order effects and arbitrary coherence

As mentioned earlier, we suspected the group treatment in the A/G-session would induce a greater difference in risky decisions than it would in the G/A-session. The regression in the previous section (Table 16) indicates that that the treatment effect is diminished in the G/A-session with the positive coefficient on the interaction

[90]Consistent subjects start their menu choices by choosing the deciding on the lottery and switch to risk-free options and remain there after a certain value. Certainty equivalent in our context refers to this point.

Table 17: Subject Level Between Treatment Risk Attitudes

	A/G Sessions	G/A Sessions	All Sessions
More Risky Choices in Alone treatment	28	14	42
More Risky Choices in Group treatment	10	12	22
Equal number of Risky Choices in Both	10	22	32

term. Further, a conditional random effects logistic regression on the G/A-session data (not reported here) does not yield significant results for the coefficient on the group treatment dummy variable even though the sign is still as expected.

Our suspicion is further confirmed in the summary measures from Table 15, the difference between treatments in the G/A-session is nearly half that of the A/G-session. Performing Wilcoxon sign-rank tests on the A/G and G/A-sessions separately yields p-values for the test statistics of 0.003 and 0.505 respectively.[91] The increased caution in the G/A-session is not significant enough for us to conclude a that there is a cautious shift even though the direction of change is as expected.

The order effect manifests itself as well in subject level data. Table 17 shows that the G/A-session has double the number of subjects with the identical number of risky choices in each treatment compared to the A/G-session. For G/A-session the number of subjects having more risky choices in the alone treatment is roughly the same as the number of subjects with more risky choices in the group treatment. This indicates that the choice shifts in the G/A-session may indicate merely random noise in subject choices and not systematic differences between treatments.

A possible explanation for this order effect may involve the concept of *arbitrary coherence* (Ariely, Loewenstein, and Prelec 2003), a form of anchoring where arbitrary initial cues may cause subsequent decisions to cohere to the cues along their dimensions of similarity. In the current context, when the group treatment is encountered first, the mental process to determine which option to vote for does not necessitate precise knowledge of ones preferences outside of the group context. Since the task in the group treatment is nearly identical to the task in the alone

[91]The A/G-sessions were conducted for 48 subjects over 4 experimental sessions and the results hold at the $\alpha = 0.025$ level if we restrict our test to any 3-session subset.

Table 18: Stated versus Actual Risk Attitudes Between Treatments

	Is more risky alone	Is same in both	Is less risky alone	Total
Says "more risky alone"	15	4	2	21
Says "same in both"	27	27	17	71
Says "less risky alone"	0	1	3	4
Total	42	32	22	96

treatment, the vote in the group treatment may serve as an anchor for the determination of noisy individual preferences antecedent to the choices in the alone treatment. The effect of voting in the group treatment thus may impose a degree of arbitrary coherence on individual choices in the ensuing alone treatment. The imposition of coherence on decisions in the A/G-session is likely to be less strong as individual preferences are determined in the first treatment and the mental process to determine which option to vote for in the ensuing group treatment can be made simply using one's (noisily) remembered decisions from the first treatment as a benchmark to adjust to under prevailing social norms.

2.4.3 The predictive impact of social norms and Allais' Paradox

There is a strong indication that subjects are aware of some form of injunctive social norm in the experiment. In Table 18 we report a frequency table which records each subject's stated between treatment difference in risk attitude (with the phrasing from the post-experiment questionnaire) and their actual between treatment difference. Subjects violating the injunctive social norm– subjects that were more risky in the group (less risky alone)– correctly reported their behavior only 13.6% of the time and were most likely to say they had the same behavior in each treatment (77%). On the other hand subjects adhering to the social norm– subjects that were less risky in the group (more risky alone)– correctly reported their behavior 35.7% of the time and they were still most likely to say they had the same behavior in each treatment (64%).

Choices of subjects who are observed to exhibit Allais' paradox during the questionnaire, seem to differ from others in terms of in-group caution level. Table 19 shows that majority of these subjects had the same frequency of risky decisions

Table 19: Subject Level Between Treatment Risk Attitudes

	Exhibits A.P.	Do not Exhibit A.P.
More Risky Choices in Alone treatment	10	32
Equal number of Risky Choices in Both	15	17
More Risky Choices in Group treatment	8	14

in both treatments whereas the majority of remaining subjects behaved more cautious in groups compared to decisions they made alone. In addition to this, Allais' paradox doesn't seem to be a predictor of risky shifts since the fraction of subjects who voted more risky in groups is nearly identical for subjects who exhibit Allais' paradox and those who don't.

With the indication that subjects may be influenced by social norms and that subjects who exhibit Allais' paradox are less likely to decide on the risk-free option more often in the group treatment than subjects who do not exhibit the paradox[92], we set out to model these and other factors in an ordered logistic regression model of individual subject behavior where the ordinal dependent variable, *Relative Group Caution Level* takes on three values, 1 if the subject picks the risky option more often in the group treatment, 2 if the subject picks risky and risk-free with the same frequency in each treatment, and 3 if the subject picks risk-free more frequently in the group treatment.[93] As can be seen in Table 18, the independent variable takes value 1 for 22 subjects, 2 for 32 subjects and 3 for 42 subjects. Our independent variables are the following: *subjectively riskier than others*, an indicator variable equal to one if on the post-experiment questionnaire the subject responded that he believed he was more risky than other subjects when choosing for himself, this variable is meant to assess the subject's view of their risk attitude relative to the social norm; *guesses* $(\$5, \$15; .5)$ *is more popular than* \$8, an indicator variable equal to 1 if the subject believes the lottery with an even chance of \$15 and \$5 is more popular than \$8 for sure, this meant to assess a subjects view of the descriptive social norm for this decision from;*guesses* $(\$5, \$15; .5)$ *is more popular*

[92]This does not imply subjects who exhibit Allais' paradox choose risky more often than others

[93]Note that again that these are good measures of risk level as the menus impose consistency on decisions and the 27 decision frames are identical between treatments.

Table 20: Ordered Logit: *Relative Group Caution Level*; $N = 95$, $I = 95$

	LR $\chi^2 = 19.72$	Prob $> \chi^2 = 0.0031$	
Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value
subjectively riskier than others	0.774 0.608	1.27	0.203
guesses $(\$5, \$15; .5)$ is more popular than \$8	-1.384 0.797	-1.75	0.080
guesses $(\$5, \$15; .5)$ is more popular than \$10	-0.171 0.510	-0.34	0.737
exhibits Allais' paradox	-0.735 0.424	-1.73	0.083
objectively riskier than others	1.312 0.460	2.85	0.004
G/A-session	-1.020 0.416	-2.45	0.014
intercept 1	-2.83 0.877		
intercept 2	-1.16 0.841		

than \$10, similar to the previous description;*exhibits Allais' paradox*, an indicator variable equal to one if the subject violated the independence axiom by exhibiting Allais-type behavior on a lottery-choice task; *objectively riskier than others* an indicator variable equal to one if the subject's frequency of choosing risky in the alone treatment is higher than the median; *G/A-session* an indicator variable equal to one if the session begins with the group treatment.

The regression model is presented in Table 20.[94] The first three variables in the table measure descriptive social norms and the signs on the coefficients are in the expected direction but only the coefficient on *guesses* $(\$5, \$15; .5)$ *is more popular than* \$8 is significant, indicating that subjects, who view their cohorts as

[94]We ignore one outlier subject with inconsistent menu choices.

Table 21: Logit: Cautious Shifts; $I = 95$,$N = 1110$

	Wald $\chi^2 = 70.49$	Prob $> \chi^2 = 0.0000$	
Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value
constant	-4.188 1.067	-3.92	0.000
subjectively riskier than others	1.146 0.539	2.12	0.034
guess ($5, $15; .5) is more popular than $8	-1.232 0.694	-1.77	0.076
guess ($5, $15; .5) is more popular than $10	-0.904 0.438	-2.06	0.039
exhibits Allais' paradox	-0.286 0.424	-0.67	0.500
objectively risker than others	0.894 0.374	2.39	0.017
G/A-session	-0.684 0.356	-1.92	0.055
value of safe option	0.896 0.134	6.66	0.000
probability of high outcome	-6.093 0.887	-6.87	0.000

more risk-seeking than how other subjects view them, exhibit have a lower *Relative Group Caution Level*. The coefficient on the fourth variable in the table, *exhibits Allais' paradox*, is significant at the 8% level and indicates that Allais-type subjects have a lower *Relative Group Caution Level*, giving mild support to (Eliaz, Ray, and Razin 2006). The significantly positive coefficient on *objectively riskier than others* indicates that risk-seeking subjects have a higher *Relative Group Caution Level* than risk-averse subjects, with the possible indication that they may be aware of their relative risk attitudes. The significance of *G/A-session* re-affirms the order effect mentioned elsewhere.

