Comparing experimental loss framing methods in a multi-leveled lying context

Nicholas Skelley

This version: June 7, 2023

Abstract

Considering recent contradictions in the literature concerning the influence of loss aversion on cheating behavior, I study the extant methodological differences using the die-roll paradigm while comparing two common methods for framing outcomes as losses: the "Expected Value" method relying on the *ex-ante* expected earnings from the experiment and the "Money Manipulation" method that differentiates between participants who start with a zero or non-zero endowment. I find no significant evidence that loss affects (dis)honesty in Expected Value framing, contrasting previous evidence from coin-toss experiments. However, I find evidence that loss *decreases* lying under Money Manipulation framing, consistent with existing die-roll literature and significantly differing from the results of the Expected Value framing method. I consider these differences in the context of defaults, trust, and possible detection limitations. Comparing these results to prior research, I argue that further replications and direct comparisons between experimental methods used to study cheating behavior are necessary for lending broader generalizability and applicability to the results of future cheating experiments.

JEL: D90, D91, D84, C91

1 Introduction

When confronted with asymmetric information, individuals face a decision about what message to send and how much knowledge to share. They can choose to behave honestly, accurately disclosing information to others without withholding influential details. Alternatively, they can behave dishonestly and choose to either misreport their private information or avoid sharing the complete truth of what they know. Examples from numerous contexts abound: a student explaining where their homework is, a salesperson attempting to sell a product, and a job applicant describing their resume in an interview are all situations in which agents have an information advantage and can choose between sending an honest signal and a dishonest one. While an individual's desire to maintain the self-image of a good and honest person may motivate them to avoid dishonesty, incentives to lie pervade everyday life. The threat of a failing grade, the promise of a commission check, or the need for a steady income can drive a person's behavior toward self-interest.

Lying can taint research, unnecessarily raise the cost of goods, and lead consumers to make sub-optimal decisions. Researchers and policymakers frequently rely on survey responses to identify problems and evaluate programs' effectiveness at alleviating those problems. When participants in those surveys lie, it can lead researchers to draw incorrect conclusions about the populations they are studying and thwart attempts to implement helpful policies. In one notable example, research on poverty-alleviation policies in developing economies (Banerjee et al., 2020; Martinelli and Parker, 2009) has uncovered evidence that households underreport their ownership of luxury goods that may lead to their being deemed ineligible for program assistance.

Concerning markets, Akerlof's (1978) famed "market for lemons" is a prime example of a situation in which lying can lead to poor economic decisions. If the seller of a used car knows that their car is a lemon, the seller could dishonestly convince a buyer that the car is in fact good and sell it for a higher price than its value, leading to the buyer's wasting money on a low-value car. Conversely, insurance fraud represents a situation in which consumers can lie to improve their outcomes (Djawadi and Fahr, 2015), possibly hiding preexisting medical conditions or property damage in order to reduce their premiums or get the insurance company to pay for repairs. In the labor market, lying on a resume or in an interview can increase the likelihood of an applicant being offered a job (Djawadi and Fahr, 2015), while lying about the workplace environment, opportunities for advancement, or benefits offered could persuade an applicant to accept a job offer. Without external interventions like lemon and insurance fraud laws, lying within a market can lead to its collapse.

While traditional economic theory suggests that agents will lie when given an incentive to do so, people are frequently honest despite seeming incentives to behave otherwise. Two common explanations for not lying despite incentives to do so are risk aversion—specifically, avoiding potential "reputational costs" (Crede and Bieberstein, 2020)—and the notion of a "moral cost" of lying (Lundquist et al., 2009; Dai et al., 2019). Reputational costs relate to social image (Crede and Bieberstein, 2020; Bursztyn and Jensen, 2017) and agents' desire to be perceived as fair (Andreoni and Bernheim, 2009) and honest (Mazar et al., 2008); when an agent is caught in a lie, they suffer an adverse reaction from the damage done to their public image. However, there is no guarantee that a lying agent will be caught, so the agent suffers the reputational cost with a probability less than one and must make the decision to lie or not under risk. Moral cost, by contrast, is an intrinsic aversion to lying (Lundquist et al., 2009)—the feeling of guilt experienced due to a lifetime of socialization. Due to its intrinsic nature, a lying agent is certain to experience the moral cost of lying.

Considering reputation and moral costs as disincentives to lying, it stands to reason that incentives of sufficient magnitude and certainty could overcome the costs associated with lying and lead an agent to lie. In light of Kahneman and Tversky's (1979) theory of loss aversion—that "losses loom larger than gains"—a worthwhile question is whether potential losses are more convincing incentives to lie than gains. Theoretically, if the *magnitude* of the utility derived from losing x is greater than that obtained by gaining x (true for loss-averse agents), one should be willing to incur a greater cost to avoid losing x than to gain x. Therefore, a potential loss of x would be a greater incentive to lie than a potential gain of x for a loss-averse agent, so we would expect the agent to lie to avoid losing x if they are on the margin for lying to gain x.

While the theory is clear, the influence of loss aversion on lying is uncertain in reality. Certainly, there are numerous instances in which the potential for loss could be an incentive to lie or otherwise behave dishonestly—lying to avoid getting fired from a job for actions at work and cheating on a test to avoid an unexpectedly bad grade among them. However, existing experimental research on the intersection of loss aversion and lying features a surprising amount of yet-unresolved disagreement. Because researchers are trying to study both loss and dishonesty, they employ disparate methods of framing outcomes as losses and rely on different games that allow participants to lie. A hallmark of scientific research is the ability to reproduce results and methods in a variety of contexts, a feat that has yet to be accomplished in considering the intersection of loss and lying. Before generalizing experimental results to practical questions, experimenters must reach a consensus without abundant unexplained contradiction. I aim to identify whether the difference in loss-framing method or lying activity is responsible for the discrepancy in experimental conclusions and in doing so offer possible explanations for the existing disagreement. I find evidence that the paradigm used to study lying is responsible for the main difference (that some research shows evidence of loss aversion while others do not) but that the framing method is instrumental in *reducing* lying in the loss condition compared to the gain treatment. In Section 2, I describe the existing literature related to lying and explore the disagreement over the influence of loss aversion on dishonesty further.

2 Background

There are three paradigms that economists have used most frequently to study deceptive behavior: the coin-toss paradigm (Bucciol and Piovesan, 2011), the die-roll paradigm (Fischbacher and Föllmi-Heusi, 2013), and the matrix task (Mazar et al., 2008). In the coin-toss paradigm, participants learn that their payout is determined by the outcome of a coin toss (Bucciol and Piovesan, 2011); they privately flip a fair coin and report the observed result. Because the experimenters do not observe the coin toss or the outcome, there is no way of knowing whether an individual subject was honest or not in reporting their observation. The result of the coin-toss paradigm is a dichotomous outcome: participants receive a full amount for observing the "good" outcome and nothing otherwise. Consequently, participants also face a binary decision and cannot choose to lie sub-maximally (meaning that there is not an opportunity to misreport the outcome so that the participant earns a nonzero amount that is still less than the maximum payout). In their meta-analysis of experimental deception paradigms, Gerlach et al. (2019) argue that the dichotomous nature of the coin-toss paradigm may decrease the rate of lying relative to other paradigms (namely, the die-roll paradigm) due to the high moral cost of maximal lying.

Similar to the coin-toss paradigm, the die-roll paradigm (Fischbacher and Föllmi-Heusi, 2013) relies on subjects privately observing a random outcome. Participants receive six- (Fischbacher and Föllmi-Heusi, 2013) or ten-sided (Charness et al., 2019) dice and earn a different payoff for each possible outcome. Unlike in the coin-toss paradigm, subjects in the die-roll paradigm can observe outcomes that result in payoffs less than the maximal amount but greater than the minimum. As a result, subjects have the option to lie along a scale (sub-maximally) instead of choosing only between maximal lying and not lying at all. This feature of the die-roll paradigm adds complexity to the decision and, as Gerlach et al. (2019) note, generally produces a higher rate of lying than observed in coin-toss experiments.

The matrix task (Mazar et al., 2008) differs from the coin-toss and die-roll paradigms in that the outcome for each participant depends on their performance in the task. Each subject views a series of pairs of three-by-three matrices containing non-integer entries. They must then identify pairs of entries (the (i, j)th entry in the first matrix and the corresponding (i, j)th entry in the second matrix) whose sum is an integer. For example, if the top left entry in the first matrix was 9.23 and the top left entry in the second matrix was 1.77, the subject would need to identify that those entries comprised a pair of corresponding entries summing to an integer. In the matrix task, the number of entry pairs participants find determines how much they earn. Since individual performance, not a random event, decides payoffs, subjects in matrix activity experiments likely view the decision to lie as being dishonest about their personal skills and abilities instead of a random outcome that was equally likely to yield a different result. Gerlach et al. (2019) provide evidence that lying is less common in experiments using the matrix activity, arguing that individuals are less inclined to lie about their performance and abilities than they are about events outside of their control.

In addition to the three paradigms described above, economists have used other activities to study dishonesty in experiments. In sender-receiver games (Gneezy, 2005), pairs of participants are assigned to sender and receiver roles. Senders view two possible options for payoffs for each player, and receivers (without having seen the payoffs for each) select the option for both players. Before receivers make their decisions, senders have the opportunity to send a message to the receiver to try to influence their decision. Importantly, the best payoff for the receiver also results in the worst payoff for the sender, and vice-versa. Numerous other, less common methods have been used in experiments studying dishonesty as well, including online quizzes where participants can cheat (Hugh-Jones, 2016) and activities in which each participant randomly draws a number that determines their payoff from an envelope (Gneezy et al., 2018). I have chosen to focus on the die-roll and coin-toss paradigms because each has been used often to study deception and because they avoid many of the confounding factors involved in interpersonal interactions (the sender-receiver game) and lying about personal performance (the matrix activity).

In recent years, the literature concerning the influence of loss aversion (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992) on dishonest behavior has grown substantially. Unfortunately, the variety of possible paradigms for studying dishonesty intersected with the different methods for framing outcomes as losses has yielded ambiguity and conflicting results. While Charness et al. (2019) and Ezquerra et al. (2018) use the die-roll paradigm and could not show results indicative of the effects of loss aversion, numerous publications (Garbarino et al., 2019; Duc Huynh, 2020; Gneezy et al., 2018; Grolleau et al., 2016) using the coin-toss paradigm and other, less common activities have offered evidence that loss aversion increases deceptive behavior. For replication purposes, I focus most of my attention on Charness et al. (2019) and Garbarino et al. (2019), hereafter referred to as "CBER" and "GSV," respectively. Of the two main studies that use the die-roll paradigm while studying loss aversion (Charness et al., 2019; Ezquerra et al., 2018), CBER provide the more thorough explanation of their experimental design and results and offer more insights and potential explanations in their discussion. In the coin-toss paradigm, GSV's study is unique in not studying the interaction of additional factors (as, for example, Duc Huynh (2020) does by considering the effect of poverty on deception) and in specifically studying loss aversion. Notably, CBER use a different method for framing outcomes as losses than GSV do. This discrepancy raises the question of whether the lack of loss aversion effects in CBER was the product of the experimental paradigm or the loss framing method.

Under the CBER design, players are assigned to either a money manipulation (MM) or no money manipulation (NMM) treatment arm. From there, the experimenters assign them to either a loss frame (MM-L, NMM-L) or a gain frame (MM-G, NMM-G). In the NMM treatment arm, outcomes in both loss and gain domains are not realized until after the experiment; NMM-L (NMM-G) participants expect to receive a fixed payout (nothing), minus (plus) whatever they "lose" ("gain") in the game. Conversely, both sets of players in the MM treatment arm receive physical cash before playing the game. MM-G players receive an empty envelope for their earnings as well as a full envelope of bills containing their potential earnings, while MM-L players receive a full envelope for their earnings and an empty envelope for money they will *return* to the experimenters.

An anonymous referee for CBER expressed concern that the NMM condition represented a gain domain and the MM condition represented a loss frame, as all NMM participants received their payments after finishing the experiment and all MM participants were given envelopes of cash at the beginning of the activity. However, the results within the MM treatments are significantly different, suggesting the presence of a distinction between the loss and gain framing of the MM condition. Namely, CBER show that while MM-G participants demonstrated dishonest behavior (albeit at a somewhat lower level than both groups of NMM players), MM-L participants did not behave significantly differently from players in the baseline group, who appear to have behaved honestly. CBER consider that giving the participants money could have implicitly imparted a burden of trust onto the players, making those in the MM treatment arm (and especially those in MM-L) want to behave more honestly.

GSV describe a model for loss aversion in risky situations and validate their model using the probability of a "good" outcome as the instrument for framing an outcome as a gain or loss. The model put forth by GSV asserts that for a given lottery of possible outcomes, agents set the lottery's expected value as their reference point. Under this design, an agent facing a low probability of a high payoff and a high probability of a low payoff would have a relatively low reference point. Thus, receiving the low payoff would be a loss, but the loss experienced after having a high *ex-ante* probability of a high payoff and receiving a low payoff would be substantially greater in magnitude.

To validate their model, GSV conduct an experiment using the coin-toss paradigm in which players flipped a coin four times and received payment based on the number of correct *ex-ante* guesses they made about the outcome of the coin toss. While the sequence of four coin tosses introduces the possibility of designing a game in which players can lie at sub-maximal levels, GSV maintain the dichotomous structure of the typical coin-toss paradigm. In the high-probability treatment (H), players earned $2 if they correctly guessed the outcome at least twice and nothing otherwise. In the low-probability treatment (L), players earned $2 if they correctly guessed the outcome at least three times and nothing otherwise.

A possible concern related to the GSV experiment is that payoffs depended on players' *guesses* about the observed outcome of the coin flip. While the correctness of a guess is determined randomly in reality, it is possible that subjects perceive it as a measure of skill. A meta-analysis of deception paradigms shows that participants lie less in games when they view their payout as the result of skill (Gerlach et al., 2019). In light of the literature concerning skill-based payoffs in dishonesty activities, it seems likely that the GSV design would decrease deceptive behavior, so we would expect to see more dishonesty by removing the perceived skill component.

As noted in Section 1, the purpose of this research is to address the disagreement in the methodological literature studying the impact of loss aversion on dishonesty. In particular, I seek to compare the methods used by GSV and CBER to identify why GSV find evidence of loss aversion increasing lying but CBER do not. In Section 3, I present a model adapting GSV's model to be applied to the die-roll paradigm in which multiple levels of lying are available. In Section 4, I describe the experiment I carried out to evaluate the differences in experimental methods, and I highlight some critical methodological tradeoffs. Section 5 details the results of the experiment, and Section 7 concludes.

3 Theoretical Model

While GSV present and validate a model of the decision to lie for loss-averse agents, their model considers only two possible outcomes: a high and a low payoff. One of the advantages of the die-roll paradigm over the coin-toss paradigm is the ability to examine lying behavior across multiple *levels* of lying and the potential for sub-maximal lying. In order to use the die-roll paradigm, I adapt the GSV model to the polytomous die-roll case.

Suppose there is a vector of possible payoffs $\mathbf{x} \in \mathbb{R}^n$. For each outcome $x_i \in \mathbf{x}$, let $x_i < x_{i+1}$. Assigning a uniform distribution to the possible outcomes, the probability of observing each payoff is $\Pr(x_i) = \frac{1}{n}$. In the six-sided die case with distinct payoffs for each outcome, $n = 6$. Then the *ex-ante* expected payoff is $x_e = \mathbb{E}(\mathbf{x}) = \frac{1}{n}\sum_{i=1}^{n} x_i$. In accordance with the GSV model, I assume that an agent sets their reference point (Kahneman and Tversky, 1979) at x_e. Under the assumption that an agent is hurt more by a loss of x than is benefited by a gain of x, an exogenous increase in the *ex-ante* expected payoff would intuitively worsen the sense of loss experienced by observing a constant low outcome (below x_e) and thus increase the disincentive associated with being honest.

Assume an agent's utility from reporting a payoff of x_R after observing a payoff of x_O, $U(x_R \mid x_O)$, is a function of the moral cost of lying, $m(\cdot)$, and the utility of the loss, $l(\cdot)$, or gain, $g(\cdot)$, relative to the reference point, x_e:

$$U(x_R \mid x_O) = g(x_R - x_e) \cdot \mathbb{1}(x_R \geq x_e) - l(x_e - x_R) \cdot \mathbb{1}(x_R \leq x_e) - m(x_R - x_O) \cdot \mathbb{1}(x_R \neq x_O) \quad (1)$$

where $\mathbb{1}(x)$ is a function equal to 1 if x is true and 0 if x is false. Also assume that g, l, and m are twice differentiable and that $g', l' > 0$ and $g'', l'' < 0$. Fischbacher and Föllmi-Heusi (2013), Lundquist et al. (2009), and Mazar et al. (2008) offer evidence that $m' > 0$, so I will also assume (consistent with GSV) that $m' > 0$. By the nature of utility functions, let $g(0) = l(0) = m(0) = 0$ and $g(x), l(x), m(x) > 0$ when $x > 0$. Finally, we must assume that the agent is loss averse—that is, that $l(x) \geq g(x)\ \forall\, x \geq 0$ and that $l'(x) > g'(x)\ \forall\, x \geq 0$ (effectively, that l is steeper than g).

