# Cheating, incentives, and money manipulation

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**Abstract**

We use different incentive schemes to study truth-telling in a die-roll task when people are asked to reveal the number rolled privately. We find no significant evidence of cheating when there are no financial incentives associated with the reports, but do find evidence of such when the reports determine financial gains or losses (in different treatments). We find no evidence of loss aversion in the standard case in which subjects receive their earnings in a sealed envelope at the end of the session. When subjects manipulate the possible earnings, we find evidence of *less* cheating, particularly in the loss setting; in fact, there is no significant difference in behavior between the non-incentivized case and the loss setting with money manipulation. We interpret our findings in terms of the moral cost of cheating and differences in the perceived trust and beliefs in the gain and the loss frames.

*Keywords:* Cheating, lying, incentives, loss aversion, framing, experiment.

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# 1. Introduction

Many economic interactions require that individuals disclose information that they possess. Examples include a car dealer selling a used car, a broker giving advice on the best mortgage, or a professor writing a reference letter for a student or a colleague. In all of these environments with asymmetric information, it may be socially-optimal for individuals to reveal their information truthfully. However, economic and personal incentives may lead people to deliberately misreport such information. One may shade the truth (so common in reference letters as to be the norm) or simply prevaricate. Such dishonesty is a form of *cheating behavior,* a term that also includes activities such as lying, theft, embezzlement, and bribery.

There are noteworthy economic consequences associated with cheating behavior. Indeed, Cohn, Fehr and Maréchal (2014) point to cheating in the business culture as a force that is plausibly responsible (at least in part) for the all-too-common scandals in business and politics. Tax evasion and avoidance led to diminished tax revenue of approximately €1 trillion in the Eurozone, according to the European Commission, and the IRS estimates the overall tax gap in the U.S represents about 16% of the estimated actual tax liability. In the developing world, corruption and cheating are quite prevalent; numerous dictators (e.g., Suharto, Marcos, and Duvalier) have shamelessly looted their countries, which have suffered greatly after the fall of the dictatorship; such corruption hinders investment and growth. In addition, recent experimental evidence has demonstrated that setting goals (Schweitzer, Ordóñez and Douma, 2004; Cadsby, Song and Tapon, 2010) or using policies such as team incentives (Conrads et al., 2013), random bonuses (Gill, Prowse and Vlassopoulos, 2013), or performance-based bonuses (Jacob and Levitt, 2003; Martinelli et al., 2018) can exacerbate cheating behavior.

In this paper, we study the effects of incentives, moral costs, and framing on cheating behavior in gain and loss domains when people are asked to reveal a piece of private information. This information concerns a state of the world, whose report may only determine the payoff of the reporting agent. We focus on behavior when the money to be received is framed alternatively as a gain or a loss to study the effects of loss aversion (Kahneman and Tversky, 1979). There is evidence that loss contracts (i.e., up-front bonuses that workers can lose) increase workplace productivity (Brooks, Stremitzer and Tontrup, 2012; Hossain and List, 2012; Fryer et al., 2012). Further, workers might prefer loss contracts as a way to improve their performance and thus increase their expected earnings (Imas, Sadoff and Samek, 2016). Although these findings highlight some noteworthy effects of up-front incentives, it seems crucial to improve our understanding of other possible effects. For example, Cameron, Miller and Monin (2010) argue that paying people in advance for performing a task might induce a feeling of entitlement or deservingness, and this might facilitate or even encourage unethical behavior.

We investigate whether people cheat more in a loss frame when their private information concerns the state of the world. Since a loss contract usually requires that people receive the money in advance, we conducted treatments (in the gain and the loss frame) with and without having the subjects be in the presence of the money before making a decision. The aim of this design choice is twofold. On the one hand, our approach is methodological. Researchers interested on the predicted power of loss aversion may be tempted to affect both the reference point (by framing earnings as gain or losses) and the "sense of ownership" (by allowing subjects to manipulate the potential earnings in the loss domain). Our study is an attempt to tease apart the possible effect of these two variables (frame and manipulability) on cheating behavior. On the other hand, the manipulability of money may affect the "sense of ownership" but also could affect the "moral cost of lying".

People may feel more entitled to keep the money that they manipulate, in line with the idea of deservingness in Cameron, Miller and Monin (2010). However, it may be more difficult to cheat and maintain a positive self-image after handling the money; thus people may refrain from cheating in this case. The question of how money manipulation affects cheating behavior is relevant empirically, since workers with different roles or in different sectors may manipulate money to different degrees. For example, taxi drivers or cashiers in a department store typically handle money, while Uber drivers or workers in a finance department do not (or do it only on paper). Similarly, some professionals do not manipulate money in their workplace while others do; e.g, teachers, policeman or surgeons for the former and dentists, businessmen or pharmacists for the later. Although of course the opportunities for these people to cheat vary along other dimensions, it seems useful to investigate whether the manipulability of money has an effect on cheating behavior.

We consider an environment where the outcome is independent of one's level of talent or ability, so that there should in principle be no measure of one's worth attached to the report (considerations of self-image and social image may nevertheless affect the reports). We use a variant of the seminal design developed by Fischbacher and Föllmi-Heusi (2013). Each participant is asked to privately roll a die (6-sided in the original experiment, 10-sided in ours), so that the experimenter cannot determine the veracity of the subsequent report.[1] The beauty of this

[1] Studies by, e.g., Greene and Paxton (2009), Shalvi et al. (2011), Conrads et al. (2013), Gravert (2013), Jiang (2013), Ploner and Regner (2013) and Hao and Houser (2017) have also used the die-rolling task. See Abeler, Nosenzo and Raymond (2016) for a recent meta-study. Other studies have used the sender-receiver game where cheating is strategic (i.e., the sender needs to send a message to the receiver about the real state of the world and the receiver may believe it or not). This includes, among others, Gneezy (2005), Sutter (2009), Lundquist et al. (2009), Erat and Gneezy (2012), Erat (2013) and Vanberg (2017). Other studies in which subjects can misreport their private information or send cheap-talk messages include Charness (2000a), Croson, Boles and

design is that while the experimenter cannot know whether an individual is lying, statistical tests on the aggregate data show the extent to which the experimental population distorts the truth. Standard economic models predict that people will cheat in the absence of punishment when there is incentive to do so, but will otherwise be indifferent regarding telling the truth.