In Table 21 we present the results of a logistic regression that models cautious shifts directly. We condition on the data (1110 observations) where subjects chose risky for themselves in the alone treatment. Clustered robust standard error are used here to account for non-independence of observations coming from the same subject. The dependent variable *cautious shift* is equal to 1 if the subject chose the risk-free option in the group treatment and zero otherwise. The independent variables include the subject-level and session-level measures from the previous regression (Table 20) and also include the decision-level measures *value of safe option* and *probability of high outcome*. As can readily be observed the independent variables representing descriptive social norms are significant; subjects who perceive themselves as riskier than others and subjects who guess safe options were more popular than lotteries among other group members are more likely to exhibit a cautious shift. Allais-type subjects do not appear to be significantly different in this regression, while risk-seeking subjects exhibit more cautious shifts. For choices made in group treatments preceding the alone treatment, cautious shifts become less likely, as expressed by the negative coefficient of *G/A-session*. As expected, the relative quality of the lottery is significant as evidenced by the p-values on the two decision parameters variables *value of safe option* and *probability of high outcome*.

When a similar regression is repeated for risky shifts, no variable turns out to be significant. This is mainly due to to low number of risky shifts and confirms our belief that the meaningful shifts, predicted by subject characteristics, would be overwhelmingly cautious and it is likely that risky ones are random errors in choices.

2.5 Discussion

This study is an attempt to show how attitudes toward risk can change under circumstances where consequences of risky decisions are certain to fall upon other people as well as the decision maker. The group treatment in our experiment induces this kind of a decision environment whereas decisions in alone treatment matters only for the subject himself. The behavior we observe in the experimental data show that choice shifts in groups are overwhelmingly cautious whereas risky shifts appear to be random noise in subject behavior. An immediate extension of our study could be modifying the group choice function such that risky decisions require unanimous agreement whereas risk-free ones bind every other member. The results of the proposed study will help to understand whether individuals are reluctant to impose every type of decision or only risky ones on other people.

Although we didn't disentangle the effects of descriptive and injunctive norms[95], we see a moderating effect of both on choice shifts made in the context of groups. Subjects are observed to be aware of and motivated by injunctive norms calling for increased caution. For the case of descriptive norms, subjects acted more cautious in groups when they believed other group members were more risk averse, and less cautious when they thought risky options were popular among others. We conclude that exclusion of social norms from the models limit the predictive power of theory, since many individuals concede their preferences for the social norm. Addition of this factor to existing models would also strengthen the predictions offered by theoretical frameworks.

Role of social norms for economic decisions has been previously discussed for a variety of settings (see Elster (1989)). Motivated by our findings, we believe that extended models specifying norms in two dimensions as "what most others do" and "what is generally expected" would be more realistic since in our experiment, adherence to both types of norms are observed to change individual decisions.

Besides the effect social norms, another concern for decisions made in groups is other-regarding preferences. Reciprocity and inequity aversion are two most popular aspects in modeling or estimating these type of preferences (see Charness and Rabin (2002)). Even though our experimental setting doesn't leave room for existence of these concerns, the possibility of affecting the well-being is observed to change risk attitudes of our subjects. We think that our results point out the necessity of adding "fear of blame" as another important factor determining the nature of other-regarding preferences and inducing suboptimal risk taking when others' payoffs are at stake.

[95] This would be possible when two types of norms dictate different behaviors and the salience of both norms can be manipulated appropriately

3 The Relative Importance of Decisions and Outcomes in Accountability Judgments: Gender Differences[96]

3.1 Introduction

The way in which people hold others accountable for their actions influences economic transactions. Men and women may differ systematically in how they hold others accountable. In our study we measure differences between men and women in how they respond to others who are choosing for them. We do so by observing how subjects assign payments to their partners who are choosing for them in a game. The actions of their partners and the outcomes that follow vary, and thus we can see how men and women differ in incorporating these factors into their evaluations.

Our experimental environment is a game which we term the "accountability game". The game involves a one-shot pairing of two subjects who move in sequence over two stages. The first mover subject, whom we term the agent, chooses costlessly between a lottery (determined by the roll of a physical die) and a certain payment. The payoffs from the choice accrue to their partner, the second mover subject, termed here the principal. Next the physical die is rolled determining the outcome of the lottery. The decision of the first mover and the die outcome are commonly observed. Finally the principal responds by costlessly assigning payment to the first mover. We manipulate whether the responses of the principal are made contingently before the first mover decides or spontaneously after the outcome of the die is revealed.

We show that the reward principals assign to their agents depends on the outcome of the lottery and that more weight is given to the outcome than the decision. This behavior does not indicate a reasonable evaluation of accountability as the decision of the agent is fully observable and therefore the outcome of the lottery does not provide any additional information about the decision of the agent.

[96] Analysis of an experiment conducted jointly with Mehmet Y. Gurdal and Aldo Rustichini

In this sense we can state that outcomes influence accountability judgments beyond what their information content can justify. In addition, while the outcome of the lottery remains important we find that it is a lessor factor when the principal determines their payment to the agent by filling out a contingent contract to pay the agent before the agent chooses. These results are consistent with the notion that subjects carry their norms, heuristics and optimal decision rules from more commonly experienced environments into novel environments where they may not be applicable.[97]

More central to the present analysis, we observe differences in principal payment behavior by gender: women hold their partners accountable for risky decisions depending on whether the lottery outcome is high or low and pay less than men do if the outcome is low, while if the certain amount is chosen women do not hold their partners accountable for the outcome of the lottery, while men do. In addition, women put relatively more weight than men on payoffs than the quality of the decision, while men but not women modulate the relative weight they place on payoffs and decision quality when they win by putting less weight on the quality of the decision.

3.2 Accountability in Economic Behavior: Decisions vs. Outcomes

There exists ample experimental evidence showing that people will engage in costly behavior that yields zero monetary payoff in order to respond to the actions of others. This behavior has been highlighted most prominently in the "ultimatum game" (Guth, Schmittberger, and Schwarze 1982), in the "gift exchange game" (Fehr, Kirchsteiger, and Riedl 1993), and in the "trust game" (Berg, Dickhaut, and McCabe 1995). These responses have been alternatively conceived of as a form of reciprocation, fairness or reputation building. Recent studies have indicated that reciprocation is not a response to the outcome delivered but to the action itself. In Falk, Fehr, and Fischbacher (2008) it was shown that subjects will not engage

[97] This idea can be captured with the ideas of *Case Based Decision Theory* (Gilboa and Schmeidler 1995) or the notion a *salient perturbation* of a decision problem (Myerson 1991).

in costly reciprocation for involuntary actions of their partner, but they will for voluntary ones. Charness and Levine (2007) show that subjects will engage in costly reciprocation for actions with good intentions that yield poor outcomes due to bad luck, but will not do the same for actions that yield the same outcome which is relatively good as they arise from bad intentions and good luck. From these results we may infer that individuals will engage in costly responses to the actions of others only when the actions of others reveal that they engaged in voluntary costly sacrifice or selfish opportunism. The conclusion from these studies is that when individuals respond to others, the nature of their response depends not on the outcomes, but the intentions that led to them.[98]

While these studies are important, it is often the case that the presence of sacrifice cannot be inferred from actions. Further, when it is clear that some sacrifice is present, the level of sacrifice is seldom measurable. Even in cases when there is some external measure of sacrifice, its true cost in utility terms is not measurable, as utility is never observable. The question naturally arises, if subjects discount the influence of outcomes and hold others accountable for actions that reveal good or bad intentions as indicated by Charness and Levine (2007) and Falk, Fehr, and Fischbacher (2008), will they do the same for actions which reveal good or bad decisions, but mask intentions? A recent experimental study by Gurdal, Miller, and Rustichini (2007) (Chapter 2) shows that when responding to others, subjects put substantially more weight on the quality of the outcomes they receive than on the quality of the decisions that led to them.[99] These effects are mitigated when subjects decide in advance how they will respond under each contingency, but potential outcomes continue to influence response behavior.