3.1 Effect of increasing *ex-ante* expected payoff on lying

Suppose x_1 and x_2 are fixed payoffs such that $x_1 < x_2$. Let x_e be the expected payoff of unknown value between x_1 and x_2. Let $U(L) = U(x_2 \mid x_1) - U(x_1 \mid x_1)$ represent the *net* utility of dishonestly reporting an outcome of x_2 when observing x_1, or the benefit of lying over telling the truth (i.e., $U(\text{lie}) - U(\text{truth})$). Then

$$\begin{aligned} U(x_2 \mid x_1) =& g(x_2 - x_e) - m(x_2 - x_1) \\ U(x_1 \mid x_1) =& -l(x_e - x_1) \end{aligned}$$

so

$$U(L) = g(x_2 - x_e) + l(x_e - x_1) - m(x_2 - x_1) \tag{2}$$

Then an agent should lie when $U(L) > 0$. I now consider how $U(L)$ is affected by changes in x_e, holding all else constant.

$$\frac{\partial U(L)}{\partial x_e} = l'(x_e - x_1) - g'(x_2 - x_e) \tag{3}$$

Then $U(L)$ is increasing on x_e when $l'(x_e - x_1) > g'(x_2 - x_e)$. Without knowing the exact utility functions involved, it is impossible to know the specific domain on which $U(L)$ is increasing on x_e. However, since $l' > g'$, we know that $l'(x_e - x_1) > g'(x_2 - x_e)$ at least when $x_e - x_1 \leq x_2 - x_e$.[1]

To identify more about the model, I assume CRRA utilities for losses and gains, with l steeper than g by a factor of γ. Then $g(x) = x^\rho$, $l(x) = \gamma x^\rho$. I assume $\rho \in (0, 1)$ and $\gamma > 1$. I also temporarily neglect m.

$$\frac{\partial U(L)}{\partial x_e} = \gamma\rho(x_e - x_1)^{\rho-1} - \rho(x_2 - x_e)^{\rho-1} \tag{4}$$

Now it is clear that $U(L)$ is increasing on x_e when

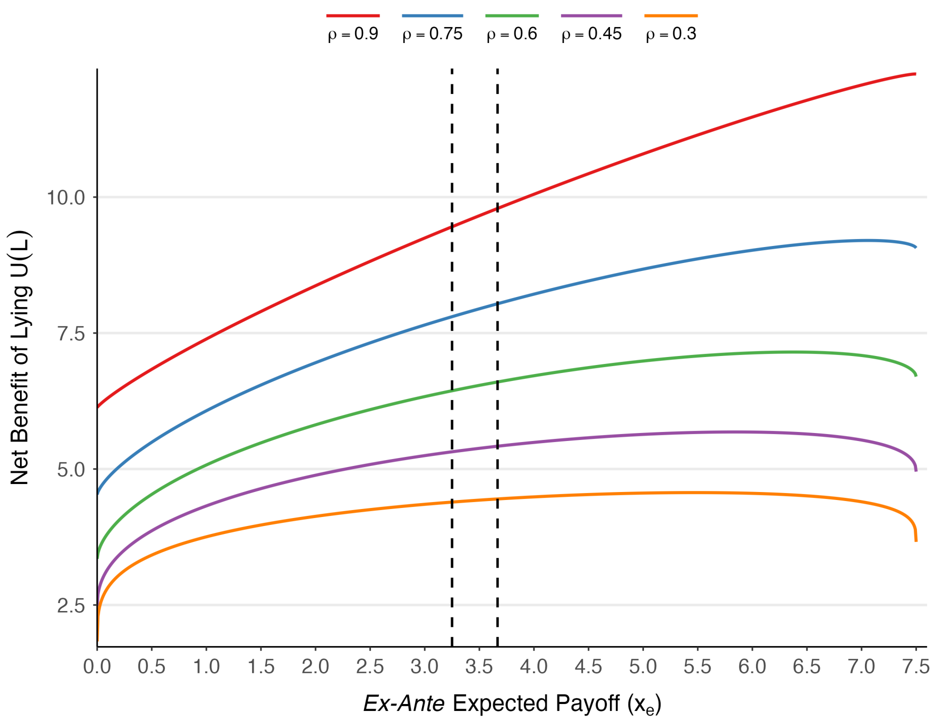
$$x_e < \frac{x_2 + x_1\gamma^{\frac{1}{\rho-1}}}{1 + \gamma^{\frac{1}{\rho-1}}} = x_e^*$$

Notably, $U(L)$ is then *decreasing* on x_e when $x_e > x_e^*$. This result initially seems counterintuitive but reflects the fact that as x_e approaches x_2, the utility gained from lying approaches zero while the effect of the loss suffered marginally diminishes. Under the model without a moral cost associated with lying, agents should always lie when faced with an

[1] See Appendix C for a proof of this property.

opportunity to do so,[2] aligning with traditional theory.

Figure 1: Net benefit of lying as a function of x_e



Note: Utilities of losses and gains are CRRA with $l = 2g$. x_1 and x_2 span the entire range of possible payoffs ($x_1 = 0$, $x_2 = 7.5$) in the Expected Value treatments (see Section 4) of this experiment. Vertical lines represent the values of x_e, 3.25 and 3.67, I use for gain and loss, respectively. Moral cost is omitted.

In this experiment, I have chosen *ex-ante* expected payoffs of 3.25 and 3.67. Figure 1 shows the utility of lying ($U(L) = U(x_2 \mid x_1) - U(x_1 \mid x_1)$) with a variable *ex-ante* expected payoff for three values of ρ used in $l(\cdot)$ and $g(\cdot)$. Vertical lines at 3.25 and 3.67 represent the *ex-ante* expected payoffs I use in this experiment and show that $U(L)$ is increasing on x_e on the interval from 3.25 to 3.67.

Reintroducing m, assume $m(x) = kx^\alpha$ for $k > 0, \alpha > 0$. Now agents should only lie when

$$(x_2 - x_e)^\rho + \gamma(x_e - x_1)^\rho > k(x_2 - x_1)^\alpha \tag{5}$$

[2] $U(L) = g(x_2 - x_e) + l(x_e - x_1)$.

3.1.1 Numeric example of increasing the *ex-ante* expected payoff

Equation 5 represents a general situation in which agents should lie. Turning instead to a more concrete example, suppose Tom and Stacey are participants in this experiment. Tom was raised outside of society and was not conditioned to experience guilt when lying, so his moral cost is zero ($m(x) = 0$). Tom has the utility functions $g_T(x) = x^{0.5}$, $l_T(x) = 2x^{0.5}$. The *ex-ante* expected payoff for both players is \$3.25. Suppose Tom observes a payoff of \$0 but knows he can lie and report a payoff of \$7.50. Then $U_T(7.5 \mid 0) - U_T(0 \mid 0) = \sqrt{4.25} + 2\sqrt{3.25} \approx 5.67$, so Tom should lie and report earning \$7.50. Suppose that instead, the *ex-ante* expected payoff was \$3.67. Then if Tom observed a payoff of \$0, $U_T(7.5 \mid 0) - U_T(0 \mid 0) = \sqrt{3.83} + 2\sqrt{3.67} \approx 5.79$, so Tom should lie. That Tom should lie when the expected payoff is higher is no surprise given that he was already going to lie with the low expected payoff. However, note that his utility gained from lying increases from $x_e = 3.25$ to $x_e = 3.67$.

In contrast to Tom, Stacey experiences guilt when lying. Suppose Stacey has the same utilities as Tom for losses and gains ($l_S(x) = 2x^{0.5}$, $g_S(x) = x^{0.5}$) but also a moral cost of $m_S(x) = \frac{1}{8}x^{1.9}$. If Stacey observes a payoff of \$0 but considers lying to obtain \$7.50 under an *ex-ante* expected payoff of \$3.25, she should lie if and only if $U_S(7.5 \mid 0) - U_S(0 \mid 0) > m_S(7.5)$. Since $\sqrt{4.25} + 2\sqrt{3.25} - \frac{1}{8}(7.5)^{1.9} \approx -0.08$, Stacey should not lie to obtain a payoff of \$7.50.[3] Consider that the *ex-ante* expected payoff was instead \$3.67. Then Stacey should lie to obtain \$7.50 if and only if $\sqrt{3.83} + 2\sqrt{3.67} - \frac{1}{8}(7.5)^{1.9} \approx 0.04 > 0$, which is true. Then Stacey should lie to obtain \$7.50 when $x_e = 3.67$ but not when $x_e = 3.25$. As with Tom, Stacey's utility of lying increases as x_e increases.

In Section 4, I test this model by implementing a loss and a gain treatment that rely on the *ex-ante* expected payoff. In the loss treatment, $x_e \approx 3.67$, and in the gain treatment, $x_e = 3.25$. The model described above predicts an increase in lying among participants who face the higher *ex-ante* expected payoff. However, note that in the example of Stacey's decision, Stacey was very close to the margin of lying to begin with; it is unclear how sensitive agents are to changes in x_e without identifying specific values (or intervals) for ρ and α in particular.

[3] However, Stacey would be willing to lie for a payoff of up to \$7.43 if given the option to do so.

3.2 Effect of increasing the reported value on lying

Suppose x_1 and x_2 are payoffs such that $x_1 < x_2$. Unlike in the previous subsection, we will consider the case when x_1 is fixed and x_2 is variable. Let x_e be the *ex-ante* expected payoff of fixed value such that $x_1 < x_e$. Consider the case when the agent observes x_1 and considers falsely reporting having observed x_2. Then the agent strictly prefers to falsely report x_2 instead of honestly reporting x_1 if and only if $U(L) = U(x_2 \mid x_1) - U(x_1 \mid x_1) > 0$. Then $U(L) = U(x_2 \mid x_1) + l(x_e - x_1)$. There are three cases: when $x_2 < x_e$, $x_2 > x_e$, and $x_2 = x_e$.

When $x_2 < x_e$, falsely reporting x_2 still results in a loss, albeit a smaller loss than reporting the truthful x_1. When $x_2 > x_e$, we see the case from the previous subsection, when reporting x_2 results in a gain, while reporting x_1 remains a loss. Finally, when $x_2 = x_e$, the utility of reporting x_2 is zero, but the moral cost of lying remains, and the agent does not incur the loss associated with reporting x_1. Thus,

$$U(L) = \begin{cases} -l(x_e - x_2) - m(x_2 - x_1) + l(x_e - x_1), & x_2 < x_e \\ g(x_2 - x_e) - m(x_2 - x_1) + l(x_e - x_1), & x_2 > x_e \\ -m(x_e - x_1) + l(x_e - x_1), & x_2 = x_e \end{cases} \tag{6}$$

Suppose a decision-maker considers the optimal outcome to report having observed and that in doing so, they first consider not lying ($x_2 = x_1$) and then progressively increase the hypothetical reported payoff (x_2). Then it is critical to understand how increasing x_2 affects the net benefit of lying, $U(L)$. From Equation 6, it is evident that the partial derivative of $U(L)$ with respect to x_2 depends heavily on x_1, x_e, and the functional forms of losses, gains, and the moral cost of lying:

$$\frac{\partial U(L)}{\partial x_2} = \begin{cases} l'(x_e - x_2) - m'(x_2 - x_1), & x_2 < x_e \\ g'(x_2 - x_e) - m'(x_2 - x_1), & x_2 > x_e \\ 0, & x_2 = x_e \end{cases} \tag{7}$$

Imposing the same assumptions about the utility functions for loss, gain, and moral cost of lying, we have $g(x) = x^\rho$, $l(x) = \gamma x^\rho$, $m(x) = kx^\alpha$ for $\rho \in (0, 1)$, $\gamma > 1$, and $\alpha, k > 0$. Then $U(L)$ is increasing on x_2 when

$$\frac{\partial U(L)}{\partial x_2} > 0 \iff \begin{cases} \gamma\rho(x_e - x_2)^{\rho-1} > \alpha k(x_2 - x_1)^{\alpha-1}, & x_2 < x_e \\ \rho(x_2 - x_e)^{\rho-1} > \alpha k(x_2 - x_1)^{\alpha-1}, & x_2 < x_e \end{cases} \tag{8}$$

Instead of deciding between lying and not lying as the evaluation of when $U(L) > 0$ describes, suppose the agent is identifying the optimal payoff to report. Then the agent is only considering $U(x_2 \mid x_1)$, given a fixed observation x_1 and *ex-ante* expected payoff x_e. Then the agent's optimal reported payoff is

$$\arg\min_{x_2} \quad U(x_2 \mid x_1) = \begin{cases} -l(x_e - x_2) - m(x_2 - x_1), & x_2 < x_e \\ -m(x_e - x_1), & x_2 = x_e \\ g(x_2 - x_e) - m(x_2 - x_1), & x_2 > x_e \end{cases} \tag{9}$$

$$\text{s.t.} \quad x_{min} \leq x_2 \leq x_{max}, \quad \text{for fixed payoff bounds } x_{min} \text{ and } x_{max}$$

Considering the optimization problem for the continuous segments of $U(x_2 \mid x_1)$, the agent maximizes their utility by reporting x_2 such that $l'(x_e - x_2) - m'(x_2 - x_1) = 0$ or $g'(x_2 - x_e) - m'(x_2 - x_e) = 0$. Again imposing the functional forms described above, the agent should choose x_2 such that $\gamma\rho(x_e - x_2)^{\rho-1} = \alpha k(x_2 - x_1)^{\alpha-1}$ or $\rho(x_2 - x_e)^{\rho-1} = \alpha k(x_2 - x_1)^{\alpha-1}$.

3.2.1 Numeric example of increasing the value to report

In the previous subsection, I used the hypothetical agents of Tom and Stacey to illustrate the effect of an increase in the *ex-ante* expected payoff on their desire to lie at the maximal level. I now use these same agents to demonstrate the optimal reported payoff values under specific utility functions.

Tom has $g_T(x) = x^{0.5}$, $l_T(x) = 2x^{0.5}$, and $m_T(x) = 0$ (Tom does not experience a moral cost from lying). Suppose payoffs can only fall between \$0.00 and \$7.50, and that payoffs are possible if they fall in the continuous interval from 0 to 7.5. Further, assume the *ex-ante* expected payoff is \$3.25. If Tom observes a payoff of 0, then Tom's optimal report solves the following optimization problem:

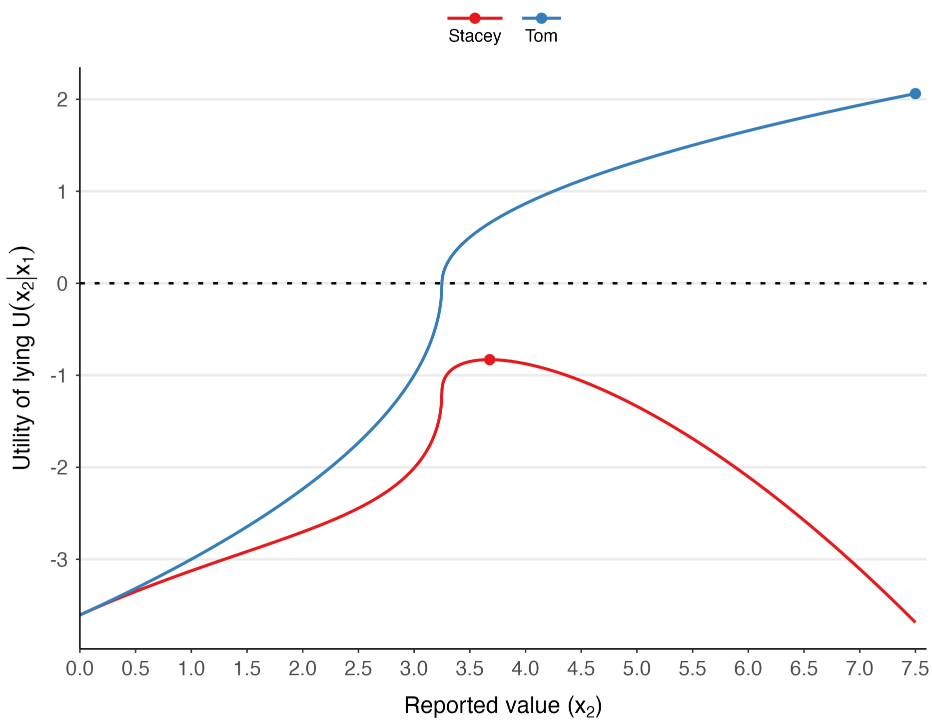
$$\min_{x_2} \quad U(x_2 \mid 0) = \begin{cases} -2(3.25 - x_2)^{0.5}, & x_2 < 3.25 \\ 0, & x_2 = 3.25 \\ (x_2 - 3.25)^{0.5}, & x_2 > 3.25 \end{cases}$$

Then the payoff constraint is binding for Tom, so he should report a payoff of \$7.50. Stacey has identical utility functions for gains and losses ($g_S(x) = x^{0.5}$, $l_S(x) = 2x^{0.5}$) but has a nonzero moral cost of lying: $m_S(x) = \frac{1}{8}x^{1.9}$. Then if Stacey observes a payoff of 0, her optimal report must solve the following optimization problem:

$$\min_{x_2} \quad U(x_2 \mid 0) = \begin{cases} -2(3.25 - x_2)^{0.5} - \frac{1}{8}(x_2)^{1.9}, & x_2 < 3.25 \\ -2.889, & x_2 = 3.25 \\ (x_2 - 3.25)^{0.5} - \frac{1}{8}(x_2)^{1.9}, & x_2 > 3.25 \end{cases}$$

Then Stacey should report a payoff of x_2 such that either (a) $-(3.25-x_2)^{-0.5}-\frac{19}{800}(x_2)^{0.9} = 0$ or (b) $\frac{1}{2}(x_2-3.25)^{-0.5} - \frac{19}{800}(x_2)^{0.9} = 0$. Only (b) has a solution (of $x_2 \approx 3.6756$), so Stacey should report a payoff of \$3.68. In the previous subsection, I noted that Stacey would be willing to report up to \$7.43. By reporting \$7.43, Stacey's net benefit from lying ($U(L)$) is zero, while reporting \$3.68 yields a net benefit of lying of approximately 2.775. Figure 2 shows the utility of lying at different values of x_2 for Tom and Stacey.

Figure 2: Utility of lying as a function of x_2



4 Method

I use a cross-sectional experimental design to test the theoretical model outlined in Section 3 and evaluate the influence of loss aversion of lying. I recruited 410 participants through

Amazon's Mechanical Turk (MTurk). MTurk workers tend to be older on average than the college students typically recruited for experimental studies; from an age perspective, we would then expect MTurk workers to then be more representative of the general population than ordinary experimental samples. Additionally, the MTurk platform allows for workers to participate anonymously and without any chance of being observed, a characteristic that is critical for avoiding the influence of possible fears of observability and reputational costs. However, Gerlach et al. (2019) raise concerns that participants obtained through MTurk may be "professional survey takers" and thus behave more deceptively than student samples, since they can be more skilled or experienced players and may derive their primary income from study payments. I discuss these concerns in more detail in Section 6.

Each participant had an equal probability of being assigned to each of the five treatment arms:

No Incentive (C): Participants earn \$2.50, regardless of what they roll.