We first compare behavior in a Baseline treatment where subjects receive a fixed amount regardless of their report with the behavior of people when their financial payoff depends on the reported outcome. To our knowledge, this is the first paper that directly tests whether subjects lie in the absence of incentives when their behavior does not impose any payoff externality on others. We then proceed to examine behavior in the loss and the gain domains, with and without money manipulation. In our standard treatments with no money manipulation, we give subjects their earnings in a closed envelope at the end of the session. We do change the reference point: In the gain setting, the reported number determines the amount to be placed (by the experimenter) into the envelope, while in the loss setting all envelopes contain the maximum possible earnings and we subtract an amount that depends on the reported number. Arguably, loss aversion would predict more cheating with the loss framing, since giving up money would seem to be more unpleasant than simply not receiving money.[2] To implement the treatments with money manipulation, participants were asked to either take their earnings from an envelope (gain treatment) or put money into an envelope after having received the maximum possible payoff at the beginning of the session (loss treatment).

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Murnighan (2003), and Charness and Dufwenberg (2006). For recent revisions of the literature on cheating see Rosenbaum, Billinge and Stieglitz (2014) and Jacobsen, Fosgaard and Pascual-Ezama (2017).

[2] Garbarino, Slonim, and Villeval (2016) derive a prediction that people will lie more frequently when the probability of a (the) bad outcome is lower, since the higher expected payoff means that the "loss" avoided by lying compared to reference point is greater. They find support from an analysis of studies in the literature as well as new experiments. See also Abeler, Nosenzo and Raymond (2016) and Gneezy, Kajackaite and Sobel (2018) for other experiments that vary the probability of a (the) bad outcome.

How would loss aversion actually apply in a cheating environment? Cameron and Miller (2009), Cameron, Miller and Monin (2010) and Grolleau, Kocher and Sutan (2016) observe that loss aversion encourages cheating in real-effort tasks. We challenge these findings by considering a setting in which subjects do not have to report their own performance on a task. Their tasks differ from ours in that one would expect more concern about one's social image when one's ability (or "honor") is at stake. Previous experimental evidence (e.g., Ertac, 2011; Charness, Rustichini, and van de Ven, 2018) shows that people are much less accurate in processing information when this information is self-relevant than when it concerns an outcome that is unaffected by one's level of talent. Thus, we might expect more cheating in a self-relevant performance task than in our task (Gravert, 2013).[3]

Perhaps the closest paper to ours is Schindler and Pfattheicher (2017). They ask subjects to roll a 6-sided die 75 times and then report the number of '4s' they have obtained. While subjects report more '4s' in the loss frame, the authors find no evidence of cheating in the gain frame, contrary to other experimental evidence (Abeler, Nosenzo and Raymond, 2016). A salient difference between our designs is that we ask subjects to report the outcome of one die roll, while the multiple die rolls in Schindler and Pfattheicher (2017) allows subjects to cheat more than once (see also Shalvi et al. 2011, Fischbacher and Föllmi-Heusi 2013).[4] In addition, we complement their findings by looking at the effects of money manipulation and moral costs on the reported outcomes, as well as reporting behavior when subjects received fixed pay.

[3] On the other hand, Kajackaite (2016) finds the opposite. It is also possible that people cheat more in the die-task because they can attribute a bad outcome to bad luck; it is much more difficult to do so with a real-effort task. Thus, cheating with a die roll may well have a lower moral cost than with a real-effort task. Serra-Garcia, Van Damme and Potters (2013) is another study that compares how people cheat about what they *observe* and what they *do*. In their setting, two subjects play a public-goods game with asymmetric information. In one of the conditions, the informed subject knows the return from contributing and can lie about it, while in another treatment the informed subject can lie about his contribution to the public good. Although there should be no difference across these two conditions (messages are cheap talk and do not affect the payoffs), Serra-Garcia, Van Damme and Potters (2013) find that subjects lie more about what they observe than about what they did.

[4] Schindler and Pfattheicher (2017) consider a second study in mTurk, where subjects self-report the outcome of tossing a coin (Bucciol, and Piovesan, 2011). In this task, where cheating is a binary decision, Schindler and Pfattheicher (2017) find that cheating occurs in both the gain and the loss frame, with more cheating being observed in the later.

A moral cost of cheating has been useful to explain why we observe truth-shading rather than universal reporting of either the true value or the maximum value in previous experiments. With the mounting evidence on lying behavior, some recent models include a term for the moral cost of lying but restrict this to be a function of the distance between the material payoff from lying and that from not lying (e.g., Lundquist et al. 2009; Hilbig and Hessler 2013). However, it seems likely that other arguments should be present in a function reflecting the moral cost of dishonesty. For example, Utikal and Fischbacher (2013) find that nuns tend to under-report the die roll, perhaps wishing to appear modest in their demands. Subjects also refrain from cheating when the opportunity is made particularly salient (Mazar, Amir and Ariely 2008; Gino, Ayal and Ariely 2009). The meta-study in Abeler, Nosenzo and Raymond (2016) indeed concludes that the desire to appear honest may be a key driving force in explaining cheating behavior in the die-rolling task.[5] We assume that the moral costs are the same in the Gain and Loss treatments and depend only on the extent to which subjects distort the truth; i.e., the moral cost is a function of the difference between the true outcome and the reported one. We expect, however, that moral concerns will interact with the motivation to cheat. Our study is an attempt to better understand the interplay between incentives to cheat in the gain and loss domain and the moral costs of cheating.