A question that has not been addressed previously is whether considerations of outcomes and decisions motivate the responses of women differently than men. Previous studies have shown that men and women differ systematically in ways that may affect economic behavior. One of the more robust results in the experimental economic literature on gender differences is that women are more risk-averse than

[98]To the extent that actions and intentions are observable, discussed shortly.

[99]The road to hell is paved with good intentions. -anonymous

men (Eckel and Grossman 2003). This difference has been attributed to a related finding that women experience emotions more strongly than men do; as attitudes towards risk can be shaped by emotional reactions to risky environments, women may be more risk averse than men (see Croson and Gneezy (2008) for a discussion). In the context of the present study, when evaluating the choice between a risky and risk-free option, the aforementioned differences indicate that women may judge risky-decisions more harshly and risk-free decisions more favorably.[100]

There are more reasons to expect women to differ from men in accountability judgments. Accountability judgments involve social behavior which depend on more than merely attitudes toward risk. Women have been shown to differ systematically in their social behavior. In dictator games it has been shown that women make more of an attempt to equalize earnings than men, who instead focus on efficiency (Andreoni and Vesterlund 2001; Dickinson and Tiefenthaler Oct., 2002). As risky decisions involve variability in payout, we may expect women to be more variable in their responses when risky decisions are made, while less variable when risk-free decisions are made, regardless of the outcome of a forgone risky option, as payoffs do not change. Furthermore, a noteworthy claim was made by Gilligan (1982) that men tend to make decisions on the basis fixed principles, and seek a system of rules or laws to follow, while women exhibit a mode of behavior that is more situational, and tend to look for decisions that spread the cost or benefits of the decision so as to preserve the relationships among the people involved. In our context principles are present before the outcome is revealed, measured here as the *E(utility premium)* and after the outcome is revealed, measured here as the *utility premium*. Before the outcome is revealed one can determine if someone could have chosen better, after the outcome is revealed one can determine whether someone could have done better. The actual payoffs (which women may attend more to) are not perfectly correlated with either principle measures and therefore we should be able to infer if men pay more attention to these factors. In addition, as responses to payoff outcomes are perceived as reciprocation for "delivering the goods", women have been shown to reciprocate more than men and thus we

[100] we should control for female risk preferences

may expect them to be more responsive to payoffs Ben-Ner, Putterman, Kong, and Magan (2004). Furthermore, to the degree that individuals punish for poor payoffs outcomes, we may expect that women will punish more than men, as a study by Eckel and Grossman (1996) demonstrated that women are more likely than men to reward or punish when the costs of rewarding and punishing are low, which they are in our environment. In addition, the anonymity of computer based experiments may lead to less care or concern for others on the part of women (see Eckel and Wilson (2002)).

3.3 Experiment Design

Our experimental design for each two-player treatment is a restricted version of the principal agent problem with observable agent actions, costless decisions on the part of the agent, costless contracting on the part of the principal, and no participation constraint. Each treatment consists of 10 periods with each period involving a choice stage where a subject makes a decision $D \in \{R, Rf\}$, where R is a risky option and Rf is a risk-free option. The risky option consists of a lottery L with support $\{h, \ell\}$, where h is the high outcome with probability p and $\ell = 0$ is the low outcome with probability $1 - p$. The risk-free option consists of a certain payment c. The probability and high outcome of the lottery and the certain payment vary across periods, with $h > c > \ell = 0$ and $p \in \{.25, .5, .75\}$. The outcome of the lottery is determined by a 4-sided physical die. Each period's choice stage is identical across all subjects, all treatments, and all sessions and they are listed in Table 22. The outcome of the risky option in a given period is determined by a single physical die for all subjects. In order to increase payoff diversity for risky options, the mapping between die numbers and payoffs was randomly permuted across subjects so that realized outcomes vary across subjects[101]. The lotteries were designed so that nearly risk-neutral subjects would predominately pick the risk-free option in Periods $1, 6, 7, 8$ and the risky option in Periods $2, 4, 9$ and would

[101] For example, in period 1 all subjects have the choice between \$9 and (\$30, .25; \$0, .75), but a given four subjects may each see a unique representation of the lottery: for the first subject die face 1 may yield \$30 with the other die faces yielding \$0, for the second subject die face 2 may yield \$30 with the other die faces yielding \$0, etc.

Table 22: The Binary decision problem for each period

	Risky Option (R)			Risk-Free Option (Rf)
Period	high	probability high	low	certain
1	\$30	0.25	\$0	\$9
2	\$20	0.50	\$0	\$10
3	\$20	0.25	\$0	\$4
4	\$10	0.75	\$0	\$7
5	\$30	0.50	\$0	\$15
6	\$20	0.25	\$0	\$8
7	\$10	0.25	\$0	\$5
8	\$30	0.25	\$0	\$12
9	\$20	0.50	\$0	\$6
10	\$10	0.25	\$0	\$3

Table 23: Experimental design and number of subjects

	Principal		Agent	
Treatment	Female	Male	Female	Male
Ex-Post(EP)	25	51	29	47
Hidden Contract (HC)	41	64	39	66
Revealed Contract (RC)	16	13	10	19
Alone (A)	41	64	39	66

be nearly split between risk-free and risky for Periods $3, 5, 10$.

Ex-post (EP) Treatment

This treatment consists of 10 periods. At the beginning of each period, subjects are matched with a new partner randomly and anonymously without repetition. In the first stage of a period the agent chooses between a risky option (R) and a risk-free option (Rf) for the principal, who is idle at that stage. In the second stage the principal observes the decision $D \in \{R, Rf\}$ of the agent and a die is thrown so all subjects may view the die and infer the outcome of their lottery $L \in \{h, \ell\}$. The principal wins \$$c$ if the agent chooses $D = Rf$. If the agent chooses $D = R$ then the principal wins \$$h$ if the lottery is high or \$0 if the lottery is low. In the third stage of the period the winnings are presented on the screen of the principal who, having seen the decision and outcome (D, L), is prompted to assign a payment \$$\pi_a(D, L) \geq 0$ to the agent and \$$\pi_{ro}(D, L) \geq 0$ to a random other agent in the room, where $\pi_a + \pi_{ro} \leq 15$ with any unassigned money kept by the experimenter. Next the agent views the payment for the principal. Finally, both agent and principal rate how they feel about their partner's decision on a $1 - 10$ scale from very bad to very good.

Hidden Contract (HC) Treatment

Subjects are paired randomly and anonymously without replacement for 10 periods. In the first stage the agent chooses between the risky option and the risk-free option for the principal while the principal simultaneously completes a hidden contract, which specifies how to assign the $15 to the agent and the random other for each *payoff contingency* of high, low and certain, i.e. the principal chooses $\pi_a(D, L), \pi_{ro}(D, L)$ for each $(D, L) \in \{R, Rf\} \times \{h, \ell\}$ such that $\pi_a(D, L) + \pi_{ro}(D, L) \leq 15$ and payments are contingent on payoffs only constraining the payments when safe is chosen to satisfy: $\pi_a(Rf, h) = \pi_a(Rf, \ell)$ and $\pi_{ro}(Rf, h) = \pi_{ro}(Rf, \ell)$. In the second stage the principal observes the choice of the agent and rates how she feels about it on a scale of 1 to 10 while the agent observes the portion of the principal's contract still relevant after his choice and rates how he feels about it on a scale of 1 to 10. Finally, the die is thrown in front of all subjects, the lottery outcome is revealed, and the payoffs are displayed.

Revealed Contract (RC) Treatment

Subjects are paired randomly and anonymously without replacement for 10 periods. In the first stage the principal completes the payoff contingent contract just as in the HC-treatment while the agent waits. In the second stage the contract is revealed to the agent before the agent chooses between the risky option and the risk-free option for the principal. Next both agent and principal rate how they feel about their partner's decision on a scale from 1 to 10. Finally, the die is thrown so all subjects may view the outcome and the payoffs are displayed.