Money Manipulation Gain (MG): Outcomes are framed as gains. A participant who rolls the number n earns $\$(n-1)$ (i.e., rolling a 6 is worth \$5.00, rolling a 2 is worth \$1.00, etc.).

Money Manipulation Loss (ML): Participants earn the same amount for each roll that they would in the Money Manipulation Gain arm, but I frame the outcome as a loss by informing them that they are endowed with \$5.00 to start and will lose the corresponding amount based on their roll (e.g., rolling a 6 is a *loss* of \$0, rolling a 2 is a *loss* of \$4).

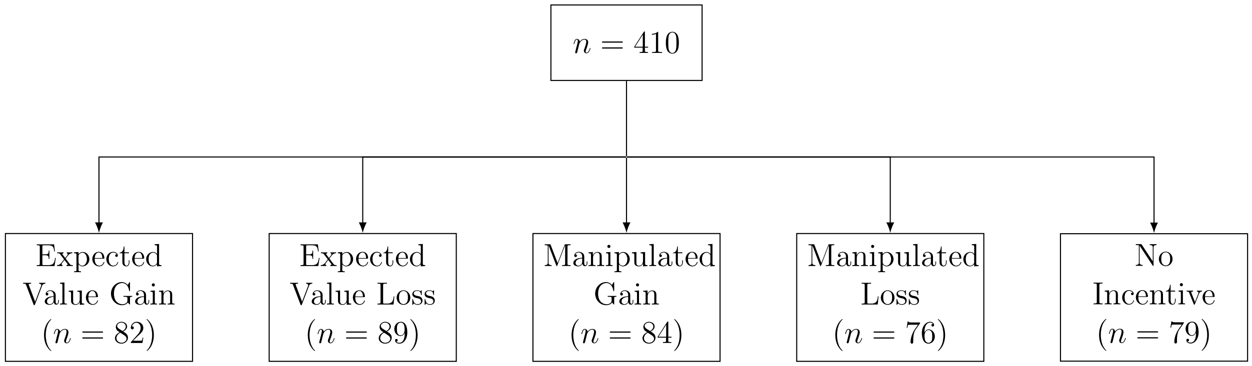
Expected Value Gain (EVG): The instructions describe payoffs as positive earnings (gains), but the *ex-ante* expected payoff is \$3.25 with a maximum payoff of \$7.50.

Expected Value Loss (EVL): The instructions describe payoffs as positive earnings (gains), but I increase the *ex-ante* expected payoff to \$3.67 with a maximum payoff of \$7.50.

4.1 Experimental Design

Participants completed the experiment individually on a personal computer, mobile phone, tablet, or other device capable of accessing the Internet and MTurk through the Qualtrics

Figure 3: Treatment Arms



survey platform. There were no synchronous experimental sessions for participants to attend; instead, each participant completed the activity on their personal devices at their convenience. While completing the experiment, participants observed their private (condition-dependent) instructions and were directed to a digital, six-sided die[4]. I used an external website for the die roll instead of alternative options, like a random number generator embedded within the Qualtrics survey, for two main reasons. First, I structured the external site so that participants would feel a sense of agency from pressing the “Roll” button and see the visual cue of the result of their roll analogous to what they would have experienced if they had rolled a physical die. Second, I was concerned that if participants observed their roll embedded in the survey, they might doubt that the roll was private to them or that it was truly random. While I created the website used for the virtual die roll, there was no way of tracking participants as they rolled the die, and I did not collect data directly from the die roll website.[5] Creating the website for the die roll game carries with it the risk of leading some participants to believe I would be able to observe their individual rolls. However, most existing, easily accessible sites had too many settings to change (the number of dice or the number of sides on each die, for example) or ads and other features. To avoid confusing settings and the resulting need to further complicate the instructions, and to avoid ads that

[4]The instructions gave subjects the option to roll a physical die if one was easily accessible. An archived version of the website participants were directed to can be viewed at https://bit.ly/dierollarchive. Images of the rolled die also appeared on the site; images of the die appear in the archived version as well but are not preloaded and may not immediately appear until they have loaded for the first time and been cached.

[5]Furthermore, the site’s code was entirely client-sided, so participants could have viewed the code on their own and verified that the die roll was random and was not recorded or saved.

may influence decisions,[6] the only reasonable option was to create the website myself. Figure A.1 shows the outcomes of 10,000 individual digital die rolls performed on the website. The result resembles a uniform distribution,[7] indicating that the digital die roll is an accurate representation of rolling a fair, six-sided die.

In the instructions, participants learned that they could roll the virtual die as many times as they would like but that they should record only the first number they saw (1, 2, 3, 4, 5, or 6) in the online survey. While allowing subjects to roll the die multiple times is standard in the experimental literature, and preventing or detecting multiple rolls would not have been feasible under this design, Shalvi et al. (2011) find evidence that lying increases when subjects observe "desired counterfactuals" and hypothesize that observing a higher die roll than the first (the roll they're *supposed* to report) allows participants to justify lying thus reduce the sense of unethicality and immorality associated with the decision. There was a "practice round" in which participants were told that they had rolled a four and recorded what their hypothetical earnings would be based on their treatment arm. In addition to ensuring that subjects understood the instructions, the practice round guaranteed that they were paying attention to the survey, as Qualtrics would not let them advance to the activity until they correctly completed the practice round.

To translate the Expected Value method for loss framing (GSV) from the coin-toss paradigm to the die-roll paradigm, I manipulate the *ex-ante* expected payoff of the die-roll activity. Based on the models presented by GSV and in Section 3, the increase in the expected value of rolling the die theoretically increases the probability of an individual lying. In the EVL treatment, I increase payoffs by 50% for rolls of three or higher, yielding an *ex-ante* expected payoff of \$3.67. In the EVG treatment, I increase payoffs by 50% only for rolls of 5 or higher. The *ex-ante* expected payoff in the EVG treatment is \$3.25. There are numerous alternative schemas for changing the expected payoffs between loss and gain framing, including adding a scalar to all payments in the loss frame, adding a scalar to only some payments in the loss frame, and multiplying all payment values by a scalar greater than one in the loss frame. However, the method I have chosen preserves the minimum and maximum payments between the two frames (\$1.00 if paid, and \$7.50, respectively) and, in eight of the 10 total cases, allows marginal benefits of lying of only \$1.00 or \$1.50. According to the model described in Section 3, the higher expected value in the EVL treatment makes

[6]For example, Mazar et al.'s (2008) results from Experiment 1 indicate that an ad for a religious text could induce a subject to report their die roll honestly when they otherwise might not.

[7]A Kolmogorov-Smirnov test comparing the distribution to a discrete uniform distribution over the integers from 1 to 6 reports a p-value of 0.939.

low payoffs more painful and theoretically produces more lying than would be present in the EVG treatment. For low rolls, subjects in the loss frame feel a disappointment greater than those in the gain frame by a magnitude of about what they'd experience after losing \$0.42. While \$0.42 may not be sufficient to drive a noticeable shift in behavior, GSV find substantial behavioral differences under a difference of \$0.75. The full instructions can be found in Appendix E.

Table 1 shows the amount earned by participants based on their treatment group and the number they report having rolled. After completing the activity and survey, I selected slightly more than half of the subjects at random to receive the payoff they earned in the experiment.

Table 1: Payoffs by Treatment

		Die roll value					
		1	2	3	4	5	6
Payoff	EVG	\$0.00	\$1.00	\$2.00	\$3.00	\$6.00	\$7.50
	EVL	\$0.00	\$1.00	\$3.00	\$4.50	\$6.00	\$7.50
	MG	\$0.00	\$1.00	\$2.00	\$3.00	\$4.00	\$5.00
	ML	\$0.00	\$1.00	\$2.00	\$3.00	\$4.00	\$5.00
	C	\$2.50	\$2.50	\$2.50	\$2.50	\$2.50	\$2.50

After participants rolled the die and recorded the outcomes, they completed a survey asking about demographics, risk attitudes and behaviors, and extraneous questions designed to distract from the risk attitudes question. The question about risk attitudes[8] comes from the 2004 German Socio-Economic Panel (SOEP) questionnaire and has been experimentally validated as a measure of risk aversion (Dohmen et al., 2011). Extraneous questions come from the General Social Survey questionnaire, and the survey platform placed all of the survey questions, including the SOEP risk question, in random order. Appendix E contains the full experimental instructions, and Subsection E.6 shows the survey instrument participants completed.[9]

I recruited participants and ran the experiment between February and March of 2022. Based on the time spent in the Qualtrics survey, subjects completed the experiment with a

[8] "Please indicate the degree to which you agree or disagree with the following statement: I am generally a person who is fully prepared to take risks."

[9] The Emory University Institutional Review Board approved this experimental design (Study No. 00003850) on Feburary 21, 2022.

median duration of 3 minutes and 43 seconds, and the mean of the middle 98% of durations was 4 minutes and 37 seconds. Due to the online format of the experiment, there are a small number of participants with extreme durations lasting several hours, including a maximum observed duration of more than 12 hours.[10] I chose 210 subjects (approximately 51.2%) at random to receive the payments they reported having earned in the game.[11] The average payment amount was $3.84 among those who were paid. Based on the number of subjects paid in each treatment condition (see Appendix A for group-specific payment statistics and a comparison of paid and unpaid amounts), the expected payment amount under the assumption that none of the participants lied would have been $2.90; thus, the realized average payment was about 32% higher than expected in a completely honest set of participants.

4.2 Hypotheses

The two main questions at the heart of this research ask how loss aversion affects dishonest behavior (if at all) and how the Expected Value and Money Manipulation methods for loss framing compare in an activity that allows multiple levels of lying.

Hypothesis 1. *Loss will not produce more extreme lying, but it will increase the incidence of lying.*

In evaluating why they were unable to observe behavior consistent with loss aversion using a loss framing method other than money manipulation, CBER demonstrate that most of the participants who lied in both the loss and gain treatments did so at the maximal level. Therefore, it was impossible for them to find evidence of loss framing leading to more extreme lying. Since CBER also used the die-roll paradigm, it would be reasonable to expect similar behavior in this experiment. However, I do expect to find evidence that more participants lie in the loss treatments than in the gain treatments (i.e., that the rate of lying increases), even if the lying itself cannot possibly be more extreme. I form this hypothesis based on the results of GSV, who find evidence of increased lying to avoid a bad outcome in their (Expected Value) loss treatment compared to their gain treatment. While I am conducting this experiment using the die roll paradigm and therefore do not have a single "good outcome" and a single "bad outcome," I still expect the Expected Value method

[10] The presence of outliers necessitates reporting summary statistics for the first-through-99th percentile of results for outlier-irresistant measures. Additional duration statistics, including full-sample measures and statistics for individual treatment conditions, are available in Appendix A.

[11] Based on the budget constraints associated with this experiment, I maximized the number of participants I could pay if chosen at random by paying 210.

to produce loss aversion, as seen in GSV. In the Money Manipulation methods, I similarly expect to see the rate of lying rise in the loss treatment compared to the gain treatment, consistent with results outside of the die-roll paradigm (Gneezy et al., 2018; Garbarino et al., 2019; Duc Huynh, 2020; Grolleau et al., 2016). However, as I note in Hypothesis 2, I do not expect lying as a whole to be greater in the ML treatment than the MG, so I anticipate that while fewer participants will be honest in the ML treatment, liars in the ML treatment will also predominantly lie sub-maximally (consistent with CBER).

HYPOTHESIS 2. *Money manipulation will not show more lying in the domain of losses than in gains.*

It can be reasonably argued that Money Manipulation treatments will not demonstrate evidence of loss aversion affecting lying, in line with the results from CBER. Since Ezquerra et al. (2018) find the same results as CBER while using a similar experimental design, it is highly unlikely that the results presented by CBER represent one-time random null results. However, there is also a strong argument that the Money Manipulation treatments will be perceived differently in this experiment than in those of CBER and Ezquerra et al. (2018). Since I have not given participants physical cash and have instead only informed them of their initial endowments, it is possible that they will not feel that they have been entrusted with money as CBER argue is a possible explanation for their results. Since experiments across coin-toss (Duc Huynh, 2020; Garbarino et al., 2019) and other paradigms (Grolleau et al., 2016; Gneezy et al., 2018) have asserted that lying is more common when outcomes are framed as losses, it would be surprising not to observe similar results when additional factors, like a feeling of being entrusted with money, have been eliminated. Still, participants in this experiment may have felt that their endowments reflected trust being placed in them to behave honestly, so I predict that the effects of the Money Manipulation treatments will be consistent with the current literature. While the discrepancy in experimental settings and methods is a weakness in the replication of the CBER method, it could also shed light on the mechanisms responsible for the results they observed.

HYPOTHESIS 3. *Loss in Expected Value treatments will increase lying relative to gains.*

I expect the Expected Value treatment to yield a significant increase in dishonesty among participants in the loss treatment. GSV found evidence consistent with loss aversion using the coin-toss paradigm, and Gerlach et al. (2019) demonstrate that experimental subjects are less likely to lie when sub-maximal lying is not possible. Therefore, I expect that by bringing the GSV method of loss framing into a paradigm that allows for sub-maximal lying, I will observe levels of dishonesty no lower than those found by GSV. Further, GSV validate a theoretical model of the influence of the expected value of a random variable on dishonest behavior under the assumption of a loss-averse agent. In Section 3, I have adapted their validated model to the die-roll case and arrived at the same conclusion, indicating that the same theory that was validated with the coin-toss paradigm should hold with the die-roll paradigm.

5 Results

I begin by examining the general behaviors exhibited in the experiment. Figure 4 shows the distributions of reported die rolls within each treatment group. Red horizontal lines at $\frac{1}{6}$ represent the expected frequency of each reported roll under a uniform distribution, how we would expect die rolls to be distributed if no one lied. Visually, it is clear that the distributions of reported die rolls are not uniform for the Expected Value Gain (EVG), Expected Value Loss (EVL), and Money Manipulation Gain (MG) treatments, and that the distribution may be non-uniform for the Money Manipulation Loss (ML) treatment as well. Table A.1 shows the p-values from Kolmogorov-Smirnov tests comparing the observed distributions to the theoretical uniform distribution and confirms that we can conclude that all of the distributions except for the Money Manipulation Loss and control groups did not come from a uniform distribution.[12] In particular, there appears to be evidence of underreporting "low" values (1-3) and overreporting "high" values (4-6) in all of the treatments except for the ML and control groups. The uniformity of the control group supported by Figure 4 and Table A.1 indicates that the control group is a valid comparison group and that participants in the control group did not experience any incentive to lie about their observed die roll. Comparing the other distributions to the control group, it is evident that, as expected, participants in the EVG, EVL, and MG treatments made false reports of what number they rolled.

In Table 2, I present the mean and standard error of the die rolls from each treatment group, and I report the cumulative density of each roll. As expected from Figure 4, the control group's mean die roll of 3.67 is the closest to 3.5 (the expected mean under no lying) and doesn't display any unexpectedly large increases in the cumulative density from roll to roll; the largest marginal change in density, between 3 and 4, is only 29% larger than expected. By contrast, the largest jump in the Money Manipulation Loss condition comes

[12]From Table A.1, the Money Manipulation Loss group's p-value (0.083) is substantially closer to the threshold for concluding that it does not come from a uniform distribution than the control group's (0.838).

between 5 and 6, and, at a magnitude of 0.25, is about 50% larger than expected. While Table A.1 shows that the Money Manipulation Loss distribution is not significantly different from the uniform distribution at the 5% significance level, only 35.5% of observations come in the lowest-paying half of die rolls (1-3). While not as extreme as the other treatment groups, proportion remains 29% lower than expected in the absence of lying. This effect, combined with the higher mean die roll, suggests that lying may still be taking place in the Money Manipulation Loss treatment.

Figure 4: Observed Die Roll Distributions

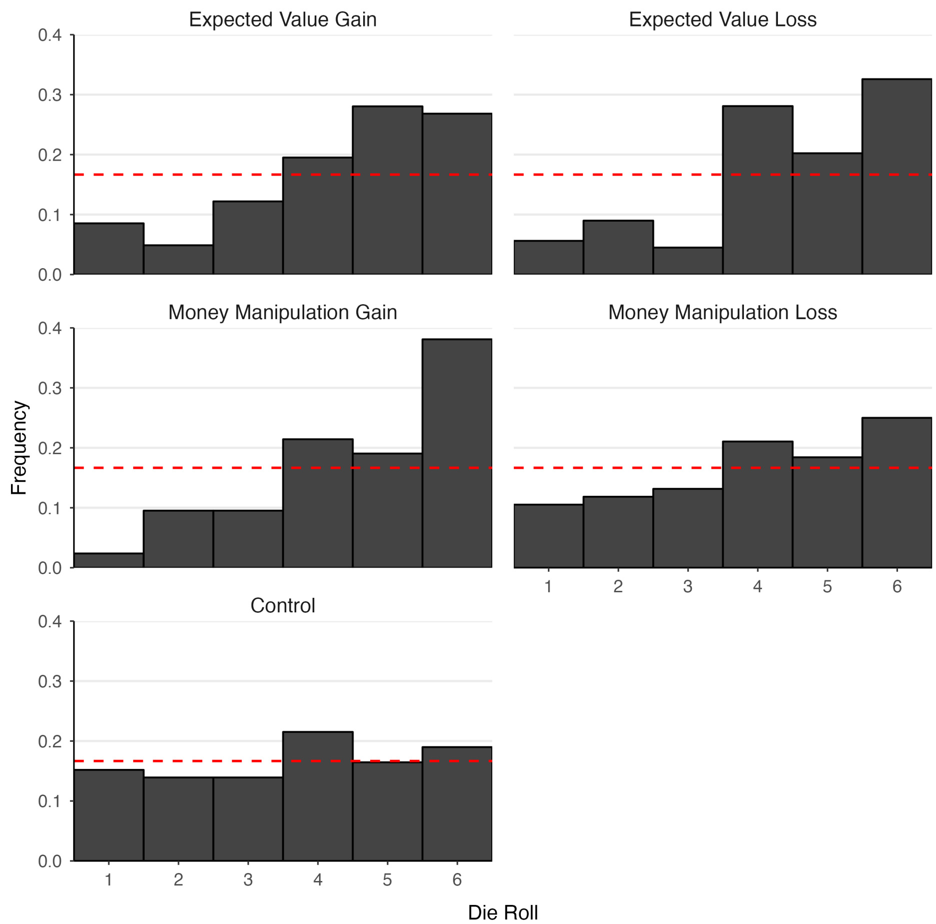


Table 3, along with Table A.6 using OLS, shows the results of two-sided Mann-Whitney tests comparing (a) the mean die roll and (b) the estimated lower bound of the rate of honest reports in each treatment group to the control group. Under Gerlach et al.'s (2019) assumption that subjects never lie to reduce their earnings (that is, that they always report *at least* what they actually observed), I treat the share of subjects who report rolling a one,

Table 2: Cumulative Roll Density

	Obs.	Mean	SE	1	2	3	4	5	6
C	79	3.671	0.192	0.152	0.291	0.430	0.646	0.810	1.000
EVG	82	4.341	0.169	0.085	0.134	0.256	0.451	0.732	1.000
EVL	88	4.500	0.155	0.045	0.136	0.182	0.466	0.670	1.000
MG	84	4.595	0.157	0.024	0.119	0.214	0.429	0.619	1.000
ML	76	4.000	0.191	0.105	0.224	0.355	0.566	0.750	1.000

relative to the share *expected* to roll a one if no one lied, as a lower bound for the rate of honesty.[13]

Table 3: Mean Differences Between Treatments and Control (Mann-Whitney)

	Outcome	
	Die Roll	Honesty Rate
Expected Value Loss	0.790***	−0.524***
	(1.493)	(0.488)
Expected Value Gain	0.671**	−0.357*
	(1.533)	(0.519)
Money Manipulation Loss	0.329	−0.242
	(1.665)	(0.506)
Money Manipulation Gain	0.924***	−0.714***
	(1.441)	(0.363)
Control Mean	3.671	0.857
Control SD	1.708	0.363

*p< 0.1, **p< 0.05, ***p< 0.01

Note: Coefficients represent the difference in mean outcome value between the specified treatment group and the control group. p-values were obtained through Wilcoxon rank sum (Mann-Whitney) tests. Outcome standard deviations for each treatment are specified in parentheses.