In fact, some of our experimental results seem surprising. We do find evidence across many of our treatments that cheating is more frequent when this affects the reporter's material payoffs than when it doesn't. However, in the standard treatments where participants simply receive their payoffs in an envelope at the end of the session, we find no evidence of more lying with a loss frame than with a gain frame. We feel that two elements could have helped to induce

[5] See also Rosenbaum, Billinge and Stieglitz (2014), Dufwenberg and Dufwenberg (2016), Kajackaite and Gneezy (2017) and Jacobsen, Fosgaard and Pascual-Ezama (2017) for related evidence, and Mazar, Amir and Ariely (2008) for a theory of self-concept maintenance.

this finding. First, reports are constrained to be one of the 10 possible outcomes of the die roll. If subjects cheat maximally in the gain treatment (given their moral costs), one cannot observe *more* cheating in the loss frame. Second, the sense of ownership might have been weak in this design.

We addressed these points in our treatments where the participants actually physically handle the money. Indeed, requiring the participants to do so led to less cheating. To our surprise, however, this had greater effects in the loss frame than in the gain frame, as we find substantially *less* cheating in this loss treatment than in the corresponding gain treatment. In fact, there is no significant difference between behavior in the baseline treatment and in the loss treatment with money manipulation! Our finding that money manipulation is important to cheating behavior also holds when we consider an alternative viewpoint of our treatments; e.g., we can interpret that subjects who manipulate the money in the gain frame (by subtracting the money from the envelope) are indeed taking their decision in the domain of losses. Loss aversion predicts that they will cheat more than when money is not in front of them, which does not seem to occur. Our sense is that this is not a refutation of loss aversion *per se*, but rather indicates how the moral costs of being dishonest vary across conditions and affect behavior. It may well feel like much more of a real breach to pocket money with which one feels one has been entrusted in advance than money one has found.

The remainder of the paper is organized as follows. We present the experimental design, implementation, and hypotheses in Section 2, and describe the experimental results in Section 3. We provide some discussion in Section 4. Section 5 concludes.

## 2. Experimental Design and hypotheses

### 2.1. Experimental design

All sessions were run at the Laboratory for Research in Experimental Economics (LINEEX) at the University of Valencia. A total of 426 subjects were recruited to participate using the electronic recruitment system of the laboratory. Following Fischbacher and Follmi-Heusi (2013), we added our experiment to the end of a previous experiment that took around 90 minutes.[6] These were aimed at eliciting social time preferences (Rodriguez-Lara and Ponti, 2017) and examining behavior in a bank-run problem (Kinateder, Kiss and Pintér, 2018; Kiss, Rodriguez-Lara and Rosa-Garcia, 2018). Our experiment was presented as an independent task to subjects, in which they could earn some additional money. Subjects did not receive any feedback about the previous experiment when they decided to participate in our task (all subjects decided to participate).

Subjects received a 10-sided die and a copy of the experimental instructions.[7] Their task consisted of rolling the die privately in their cubicles and reporting the number from the first roll on the computer screen. Subjects could roll the die as many times as desired, but were told that only the first throw was relevant for their payment. We used the z-tree software (Fischbacher, 2007) to collect information about the reports.

**Table 1:** Payoffs (in Euros) in each treatment depending on the reported number (0 to 9)

| Reported number | **0** | **1** | **2** | **3** | **4** | **5** | **6** | **7** | **8** | **9** |
|---|---|---|---|---|---|---|---|---|---|---|
| Baseline | 2.5 | 2.5 | 2.5 | 2.5 | 2.5 | 2.5 | 2.5 | 2.5 | 2.5 | 2.5 |
| Gain treatments | 0 | 1 | 1.5 | 2 | 2.5 | 3 | 3.5 | 4 | 4.5 | 5 |
| Loss treatments | -5 | -4 | -3.5 | -3 | -2.5 | -2 | -1.5 | -1 | -0.5 | 0 |

[6] This procedure is frequently used in the literature due to the short nature of the task and the opportunity to avoid paying two separate show-up fees. See Section B1 in Appendix B for further details about the previous experiments.
[7] We use a 10-sided die to increase the number of possible outcomes and have more variability in our data. A translated version of the instructions can be found in Appendix A.

We had five different treatments, which varied the payoffs that subjects received for reporting the outcome of the dice and the extent to which subjects handled their potential earnings. In the Baseline treatment, subjects received a fixed payment of 2.5 Euros in a sealed envelope at the end of the session, regardless of the reported outcome. In the Gain (Loss) treatments, payoffs associated to each report were framed as positive (negative) monetary payoffs to be received (retuned) by subjects (see Table 1). The experimenter was in charge of paying or subtracting these payoffs from subjects in treatments with no money manipulation. In treatments with money manipulation, subjects needed to pay or return the corresponding amount themselves. This was implemented by using envelopes. In treatments with no money manipulation, the experimenter was in the possession of the envelopes for the payments, as in the Baseline treatment. In treatments with money manipulation, subjects had the envelope on their tables and needed to take money or return money depending on their reports.

We attempt to give loss aversion its best chance, thus the initial endowment was mentioned to all subjects participating in the Loss treatments at the beginning of the session (*"You have received an initial amount of 5 Euros for participating in an experiment that follows the one that is about to start. From now, this money belongs to you. Next, we will explain to you the instructions of the first experiment.")*. In the treatment with no money manipulation, this message was read aloud at the beginning of the session. Subjects participating in the loss treatment with money manipulation were also physically given the 5 Euros when they entered the room.

Next, we summarize our treatment conditions and the procedures:

- **Baseline treatment (Baseline)**: At the end of the session, subjects received a sealed envelope with a fixed amount (2.5€), regardless of the number they reported.
- **Gain with no money manipulation (Gain-NO):** As in the Baseline, subjects received their earnings in a sealed envelope at the end of the session. In this treatment, earnings ranged between 0€ (when reporting 0) and 5€ (when reporting 9).