Alone (A) Treatment

In every experimental session, each subject participates in this treatment as the third and final treatment of the session. Subjects are not paired, there are no roles, and they choose independently only for themselves. Over the course of 10 periods subjects choose between the same risky and risk-free options viewed in the previous treatments (with the same order). The payoffs are not determined while the subjects choose. In addition to the payoff selected from the other treatments, a single period is chosen to count for payment from this treatment with a physical die thrown at the end of the experimental session.

The subjects and procedure

The experiment was programmed and conducted with z-Tree experimental software (Fischbacher 2007). For each experimental session we visited the last 5 minutes of a 200-500 student principles of economics class and informed the students that they may participate in a 2 hour economic choice study beginning immediately. They were also told that they may receive between $8 and $70 for their participation, with more than half of the subjects receiving between $20 and $40. Typically, 30-40 subjects were recruited and walked over with the experimenter to the Social and Behavioral Sciences Laboratory (SBSL) at the University of Minnesota. Upon arrival, students were seated and given consent forms and payment receipts[102]. The subjects were seated at individual computer carrel booths that isolated their screens from the view of other subjects. Subjects were informed only that they would participate in three 10-period treatments, after which they would be requested to respond to feedback questions and finally paid. Subjects were told that we would be randomly selecting (with physical dies) one decision period from the first two treatments and one decision period from the final treatment to count for payment. The session began by privately and permanently assigning students to the role of either principal or agent and then commencing the first treatment. Each treatment was preceded by flash video instructions presented individually with headphones streaming via a web browser and distribution of a sheet presenting a summary of instructions. Next, using z-Tree experimental software we ran $1-2$ periods of practice, and questions (with individualized feedback) to check for understanding of instructions — no details regarding later treatments were given. After these preliminaries we launched the $15-25$ minute treatment in z-Tree. A total of 210 subjects (105 principals and 105 agents) participated,

[102]see supplementals for details

with each subject participating in one of the following three experimental sessions: 1) The EP/HC-session, where the order of treatments were the EP, HC, followed with the A-treament (70 subjects, 2 days); 2) HC/EP-session where the order of treatments were the HC, EP, followed with the A-treament (82 subjects, 4 days); 3) The RC/HC-session where the order of treatments were the RC, HC, followed with the A-treatment (58 subjects, 2 days). When the treatments were completed the subjects typed open-ended feedback to questions and then were paid. A break down of gender frequency by treatment and subject type is presented in Table 23

3.4 Results

Data from all experimental sessions are pooled to analyze how the gender factor influences principal and agent behavior. As there is no significant difference between subjects across sessions we are able do this.[103] We first examine differences in risk aversion between men and women, then demonstrate differences in choosing for others between lotteries and certain amounts, and finally the central result of how women and men differ in incorporating decision quality and outcome information into their assignment of payments.

Risk Aversion

Men are more risk seeking than women in choosing for themselves. In Figure 6 we present the average proportion of choices that were risky in the decision periods where the decision between risky and risk-free options was "close", where a close decision is one where one option is not chosen by an overwhelming majority.[104] As can be seen, men choose the risky option significantly more often than women in these periods.

In order to more robustly test for the significance between genders in risk-aversion we may model the subjects' choice between the risky option and the risk-free option as a Bernoulli random variable. We can model the Bernoulli probability of choosing risky parametrically by imposing a probability structure on a CRRA

[103]There is an order effect for treatments.

[104]Only periods 3,5, and 10 are close by this measure

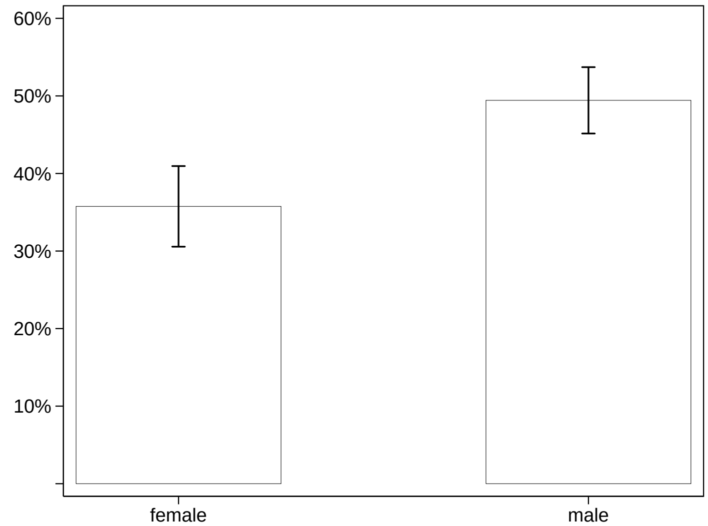

Figure 6: Frequency risky chosen by gender

Table 24: MLE CRRA probabilistic choice estimation of risk aversion in the alone treatment: Female vs. Male (N=630 with 210 clusters*)

	Logistic ($\chi^2 = 4.92$)		Probit ($\chi^2 = 5.28$)		Luce Ratio ($\chi^2 = 5.55$)	
Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	p-value	$\hat{\beta}/se_{\hat{\beta}}$	p-value	$\hat{\beta}/se_{\hat{\beta}}$	p-value
constant	0.118	0.000	0.086	0.000	0.377	0.006
	(0.032)		(0.018)		(0.138)	
male	-0.080	0.027	-0.048	0.022	-0.401	0.018
	(0.036)		(0.021)		(0.170)	

*Note: regression estimated on data set restricted to periods $3, 5, 10$. Parameters are sensitive for decisions in other periods that are far from the indifference point. A linear random effects log-odds model of binary choice has gender significant as well, but including the certain payment and the probability that risky is high is problematic for these choices as they are perfectly correlated in periods $3, 5, 10$.

utility specification, $u(x) := x^{1-\sigma}/(1-\sigma)$. Let *risky choice*$_t$ be equal to 1 if the risky option is chosen and 0 if the risk-free option is chosen. We model:

$$risky\ choice_t := \begin{cases} 1 & \text{with probability} \quad F(\phi(\boldsymbol{\beta}, \boldsymbol{x}_t)) \\ 0 & \text{with probability} \quad 1 - F(\phi(\boldsymbol{\beta}, \boldsymbol{x}_t)) \end{cases}$$

Where $\boldsymbol{x}_t$ is a column vector consisting of the scalar 1 and the values of the independent variables in period t. Let the payoffs from the lottery L is defined to be h with probability p and 0 with probability $1-p$ and the payoff from the certain amount be defined by c. The independent variables in our model consist of the high payoff from the lottery (h), its probability (p) and the payoff of the certain amount (c). The vector $\boldsymbol{\beta}$ consists of the parameters of the model, and both ϕ and F are a functions, with F between zero and one. The parameters $\boldsymbol{\beta}$ are estimated using the maximum likelihood procedure.

For our first two specifications ϕ measures the utility premium of choosing risky and F is a cumulative distribution function (cdf) than transforms the utility premium to the $[0, 1]$ scale. The first specification is Logit where F is the logistic cdf, $F(\phi) := 1/(1 + e^{-\phi})$ and the second specification is Probit where F is the standard normal cdf, $F(\phi) := \int_{-\infty}^{\phi} \frac{1}{\sqrt{2\pi}} e^{\frac{x^2}{2}} dx$, both generating probabilities with the following form:

$$\mathbb{P}(\text{choosing risky}) := F\left(E\left[\frac{L^{1-\sigma}}{1-\sigma}\right] - \frac{c^{1-\sigma}}{1-\sigma}\right)$$

In both cases $\boldsymbol{\beta} = [\beta_0\ \beta_1]'$, and $\sigma := \beta_0 + \beta_1 \cdot male$. In the third specification we model the probability of choosing risky as according with the Luce ratio:

$$\mathbb{P}(\text{choosing risky}) := \frac{E[\frac{L^{1-\sigma}}{1-\sigma}]}{E[\frac{L^{1-\sigma}}{1-\sigma}] + \frac{c^{1-\sigma}}{1-\sigma}}$$

where $\boldsymbol{\beta}$ and σ are defined as before. We seek β_0, β_1 so as to maximize the joint likelihood of the data (clustering for repeated observations). The results of each estimation are reported in Table 24. The results confirm that men are less risk averse than women, regardless of how we model the Bernoulli probabilities.