Tables 3 and A.6 further confirm the conclusions evident from Figure 4 and Table A.1, that participants in the treatment groups lied about their observed die rolls. In addition to supporting this result, Table 3 reveals the magnitude of lying and an approximation of the rate of lying within each condition. Notably, there appear to have been more liars in the EVL treatment than EVG, yielding a higher mean die roll. Conversely, the ML treatment

[13]The exact formula Gerlach et al. (2019) use to estimate M_{honest}, the rate of honesty, is $M_{honest} = \frac{C(t_{min})}{\mathbb{E}(t_{min})}$, where $C(t_{min})$ is the proportion of participants who claimed the lowest-paying outcome (in this experiment, a one), and $\mathbb{E}(t_{min})$ is the *expected* incidence of the lowest-paying outcome, assuming no one lies (in this experiment, $\frac{1}{6} \approx 0.167$). Thus, I estimate M_{honest} using $M_{honest} = 6 \cdot C(t_1)$, where $C(t_1)$ represents the proportion of subjects who report rolling a one.

produced an increase in the mean die roll that is not statistically significant, and we cannot conclude that the ML treatment produces statistically distinguishable effects on the rate of lying compared to the control group.

Result 1. *There is no significant evidence that loss increases the incidence of lying in Expected Value treatments.*

Concerning the first hypothesis, that "loss will not produce more extreme lying, but it will increase the incidence of lying," Table 4 shows that two-sided Mann-Whitney tests comparing loss and gain treatments do not find significant differences in either outcome for Expected Value treatments. Table A.7 shows a similar lack of statistical significance using OLS regressions. Although the results are not significant, Figure 4 visually indicates that both gain and loss treatments exhibit a substantial number of participants lying at the maximal (or near-maximal) level. Additionally, lying does not appear to be more extreme in the Expected Value Loss treatment than in the Expected Value Gain treatment. However, the mean reported roll in the EVL treatment is greater than the mean reported roll for EVG participants. While this difference is not statistically significant, it is clear that within this sample, there were more liars in the loss treatment than in the gain. The difference of 0.167 in the rate of honesty, for instance, indicates that there were 16.7 percentage points (or about 33.4%) more liars in EVL compared to EVG.

Because the differences in mean roll and honesty rate are small relative to the standard deviation in the outcomes in the EVG treatment (both are less than 0.35 standard deviations from the EVG value), it is worth considering the minimum detectable effect size in this sample, an issue I address further in Section 6.

Result 2. *Money manipulation does not show more lying in the domain of losses than in gains.*

Table 4 shows evidence in support of my second hypothesis: in Money Manipulation treatments, lying is not more common in the loss treatment than in the gain treatment. In fact, the loss treatment exhibits a significant *decrease* in lying compared to the gain treatment. From the increase in the proportion of participants who reported rolling a one, it is likely that the decrease in mean die roll is the product of both fewer participants in the ML treatment lying and liars in the ML treatment doing so at a less extreme level than in the MG treatment.

Result 3. *There is no significant evidence that loss in Expected Value treatments increases lying relative to gains.*

Table 4 shows that the estimated rate of honesty is lower in the EVL treatment than the

EVG treatment (33.3% compared to 50.0%). This difference suggests that there were more liars in the EVL treatment than in the EVG treatment. While both estimates are only *lower* bounds for honest, these rates suggest that only about 66.7% as many EVL subjects as EVG subjects performed honestly. Table 3, however, shows that only the honesty rate among the EVL participants is statistically distinguishable from the control group's at the 5% level, though EVG's honesty rate is marginally significant. Further, a two-sided Mann-Whitney test reveals that the difference between EVG and EVL rates is not statistically significant. In light of the mixed evidence of differences in honesty rates, it seems plausible, albeit not statistically verified here, that the Expected Value treatments yield different rates of honest reporting.

Comparing the apparent effects of loss on lying in the Expected Value treatments to those in the Money Manipulation condition, the OLS regression in the final row of Table 4 demonstrates that the observed effects from each of the loss-framing methods are significantly different from one another. Specifically, the effect of loss on the mean die roll value in the Money Manipulation condition represents a significant decrease compared to the effect of loss in the Expected Value treatments. From the difference in estimated honesty rates, it is also evident that ML's apparent increase in honesty compared to MG is significantly different from EVL's decrease in honesty relative to EVG.

5.1 Replication Validity

Due to the methodological differences between this experiment and the one conducted by CBER, it is critical to draw statistical comparisons between our results. A major obstacle to comparing these results to CBER's is that they use a ten-sided die in their experiment, while I use a six-sided die. I take two approaches to addressing this discrepancy: coercing CBER's possible die roll values to mine *by payoff amount* and applying Gerlach et al.'s (2019) standardized report measure of reported values across die-roll experiments.

First, I coerce the 10 possible payoffs available in CBER to the six available in my Money Manipulation conditions. Table 5 shows the payoffs CBER used for each reported die roll value.

Because CBER use increments of 0.5 that I do not include in my Money Manipulation treatments, I use two methods to obtain comparable data: "rounding up" and "rounding down" the payoffs to the nearest whole currency unit value. Table 6 shows what each value on the ten-sided die is translated to on the six-sided die under each conversion method. Assuming participants report their die rolls to obtain a particular payoff, rounding up implies

Table 4: Mean Differences Between Loss and Gain Treatments (Mann-Whitney)

	Outcome	
	Die Roll	Honesty Rate
Expected Value		
Gain	4.341	0.500
	(1.533)	(0.519)
Loss−Gain	0.119	−0.167
	(1.493)	(0.488)
Money Manipulation		
Gain	4.595	0.143
	(1.441)	(0.363)
Loss−Gain	-0.595^{**}	0.473^{**}
	(1.665)	(0.506)
Effect Size Differences		
Money Manipulation−Expected Value	-0.714^{**}	0.639^{**}
	(0.337)	(0.253)

*p$<$ 0.1, **p$<$ 0.05, ***p$<$ 0.01

Note: Coefficients in the "Gain" rows represent the mean outcome value in the gain treatment group. Coefficients in the "Loss−Gain" rows represent the mean outcome value of the gain treatment group subtracted from the mean outcome value of the loss treatment group. In the "Gain" rows, the gain treatment group's standard deviation is reported in parentheses; in the "Loss−Gain" rows, the standard deviation of the loss treatment group's outcome is reported in parentheses. Coefficients in the "Effect Size Differences" panel represent the differences between Expected Value and Money Manipulation effect sizes (OLS), and parentheticals represent the standard error of the regression coefficients.

Table 5: CBER Payoffs

	Reported die roll									
	0	1	2	3	4	5	6	7	8	9
Payoff (EUR)	0.00	1.00	1.50	2.00	2.50	3.00	3.50	4.00	4.50	5.00

that participants at half-value amounts would choose to report the die roll giving them a higher payoff if given the choice. Conversely, rounding down implies that those same participants would instead choose to report the die roll giving them a lower payoff. The behavior participants at half-value amounts would truly report likely falls somewhere between the extremes of the rounding up and rounding down methods, depending on what value they actually observed and how prone they are to lying. Because these factors are impossible to determine without substantially more information, I use the extreme cases for reference points.

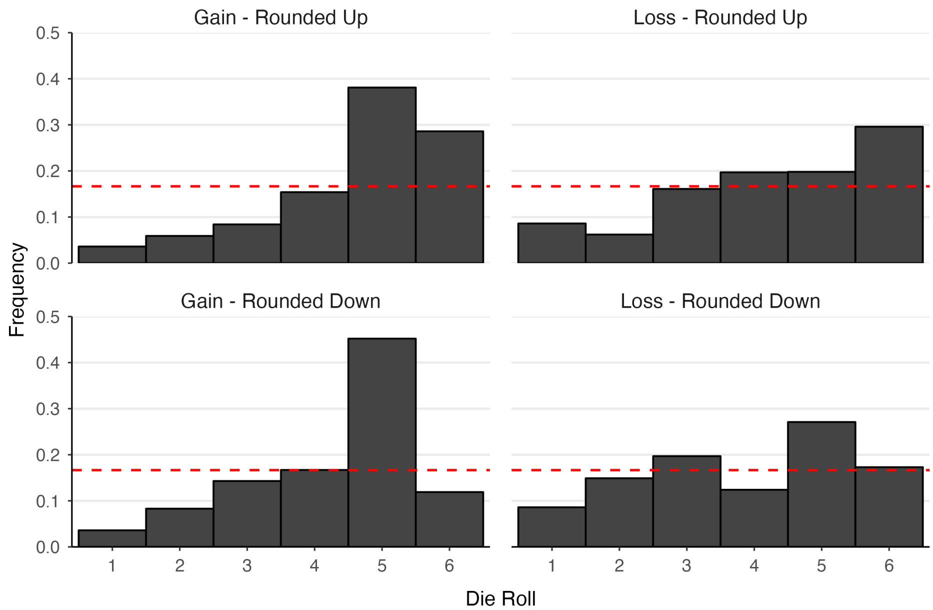
Using the cumulative density of reports at each roll value provided by CBER, I produce replicated discrete density distributions for six-sided dice pictured in Figure 5. Based on a visual comparison of these histograms to those presented by CBER, it appears that the rounding down method is more accurate to CBER's results due to the sub-maximal spike in reports in the gain treatment and the more uniform distribution of reports in the loss treatment. Nevertheless, I compare the results of both conversion methods to my observed results.

Table 6: CBER Payoffs

		Reported die roll									
		0	1	2	3	4	5	6	7	8	9
6-sided die	Rounded Up	1	2	3	3	4	4	5	5	6	6
	Rounded Down	1	2	2	3	3	4	4	5	5	6

Table 7 displays p-values resulting from Kolmogorov-Smirnov tests comparing the observed distributions of reports to the reconstructed distributions from results that CBER obtained. From these results, it appears that the rounding up method is more representative of what I observed than the rounding down method. Importantly, we cannot say that the ML distribution of reports comes from a different distribution than the CBER loss treatment distributions from either conversion method, supporting the validity of this experiment's replication of CBER.

Figure 5: Hypothetical Roll Distributions



Although the ML distribution is consistent with the CBER distributions, the MG distribution is significantly different from the CBER gain distribution after rounding down. This discrepancy likely arises because the sub-maximal lying tendency in the gain treatment that CBER observed is not present in my observed results. Instead, MG participants in this experiment heavily favored maximal lying, as is evident in Figure 4. While this is an important inconsistency in the replication, it is at least partly attributable to focusing only on the two extreme methods to convert the ten-sided die rolls to six-sided dice. To address the gap between rounding up and rounding down, I create twenty artificial distributions in which a proportion $q \in [0, 1]$ of participants at half-value payoffs are rounded up and the remaining $1 - q$ are rounded down. Figure 6 shows the p-values resulting from Kolmogorov-Smirnov tests comparing the observed distributions of reported die rolls in the Money Manipulation condition to hypothetical CBER distributions at different values for q.

Based on the results from Table 7, it is unsurprising that Figure 6 shows that the observed distribution in the ML frame does not significantly differ from the hypothetical distributions generated by any rounding up proportion q. However, it is also evident from Figure 6 that the observed distribution from the MG frame is significantly different from the hypothetical distributions when q is low (i.e. mostly rounding down). When payments are mostly rounded down, the resulting distributions emphasize sub-maximal lying; as Figure 4 shows, maximal lying is the modal outcome by far in the observations from the gain frame of the Money

Table 7: Kolmogorov-Smirnov Tests with CBER Replicated Distributions

	Kolmogorov-Smirnov p-values	
	Loss Domain	Gain Domain
Rounded Up		
Expected Value	0.168	0.202
Money Manipulation	0.777	0.427
Rounded Down		
Expected Value	0.000	0.052
Money Manipulation	0.758	0.000

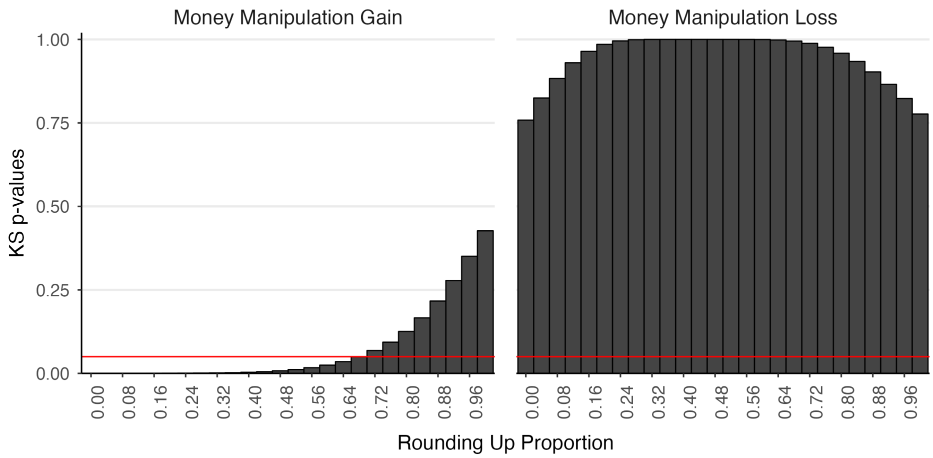
Note: In the "Loss Domain" ("Gain Domain") column, the numbers reported represent the p-value obtained from Kolmogorov-Smirnov tests comparing the observed distribution of die rolls in the loss (gain) frame of the indicated treatment type to the reconstructed money manipulation loss (gain) treatment from CBER. In the "Rounded Up" ("Rounded Down") panel, reconstructed distributions from CBER were obtained through the rounding up (rounding down) method described above.

Manipulation condition, and sub-maximal reports of high payoffs are no more common than expected (each has a frequency slightly greater than $\frac{1}{6}$). As CBER show a high degree of sub-maximal lying and less maximal lying in their gain treatment, it requires substantial rounding up[14] to fail to reject the similarity between hypothetical and observed distributions.

From the results shown in Figure 6, I conclude that participants my ML treatment behaved similarly to those in CBER's loss frame and that participants in my MG treatment were more likely to lie maximally than those in the CBER gain condition. The disparity in maximal lying in the gain frames of this experiment and CBER's may suggest that participants in this study were more prone to or comfortable with dishonesty than in CBER's, a possibility I discuss in detail in Section 6. Another possibility follows from Gerlach et al.'s (2019) claim that maximal lying is more costly than sub-maximal lying. If agents consider the levels of lying in terms of the nominal level instead of the actual payoff, it is possible that lying maximally at a level of 10 incurs a greater moral cost than lying maximally at a level of 6, even if the resulting payoffs are identical.

[14] Approximately 72% of observations at half-value payoffs must round to the nearest whole-value payoff greater than the observed payoff for the observed MG distribution not to differ significantly from the hypothetical CBER distribution.

Figure 6: Replicated Distribution Comparion p-values



Note: Each bar represents the p-value generated from a KS test comparing the observed die roll distribution to one of 20 hypothetical distributions obtained by rounding a proportion q of each half-value payoff's density up to the nearest whole value and a proportion $1 - q$ *down* to the nearest whole value. Red horizontal lines are at p-values of 0.05.

In addition to using rounding of payoffs to reduce CBER's 10-payoff outcomes set to a six-payoff set analogous to my Money Manipulation conditions, I standardize the reports seen in both studies to compare them. Gerlach et al. (2019) define the standardized report, M_r, as

$$M_r = \frac{m - t}{t - t_{min}} \text{ if } m < t$$

and

$$M_r = \frac{m - t}{t_{max} - t} \text{ if } m \geq t$$

when m is the observed within-study mean reported die roll, t is the expected mean report if no one lied, t_{min} is the minimum possible report, and t_{max} is the maximum possible report. In Appendix B, Gerlach et al. (2019) note that in die-roll experiments where payoffs follow an increasing, nonlinear function of the reported roll, they calculate M_r using the reported die roll value instead of the realized payoff. Therefore, in this study, $t = 3.5$, $t_{min} = 1$, and $t_{max} = 6$. For CBER's standardized reports, I use $t = 4.5$, $t_{min} = 0$, and $t_{max} = 9$.