- **Loss with no money manipulation (Loss-NO):** Before starting the session, subjects were informed that they had been allocated with an initial endowment of 5€ to be kept in an envelope by the experimenter. Subjects were told that this would be used in a subsequent experiment. After finishing the first experiment (90 minutes), subjects were reminded about their 5€ and presented with our task. Subjects knew that the reported number would determine the amount to be deducted from the envelope. The envelope was given to subjects at the end of the session.

- **Gain with money manipulation (Gain-MM):** Again, earnings increased with the reported number but subjects had an envelope with 5€ on their desk before rolling the die. Each subject had to extract their earnings from the envelope upon rolling the die and reporting the outcome on the computer screen.

- **Loss with money manipulation (Loss-MM):** Subjects received the initial endowment (5€) at the beginning of the session and were informed that they had been allocated with this initial endowment, as in the Loss-NO treatment. After finishing the first experiment (90 minutes), subjects were given an empty envelope. They rolled the die, reported the outcome, and placed the amount to be returned in the envelope before leaving the room.[8]

Before proceeding to the hypotheses, there are some aspects of our experimental design that are worth mentioning. First, we explained nothing about the cheating experiment when subjects participated in our first experiment; i.e., the instructions of our experiment were presented to subjects only once the first experiment had been completed. Second, earnings associated with each reported number were equivalent in the Gain and the Loss treatments. Third, subjects in the money-manipulation treatments had a second opportunity to cheat by misreporting the amount of money they had to take from or leave in the envelope. In this respect, our evidence is consistent with Cameron and Miller (2009) or Schindler and Pfattheicher (2017); we do not find that subjects recorded an outcome that did not correspond to the amount of money they took from or left into the envelope. Finally, we should acknowledge the alternative view of one of the referees, who suggested that our treatments with no money manipulation could be considered to

[8] When giving them the initial endowment, we observe that roughly 1/3 of the subjects decided to leave the money on the table, while the rest put it in their pockets. We cannot determine if these subjects behave differently but we tried to minimize this problem by asking participants in the Loss-MM to put their initial endowment on the table before rolling the die.

be Gain treatments because subjects received their earnings at the end of the session, while money-manipulation treatments are Loss treatments because subjects had their earnings in front of them. We believe that it would not be easy to rationalize the announcement of the initial endowment of 5 Euros and the negative payoffs associated with the reports in the Loss-NO to a Gain frame. However, we discuss how our data reconcile with this viewpoint when we present our hypotheses and our results below.

### 2.2. Model and hypotheses

After rolling the die and observing the (true) outcome $T$, subjects can report any number $R$, where $T, R \in X = \{0, 1,\ldots, 9\}$. Let $m: X \rightarrow \mathbb{R}$ denote the monetary outcome associated each possible outcome of the die, where $m^T \geq 0$ ($m^R \geq 0$) stands for the earnings associated with the true (reported) outcome, respectively. We assume that subjects have a separable utility function as follows:

$$(1)\quad U(T, R) = v(m^R \mid e) - c(T, R)$$

where $v(m^R \mid e)$ denotes the material utility associated with the report and $c(T, R)$ measures the moral cost of cheating; this determines the disutility subjects experience when reporting an outcome that differs from the observed one. We build on the idea of reference-dependent utility (e.g., Kőszegi and Rabin, 2006, 2009; Abeler et al., 2011) and loss aversion (e.g., Kahenman and Tversky, 1979; Köbberling and Wakker, 2005) to model the way in which subjects assess their earnings. As any report $R \in X$ is associated to a monetary outcome $m^R \geq 0$, we denote $v(m^R)$ the utility that subjects derive from reporting $R \in X$. In addition, subjects who received an initial endowment $e > 0$, use it a reference point; thus any amount $m^R$ that exceeds (falls below) their initial endowment will be evaluated as a gain (loss) in utility using the function $\beta\, v\left(m^R - e\right)$ with $\beta \geq 0$. This, in turn, implies that material utility $v\,(m^R \mid e)$ associated with the report can be written as follows:

$$(2)\ v\,(m^R \mid e) = \begin{cases} v\,(m^R\,) & \text{if } \ e = 0 \\ v\,(m^R) + \beta\, v\left(m^R - e\right) & \text{if } \ e > 0 \end{cases}$$

where $v\,(m^R - e)$ is positive whenever the amount associated with the report exceeds the initial endowment, whereas $v\,(m^R - e)$ is negative when the amount associated with the reports are below the initial endowment. We further assume that the value function $v\,(x)$ is increasing in $x$ (i.e., $v'(x) > 0$), concave for gains (i.e., $v''(x) > 0$) and convex for losses, ($v''(x) > 0\ for\ x < 0$). To capture the idea of loss aversion, $v\,(x)$ is steeper for losses than for gains to capture, i.e., $v'(-x) > v'(x) > 0$, for $x > 0$). We also assume that $v\,(x)$ is a piece-wise function with $v\,(-x) = -\lambda\, v\,(x)$, where $\lambda > 1$ indicates that losses loom larger than equal-sized gains (Köbberling and Wakker, 2005).[9] The cost function $c(.)$ is non-negative, $c\,(T, T) = 0$ and $c\,(T, R)$ is increasing in $T - R$ (see, e.g., Abeler, Nosenzo and Raymond, 2016; Gneezy, Kajackaite and Sobel, 2018).[10]

We can compare the utility of subjects when they report the true outcome and any other outcome $R > T$. It follows that subjects cheat if:

$$(3)\ v\,(m^R \mid e) - v\,(m^T \mid e) > c\,(T - R)$$

[9] We assume separability between the benefits associated to the report and the cost of cheating (see, e.g., Gneezy, Kajackaite and Sobel 2018). The assumption that $v\,(x)$ is piece-wise linear is common for small values of $x$ (e.g., Abeler et al. 2011).

[10] Consistent with previous theoretical models and the preponderance of the observed evidence, we assume subjects never report an outcome $R < T$ in our model.