Choosing for others

When agents choose for others they may 1) choose what they believe to be objectively the best, 2) choose what they think their partner finds most desirable, 3) choose what they find most desirable, or 4) randomize. There is no evidence that that agents randomize in their choices when choosing for others in the ex-post and hidden contract treatments.[105] If agents choose what they believe to be objectively

[105] note that agent choices in the revealed contract treatment are not of interest as they may merely follow the contract assigned to them

Table 25: MLE CRRA probabilistic choice estimation of agent risk aversion in the HC and EP treatments: Female vs. Male (N=630 with 210 clusters*)

	Logistic ($\chi^2 = 9.24$)		Probit ($\chi^2 = 11.61$)		Luce Ratio ($\chi^2 = 11.93$)	
Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	p-value	$\hat{\beta}/se_{\hat{\beta}}$	p-value	$\hat{\beta}/se_{\hat{\beta}}$	p-value
constant	0.081 (0.038)	0.035	0.064 (0.021)	0.002	0.436 (0.178)	0.014
male	-0.080 (0.044)	0.068	-0.050 (0.025)	0.047	-0.530 (0.221)	0.017
ex-post	0.121 (0.068)	0.077	0.063 (0.033)	0.054	0.329 (0.237)	0.165
ex-post × male	-0.102 (0.074)	0.167	-0.051 (0.037)	0.176	-0.255 (0.281)	0.365

*Note: regression estimated on data set restricted to periods 3, 5, 10. Parameters are sensitive for decisions in other periods that are far from the indifference point. A linear random effects log-odds model of binary choice has gender significant as well, but including the certain payment and the probability that risky is high is problematic for these choices as they are perfectly correlated in periods 3, 5, 10.

the best or what they believe their partners find most desirable we need a model of how they define best or a model of how they intuit the preferences of others. It is possible that their own preferences serve as proxies for this, in which case 1) and 2) are indistinguishable from 3).

The present study was not designed to disassociate these influences. Instead were merely check to see if women are more risk-averse than men when choosing for others in the ex-post and hidden contract treatments. In addition, as it was found in Gurdal, Miller, and Rustichini (2007), agents exhibit behavior that would be characterized as more risk-averse in the ex-post treatment than in the hidden contract treatment, we are interested if women are differentially influenced by this treatment effect. In Table 25 we present the estimation of the models presented in Table 24 applied to the ex-post and hidden contract treatments where we control for gender, treatment, and their interaction, i.e. we parameterize risk aversion in our MLE model according to:

$$\sigma := \beta_0 + \beta_1 \cdot \text{male} + \beta_2 \cdot \text{ex-post} + \beta_3 \cdot \text{ex-post} \times \text{male}$$

We see immediately that when choosing for others, womens' choices exhibit a greater degree of risk-aversion than men's choices. In addition, the treatment

Table 26: Mean (st. dev.) principal payments by treatment and gender

Decision	Gender	Lottery High			Lottery Low		
		EP	HC	RC	EP	HC	RC
risky	female	12.20 (3.37)	9.69 (5.14)	7.76 (5.09)	2.48 (3.04)	3.04 (4.32)	3.46 (4.27)
	male	12.23 (3.29)	9.93 (4.91)	8.04 (6.23)	5.26 (5.35)	4.93 (5.05)	4.91 (5.69)
risk-free	female	8.65 (3.45)	7.48 (4.16)	7.34 (3.65)	9.22 (4.02)	n/a	n/a
	male	8.56 (4.01)	9.05 (4.64)	9.56 (5.85)	11.10 (3.78)	n/a	n/a

effects remain when we control for gender, but gender does not interact significantly with treatment, therefore we cannot conclude that women respond differentially to the experimental treatments.

Principal payments

In Table 26 we present a summary of principal payments to agents across Treatment, decision, lottery outcome, and gender. We discuss each treatment separately. In our discussion we will reference various regression models over the following variables:

Dependent variable

partner pay	- amount assigned to parter \$0-\$15

Explanatory variables

male	- equals 1 if male, 0 if female
lottery was chosen	- equals 1 if the lottery was chosen, 0 otherwise
ex-post	- equals 1 if it is the ex-post treatment, 0 otherwise
hidden contract	- equals 1 if it is the hidden contract treatment, 0 otherwise
E[utility premium]	- Eu(chosen option)-Eu(forgone option), an ex-ante measure relative quality
utility premium	- u(payoff received)-u(forgone payoff), an ex-post measure relative quality

Where *E[utility premium]* and *utility premium* are calculated under the assumption that utility is a CRRA utility function with risk-aversion parameter estimated from

Table 27: Model of principal payments to partner in ex-post treatment if risky chosen and lottery is low

	Partial		Full	
Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	p-value	$\hat{\beta}/se_{\hat{\beta}}$	p-value
constant	2.387 (0.486)	0.000	0.892 (0.744)	0.230
male	2.448 (0.821)	0.003	5.476 (1.381)	0.000
E[utility premium]	0.289 (0.141)	0.041	0.331 (0.439)	0.451
E[utility premium] × male	0.478 (0.234)	0.041	-0.063 (0.826)	0.939
utility premium			-0.215 (0.089)	0.016
utility premium × male			0.408 (0.143)	0.004
E[utility premium] × utility premium			-0.005 (0.049)	0.919
E[utility premium] × utility premium × male			-0.054 (0.106)	0.610

(60 female observations), 116 male observations)

the data in the alone treatment ($\sigma = .025$).[106]

A cursory inspection using Table 26 shows that in the ex-post treatment male subjects pay more than female subjects when the lottery is chosen for them by their agent and it yields the low outcome. As these summary measures are collapsed across decision periods they may confound the role for the option desirability and the payoffs. Unfortunately the event when the agent chooses the lottery for their partner and it yields the low outcome is not frequent enough in any given period for us to have enough observations to test for significance in that period, although the difference between gender maintains. We can pool and utilize data from all periods in a statistical test for these differences if we control for confounded factors that differ across periods. In Table 27 we present a panel data random effects

[106]See the calculation in Appendix A for more details. Note it may be more appropriate to use different σs for men and women

regression model of principal payments to the agent when the agent chooses the lottery and the payoff is low. Since the payoff to the principal is $0 for every observation in this model we may look at only how gender and decision quality influence payments. In the left column we present this model of principal payments, notice that men pay significantly more than women when they receive $0 from the lottery and that they are significantly more sensitive to the quality of the decision than women are. Although the payoffs to the principal doesn't vary in this restricted set of observations, the ex-post utility premium does vary since the certain payments are not constant across decision periods. If we add the ex-post utility premium as an explanatory variable, along with its interactions we see that *E[utility premium]* (decision quality) is no longer significant, and the results are being driving primarily by *utility premium*, with men continuing to pay more than women for losses independent of other factors and men also paying relatively more in periods when the forgone option is not as good (but not women).

Table 26 reveals another prominent difference in the ex-post treatment: males pay more than females when the certain payment is chosen and the lottery yields the low outcome. When we break it down by period the male mean is higher than the female mean in every period. For the periods with more than 10 females observations of this event (Periods $1, 3, 6, 7, 8, 10$) it is significant at the 5% level in 4 out of 6 (between subjects ranksum test). We may perform the same pooling done for the risky choice data with the risk-free choice data in order to utilize observations from all periods and test for these differences. In Table 28 we present a panel data random effects regression model of principal payments to agent when the agent chooses the risk-free option. We can see that men pay significantly more than women when the certain payment is better than the payment from the outcome on the lottery forgone.