Table 8 shows the standardized reports for each condition in this study, along with their associated standard errors. Gerlach et al. (2019) compute the standard error of standardized reports, SE_r, as

Table 8: Observed Standardized Reports

Treatment	M_r	SE_r	Obs.
C	0.068	0.077	79
EVG	0.337	0.068	82
EVL	0.384	0.063	89
MG	0.438	0.063	84
ML	0.200	0.076	76

$$SE_r = \frac{s_m}{(t - t_{min})\sqrt{n}} \text{ if } m < t$$

and

$$SE_r = \frac{s_m}{(t_{max} - t)\sqrt{n}} \text{ if } m \geq t$$

where s_m is the standard deviation of the observed reports in the study and n is the number of participants.

Table 9: Study and CBER Standardized Reports

Study Condition	CBER Condition	M_r (Study)	M_r (CBER)	SE_r (CBER)
MG	Gain MM	0.438	0.323	0.060
ML	Loss MM	0.200	0.155	0.074

Comparing my two Money Manipulation conditions to CBER's, I find the standardized reports and associated standard errors shown in Table 9. Similar to the previous comparison based on coerced die-roll distributions, the differences in standardized reports indicate that the Money Manipulation Loss condition was very similar to CBER's analogous loss treatment arm at only 0.61 CBER standard errors above the CBER standardized report.[15] However, there is reason to suspect that the Money Manipulation Gain group may have behaved differently than its CBER counterpart; the standardized report in this study is 1.912 CBER standard errors (or 1.825 study standard errors) above the standardized report CBER observe. This difference does not meet the threshold for statistical significance at $\alpha = 0.05$ in a two-sided test, but it is worth noting nonetheless, especially considering the results of the coercion-based comparison discussed above. In Appendix D, I discuss additional comparisons based on standardized reports to the die-roll literature as a whole and

[15]This is 0.592 standard errors above the CBER standardized report, in terms of ML standard errors.

GSV's results in the coin-flip context.

6 Discussion

In Section 5, I offer ample evidence demonstrating that participants in the treatment groups lied about their observed die roll value in order to increase their payoffs in the experiment. This conclusion is expected but important nonetheless, as it confirms that participants in this experiment behaved in a way that is generally consistent with the decisions observed in prior research on lying. Some—but not all—subjects lied about their die roll, and some dishonest subjects lied at the payoff-maximizing level (in this case, dishonestly reporting a six) while others lied at sub-maximal levels.

Importantly, the control group in this experiment did not exhibit any clear lying behavior. In addition to allowing the control group to serve as a comparison group for all other treatments, this outcome further confirms this experiment's validity in assessing dishonest behavior under incentives to lie: because the control group was not given any incentive to lie, it would be surprising and indicative of a methodological error if participants in the control group regularly lied. Instead, I have not found any substantial evidence of lying in the control group; the standardized report for the control group is suggestive of only 6.8% of the over-reporting possible in the game. I also test the robustness of the control group as a baseline comparison for the treatments in Appendix B by creating 10,000 simulated control groups drawn from a discrete uniform distribution of possible die rolls and comparing the actual treatment results to these simulated control groups. The main conclusion from this simulated robustness test is that the real control group in this experiment is a valid comparison group. Additionally, none of the significant results that the Mann-Whitney tests identified in Section 5 appear to have been produced by random characteristics of the observed control group.

In comparisons between treatment groups, I focused on two outcomes: the mean die roll and rate of honest report, as estimated by the proportion of subjects who reported rolling a one. There is no doubt that the mean reported die roll is useful for evaluating dishonest behavior within treatments, but it does not distinguish between the number of participants who lied and the "extremeness" of each lie—that is, the absolute difference between the true observed die roll and the reported number. To make this distinction, I include the estimated lower bound of the honesty rate based on the assumption that subjects never lie to decrease their earnings (and therefore they that they always report at least what they observe). I do not find any evidence to the contrary of this assumption based on the results from the Money Manipulation Loss treatment of this experiment, and I have been unable to find a substantial body of literature contradicting Gerlach et al.'s (2019) assumption. Therefore, I am comfortable viewing the honesty rate, as computed here and in Gerlach et al. (2019), as a valid estiamte for the lower bound of the proportion of honest reports within each condition.

In Section 4, I predicted statistically significant differences between the loss and gain treatments of the Expected Value method of inducing a sense of loss. Neither of these results came to fruition. By contrast, the results from the EVG and EVL treatments are nearly statistically indistinguishable. While contradictory to my initial hypotheses, this outcome is in line with the conclusions that CBER and Ezquerra et al. (2018) draw from their die-roll experiments. Specifically, the lack of distinction between the EVL and EVG results suggests that loss aversion may not play a role in the decision to lie—at least when there are five or more possible levels of lying.

CBER and Ezquerra et al. (2018) put forward evidence contrary to the hypothesis that framing an outcome as a loss increases the likelihood of lying to avoid it compared to framing the same outcome as a gain. However, CBER suggest that loss framing may actually *decrease* lying relative to gain framing, a result that evidence from my Money Manipulation treatments appears to support. While my ML and MG treatments align with CBER's results, I cannot identify any significant difference between my EVL and EVG outcomes. The comparison of the Expected Value and Money Manipulation treatments yields two important conclusions.

The first conclusion is that the Money Manipulation and Expected Value methods for framing outcomes as losses (or gains) differ in both method and result. Where dishonesty appears to decrease from the MG to ML treatments, it stays the same or increases between EVG and EVL treatments. This determination is expected but still meaningful for designing experiments relating to lying and loss aversion in the future; to definitively argue the influence of loss aversion on dishonesty in applied contexts, it is essential to consider both methods of framing random outcomes as losses. Additionally, further research and replications are necessary to lend support to the conclusion that the Money Manipulation and Expected Value methods suggest different effects of loss aversion on lying behavior.

I have argued that the Expected Value and Money Manipulation treatments yield different results. However, the outcomes of both treatment types support the conclusion previously asserted by CBER and Ezquerra et al. (2018)—that loss aversion does not lead agents to lie more when faced with a loss than a gain. Such a conclusion contradicts a seminal theory of human behavior and seems to be refuted by numerous other lying experiments (Duc Huynh, 2020; Grolleau et al., 2016; Gneezy et al., 2018; Garbarino et al., 2019). Within the domain of dishonesty, there must be an explanation for why loss aversion seems to increase lying in some studies and not in others. Momentarily setting aside study-specific methodological and environmental explanations, one possibility worth further consideration is the role of defaults (Johnson and Goldstein, 2003). One of the defining characteristics of the die-roll paradigm is that there are more than two possible levels of lying available, including several sub-maximal levels. Because the difficulty of making a decision increases as the set of alternatives grows (Iyengar and Lepper, 2000; Van 't Veer et al., 2014), it is plausible that agents view their true observed roll as the default option and elect to report that default instead of spending energy choosing a different number. Comparing dishonesty in the domain of both losses and gains as the size of the alternatives set increases along a continuum is a logical avenue for further research.

Turning instead to considerations of the method and experimental context involved in studying this question, one possible explanation for loss aversion's seeming lack of effect on dishonesty is that the marginal benefit of reporting a higher payoff is not large enough to overcome the moral cost of that higher report. As a result, a question that remains to be answered is whether raising the stakes in the experiment and widening the gap between possible payoffs leads to different behaviors. The answer to this question is not evident without further research, as the moral cost of lying is believed to be increasing on the magnitude of the marginal benefit (Fischbacher and Föllmi-Heusi, 2013; Lundquist et al., 2009; Mazar et al., 2008).[16] Another explanation—also addressed by CBER—is that so many participants were already maximally lying when the outcome was framed as a gain that reasonable changes in dishonesty among loss-treatment participants were undetectable.

Although the distributions of reported die rolls appear similar for the EVG and EVL treatments, it is clear from Table 4 that in this experiment, participants in the EVL treatment lied more than those in the EVG treatment. As stated in Section 5, this difference is not statistically significant. Given the relatively large standard deviations compared to the difference in mean, it is unclear whether the result is null because there is truly no effect present or because the sample does not lend enough power to the test to identify the result as significant. Assuming an underlying normal distribution of estimators, I find a minimum detectable effect of 0.649 (i.e., the mean reported die roll would need to be about 0.65 "pips" higher in the EVL group than in the EVG group to find a statistically significant increase

[16] Recall from Section 3 that $m' > 0$ for the function $m(x)$ describing the moral cost of lying to gain an additional payoff of x.

with $\alpha = 0.05$ and 80% power).

A difference of means of 0.65 or greater is unrealistic in this experiment for two reasons. First, it is a relatively large difference; the difference between the EVG and control groups' mean roll is only 0.671. Second, die rolls are constrained to a limited range of values. Since we already expect lying to be prevalent in the domain of gains, it would be exceedingly difficult to observe such a large increase in lying on top of the dishonesty from the gain treatment. Due to the limitations involved in testing whether the EVG and EVL treatments exhibit differences in lying behavior, I cannot conclude that the failure to identify a significant difference is strong enough evidence to argue that loss aversion does not play a role in the decision to behave dishonestly. The minimum detectable effect for the honesty rate between Expected Value treatments (52 percentage points) is even less realistic—with a baseline honesty rate of 50% in the EVG condition, the EVL group would have to have an honesty rate below -2% or above 102% (outcomes that are clearly impossible to observe) for a two-sided parametric test to detect an effect.

In Section 5 (Subsection 5.1), I offered a discussion of how well this experiment replicated the results observed by CBER. A noteworthy distinction between these results and CBER's is that participants in this experiment appeared to lie more than those in CBER's experiment. In the gain treatment, CBER observe a sub-maximal modal die roll, while I find that the modal die roll in this study is the maximal value (six), clearly indicating that gain-treatment subjects in this experiment were more likely to maximally. The potentially anomalous nature of my MG group's behavior is further evidenced by its deviance from the general pattern of behaviors across numerous similar die-roll games, as I discuss further in Appendix D. Concerning the loss treatment, the differences between CBER's results and mine are not statistically significant, but they are worth discussing, nonetheless. Where CBER do not find evidence that participants in their loss treatment lie, I find some weak evidence that participants in my loss treatment may lie to increase their earnings.

Noting that lying may be more common in this experiment than in that of CBER, I lay out three fundamental possibilities to explain this difference. This experiment was conducted entirely online, so I was unable to provide endowments to loss-treatment (ML) participants in physical cash the way CBER did. If CBER are correct that participants lie less because of a feeling of having been entrusted with their endowments, an intangible endowment may be less likely to lead to that same sense of trust, thereby resulting in a higher incidence of lying. If, however, none of my participants felt that they had been entrusted with their endowments, there would seemingly be little distinguishing the Money Manipulation and Expected Value methods of framing outcomes as losses. Therefore, I would expect the results to be similar to those observed in the Expected Value treatments. The Money Manipulation results do not resemble the Expected Value results, so I must conclude that some degree of the sense of being entrusted or another defining trait of the Money Manipulation method survived the transition from a physical experiment to online.

Another plausible explanation for the increased lying observed in this experiment compared to CBER is the use of MTurk for recruiting participants. Gerlach et al. (2019) find evidence that MTurk participants have a higher rate of lying than student participants across all experimental paradigms for studying dishonesty. Specifically, MTurk participants have an average rate of liars that is eleven percentage points (Gerlach et al., 2019) higher than that of student populations. One possibility for why MTurk participants are more likely to lie is that they are "professional survey takers" (Gerlach et al., 2019), so they may have participated in a similar experiment or be more prepared to identify ways to maximize their earnings. In a related explanation, MTurk participants may use MTurk as their primary source of income, a hypothesis anecdotally supported by the reasoning given by some participants[17] for why they chose to report the die roll value that they did (see Appendix F for more). While students may have other sources of income and therefore be less dependent on what they earn from the experiment, MTurk participants may rely on earnings from the tasks they complete to pay for basic living expenses. Consequently, an important extension on this research is to use student and MTurk samples to draw comparisons between the behaviors exhibited by members of each population.

Finally, there were not any virtual or in-person experimental sessions for completing this study. Instead, all participants completed the experiment on Qualtrics at their leisure using their own devices. This arrangement grants participants a greater degree of privacy than they could expect to experience under any other format. Gneezy et al. (2018) and Crede and Bieberstein (2020) find evidence that participants are less likely to lie when the experimenters can observe their decisions, avoiding the "reputational cost" (Crede and Bieberstein, 2020) of being caught lying. Gneezy et al. (2018) also focus on the situation in which loss aversion can influence participants' decisions and find similar results. Therefore, this experiment's relatively high degree of privacy for participants may have led to some increase in lying. That said, experimenters in physical experiments have gone to great lengths to ensure privacy (Van 't Veer et al., 2014; Duc Huynh, 2020; Lewis et al., 2012;

[17]Appendix F contains all given reasons, but one notable example explicitly states "MAIN INCOME SOURCE."

Shalvi et al., 2011), including by having participants roll the die or observe their random outcome in a covered cup. Consequently, it seems unlikely that the use of personal devices had a large impact on dishonesty.

So far, I have discussed the results as they relate to CBER's findings in great detail. Most existing evidence in support of loss aversion's impact on dishonesty comes from experiments using other paradigms (Duc Huynh, 2020; Garbarino et al., 2019; Gneezy et al., 2018; Grolleau et al., 2016). To answer the fundamental question of whether the discrepancy in the influence attributed to loss aversion is the consequence of the difference in paradigms or the methods for inducing loss, I now briefly discuss the comparison of this experiment's results to those of GSV.

Because GSV use the coin-toss paradigm, their experiment allows a dichotomous outcome for participants that they can separate into a "good outcome" (earning money) and a "bad outcome" (earning nothing). In their experiment, GSV show that participants with a high *ex-ante* expected payoff are more likely to lie to avoid the bad outcome than participants with a low *ex-ante* expected payoff. By contrast, I am unable to find any significant difference in behavior between participants with a high *ex-ante* expected payoff (EVL) and those with a low *ex-ante* expected payoff (EVG). A critical distinction between GSV's method and this experiment's is that the marginal benefit of lying in GSV is equal to the maximum possible payment where in this experiment, the marginal benefit of lying is only one-fifth the maximum possible payment.[18] Furthermore, I introduce a jump discontinuity in the shape of the Expected Value payoffs curves that could inhibit the comparability of this study's Expected Value groups to the results found by GSV and in my Money Manipulation treatments. Based on the results of this experiment, it appears that the smaller marginal benefit of lying and the ability to lie at multiple sub-maximal levels nullified much, if not all,[19] of the effect of loss observed by GSV.

7 Conclusion

In this paper, I investigate the influence of loss aversion on dishonesty when experimental subjects are asked to provide information that does not affect other participants, involve social interactions, or reflect their personal abilities. In the control group, participants do

[18]This also represents only one half the marginal benefit of lying in GSV.

[19]As discussed above, the question of minimum detectable effect size precludes me from arguing that loss does not affect lying behavior.

not receive an incentive to lie, and the control group's results are consistent with the expected die rolls obtained from a uniform distribution (no lying). When reported information affects participant payoffs, subjects show statistically significant signs of lying to increase their earnings.

Regarding loss aversion, I seek to explore the discrepancy in the existing literature exemplified by the disparity between the results from GSV and CBER. While GSV found evidence in support of loss aversion increasing lying, CBER find no such evidence. Because GSV and CBER use different methods for both framing outcomes as losses and connecting participant outcomes to self-reported information, I compare their loss-framing methods in the context of the die-roll lying paradigm. I find results consistent with those of CBER when using their method for framing outcomes as losses. However, I find results contradictory to those of GSV when using their loss-framing method.

From the results of this experiment, it appears that the experimental paradigm used to allow participants to lie to alter their earnings changes the apparent influence of loss aversion on dishonesty. One explanation for this conclusion is that using the coin-toss paradigm (as GSV do) instead of the die-roll paradigm increases the marginal benefit of lying, thus encouraging more lying. As another explanation, I suggest that agents' utilities are not sensitive enough to respond differently (at least at a statistically significant level) to a small change in the *ex-ante* expected payoff.

It is evident that the methodological differences between dishonesty studies are responsible for their inconsistent results. As such, the experimental methods used to investigate lying and cheating must be researched and compared further. In particular, researchers should examine the role of default effects on (not) lying and whether the stakes and marginal benefits of lying increase dishonesty. Additionally, more research on the characteristics of the moral and reputational costs of lying is necessary to derive an accurate model of cheating.

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A Supplemental Figures and Tables

Figure A.1 represents the distribution of 10,000 die rolls conducted on dieroll.net (the website participants used for obtaining their die rolls). A Kolmogorov-Smirnov test comparing this distribution to the theoretical discrete uniform distribution yields a p-value of 0.9386.

Figure A.1: Die Roll Randomness

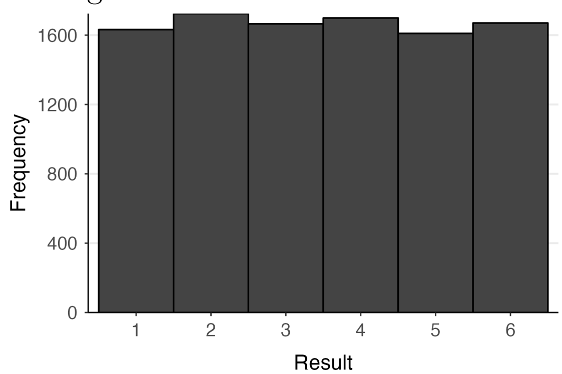


Table A.1: Kolmogorov-Smirnov p-values

	p-value
Compared to Uniform Distribution	
C	0.838
EVL	0.000
EVG	0.000
ML	0.083
MG	0.000
Loss Compared to Gain Distribution	
EV	0.717
M	0.097

Note: In the "Compared to Uniform Distribution" panel, p-values were obtained from Kolmogorov-Smirnov tests comparing the indicated treatment group's distribution of die rolls to the theoretical discrete uniform distribution over the integers from 1 to 6. In the "Loss Compared to Gain Distribution" panel, p-values were obtained from Kolmogorov-Smirnov tests comparing the Expected Value (Money Manipulation) loss treatment's distribution of die rolls to that of the Expected Value (Money Manipulation) gain treatment.