Consider first the Baseline treatment, where subjects receive no endowment and lying has no benefit; in fact, subjects receive $m^R = 2.5$ regardless of the reported outcome thus $v\ (m^R \mid e) = v\ (m^T \mid e)$ for every $R \in \{0, 1, \ldots, 9\}$. Because of the moral costs of cheating, we expect that subjects will report truthfully in the Baseline treatment; in fact, $c\ (T, R)$ can only be 0 when subjects report the true outcome (see footnote 10); thus we should observe an equal distribution of outcomes in this treatment.[11] Our first prediction is:

**Hypothesis 1:** *The distribution of reports in the Baseline treatment is not significantly different from the uniform distribution.*

In all of our treatments except the Baseline, people have a financial incentive to cheat. In the Gain treatments, $v\ (m^R \mid e) = v\ (m^R) > v\ (m^T \mid e) = v\ (m^T)$. In the Loss treatments, $v\ (m^R \mid e) = v\ (m^R) - \beta\, \lambda\, v\ (e - m^R)$ and $v\ (m^T \mid e) = v\ (m^T) - \beta\, \lambda\, v\ (e - m^T)$. Because $e \geq m^R > m^T$ it follows that $v\ (m^R \mid e) > v\ (m^T \mid e)$ for any $R > T$. If people value money and the cost of lying is not extreme, we should expect to see reports in the Gain and Loss treatments that are significantly higher than that in either the uniform distribution or the Baseline. Thus, our second hypothesis is:

**Hypothesis 2:** *The numbers reported in the Gain and Loss treatments will be significantly higher than those in the uniform distribution and in the Baseline.*

We turn to equations (2) and (3) to rationalize our hypotheses on the effects of loss aversion and money manipulation. In line with the literature on loss aversion and a plausible link between loss aversion and choices made with gain and loss frames, we expected more cheating with loss framing. This is because subjects who misreport the outcome in the Gain treatment associate it with a gain in utility $V^G = v\ (m^R \mid e) - v\ (m^T \mid e) = v\ (m^R) - v\ (m^T) > 0$. In the Loss treatment, the gain in utility includes an additional component, which stands for the extra loss that

[11] We are not aware of any other paper that directly tests for cheating behavior in the absence of economic incentives.

subjects avoid. In particular, $V^{\mathrm{L}} = (m^R \mid e) - v\,(m^T \mid e) = v\,(m^R) - v\,(m^T) + \beta\,\lambda\,[v\,(e - m^T) - v\,(e - m^R)] > V^{\mathrm{G}}$, where $e \geq m^R > m^T$. In both the gain and the loss frames, the cost of cheating $c\,(T, R)$ depends only on the difference between the outcome and the report, since $c\,(T, R)$ is a function of $T - R$. This, in turn, implies that when subjects evaluate the possibility of misreporting the true outcome in the gain and the loss frames, the cost will be the same in both domains. This leads to our third hypothesis:

**Hypothesis 3:** *The numbers reported in a Loss treatment will be significantly higher than those reported in the corresponding Gain treatment.*

While cheating has clear financial benefits in the Loss and Gain treatments, it may also have a moral cost, e.g., subjects might be averse to cheat due to social image (the choice is observed by the experimenter, as in Gneezy, Kajackaite and Sobel, 2018), a desire to hold a positive self image (e.g., Mazar, Amir and Ariely, 2008), or some form of guilt aversion (e.g., Battagalli and Dufwenberg, 2007, 2009). We aim at manipulating the cost of cheating in our experiment by affecting the cost function $c\,(T - R)$ in equation (3) through varying whether people handle the money. We feel that asking people to handle their potential earnings will tend to increase the moral cost of lying. This is because those who must take money from the envelope or place it into the envelope may feel it is more difficult to maintain their self-image after cheating, compared with the case in which the experimenter manipulates the money. In fact, there are other mechanisms that can explain why the cost function $c\,(T - R)$ may be larger in the presence of money manipulation. For example, people tend to refrain from cheating when this becomes salient (Mazar, Amir, and Ariely, 2008; Gino, Ayal, and Ariely 2009), which may apply in the treatments with money manipulation.[12] Since a higher moral cost of cheating leads to less cheating, we have the following hypothesis:

**Hypothesis 4:** *The numbers reported in the money-manipulation treatments will be lower than in the corresponding treatments without money manipulation (because of the moral cost of cheating).*

Our hypotheses 1 to 4 rely on the implicit cost-benefit analysis of equation (3). We constructed our Gain and Loss treatments so as to affect the left-hand-side of the equation, $v\,(m^R \mid e) - v\,(m^T \mid e)$, while the effects of the money manipulation on the moral costs of cheating alter the right-hand-side of the equation, $c\,(T - R)$. It is worth noticing, however, that the subjects' utility function also depends on the value of their initial endowment, which serves as a reference point to subjects. This is relevant to Hypothesis 4, where the implicit assumption is that handling the money does not affect the value of the initial endowment; that is, in the Gain and Loss treatments the value of the endowment is the same with and without the manipulation of money. One may argue that when subjects see the potential earnings on the table in the Gain-MM or receive their endowment in advance in the Loss-MM, they may feel more deserving to keep it. If such "sense of ownership" exists, it could compensate for the moral cost of cheating discussed above and result in more cheating when people handle the money, compared with the case in which the experimenter does. This hypothesis goes back to the alternative viewpoint discussed in Section 2.1, which predicts more cheating when people manipulate the endowment because earnings on the table can be viewed as potential losses. We account for this possibility in our final hypothesis, which is an alternative to Hypothesis 4.

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[12] Gneezy, Kajackaite and Sobel (2018) argue that the utility function should incorporate a term that accounts for the fact that people value honest behavior or being perceived as honest; see also Garbarino, Slonim, and Villeval (2016) or Abeler, Nosenzo and Raymond (2016). In our setting, it seems reasonable to assume that people will be perceived as being more honest when they report truthfully in treatments with money manipulation, which would lead to less cheating in these treatments, as a result.