We have shown that women have a greater disparity in payments across lottery outcomes when the lottery is chosen while men have a greater disparity when the certain payment is chosen. It is useful to compare pooled ex-post data of male and female payments separately to highlight other potential differences. In Table 29 we present the estimates for the coefficients of the independent variables (and their interactions) *lottery was chosen*, *E[utility premium]*, and *utility premium* from a

Table 28: Model of principal payments to partner in ex-post treatment if risk-free chosen and lottery is low

Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	p-value
constant	4.516 (1.066)	0.000
male	2.541 (1.387)	0.067
E[utility premium]	-0.030 (0.537)	0.955
E[utility premium] × male	0.973 (0.639)	0.128
utility premium	0.553 (0.100)	0.000
utility premium × male	-0.128 (0.136)	0.345
E[utility premium] × utility premium	0.020 (0.049)	0.689
E[utility premium] × utility premium × male	-0.086 (0.060)	0.149

random effects regression model of principal payments to their partner in the ex-post treatment. The male versus female differences suggested by this estimate can be further checked for significance and robustness by pooling all the data into a full model with interactions presented in Table 35 (Appendix C). In the separate models of Table 29 the constant term suggests that men may award more overall than women, which is not supported by the coefficient on the *male* dummy variable in the pooled model. The separate models indicate that both genders penalize if the lottery is chosen but the coefficient is negative only for women, this difference is not present in the pooled model as the gender interaction is not significant. The separate models suggest that men may place more weight on *E[utility premium]*, but this is not supported by the pooled model. Consistent with earlier results the separate models indicate that men place more weight than women on *utility premium* if the certain payment is chosen but less weight than women if the lottery is chosen, this difference remains significant in the pooled model by noting the significantly positive coefficient on *utility premium* × *male*

Table 29: Model of principal payments to partner by gender in ex-post treatment

	Male Data		Female Data	
Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	p-value	$\hat{\beta}/se_{\hat{\beta}}$	p-value
constant	8.845 (0.448)	0.000	7.958 (0.593)	0.000
lottery was chosen	-0.464 (0.437)	0.288	-1.260 (0.600)	0.036
E[utility premium]	0.755 (0.106)	0.000	0.577 (0.175)	0.001
E[utility premium] × lottery was chosen	-0.507 (0.184)	0.006	-0.581 (0.232)	0.012
utility premium	0.151 (0.028)	0.000	0.028 (0.041)	0.492
utility premium × lottery was chosen	0.238 (0.038)	0.000	0.525 (0.047)	0.000
E[utility premium] × utility premium	-0.030 (0.008)	0.000	-0.004 (0.011)	0.744

*51 males (510 observations), 25 females (250 observations)

and significantly negative coefficient on *utility premium* × *lottery was chosen* × *male*. Lastly, it appears from the separate models that only men care less about the ex-ante utility (*E[utility premium]*) when the ex-post utility (*utility premium*) is higher, which is confirmed in the pooled model with the significantly negative coefficient on *E[utility premium]* × *utility premium* × *male*.

The revealed contract treatment provides an opportunity to contrast principal payment behavior in the presence of incentives. When the contract becomes a medium through which principals may communicate their preferences their relative consideration of outcomes and actions may change. In Gurdal, Miller, and Rustichini (2007) it was demonstrated that the outcome bias was halved in the revealed contract treatment relative to the ex-post treatment. Of interest is whether gender plays a differential role in the revealed contract treatment itself and if it interacts with any treatment effects. We pool the data from both treatments together by restricting the observations in the ex-post treatment to those where risky is chosen and include only the portions of the revealed contract pertaining to the risky

Table 30: Model of principal payments to partner in revealed contract treatment

Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value
constant	5.876 (0.569)	10.325	0.000
male	1.586 (0.839)	1.892	0.059
E[utility premium]	0.341 (0.089)	3.852	0.000
E[utility premium] × male	0.318 (0.129)	2.460	0.014
utility premium	0.223 (0.021)	10.574	0.000
utility premium × male	-0.018 (0.030)	-0.588	0.556
E[utility premium] × utility premium	0.019 (0.008)	2.301	0.021
E[utility premium] × utility premium × male	-0.014 (0.012)	-1.204	0.229

*980 observations, 49 unique subjects

option.[107] In Table 30 we present a random effects model of principal payments to the agent in the revealed contract treatment. As can be seen men pay significantly more than women and account for the ex-ante appeal of the decision more than women do.[108] In an analysis performed on data in both treatments, we present the estimation results of the random effects regression of partner payments by each gender in Table 31. The gender difference results in this table are merely suggestive and must be confirmed in a pooled model with gender interactions, which is presented in Appendix C (Table 36). Although men appear to pay more overall and only women appear to have a significant overall treatment affect, both of these differences do not hold up in the pooled model. In fact, none of the magnitude

[107]Note that the portion of the contract pertaining to the risk-free option cannot depend on the lottery outcome so is therefore not comparable to the ex-post treatment observations when the risk-free option is chosen.

[108]In addition female principals have significantly more contracts that do not signal their preferences.

Table 31: Model of principal payments to partner ex-post v. revealed contract treatments by gender

	Male Data		Female Data	
Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	p-value	$\hat{\beta}/se_{\hat{\beta}}$	p-value
constant	6.971 (1.006)	0.000	5.501 (0.369)	0.000
ex-post	1.559 (1.106)	0.158	1.238 (0.520)	0.017
E[utility premium]	0.914 (0.132)	0.000	0.331 (0.122)	0.007
E[utility premium] × ex-post	-0.710 (0.198)	0.000	-0.347 (0.196)	0.077
utility premium	0.120 (0.032)	0.000	0.214 (0.028)	0.000
utility premium × ex-post	0.281 (0.047)	0.000	0.352 (0.050)	0.000
E[utility premium] × utility premium	-0.001 (0.012)	0.960	0.025 (0.011)	0.022
E[utility premium] × utility premium × ex-post	-0.036 (0.018)	0.041	-0.029 (0.017)	0.086

*64 males (480 observations), 425 females (41 observations)

differences that point at a treatment effect difference between men and women hold up in the pooled model. The only gender differences that maintain in the pooled model include the male subjects placing more weight on *E[utility premium]* and less weight on *utility premium*.

The results of the hidden contract treatment are largely comparable to that of the revealed contract treatment.[109] The analogous model to that presented in Table 30 is presented in Table 32, and as we can see the results are comparable. The treatment effects between the ex-post and revealed contract treatment are relegated to Appendix C (Table 37 & Table 38) as they are comparable. In addition we present the treatment effects between the hidden contract contract and revealed

[109]For the hidden contract treatment men pay significantly more than women for total paid to the partner and the random other person when pay safe is chosen (in 9 out of 10 periods) and there is a significantly bigger difference between the payment if risky is chosen and it is high and if it is low in 7 out of 10 periods

Table 32: Model of principal payments to partner in hidden contract treatment

Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value
constant	6.013 (0.468)	12.849	0.000
male	1.441 (0.591)	2.437	0.015
E[utility premium]	0.234 (0.064)	3.671	0.000
E[utility premium] × male	0.309 (0.083)	3.706	0.000
utility premium	0.297 (0.016)	18.078	0.000
utility premium × male	-0.075 (0.020)	-3.723	0.000
E[utility premium] × utility premium	0.005 (0.006)	0.828	0.408
E[utility premium] × utility premium × male	-0.004 (0.008)	-0.470	0.639

*2100 observations, 105 unique subjects

contract treatments in Appendix C (Table 39).

3.5 Conclusion

In this paper we investigate differences between men and women in the relative role of actions and outcomes when holding others accountable. We attempt to measure differences in how men and women treat information regarding the quality of someone's decision and the ultimate outcome in their process holding others accountable. We develop a game we term the "accountability game" where one subject, termed the *agent*, chooses between a lottery and a certain amount on behalf of their partner, termed the *principal*. The principal decides how much to pay the agent. We develop a measure of the ex-ante and ex-post desirability of the chosen option and use these measures as proxies for decision quality and outcome quality respectively. We estimate how the principals account for these two factors when they assign payment to the agent. We find that men account more for the quality of the decision and women account more for the payoff (outcome). Women are more risk-averse overall in the experiment and pay significantly less than men when the lottery is chosen for them and they lose. When the certain amount is chosen, women account more for how much they get paid while men account more for how much better the certain amount is than the outcome of the lottery.