Table A.2: Survey Duration by Treatment Condition

Treatment	Mean	SD	Min.	Median	Max.	Obs.
C	2973.13	24335.73	68.00	202.00	216531.00	79
EVG	339.24	426.97	70.00	227.50	3668.00	82
EVL	265.15	225.61	52.00	202.00	1585.00	89
MG	286.89	184.80	68.00	225.50	957.00	84
ML	1975.22	14466.16	78.00	270.00	126419.00	76

Note: All durations are reported in seconds spent in the Qualtrics survey. Statistics come from the full study sample.

Table A.3: Survey Duration by Treatment Condition (Mid-98%)

Treatment	Mean	SD	Min.	Median	Max.	Obs.
C	239.59	165.17	72.00	205.00	862.00	76
EVG	288.50	193.10	70.00	224.50	919.00	80
EVL	252.43	176.46	70.00	202.00	889.00	87
MG	289.53	184.33	78.00	228.00	957.00	83
ML	315.97	183.97	78.00	261.00	909.00	75

Note: All durations are reported in seconds spent in the Qualtrics survey. Statistics come from observations comprising the middle 98% of the *study-wide* set of durations.

Table A.4 shows additional payment summary statistics separated by treatment arm. In particular, note that the mean payment amount actually paid out is similar to the mean of the *unpaid* payment values in each condition. The proportion of participants paid is roughly balanced across conditions as well, with the ML treatment receiving an idiosyncratically low payment percentage (40.8%). I selected participants to pay programmatically in R by taking a random, unstratified sample of 210 subjects from the entire subject pool. The study-wide standard deviation of the amount paid among participants who received payment is $2.02. With a mean payment of $3.84 over the 210 paid subjects, the total amount paid was $806.50. Payments were also accompanied by a 20-percent service fee from Amazon, resulting in total expenses on subject payments of $967.80.

Table A.4: Grouped Payment Statistics

Treatment	Mean	SD	Mode	N	% Paid	Treatment N
C	2.50	0.00	2.50	45	57.0	79
	(2.50)	(0.00)	(2.50)	(34)		
EVG	4.21	2.72	7.50	39	47.6	82
	(4.91)	(2.42)	(6.00)	(43)		
EVL	5.30	2.26	7.50	46	52.3	88
	(5.10)	(2.29)	({4.50, 7.50})	(42)		
MG	3.65	1.36	5.00	49	58.3	84
	(3.51)	(1.56)	(5.00)	(35)		
ML	3.45	1.43	5.00	31	40.8	76
	(2.69)	(1.76)	({5.00, 3.00})	(45)		

Note: Mean, SD, and mode are expressed in terms of the payment currency (US Dollars). Values represent statistics among subjects in the indicated condition who were paid. Parenthetical values are statistics among *unpaid* participants.

Table A.5: Minimum Detectable Effect Sizes

	Die Roll		Honesty Rate	
Power	MM	EV	MM	EV
0.750	0.652	0.611	0.450	0.494
0.800	0.693	0.649	0.478	0.525
0.850	0.741	0.694	0.512	0.561
0.900	0.802	0.751	0.553	0.607
0.950	0.892	0.835	0.615	0.675

Note: "MM" columns represent minimum detectable effect (MDE) sizes between Money Manipulation Loss and Gain treatments; "EV" columns represent MDEs between Expected Value Loss and Gain treatments. All MDEs are at $\alpha = 0.05$ and the power specified at the start of the row.

A.1 Parametric Tests

Table A.6: Mean Differences Between Treatments and Control (OLS)

	Reported Die Roll		Honesty Rate	
	(1)	(2)	(3)	(4)
Expected Value Gain	0.671***	0.659**	−0.357**	−0.390**
	(0.247)	(0.260)	(0.171)	(0.190)
Expected Value Loss	0.790***	0.677***	−0.524***	−0.482**
	(0.242)	(0.257)	(0.168)	(0.200)
Money Manipulation Gain	0.924***	0.950***	−0.714***	−0.711***
	(0.246)	(0.256)	(0.171)	(0.185)
Money Manipulation Loss	0.329	0.328	−0.242	−0.230
	(0.252)	(0.263)	(0.174)	(0.199)
Age		−0.010		0.001
		(0.008)		(0.006)
Risk Aversion		−0.013		−0.019
		(0.048)		(0.037)
Male		−0.129		0.025
		(0.162)		(0.123)
Non-Hispanic Black		−0.029		−0.251
		(0.417)		(0.296)
Constant	3.671***	4.119***	0.857***	0.873***
	(0.176)	(0.368)	(0.121)	(0.301)
Observations	410	376	70	62

*p< 0.1; **p< 0.05; ***p< 0.01

Note: Coefficients represent OLS regression coefficients on indicators for treatment arm, relative to the control group. Standard errors for coefficients are specified in parentheses. Honesty Rate is a lower bound of honest reporting based on the assumption that subjects never lie to report earning less than they observe and computed by dividing the observed proportion of participants who reported rolling a one by the expected proportion if no one lied ($\frac{1}{6}$). Models (1) and (3) are unadjusted regressions. Models (2) and (4) control for reported demographic variables and risk aversion.

Table A.7: Mean Differences Between Loss and Gain Frames (OLS)

	Reported Die Roll				Honesty Rate			
	Expected Value		Money Manipulation		Expected Value		Money Manipulation	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Loss Framing	0.119	0.019	−0.595**	−0.620**	−0.167	−0.120	0.473***	0.515**
	(0.232)	(0.244)	(0.246)	(0.257)	(0.187)	(0.234)	(0.169)	(0.188)
Age		−0.017		0.002		−0.010		0.006
		(0.012)		(0.012)		(0.012)		(0.009)
Risk Aversion		−0.008		0.005		0.013		−0.055
		(0.083)		(0.070)		(0.062)		(0.051)
Male		−0.391		−0.119		0.180		−0.082
		(0.245)		(0.259)		(0.244)		(0.195)
Non-Hispanic Black		1.155		−0.326				−0.452
		(0.790)		(0.651)				(0.350)
Constant	4.341***	5.109***	4.595***	4.626***	0.500***	0.773	0.143	0.122
	(0.167)	(0.490)	(0.169)	(0.498)	(0.134)	(0.502)	(0.117)	(0.395)
Observations	171	154	160	148	29	24	27	24

*p< 0.1; **p< 0.05; ***p< 0.01

Note: Coefficients represent OLS regression coefficients on indicators for treatment frame (loss or gain), restricted to the indicated treatment method (Expected Value or Money Manipulation). Standard errors for coefficients are specified in parentheses. Honesty Rate is a lower bound of honest reporting based on the assumption that subjects never lie to report earning less than they observe and computed by dividing the observed proportion of participants who reported rolling a one by the expected proportion if no one lied ($\frac{1}{6}$). Models (1)-(4) are concerned with the reported die roll, and Models (5)-(8) are concerned with the lower bound of the honesty rate. Within each outcome, the first two models are restricted to the Expected Value method, and the latter two models are restricted to the Money Manipulation method. Odd-numbered models are unrestricted regressions. Even-numbered models control for reported demographic variables and risk aversion.

B Artificial Controls

For robustness, I programmatically create 10,000 artificial control groups by drawing samples of 79 (the size of the actual control group in this study) integers between 1 and 6 (inclusive) with equal probability on each.

$$\Pr(x) = \begin{cases} \frac{1}{6}, & x \in \{1, 2, 3, 4, 5, 6\} \\ 0, & \text{otherwise} \end{cases}$$

I then estimate the difference in mean reported die roll and the rate of honest reporting between the artificial control group and the five groups in this study (C, EVG, EVL, MG, and ML). Table B.1 summarizes the distribution of artificial control groups. Table B.2 summarizes the differences in means between each artificial control group and each actual treatment group observed in this study, including the p-values resulting from a parametric statistical test of the differences.

Table B.1: Summary statistics across artificial control groups

Outcome	Mean $\overline{x}_i$	Mean s_i	SD of $\overline{x}_i$	SD of s_i
Honesty Rate	0.998	2.222	0.255	0.235
Die Roll	3.502	1.707	0.194	0.084

Note: $\overline{x}_i$ represents the mean of the outcome measure for the ith artificial control group. Similarly, s_i represents the standard deviation of the outcome measure for the ith group.

Finally, Figures B.1-B.4 depict the differences in means and resulting p-values for each outcome of interest (reported die roll and rate of honest reporting) using the artificial control groups as the control (instead of the observed control group). Note that the distribution of differences in reported die roll between incentive-treated groups and the artificial controls are shifted to the right of zero and that the differences in honesty rates are shifted to the left—these outcomes contribute additional robustness and evidence that lying took place in this study. Vertical red lines on mean difference plots are at 0, indicating no observed difference between the artificial control behavior and the true observation; on the p-value plots, vertical red lines are at 0.05 and represent the threshold for statistical significance when $\alpha = 0.05$.

Table B.2: Parametric comparisons with artificial control groups

Treatment	Outcome	Mean Difference	Mean Test Statistic	Mean p-value
C	Honesty Rate	-0.087	-0.210	0.595
C	Die Roll	0.169	0.623	0.503
EVG	Honesty Rate	-0.486	-1.509	0.212
EVG	Die Roll	0.840	3.282	0.009
EVL	Honesty Rate	-0.661	-2.221	0.070
EVL	Die Roll	0.959	3.856	0.002
MG	Honesty Rate	-0.855	-3.112	0.010
MG	Die Roll	1.094	4.410	0.000
ML	Honesty Rate	-0.367	-1.072	0.347
ML	Die Roll	0.498	1.840	0.133

Note: Mean Difference is the mean of the differences in means for the given outcome-treatment pair between the artificial control group and the study condition's observed sample. Note that the Mean Test Statistic is *not* the mean of the set of absolute values of test statistics. The Mean p-value is the mean p-value resulting from two-sample z-tests for differences in means, assuming underlying normal distributions.

Figure B.1: Die Roll Mean Differences

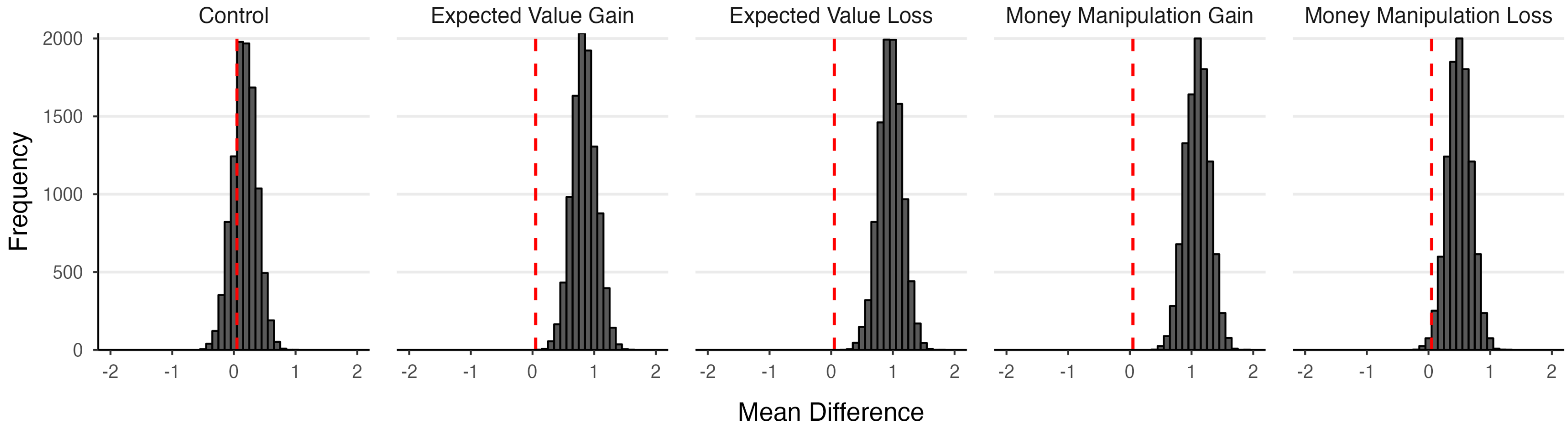


Figure B.2: Die Roll p-Values

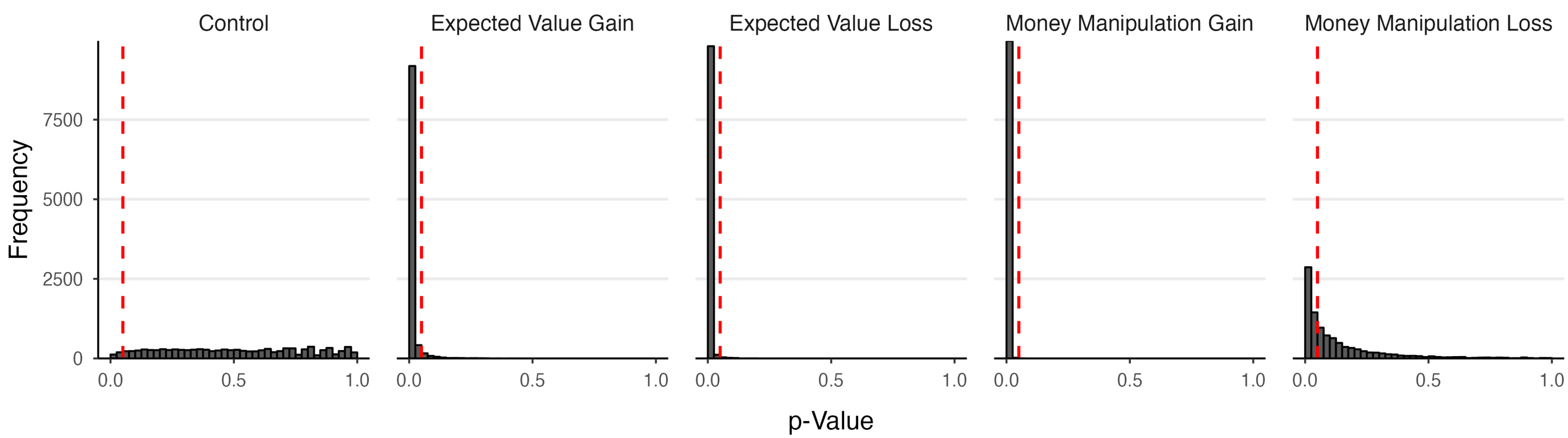


Figure B.3: Honesty Rate Mean Difference

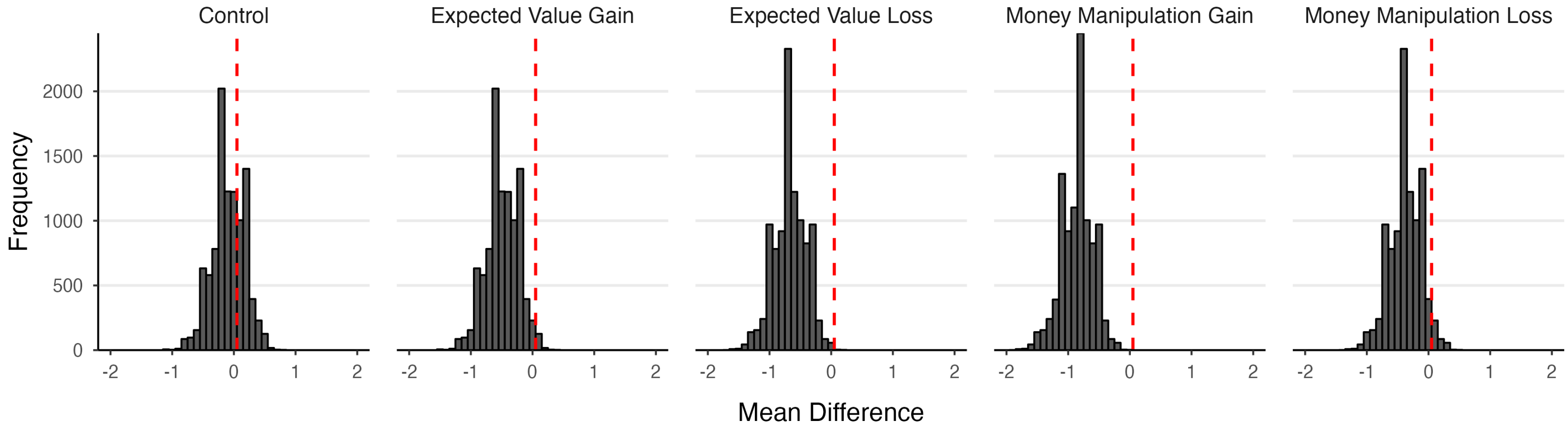
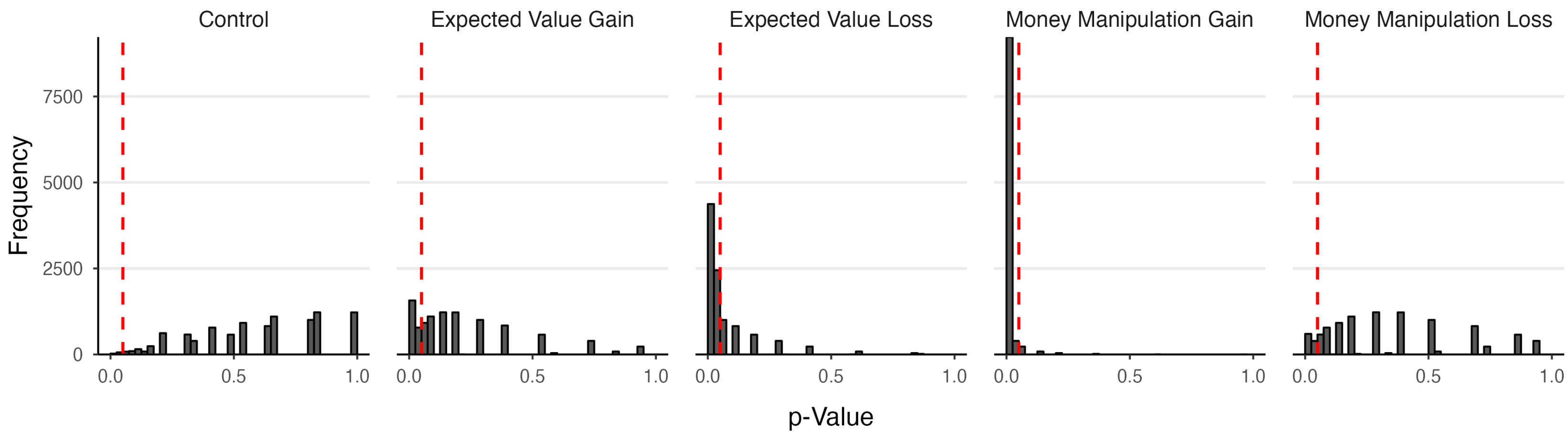


Figure B.4: Honesty Rate p-Values



C Supplemental Proof

Proposition: If $l'(x) > g'(x) \; \forall \; x > 0$, and l'', $g'' < 0$, then for any $a, b \in \mathbb{R}_{>0}$, $a \leq b \implies l'(a) > g'(b)$.