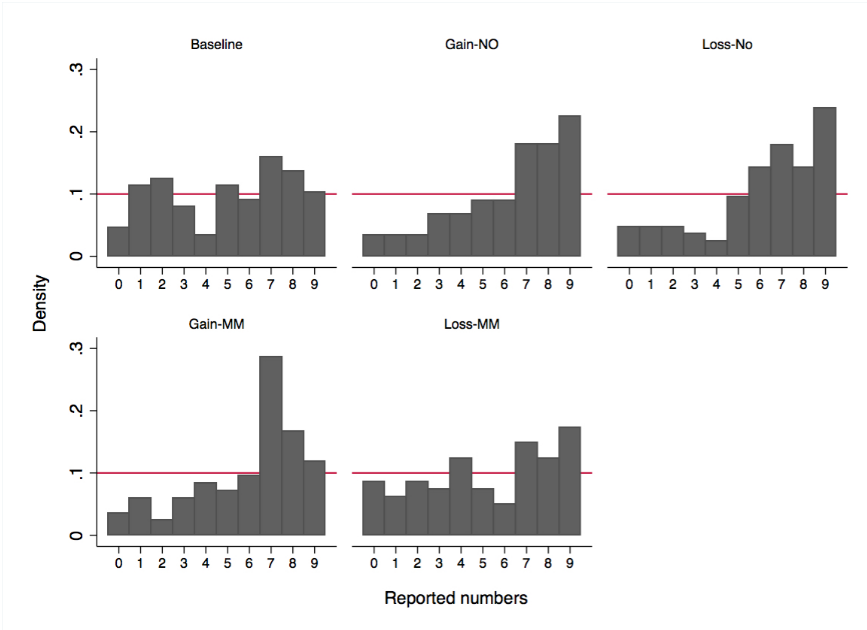
**Hypothesis 4':** *The numbers reported in the money-manipulation treatments will be larger than in the corresponding treatments without money manipulation (because subjects will feel more entitled to keep the money that they manipulate).*

To formalize this Hypothesis 4' consider that the value of the endowment with money manipulation ($e_{MM}$) is larger than the value of the endowment with no money manipulation ($e_{NO}$). In Gain-NO, subjects receive no endowment ($e_{NO} = 0$), thus any report is associated with a gain in utility $v(m^R)$. In Gain-MM, subjects see the potential earnings on their table ($e_{MM} = 5$) thus any report will be associated a loss in utility, $\beta v(m^R - e_{MM})$. By using our arguments above, we can show that $v(m^R | e_{MM}) - v(m^T | e_{MM}) > v(m^R | e_{NO}) - v(m^T | e_{NO})$ thus more cheating would be expected in the Gain treatment with money manipulation. The same holds in the Loss treatment after noticing that $v(m^R | e) - v(m^T | e) = v(m^R) - v(m^T) + \beta\lambda[v(e - m^T) - v(e - m^R)]$ and the value function $v(x)$ is concave; therefore $v(e_{MM} - m^T) - v(e_{MM} - m^R) < v(e_{NO} - m^T) - v(e_{NO} - m^R)$, where $e_{MM} > e_{NO} \geq m^R > m^T$.

## 3. Results

Figure 1 displays the distribution of the reported numbers in each of the treatments. The horizontal line at .1 indicates the expected frequency if reports followed the theoretical uniform distribution. Table 2 summarizes our data by including information on the cumulative distribution of reported numbers.

**Figure 1:** Distribution of reported numbers per treatment




The average reported number is above the mean expected outcome (that predicted by the uniform distribution) of 4.5 in all the treatments. In fact, more high numbers (5-9) are reported than low numbers (0-4) in all treatments. Consistent with previous findings (Fischbacher and Föllmi-Heusi 2013, Utikal and Fischbacher 2013, Shalvi et al. 2011, Abeler, Nosenzo and Raymond 2016), there is no single large spike at the payoff-maximizing outcome.[13]

**Table 2: Descriptive statistics**

Mean, standard error and cumulative frequency of reports

| Treatment | Obs. | Mean | SE | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Baseline | 88 | 4.977 | 0.304 | 0.046 | 0.159 | 0.284 | 0.364 | 0.398 | 0.511 | 0.602 | 0.761 | 0.898 | 1.000 |
| Gain-NO | 89 | 6.281 | 0.270 | 0.034 | 0.067 | 0.101 | 0.168 | 0.236 | 0.326 | 0.416 | 0.596 | 0.775 | 1.000 |
| Loss-NO | 84 | 6.214 | 0.291 | 0.048 | 0.095 | 0.143 | 0.179 | 0.202 | 0.298 | 0.440 | 0.619 | 0.761 | 1.000 |

[13] We also note that there is no significant gender difference in any of the five treatments. The overall average number reported by males (females) was 5.849 (5.633). The overall proportion of zeroes reported by males (females) was 0.050 (0.048), while the proportion of nines reported by males (females) was 0.171 (0.172). See, among others, Cappelen, Sorensen, and Tungodden (2012), Childs (2012), Ezquerra, Kolev and Rodriguez-Lara (2018), Gylfason Arnardottir and Kristinsson (2013), and Pascual-Ezama, Prelec and Dunfield (2015) for other studies showing no gender differences in cheating behavior.

| | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Gain-MM | 84 | 5.952 | 0.271 | 0.036 | 0.095 | 0.119 | 0.179 | 0.262 | 0.333 | 0.429 | 0.714 | 0.881 | 1.000 |
| Loss-MM | 81 | 5.198 | 0.332 | 0.086 | 0.148 | 0.235 | 0.309 | 0.432 | 0.506 | 0.556 | 0.704 | 0.827 | 1.000 |

We proceed to test our hypotheses with both non-parametric tests and regression analysis. First, the Kruskal-Wallis test rejects the hypothesis that observations in the different treatments come from the same distribution ($\chi^2_4$ = 14.95, $p$ = 0.005), thus monetary incentives and/or the manipulation of money seems to affect the reported outcomes.