The results of this experiment, while only preliminary, are striking and may hold important implications for the understanding of male and female differences in economic decision making more generally. Evaluation judgments are an integral part of many decision making environments, especially in the areas of management, regulation and supervision. Our results suggest men and women differ systematically in how they incorporate the basic components of evaluative judgment, information about decision quality (actions) and performance (outcomes), which lead to differences in how they hold others accountable. These results suggest that women in the position of shaping the behavior of others may encourage more risk-averse behavior, perhaps beyond what their own attitude toward risk may justify. Moreover, in this experiment men (but not women) hold their partners accountable for forgone payoffs even when the certain payments are chosen, inducing risk on those that they are evaluating regardless of their choice and thus possibly encouraging risk taking beyond what the circumstances may justify. An interesting follow up study would test to see if these differences are being driven primarily by individual attitudes towards risk, or something else.

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A Appendix: Chapter 1

A.1 Structural Estimation of Risk Aversion Parameters

We model decision making under uncertainty as according with one of two possible vN.M utility functions, the CRRA utility $u(x) := x^{1-\sigma}/(1-\sigma)$ and the hybrid CRRA/CARA utility known as Expo-Power utility, $u(x) := (1 - \exp(-\alpha x^{1-\sigma}))/\alpha$ (Saha 1993). The expected utility subjects choose between a certain payment c and a lottery L with distribution:

$$L := \begin{cases} h & \text{with probability } p \\ \ell & \text{with probability } 1-p \end{cases}$$

A.1.1 Utility Without Errors

We estimate utility parameters using maximum likelihood estimation under the assumption that subjects choose the risky option according to a Bernoulli probability. We define the Bernoulli probability in three alternate ways:

$$\mathbb{P}(\text{choosing risky}) := N(E[u(L)] - u(c))$$
$$\mathbb{P}(\text{choosing risky}) := \mathit{Logistic}(E[u(L)] - u(c))$$
$$\mathbb{P}(\text{choosing risky}) := \frac{E[u(L)]}{E[u(L)] + u(c)}$$

Where $N(x)$ is the standard normal CDF, $\mathit{Logistic}(x)$ is the sigmoid function $1/(1+e^{-x})$, the last specification for the Bernoulli probabilities is known as the Luce Ratio. Given the observations we maximize the log of the joint likelihood of the sample numerically by varying the utility parameters. When the routine is within our level of tolerance we have determined the estimated utility parameters.[110]

A.1.2 Utility With Errors

We estimate utility parameters using maximum likelihood estimation in a similar way, except in this case an additional parameter μ adds noise to the choices (errors).

[110] Stata procedures outlined in Harrison (2008) were employed.

Table 33: Estimation of Choice Parameters (Clustered MLE)

	Utility Specification				
	CRRA		Expo-Power		
	σ	μ	σ	α	μ
Probit	0.024	-	0.752	1.168	-
Logit	0.013	-	!	!	!
Luce Ratio	0.265	-	-0.526	0.030	-
Probit w/Fechner Error	0.017	2.001	-0.032	2.152	0.005
Logit w/Fechner Error	0.012	1.105	-0.047	1.217	0.006
Luce Error Ratio	0.025	0.199	-0.128	0.207	0.017

$$\mathbb{P}(\text{choosing risky}) := N\left(\frac{E[u(L)] - u(c)}{\mu}\right)$$
$$\mathbb{P}(\text{choosing risky}) := \mathit{Logistic}\left(\frac{E[u(L)] - u(c)}{\mu}\right)$$
$$\mathbb{P}(\text{choosing risky}) := \frac{E[u(L)]^{1/\mu}}{E[u(L)]^{1/\mu} + u(c)^{1/\mu}}$$

The first two specifications are known as Fechner errors, the last is the Luce error.

A.1.3 Estimation

Pooling the principal and agent choices together from the CFY-treatment we estimate the risk aversion parameters for all utility and Bernoulli probability specifications.

A.2 Tables

Table 34: Random effects logistic regressions for pooled agent choice data; $N = 760$, $I = 76$

	EP-treatment			A-treatment		
		$\chi^2_{(4)}$ 142.63			$\chi^2_{(4)}$ 140.39	
Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value
constant	-0.085 0.390	-0.217	0.828	0.068 0.354	0.191	0.848
h	-0.191 0.030	-6.457	0.000	-0.166 0.027	-6.048	0.000
p	-20.922 1.759	-11.893	0.000	-19.363 1.638	-11.823	0.000
c	0.675 0.074	9.173	0.000	0.669 0.070	9.493	0.000
EP/HC-session	0.447 0.313	1.429	0.153	0.361 0.321	1.124	0.261

B Appendix: Chapter 2

B.1 Proofs

Proof of Theorem 1. Let $\boldsymbol{s}_a$ represent the lottery generated by voting safe in Group treatment, and $\boldsymbol{r}_a$ represent the lottery generated by voting risky. Note that, due to the way, voting rule is designed $\boldsymbol{r}_a = \boldsymbol{r} = (p, 0, 1-p)$ where p is the probability of low outcome in the lottery. Assume the probability that all other members will vote for safe (i.e. the subject is pivotal) is given by a. Then we will have, $\boldsymbol{s}_a = (p(1-a), a, (1-p)(1-a))$. We choose the following as the subject's rank dependent expected utility function for these options:

$$U(\boldsymbol{s}_a) = [f(1) - f(p+a-ap)]v(h) + [f(p+a-ap) - f(p-ap)]v(c) + [f(p-ap)]v(l) \tag{B.1}$$

and

$$U(\boldsymbol{r}_a) = [f(p)]v(l) + [f(1) - f(p)]v(h) \tag{B.2}$$

Using B.1 and B.2 above, we can write

$$U(\boldsymbol{r}_a) - U(\boldsymbol{s}_a) = [f(p) - f(p+a-ap)][v(c) - v(h)] + [f(p) - f(p-ap)][v(l) - v(c)] \tag{B.3}$$

Now, since lemma 2 implies $f(\cdot)$ is concave and it also satisfies the equality $f(0) = 0$ by definition, we also have

$$f(p-ap) > (1-a)f(p) \tag{B.4}$$

and

$$f(p+a-ap) = f((1-a)p+a) > (1-a)f(p) + af(1) \quad \text{(B.5)}$$

Using these, and the fact that $f(1) = 1$ and $v(c) - v(h) < 0$, the first one of the two added terms on the right side of B.3 satisfy

$$[f(p) - f(p+a-ap)][v(c) - v(h)] > [af(p) - a][v(c) - v(h)] \quad \text{(B.6)}$$

Similarly since $v(l) - v(c) < 0$, the second one of the two added terms on the right side of B.3 satisfy

$$[f(p) - f(p-ap)][v(l) - v(c)] > [af(p)][v(l) - v(c)] \quad \text{(B.7)}$$

Adding up B.6 and B.7 and using B.3 we have

$$U(\boldsymbol{r}_a) - U(\boldsymbol{s}_a) > af(p)[v(l) - v(c)] + (af(p) - a)[v(c) - v(h)]) \quad \text{(B.8)}$$

Since the individual has $\boldsymbol{r} \sim \boldsymbol{s}$, his preferences satisfy:

$$(1 - f(p))v(h) + f(p)v(l) = v(c) \quad \text{(B.9)}$$

and we can write this as

$$v(h) + f(p)(v(l) - v(h)) = v(c) \quad \text{(B.10)}$$

Rewriting B.8

$$U(\boldsymbol{r}_a) - U(\boldsymbol{s}_a) > a[v(h) + f(p)(v(l) - v(h)) - v(c)] \quad \text{(B.11)}$$

Now, using B.10, B.11 simplifies to

$$U(\boldsymbol{r}_a) - U(\boldsymbol{s}_a) > 0 \quad \text{(B.12)}$$

which implies that $\boldsymbol{r}_a \succ \boldsymbol{s}_a$, so the individual will make a risky shift during the group treatment. □

B.2 Experimental Task - Example

Below, we present a screen that subjects saw during Alone treatment. This menu has a risky option which yields high and low payoffs with equal probability. The other two menus for the same treatment differ in the probabilities as explained in the design section. Risk-free option always takes values between \$6 and \$14. Group treatment screens are the same except that “You are to choose alone” label is changed with “You are in a Group”.