Proof: There are two cases: (i) $a = b$, and (ii) $a < b$. In the first case (i), suppose $a = b$. Then since $l'(x) > g'(x) \; \forall \; x > 0$, and since $a > 0$, $l'(a) > g'(a) = g'(b)$.

In the second case (ii), suppose $a < b$. Since $g'' < 0$, g' is strictly decreasing, so $a < b \implies g'(a) > g'(b)$. By (i) and the assumptions, $l'(a) > g'(a)$ must be true. Then by transitivity, $l'(a) > g'(a) > g'(b) \implies l'(a) > g'(b)$.

$\blacksquare$

D Standardized Reports in Relevant Literature

D.1 Die-Roll Literature

Gerlach et al. (2019) offer a comprehensive summary of results from lying experiments conducted in experimental and behavioral economics, and their online appendix features summary data from each study they evaluate in their investigation. Across the 129 die-roll experiments included in Gerlach et al.'s (2019) meta-analysis comprising 14,835 subjects, the mean standardized report is 28.9% with a standard error of 0.016 (both before performing additional adjustments that Gerlach et al. (2019) use). Table D.1 shows that, with two exceptions, the behaviors exhibited in this study are comparable to those seen in other, similar studies; except for the control and MG groups, 95% confidence intervals around the condition-specific standardized reports would capture the mean standardized report for all 129 die-roll studies that Gerlach et al. (2019) consider. Figure D.1 plots the standardized reports observed in this study alongside the distribution of standardized reports that Gerlach et al. (2019) evaluate in their meta-analysis.

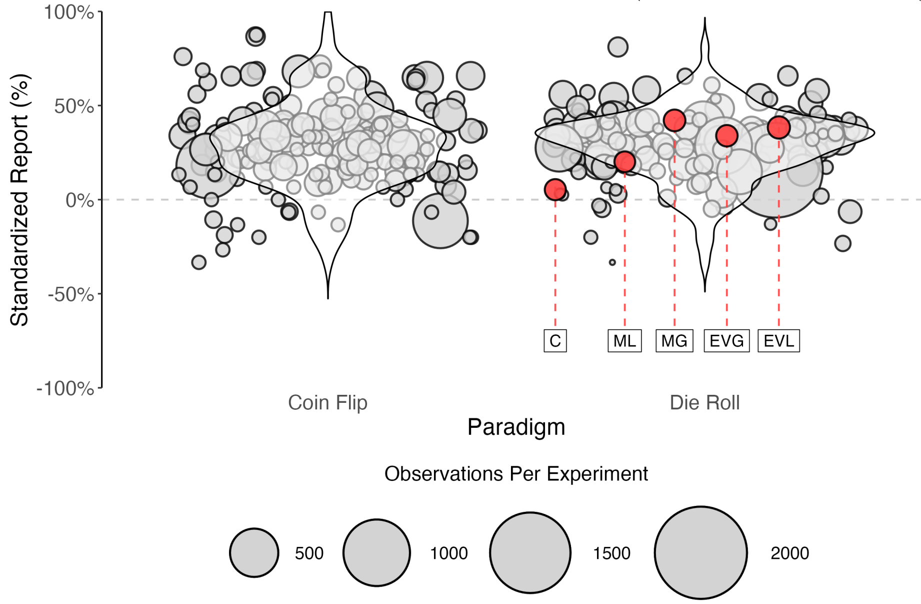
Table D.1: Study Standardized Reports Relative to Die Roll Literature

Treatment	Study M_r	Study SE_r	SE_r Above Mean M_r
C	0.068	0.077	-2.866
EVG	0.337	0.068	0.699
EVL	0.384	0.063	1.511
MG	0.438	0.063	2.365
ML	0.200	0.076	-1.172

The control group's deviation from the general trend in the literature is both expected and further confirmation of its validity as a no-incentive, control condition: the studies included in Gerlach et al.'s (2019) analysis ubiquitously feature incentives to lie that drive their standardized reports significantly above 0%. By comparison, the control group's standardized report of only 6.8% is indicative of relatively little lying, if any at all.

By contrast, the MG condition's standardized report (43.8%) is significantly greater than the literature-wide mean of 28.9%, suggesting a significantly higher incidence of lying and/or maximal lying in this study compared to related experiments. Interestingly, the two Expected Value conditions use highly idiosyncratic, increasing, nonlinear payoff functions that are similar to only two of those included in the meta-analysis, yet neither standardized report differs from the paradigm-wide mean by a statistically significant magnitude. The

Figure D.1: Distribution of Standardized Reports (from Gerlach et al. (2019))



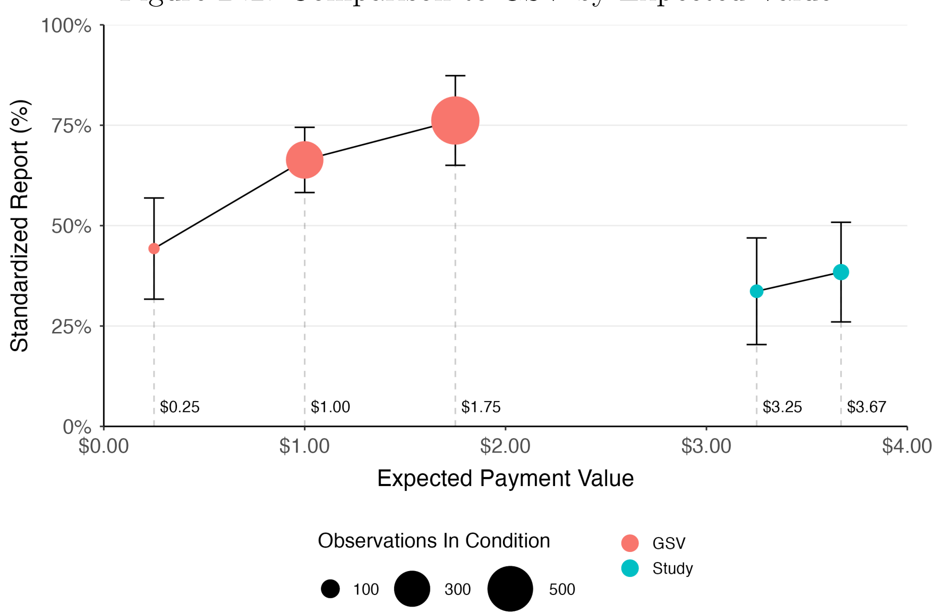
Note: The figure above is a replication of one used by Gerlach et al. (2019) to illustrate the distribution of standardized reports, restricted to experiments using the die-roll paradigm (and, for comparison, the coin-flip paradigm). Points represent the standardized reports of individual studies, and point sizes represent the number of participants in the study. I apply random horizontal jitter to the plot, per the example of Gerlach et al. (2019), for visual clarity. Red points correspond to the treatment arms that appear in this study and are labeled accordingly.

MG treatment, however, follows the typical pattern of linearly increasing payoff functions observed in the literature and sees a statistically significant deviation in behavior. This outcome may be the consequence of a Type-I error, the format of the experiment, or a particularly dishonest sample of subjects, among any number of other explanations. Its main importance within the context of this study, though, lies in interpreting the effect of loss on dishonesty resulting from comparing the ML and MG groups' behavior; in identifying an unusually high level of lying, this MG group likely overstates the (evidently honesty-inducing) effect of loss. While certainly loss within Money Manipulation framing contexts may decrease dishonesty relative to gain, and it is notable that the ML condition observes a substantial albeit not significant ($p \approx 0.121$) decrease in dishonesty compared to the relevant literature, it remains the case that lying within the ML group's behavior falls sufficiently close to the paradigm-wide mean to be unable to conclude that its difference is not attributable to mere random variation; the MG group's behavior does not.

D.2 GSV Comparison

Figure D.2 is a graphical representation of the standardized reports seen in GSV and this study's Expected Value conditions as a function of the *ex-ante* expected payment value. It is visually evident that the Expected Value groups and GSV are not readily comparable, as they feature vastly different expected payments and come from different experimental paradigms. In spite of these differences, the slopes of the lines connecting expected payment values of \$1.00 and \$1.75 (GSV's 0022 and 0222 conditions, respectively), and \$3.25 and \$3.67 (this study's EVG and EVL, respectively) appear remarkably alike. The difference in standardized reports GSV observe in the \$0.75 between expectations of \$1.00 and \$1.75 is 9.8 percentage points (a slope of 13.1 p.p.s per dollar), and I observe a difference of 4.8 percentage points in the \$0.42 between \$3.25 and \$3.67 (a slope of 11.4). Therefore, these two slopes are quite similar; the slope from this study is only about 13% lower than the one between GSV's 0022 and 0222 conditions.

Figure D.2: Comparison to GSV by Expected Value



Note: Vertical lines in the figure represent *ex-ante* expected payoff values implied by the design of each of the five conditions depicted. Error bars show the 95% confidence intervals of each standardized report estimate.

E Instructions

For each participant, one of the following sets of treatment-specific instructions was shown at random. All participants received the same survey at the end.

E.1 Expected Value Loss

You will now complete an activity in which you have the opportunity to earn real money. At the end of the experiment, several participants will be randomly selected to receive the money you earn by completing this activity.

In this activity, you will roll a six-sided die. Your earnings depend on what you roll. The table below shows what you will earn for each possible roll.

	Die roll value					
	1	2	3	4	5	6
Earnings (USD)	$0.00	$1.00	$3.00	$4.50	$6.00	$7.50

Now suppose you have rolled a 4. How much money would you have earned?

$________

You will now complete the activity.

You will roll a die and then report the number you have rolled. You may roll the die as many times as you would like, but you must only report the first number that you roll.

Your earnings depend on what you roll. The table below shows what you will earn for each possible roll.

	Die roll value					
	1	2	3	4	5	6
Earnings (USD)	$0.00	$1.00	$3.00	$4.50	$6.00	$7.50

If you have a fair, six-sided die nearby, you may roll that. If not, please go to http://www.dieroll.net in a new tab or window and click "Roll." Then return to this page and record your roll.

What number did you roll?

_____

E.2 Expected Value Gain

You will now complete an activity in which you have the opportunity to earn real money. At the end of the experiment, several participants will be randomly selected to receive the money you earn by completing this activity.

In this activity, you will roll a six-sided die. Your earnings depend on what you roll. The table below shows what you will earn for each possible roll.

	Die roll value					
	1	2	3	4	5	6
Earnings (USD)	$0.00	$1.00	$2.00	$3.00	$6.00	$7.50

Now suppose you have rolled a 4. How much money would you have earned?

$________

You will now complete the activity.

You will roll a die and then report the number you have rolled. You may roll the die as many times as you would like, but you must only report the first number that you roll.

Your earnings depend on what you roll. The table below shows what you will earn for each possible roll.

	Die roll value					
	1	2	3	4	5	6
Earnings (USD)	$0.00	$1.00	$2.00	$3.00	$6.00	$7.50

If you have a fair, six-sided die nearby, you may roll that. If not, please go to http://www.dieroll.net in a new tab or window and click "Roll." Then return to this page and record your roll.

What number did you roll?

_____

E.3 Manipulated Loss

You will now complete an activity in which you have the opportunity to earn real money. At the end of the experiment, several participants will be randomly selected to receive the money you earn by completing this activity.

You have been given \$5.00 (USD). In this activity, you will roll a six-sided die. The amount you keep from your endowment of \$5.00 depends on what you roll. The table below shows what you amount you will lose for each possible roll.

	Die roll value					
	1	2	3	4	5	6
Losses (USD)	\$5.00	\$4.00	\$3.00	\$2.00	\$1.00	\$0.00

Now suppose you have rolled a 4.

How much money would you have lost from your original \$5.00?

\$________

How much money would you have kept from your original \$5.00?

\$________

You will now complete the activity.

You will roll a die and then report the number you have rolled. You may roll the die as many times as you would like, but you must only report the first number that you roll.

The amount you keep from your endowment of \$5.00 depends on what you roll. The table below shows what you amount you will lose for each possible roll.

	Die roll value					
	1	2	3	4	5	6
Losses (USD)	\$5.00	\$4.00	\$3.00	\$2.00	\$1.00	\$0.00

If you have a fair, six-sided die nearby, you may roll that. If not, please go to http://www.dieroll.net in a new tab or window and click "Roll." Then return to this page and record your roll.

What number did you roll?

_____

E.4 Manipulated Gain

You will now complete an activity in which you have the opportunity to earn real money. At the end of the experiment, several participants will be randomly selected to receive the money you earn by completing this activity.

In this activity, you will roll a six-sided die. Your earnings depend on what you roll. The table below shows what you will earn for each possible roll.

	Die roll value					
	1	2	3	4	5	6
Earnings (USD)	$0.00	$1.00	$2.00	$3.00	$4.00	$5.00

Now suppose you have rolled a [random integer between 1 and 6]. How much money would you have earned?

$________

You will now complete the activity.

You will roll a die and then report the number you have rolled. You may roll the die as many times as you would like, but you must only report the first number that you roll.

Your earnings depend on what you roll. The table below shows what you will earn for each possible roll.

	Die roll value					
	1	2	3	4	5	6
Earnings (USD)	$0.00	$1.00	$2.00	$3.00	$4.00	$5.00

If you have a fair, six-sided die nearby, you may roll that. If not, please go to http://www.dieroll.net in a new tab or window and click "Roll." Then return to this page and record your roll.

What number did you roll?

_____

E.5 Control

You will now complete an activity for which you are receiving $2.50. At the end of the experiment, several participants will be randomly selected to be paid.

You will roll a die and then report the number you have rolled. You may roll the die as many times as you would like, but you must only report the first number that you roll.

If you have a fair, six-sided die nearby, you may roll that. If not, please go to http://www.dieroll.net in a new tab or window and click "Roll." Then return to this page and record your roll.

What number did you roll?

_____

E.6 Survey

You will now complete the survey. You may decline to answer any question. **Your responses to these survey questions will not affect your earnings**.

1. What is your gender?

   ☐ Male

   ☐ Female

   ☐ Non-binary/other gender

   ☐ Prefer not to say

2. What is your age?

   _____

3. Are you of Hispanic, Latino, or Spanish origin?

   ☐ Yes

   ☐ No

   ☐ Other (please specify): ____________________

   ☐ Prefer not to say

4. How would you best describe your race?

   ☐ American Indian or Alaska Native

   ☐ Asian

   ☐ Black or African American

   ☐ Native Hawaiian or Other Pacific Islander

   ☐ White (including Middle Eastern origin)

   ☐ Other (please specify): ____________________

   ☐ Prefer not to say

5. For the following questions, please indicate the degree to which you agree or disagree with each statement.

Strongly Disagree (1)	Generally Disagree (2)	Mildly Disagree (3)	Neither Agree nor Disagree (4)	Mildly Agree (5)	Generally Disagree (6)	Strongly Disagree (7)

The rate of TV watching among people today is alarming.

1 2 3 4 5 6 7

Only a few people have the ability to judge excellence in the arts.

1 2 3 4 5 6 7

There should be more government support for industry to develop new products and technology.

1 2 3 4 5 6 7

I am generally a person who is fully prepared to take risks.

1 2 3 4 5 6 7

Air pollution caused by cars is dangerous.

1 2 3 4 5 6 7

I rely on my religious belief system when making important decisions.

1 2 3 4 5 6 7

6. Please briefly describe why you chose the number you recorded in the die-roll activity.

____

7. Are you a participant from Emory University or MTurk?

☐ Emory University

☐ MTurk

F Decision Motivation Comments

The following are self-reported reasons for reporting the die roll value specified in parentheses.

Free Response: Please briefly describe why you chose the number you recorded as your die roll in the die-roll activity.