The results of our non-parametric analysis are summarized in Table 3. We first investigate whether the reported outcomes in each of the treatments differ from the actual expected outcomes (i.e., the equal distribution) using a $\chi^2$ test; see column (1). Using Wilcoxon rank-sum and Kolmogorov-Smirnov tests, we compare the reports in the Gain and Loss treatments with those in the Baseline; see columns (2) and (3). [14]

Hypothesis 1 states that there will be no difference between reports in the Baseline treatment and the expected outcomes of the die roll. In fact, while we do see a slight tendency towards reporting higher numbers, the $\chi^2$ test shows no significant difference between the reports in the Baseline and the actual expected outcomes ($p$ = 0.213). Thus, when there is no financial incentive for misreporting, we see no evidence of significant distortion in the reports made (see Tables B2 and B3 in the Appendix for further evidence of no cheating in the Baseline treatment).

**Table 3:** Non-parametric analysis on differences from expected true reports and baseline reports

| | Expected true reports | Baseline reports | |
|---|---|---|---|
| Treatment | (1) $\chi^2$test | (2) Wilcoxon rank-sum ($Z$) | (3) Kolmogorov-Smirnov ($KS$) |

[14] Throughout the paper, we round all $p$-values to three decimal places. The interested reader on the comparison between the reported outcomes in each treatment and expected actual outcomes using the Wilcoxon rank-sum test or the Kolmogorov-Smirnov test of cumulative distributions can consult Appendix B (Table B1). This includes information on the fraction of subjects who cheat to avoid the worst possible outcome using the estimation method in Garbarino, Slonim and Villeval (2016).

| | | | |
|---|---|---|---|
| Baseline | 12.00 (0.213) | - | - |
| Gain-NO | 38.98 (0.000) | 3.128 (0.001) | 0.195 (0.024) |
| Loss-NO | 39.57 (0.000) | 2.880 (0.002) | 0.213 (0.013) |
| Gain-MM | 45.11 (0.000) | 2.165 (0.015) | 0.070 (0.039) |
| Loss-MM | 11.72 (0.230) | 0.610 (0.271) | 0.185 (0.488) |

Note: $p$-values (parentheses) reflect one-tailed tests.

**Result 1**. *There is no significant evidence of cheating in the absence of economic incentives.*

We expected more cheating in our four treatments where there is a financial incentive to report a higher number than was actually rolled, as stated in Hypothesis 2. Indeed, the Gain-NO and Loss-NO treatments show much higher numbers than the expected true outcomes ($p$ = 0.001) or the Baseline ($p$ = 0.024). There is also evidence in the Gain-MM treatment of distortion relative to the expected outcomes ($p$ = 0.001) and the Baseline ($p$ = 0.039). However, there is surprisingly little difference between the reports made in the Loss-MM and Baseline treatments or between the reports in Loss-MM and the expected actual outcomes, even with a one-tailed test ($p$ = 0.230).

**Result 2.** *Providing financial incentives for lying increases reported numbers in all treatments except in the Loss-MM, where the distribution of reported numbers does not significantly differ from the expected actual outcomes and the Baseline distribution.*

Hypothesis 3 predicts that presumed loss aversion will manifest in more cheating in reports made in the loss frame than those made in the gain frame. As suggested in the preceding paragraph, the observed patterns do not support this hypothesis. In fact, there is very little difference in the reports across the Gain-NO and Loss-NO; the average report is in fact only slightly higher in the Gain-NO treatment (6.281 versus 6.214). The respective one-tailed test statistics and $p$-values are $Z$ = -0.089, $p$ = 0.536 and $KS$ = 0.042, $p$ = 0.500. What may be even more surprising is that there is substantially *less* cheating in Loss-MM than in Gain-MM. In any case, we have strong evidence to reject Hypothesis 3 in the money-manipulation treatments.

**Result 3.** *There is an equal amount of cheating in the gain and loss domains when there is no money manipulation. If there is no money manipulation, less cheating is observed in the loss domain than in the gain domain.*

Finally, Hypothesis 4 and Hypothesis 4' predict that money manipulation will affect the reported outcomes. We find strong support for the hypothesis that money manipulation leads to less cheating, perhaps due to an increased moral cost of lying when we compare reports made in the Loss-MM and Loss-NO treatments (Hypothesis 4). In fact, the median (modal) report in the Loss-MM treatment is 5 (7), while the median (modal) report in the Loss-NO treatment is 7 (9). The Wilcoxon test gives $Z = 2.128$, $p = 0.016$, while the Kolmogorov-Smirnov test gives $KS = 0.230$, $p = 0.008$, both one-tailed tests. The differences across the reports in the Gain-MM and Gain-NO treatments are considerably more modest and not statistically significant using a non-parametric approach, but also favor Hypothesis 4 in that people cheat less when they manipulate money; the modal report in the Gain-MM (Gain-NO) is 7 (9). Here the Wilcoxon test gives $Z = 1.144$, $p = 0.126$, while the Kolmogorov-Smirnov test gives $KS = 0.119$, $p = 0.254$, both one-tailed tests. Thus, we see that money manipulation affects the moral cost of cheating and makes a real difference with a loss frame, but much less of a difference with a gain frame.

**Result 4**. *Money manipulation reduces cheating behavior, especially with the loss frame.*

We now proceed to regression analysis. In Table 4, we report the results of a Tobit analysis, where the set of independent variables include dummies for the gain frame and the manipulation of money, as well as the interaction term.[15] The reported standard errors (in parentheses) are clustered at the session level. Our regression analysis uses the data from all treatments except the Baseline. Our first regression in column (1) considers all treatments in which there are financial incentives to cheat. Specifications (2) to (5) give the results for different Tobit models, depending on whether or not subjects manipulate the money and the frame. Recall that our task was added at the end of a previous session. Our estimates in column (a) do not account for the behavior in the previous experiment. We show in column (b) that our findings are robust after controlling for the type of the preceding experiment (time preferences or bank runs) and the earnings of subjects in these experiments.[16]

- Table 4 about here -

First, the third row shows that money manipulation leads to smaller reported numbers and so presumably less cheating. When there is no money manipulation, behavior does not differ significantly across the Gain and Loss frames. Money manipulation does reduce the reported outcomes, especially in the loss frame; in fact, subjects cheat more in the Gain than in the Loss treatment with money-manipulation.[17] While we argue that this result reflects a difference in moral costs, it nevertheless does not *per se* provide any support for loss aversion. This is true even

[15] We note that ordinary least squares regressions provide qualitatively the same results and similar levels of significance. See Appendix B (Table B3 and Table B4) for further analysis and robustness checks.