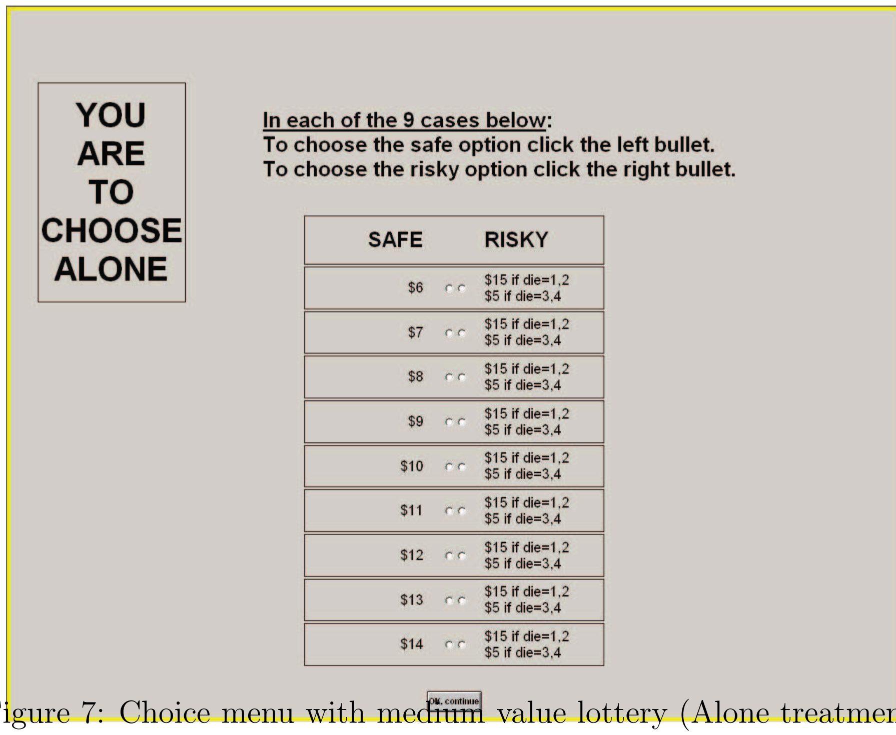

Figure 7: Choice menu with medium value lottery (Alone treatment)

C Appendix: Chapter 3

C.1 Tables

Table 35: Model of principal payments to partner in ex-post treatment ($N = 760$, 76 groups)

Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value
constant	7.978 (0.659)	12.108	0.000
lottery was chosen	-1.295 (0.586)	-2.209	0.027
male	0.872 (0.788)	1.106	0.269
lottery was chosen × male	0.826 (0.733)	1.126	0.260
E[utility premium]	0.577 (0.172)	3.349	0.001
E[utility premium] × lottery was chosen	-0.574 (0.229)	-2.506	0.012
E[utility premium] × male	0.175 (0.203)	0.862	0.388
E[utility premium] × lottery was chosen × male	0.070 (0.295)	0.236	0.814
utility premium	0.023 (0.041)	0.560	0.575
utility premium × lottery was chosen	0.525 (0.046)	11.464	0.000
utility premium × male	0.129 (0.050)	2.582	0.010
utility premium × lottery was chosen × male	-0.288 (0.060)	-4.799	0.000
E[utility premium] × utility premium	-0.004 (0.011)	-0.344	0.731
E[utility premium] × utility premium × male	-0.026 (0.014)	-1.853	0.064

Table 36: Model of principal payments to partner in ex-post v. reveal contract treatment

Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value
constant	5.501 (0.688)	7.993	0.000
ex-post	1.198 (0.836)	1.433	0.152
male	1.469 (1.101)	1.334	0.182
ex-post × male	0.341 (1.272)	0.268	0.789
E[utility premium]	0.331 (0.115)	2.874	0.004
E[utility premium] × male	0.584 (0.176)	3.314	0.001
E[utility premium] × ex-post	-0.331 (0.188)	-1.767	0.077
E[utility premium] × ex-post × male	-0.379 (0.276)	-1.370	0.171
utility premium	0.214 (0.027)	7.961	0.000
utility premium × male	-0.093 (0.042)	-2.231	0.026
utility premium × ex-post	0.337 (0.049)	6.912	0.000
utility premium × ex-post × male	-0.058 (0.068)	-0.848	0.396
E[utility premium] × utility premium	0.025 (0.011)	2.401	0.016
E[utility premium] × utility premium × male	-0.026 (0.016)	-1.649	0.099
E[utility premium] × utility premium × ex-post	-0.028 (0.016)	-1.691	0.091
E[utility premium] × utility premium × ex-post × male	-0.008 (0.024)	-0.308	0.758

Table 37: Model of principal payments to partner ex-post v. hidden contract treatments by gender

	Male Data		Female Data	
Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	p-value	$\hat{\beta}/se_{\hat{\beta}}$	p-value
constant	7.454 (0.381)	0.000	6.013 (0.367)	0.000
ex-post	1.220 (0.319)	0.000	-0.334 (0.391)	0.393
E[utility premium]	0.543 (0.053)	0.000	0.234 (0.065)	0.000
E[utility premium] × ex-post	-0.316 (0.162)	0.051	-0.205 (0.163)	0.209
utility premium	0.222 (0.012)	0.000	0.297 (0.017)	0.000
utility premium × ex-post	0.163 (0.035)	0.000	0.266 (0.045)	0.000
E[utility premium] × utility premium	0.001 (0.005)	0.749	0.005 (0.006)	0.419
E[utility premium] × utility premium × ex-post	-0.027 (0.014)	0.048	-0.015 (0.016)	0.320

*64 males (1500 observations), 41 females (925 observations)

Table 38: Model of principal payments to partner in ex-post v. hidden contract treatment ($N = 2425$, 105 groups)

Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value
constant	6.013 (0.446)	13.470	0.000
ex-post	-0.410 (0.392)	-1.044	0.296
male	1.441 (0.565)	2.550	0.011
ex-post × male	1.624 (0.506)	3.208	0.001
E[utility premium]	0.234 (0.064)	3.632	0.000
E[utility premium] × male	0.309 (0.084)	3.681	0.000
E[utility premium] × ex-post	-0.202 (0.163)	-1.240	0.215
E[utility premium] × ex-post × male	-0.114 (0.230)	-0.494	0.621
utility premium	0.297 (0.017)	17.965	0.000
utility premium × male	-0.075 (0.020)	-3.705	0.000
utility premium × ex-post	0.266 (0.045)	5.876	0.000
utility premium × ex-post × male	-0.103 (0.057)	-1.791	0.073
E[utility premium] × utility premium	0.005 (0.006)	0.820	0.412
E[utility premium] × utility premium × male	-0.004 (0.008)	-0.466	0.642
E[utility premium] × utility premium × ex-post	-0.016 (0.016)	-1.010	0.312
E[utility premium] × utility premium × ex-post × male	-0.012 (0.021)	-0.556	0.578

Table 39: Model of principal payments to partner in revealed contract v. hidden contract treatments

Independent Variable	$\hat{\beta}/se_{\hat{\beta}}$	z	p-value
constant	7.249 (0.486)	14.902	0.000
hidden contract	-1.236 (0.363)	-3.410	0.001
male	-0.898 (0.710)	-1.265	0.206
hidden contract × male	2.340 (0.574)	4.078	0.000
E[utility premium]	0.331 (0.115)	2.863	0.004
E[utility premium] × male	0.584 (0.175)	3.327	0.001
E[utility premium] × hidden contract	-0.097 (0.132)	-0.730	0.465
E[utility premium] × hidden contract × male	-0.275 (0.195)	-1.410	0.159
utility premium	0.214 (0.026)	8.110	0.000
utility premium × male	-0.093 (0.041)	-2.254	0.024
utility premium × hidden contract	0.084 (0.031)	2.684	0.007
utility premium × hidden contract × male	0.018 (0.046)	0.397	0.691
E[utility premium] × utility premium	0.025 (0.011)	2.426	0.015
E[utility premium] × utility premium × male	-0.026 (0.016)	-1.650	0.099
E[utility premium] × utility premium × hidden contract	-0.020 (0.012)	-1.669	0.095
E[utility premium] × utility premium × hidden contract × male	0.022 (0.018)	1.274	0.203

*2680 observations (105 subjects))