MTurk: Control Treatment

"I gave an honest report of my roll because I felt it was the right thing to do" (3)
"That is the number that I rolled." (5)
"I chose the largest number of options as determined by my intuition" (6)
"easy to allocate number" (4)
"I truthfully report the number I got from the website." (5)
"website randomly rolls" (5)
"Because It is the outcome in the die roll" (6)
"I rolled only once. It shows 4 as my answer. So i entered 4." (4)
"for bonus reward" (2)
"the expected value of my die roll" (2)
"It was randomly assigned to me" (1)
"This website randomly rolls a fair luckily i got number 6" (6)
"It comes when I roll the die." (2)
"It was the number I rolled" (4)
"it was the first and only number i rolled" (4)
"good game" (4)
"The virtual dice rolled a 6, so I reported a 6, there was no deeper reason than that." (6)
"4" (4)
"I reported the first outcome of the die roll." (3)
"nothing" (5)
"The instruction was stated to roll the die and mark the first obtained number. So I did follow the instructions." (5)
"I rolled a 3 on the die roll website so I recorded it as such." (3)
"interesting" (4)
"It was the first number I rolled." (3)
"I used the website to roll the dice and got 6. So recorded that number." (6)
"I have got that number." (3)
"I CHOOSE BONUS POINT" (6)

“because i like the number” (6)

“I randomly click roll button it rolled 3. So I have recorded the number 3.” (3)

“none” (1)

“Die-roll activity” (6)

“Die-roll activity” (1)

“3” (4)

“it is the num r0lled” (3)

“lucky number” (2)

“This is my thoughts” (4)

“3” (4)

“It was the number that I rolled” (4)

“Because I was instructed to write the first roll” (2)

“why wouldnt i” (3)

“It is randomly chosen number” (5)

“because in rolling one six-sided die, rolling an even number could occur with one of three outcomes:” (5)

“46” (2)

“good” (6)

“The total number of possible outcomes is the size of the sample space. When you roll one dice, for example, the sample space” (1)

“dieroll dot net site has selected the number for me” (3)

“It was the first number I rolled.” (1)

“I was asked to report the number that I rolled.” (6)

“it was the number that was rolled” (2)

“I want to some save money” (2)

“it was the first result” (6)

“Roll 4 is favourite” (4)

“thats what i really rolled” (2)

“It was the first number that came out when I rolled the die.” (2)

“i roll the die then it came” (6)

“It was an honest thing to do” (1)

“Own decision” (1)

“4” (4)

“I score bonus points” (3)

“NA” (3)
“The number that rolled for me was two.” (2)
“That was the first number when I roll a die.” (5)
“I like it.” (1)
“I have chosen number four because of the instruction given ”Enter the 1st number u get”.” (4)
“I didn’t really choose it, that is just what it rolled” (4)
“It was the first one.” (5)
“Because it is the number which I got when I rolled first.” (1)
“i think that this number is correct for me” (5)
“I had the computer roll the die and number 1 is what came up on the first roll.” (1)
“I AM FIRST DIE-ROLL NUMBER IS 5” (5)
“it is number i got when i rolled .” (4)
“Because I just followed that link and that’s the number I got. There isn’t any big reason behind it.” (1)
“by the given link” (5)
“NA” (6)
“6” (6)
“NA” (6)
“5 is a number that when it is multiplied can bring great numbers in a whole.” (5)
“4 is my lucky number” (4)
“I used a website for a random die roll and was provided with and reported 1.” (1)

MTurk: Expected Value Gain Treatment

“Believe.” (6)
“I chose this number because it was the first number that came up when I rolled the dice.” (6)
“It is the actual number that I rolled.” (5)
“website randomly rolls” (4)
“because that was the number I rolled” (1)
“The dieroll.net dice recorded a 6 when I rolled it.” (6)
“I was told to report the first roll so that’s what I did. I didn’t bother to roll more after that.” (2)
“The first die-roll in the activity” (5)
“It was the number I got randomly when I rolled the dice.” (5)

“NA” (5)

“the expected value of my die roll” (3)

“It was the number I was drawn” (6)

“to earn real money as per entering the die roll number enters in the study” (5)

“It was the number I rolled.” (6)

“I chose it because it is the first number that appeared” (1)

“4” (4)

“IT IS VERY LUCKY NUMBER.” (6)

“Easy to understand and easily get my number choice and receive chance” (6)

“This website randomly rolls a fair, six-sided die. Click ”Roll” to roll the die.” (3)

“I went to the link you mentioned. and pressed the roll button. The number that came in was 1 and I wrote this. I did it this way because it was the right thing to do.” (1)

“It is my thoughts” (4)

“Because it was my fixed number” (3)

“I just got that number in the first attempt. I can provide the screenshot if needed.” (6)

“THE INCOME WAS DEPENDENT ON THE ROLL.THE NUMBER CAME AS SOON AS THE DIE WAS ROLLED.THAT SEEMED TO BE ENOUGH.” (4)

“i roll the die first round and i record the number.” (2)

“I went to the given link and selected the number coming after pass roll.” (5)

“I achieved the bonus point” (4)

“4” (4)

“none” (2)

“Attention activity” (5)

“Just my Opinion” (4)

“lucky number” (6)

“Because I think that suppose my number when I roll it.” (6)

“I rolled the die and the number 5 came up so I recorded that.” (5)

“Its landed on the number 4, so i chose it” (4)

“give the best choice” (1)

“very like” (4)

“I was being honest” (4)

“ACTUAL INCOME” (6)

“thats the number i got” (6)

“As First time result I recorded.” (5)

“Probability is a metric for determining the possibility of an event occurring. The number of conceivable outcomes divided by the total number of possible outcomes is the probability of an event, similar to rolling an even number.” (5)
“i recorded a number which i got in first roll” (3)
“In the die-roll activity after pressing the roll button, I was provided with the number 6.” (6)
“Based on the die roll activity, I was allotted with the number 6 in the die.” (6)
“At the first time of rolling the die, I was allotted with the number 6 in the die-roll activity.” (6)
“I want to earn maximum money.” (6)
“WE CHOOSE THE NUMBER WE RECORDED AS OUR DIE IN ROLL ACTIVITY KEEPS MY UNDERSTANDING.” (3)
“i am choose for 5.” (5)
“It is very well.” (3)
“it is the num in die” (5)
“5” (5)
“I chose this number because this is what I rolled. I wanted to be honest because that is how I live my everyday life.” (2)
“Depend upon my lottery number outcome.” (5)
“It was the first one to come up.” (1)
“i choose 3 because die roll shows 3.” (3)
“Because it was the first number I rolled in the die roll simulator” (3)
“3” (4)
“I chose bouns poants” (5)
“i choose the bonus points” (4)
“I CHOOSE BONUS POINT” (6)
“I chose bouns poants” (6)
“I achieved bonus points” (5)
“It is the number that I rolled on the dice roll website.” (5)
“4” (4)
“4” (5)
“4” (5)
“I entered the number that I get when I rolled the die” (1)
“4” (4)

“I chose this number because it offers the largest bonus” (6)
“6” (6)
“When I rolled a die I got 6 and I recorded it.” (6)
“JUST ROLLED DIE ONCE AND RECORD IT” (3)
“6” (6)
“the dollar amount is 6” (4)
“3 is my favourite number” (3)
“I did as instructed and opened the link in the new tab. Because I was only supposed to report the first roll, I only rolled it once. It landed on 5, so that is what I reported.” (5)
“Its a good number.” (5)
“1” (1)
“I had a dice with me in my room and I rolled it once, and the result was 5. So I selected 5 as the answer.” (5)
“Its the first number when I rolled the die.” (4)
“Because I rolled it becomes 5” (5)

MTurk: Expected Value Loss Treatment

“I chose that number because that was the first number that was rolled on the die roll website.” (5)
“it made the most money” (6)
“It was the first and only number I rolled.” (1)
“Roll die as a very quick and easy to manage the number” (4)
“I think I could gain more money” (5)
“I used the dieroll.net website.” (6)
“To roll my lucky number four.” (4)
“i got that number so only” (4)
“its my lucky one” (5)
“Because it is the actual outcome.” (6)
“good game” (6)
“because it was the first one i rolled” (6)
“my lucky number” (4)
“It is what I rolled. I didn’t see any reason to lie. I’m a glass half empty guy so if I win then great, if not oh well then.” (4)
“Just my opinion” (5)
“That is what came up when I rolled a real die.” (6)

“good” (6)

“My best number and bonus” (4)

“I choose 6, but die-roll given as 5. Very happy.” (5)

“YES I CHOOSE NUMBER 4” (4)

“I rolled a physical die which gave me a six on roll 1” (6)

“I used the random die roller link and rolled a 6 on the first (and only) roll I did.” (6)

“when I opened the new tab to roll the die, the number I rolled the first time was the number I recorded.” (6)

“The set of all the possible outcomes is called the sample space” (5)

“I choose bonus point” (6)

“interesting” (4)

“interesting” (5)

“I choose the bonus points. so, i recorded the number” (5)

“when roll the die it shows number , so i put it” (2)

“i roll 4 because it is easy” (4)

“6” (4)

“none” (4)

“i will choose three” (3)

“Attention Activity” (1)

“Because it is the actual outcome.” (6)

“I roll the dice and i receive this no so i choose the number” (6)

“it is earn money.” (6)

“I click the die-roll, Automatically dies result is 6.So i choose 6” (6)

“i imagine that number before roll the die” (2)

“I clicked the online die roll ONE time and it came up a six! I couldn’t believe it. I reported it honestly but I wondered if it would seem dishonest to the researchers.” (6)

“6” (4)

“4” (4)

“CHOOSE THE BOUNES POIANT” (5)

“It was the number I rolled on the dice roll page. I reported my answer honestly.” (4)

“I didn’t have a dice, so I entered the link that was available, clicked to roll the dice and the number 4 came out” (4)

“Because it is the number that I rolled” (6)

“because it was the number that appeared” (6)

“I allowed my daughter to roll for me we rolled a real dice” (4)
“that is my lucky number” (2)
“Click die roll option for my luck best number in the roll..” (6)
“that is my fav no” (4)
“For my luck and best click die roll” (6)
“because it was the first one I rolled” (5)
“at first i though that it’s really easy to fake my dice roll. you can just pick six and nobody will know that you got another roll, but i was thinking that i’m not gonna do that beacuase i will feel bad later xD i still picked 6 but not because i wanted to but beacuse i rolled it :D i guess god gave me some luck for my good thoughts :p” (6)
“Nothing” (3)
“5” (5)
“That is the number i rolled on the website the hit instructed me to go to.” (5)
“I recorded that I rolled a 6 because that was the first number I rolled on the site.” (6)
“i wanted to earn a maximum bonus” (6)
“NA” (4)
“It is what was rolled.” (3)
“i roll the die for first time then six number was came.” (6)
“25” (4)
“I chose ”4” because that was the number I rolled.” (4)
“It was the first roll. The first number” (1)
“my best number” (4)
“I chose bonus points.” (4)
“I chose bonus points” (4)
“I chose bonus points” (5)
“I score bonus points” (2)
“because that is what i got by rolling dice” (2)
“I will roll the dies first the number was 1 I choose number one because I believe first priority is the best one.” (1)
“I rolled there dice they number was my favorite.” (6)
“Because my die show number 6” (6)
“lucky number” (4)
“I chose that number because it was a numbet I rolled with the dice and it is also a number which eould maximize my potential bonus.” (6)

“It was my number to rolled” (5)
“6” (6)
“i was die roll activity i choose number 5” (5)
“die indicate me on 2” (2)
“I chose that number because it was exactly what I rolled on the page I was taken to.” (5)
“That was the number that showed on dieroll.net” (5)
“very approach that number” (6)
“the instructions said to enter the first number rolled, no matter how many times you roll it” (2)
“earing money” (3)
“I WILL CHOCE NUMBER5” (5)
“NA” (4)
“2” (2)

MTurk: Money Manipulation Gain Treatment

“That was the first number I rolled.” (4)
“I should random role to the die” (6)
“I chose the number that was my die roll. It was a five. I rolled it, and along with the picture of the die it said: ”You have rolled a 5.”” (5)
“It is game for fun entertainment dice roll game” (3)
“Simulate rolling on first die” (6)
“That was the number that came out on the dice website that was provided” (6)
“got that point so I gave” (6)
“I rolled the die and it showed 5. So i entered my answer 5.” (5)
“when I rolling the die its show number six. as per given number I recorded.” (6)
“I reported the number I rolled” (6)
“6” (6)
“This was the number I rolled the first time.” (2)
“i hit the link to the site with the imaginary die and it came up ”6.” (it really did!) that was the result so that is what i recorded.” (6)
“when I rolling die its shown I recorded number” (6)
“this is my first roll number” (4)
“Which number on a die are you most likely to roll? Dice Roll Probability As you can see, 7 is the most common roll with two six-sided dice. You are six times more likely to roll a 7 than a 2 or a 12, which is a huge difference. You are twice as likely to roll a 7 as you are to

roll a 4 or a 10” (4)
“I rolled a three and recorded it as the instructions stated.” (3)
“I rolled a 5 on a die roll website. In essence, I actually put down what I rolled.” (5)
“Nothing like this” (2)
“6” (6)
“because in the link i rolled the roll” (2)
“Because that the number showed up” (5)
“because it is the number i got after I rolled” (4)
“interesting” (6)
“interesting” (4)
“Becouse its come while die roll” (4)
“Because its come” (4)
“I visited the website provided, and I actually did roll a six. In advance, I admit that I did consider reporting a higher number if I rolled a low one; however, it turned out to not be needed.” (6)
“12” (6)
“Because it was the number I rolled on the website. It was a terrible roll, but an honest one.” (1)
“because i am earning lot of money” (6)
“i clicked the roll button it shows 6 number , so i entered this no.” (6)
“I just roll the die and choose the number no other reason to choose that number.” (5)
“I am very interesting to roll die. I roll and it falls first 5 and then 6. I recorded number 6. I like very much. So. I choose this.” (6)
“when i clicked the roll die it shows number as a 4 , so i recorded the same number” (4)
“I chose the number four because that was what I rolled on my first, and only role.” (4)
“I chose the number 6, because it gives me a great benefit for me. I like very much.” (6)
“when I play the die shown that number so I recorded that number” (6)
“IT IS HIGHEST NUMBER SO I CHOSE THE NUMBER” (6)
“My lucky number” (4)
“Roll in activity” (5)
“By using link I used to roll the die. From that I receive number 2.” (2)
“most very interest” (4)
“4” (5)
“MAIN INCOME SOURCE” (5)

“it was the actual number I rolled” (6)
“I was drawn with this number on the website ”https://www.dieroll.net/”” (5)
“I recorded a number which I got in first roll” (5)
“I PLAYED THE GAME HONESTLY, IN THE INSTRUCTIONS ARE VERY CLEAR SO IAM NOTE THE FIRST NUMBER I GET.” (2)
“I click die roll option for my lucky number spin and best...” (6)
“I'm so glad, I prayed to a god, I needed 6 in the first roll, I got it, thank you, I was so happy, number 6 I got the highest bonus, I honestly got 6 in the first roll” (6)
“NA” (5)
“Because of i want earn more money.” (6)
“Nothing” (5)
“Often called transgender by outsiders, Indian society and most hijras consider.” (3)
“I rolled the dice, one time and reported that number. took a screen shot also.” (6)
“It was the first number I rolled.” (6)
“nover” (3)
“because that is the die I rolled and I wanted to be truthful” (1)
“i roll the die then six came it in the die.” (6)
“I use the link for the dice and dice have number 5.” (5)
“Because the number was rolled” (4)
“I CHOOSE BONUS POINT” (6)
“I chose bonus points” (4)
“I CHOSE BONUS POINT” (3)
“I CHOSE BONUS POINT” (5)
“That was my lucky number” (4)
“3” (3)
“Number 2. It forms the basis of a duality, it has religious and spiritual significance in many cultures.” (2)
“I entered the number that I get when I rolled the die.” (3)
“Probability is a metric for determining the possibility of an event occurring. The number of conceivable outcomes divided by the total number of possible outcomes is the probability of an event, similar to rolling an even number.” (6)
“6” (6)
“4” (4)
“Because it is the number that I rolled on the site you provided.” (5)

“i can roll the die so i can choose” (6)
“NA” (6)
“I had rolled the die and it gave a number.” (3)
“I must be honest.” (2)
“it was prefix instruction” (4)
“That was the number that I rolled, and I value being honest in my life.” (2)
“I followed the link, rolled a dice, and reported the number. I got a 6 but thought maybe I should put a lower number because everyone will probably say they got a six. But ultimately decided to go with the number I got.” (6)
“Because I was told to report the first number that is rolled.” (4)
“porque saiu no giro do dado” (5)
“It was the near sided 6” (4)

MTurk: Money Manipulation Loss Treatment

“Because I rolled and i got that number” (5)
“Because it was the number I rolled” (2)
“Belives.” (3)
“Roll on first die in the activity” (6)
“Roll and click” (1)
“Because it was the first number that came up when I used the online dice roll.” (4)
“NA” (5)
“good game” (1)
“I just reported what I actually rolled.” (3)
“It didn’t chose the number. The number recorded by me is the actual result of die roll.” (5)
“The first number I have chosen while die roll in the die-roll activity” (6)
“To roll die three,i like to roll a die.” (3)
“That is the number I rolled.” (2)
“That the mid term and helps me gain some dollars” (3)
“It is my opinion” (1)
“I reported the random outcome that I received” (5)
“2” (2)
“I was asked to roll a die and then report the number I have rolled. I can roll the die as many times as I would like , But I must report the first number that I roll. The earnings will depend on what I roll.” (2)

“I got that number while I rolled on the internet. For thr first time it showed 4. so i selected the number.” (4)
“It was the number i got from the rolling of die” (5)
“I chose it as i have got that number” (3)
“FOR TAKING A BONUS” (6)
“i achinved the bonus point” (6)
“I AM ROLE THE DIE I HAVE A NUMBER 4.SO,I WILL CHOOSE A NUMBER 4.” (4)
“I know I will lose money and although I have decided with the truth.” (4)
“I chose the number our die roll in 6 is the best confident to achieve the next $7.50 study. I am very confident to complete the next study so please give an next study.” (6)
“I LIKE” (1)
“it is very nice.i will choose number 6.” (6)
“I WILL CHOOSE NUMBER 6” (6)
“LUKY NUMBER” (4)
“1” (5)
“Because it was high meaning a lower loss, and/or, believe It or not it was my first and only roll.” (5)
“i recorded the original outcome i got.” (6)
“as you have mentioned to do so” (3)
“That’s the real one I got when rolled the dice. I dont want to lie to get the reward.” (1)
“most likely” (4)
“3” (3)
“its the number shown” (4)
“Because it was the one i rolled” (5)
“Because I want to save some money” (1)
“I want to save money” (6)
“I want to save my more money.” (6)
“i am choosing number 1 because favorite number” (1)
“i roll the activity show number 6.” (6)
“I chose the number 6 because when I clicked on the link and hit ’Roll’, it stated that I rolled a 6 on the first roll.” (6)
“4” (4)
“i roll the die then it came” (3)
“A fair die have an equal chance of getting” (2)

“6” (6)

“Roll die decision” (2)

“Its the number that was rolled by the website” (6)

“It was the number that I rolled.” (3)

“my favorite number” (4)

“i chose the bonus points” (4)

“i choose the bonus point” (2)

“I chose bouns poants” (4)

“I achieved bonus points” (6)

“I chose bouns poants” (5)

“I CHOSE BONUS POINT” (5)

“Because it was the actual number that I rolled.” (4)

“I rolled there dice its my favorite number.” (5)

“I chose the number that was given by the website.” (2)

“for playing” (5)

“it is what I rolled” (6)

“1” (1)

“I rolled the die and I got the number 4.” (4)

“It was the number I actually rolled on the website.” (4)

“It is the number I got on the die roll website” (5)

“4” (6)

“4” (4)

“because it has the smallest loss” (6)

“I choose it randomly” (4)

“it was what I rolled” (6)

“I selected 5 because, it was the result of my dice. I had a dice in my office and I rolled the dice once.” (5)

“it was the number that was rolled and i wanted to be truthful.” (3)

“The link to the random die roll website provided me with the number 2.” (2)