[16] Although participants in our experiment were not informed about their earnings in the prior experiments, one may argue that they could had formed some beliefs to be used as a reference point. The correlation between the previous earnings and the reports in each of the treatment is negative but never significant ($p > 0.165$). Appendix B presents further details about the previous experiments and show that the reports are independent of the previous task or the role of subjects in these experiments (see Sections B1 and B2).

[17] Note that the effect of the money manipulation in the Gain frames becomes significant when we control for the previous experiment.

if we consider that treatments with money manipulation to be Loss frames; in fact, our data lend no support for Hypothesis 4', that money manipulation leads to more cheating.

## 4. Discussion

Our results confirm some expected patterns. For example, people do not lie much when there is no financial incentive to report an outcome different from the one that actually occurred; we find no significant difference between the reports made in this environment and the expected distribution of actual outcomes. We also observe considerable dishonesty when there is a financial incentive to report a higher number, which is consistent with previous work. Finally, money manipulation seems to affect the moral cost of cheating and thus the reported outcomes, in a way that suggests that people care about their self-image or they way they are viewed by others.

But some other results are largely surprising. We expected to find evidence of more cheating when earnings associated with the report were framed as a loss, but we found none in a standard environment. When we conducted treatments where the participants were given envelopes with the funds and then had to physically handle the money, we observe *less* cheating in our loss frame than in our gain frame! In fact, there was only a modest and insignificant difference in the reports in the Baseline treatment and the loss frame with money manipulation.

What could explain these unexpected results? First, an important consideration regarding loss aversion is the reference point for gains and losses, which very well may be unclear in laboratory settings (Terzi et al. 2016). Second, while loss aversion and reference dependence are widely accepted, the generality of loss aversion may in fact be less than universal. A number of studies (e.g., Erev, Ert, and Yechiam, 2008; Harinck et al., 2007; Kermer et al., 2006) examining the effect of losses in decision-making under risk and uncertainty in fact find no evidence of loss aversion, as is the result in our standard treatments.[18] Third, it is possible that subjects in our gain and loss treatments were already cheating maximally (given their moral costs), thus the (constrained) task prevented us to find more cheating under loss aversion; the spikes are in fact at the maximum value in the standard treatments.

After having observed no difference in behavior across the Gain-NO and Loss-NO treatments, we made matters (literally) more hands-on by having subjects manipulate the money. We attempted to give loss aversion its best chance by announcing the 5-Euro prize they had been allocated as early as possible, in an effort to enhance the sense of ownership. Our findings lend support for the idea that money manipulation influences the moral cost of cheating. The spikes at higher numbers other than the maximum value in these treatments suggest that many people who choose to lie do not wish to be seen as a liar (in fact by far the highest spike in all of the data is the spike at 7 in the Gain-MM treatment).[19]

Trust and morality are important and people may be sensitive to small clues and considerations (Mazar, Amir and Ariely, 2008). We suspect that if one feels trusted, one is more likely to respond in an honest or trustworthy manner. This could explain why people cheat less with money manipulation, although keeping the money with which one has been entrusted may feel more like stealing than taking money that one has been invited to take.[20] If one is given money and hold it in one's possession, one may feel that one has been trusted (the mental-cheating condition could be seen as being a strong demand effect). In our experiment, one's

[18] In fact, Harinck et al. (2007) find evidence of reversed loss aversion in a series of experiments where subjects are asked to rate how (un)pleasant would be finding (losing) small amounts of money. They argue that negative feelings associated with small losses may be outweighed by positive feelings associated with equivalent small gains. A similar argument applies to our setting; the unpleasant feelings may refer to moral costs of being dishonest, while the positive feelings are associated to being honest and reporting the actual outcome.

[19] To our surprise, however, our null result does not appear to have been driven by a lack of a sense of "ownership" of the funds, since we find no evidence of loss aversion in the money-manipulation treatments.

[20] In a sense, this resembles the idea of omission-commission in Spranca, Minsk and Baron (1991). However, participants in our experiment are always asked to enter the number they have obtained in the computer screen, thus cheating requires acts of commission even in the loss condition (Cameron and Miller, 2009)

feelings about lying are also likely to be affected by one's beliefs about what is expected. If the feeling of being trusted leads to different beliefs about what is expected, it could be the case that one believes that the trustor believes that one will behave in a trustworthy manner; otherwise, one may experience disutility from guilt (Charness and Dufwenberg, 2006; Battigalli and Dufwenberg, 2007, 2009; Battigalli, Charness, and Dufwenberg, 2013).

Perhaps people in the money-manipulation treatments had different beliefs about the expectations of the experimenter than people in the other variable-pay treatments. Specifically, people in the money-manipulation loss treatment were perhaps more likely to feel trusted with the full potential payoff in the beginning and might have had different beliefs about the beliefs of the experimenter than people in the corresponding gain treatment. Having been endowed with visible money in the beginning of an experiment in the loss frame may also make cheating more salient than having an envelope from which people can later take money. Mazar, Amir, and Ariely (2008) and Gino, Ayal, and Ariely (2009) find that subjects cheat less when cheating is salient as it more difficult is to maintain the self-image of being honest. Along these lines, Gneezy, Kajackaite, and Sobel (2018) incorporate an additional term in the utility function so that subjects value honest behavior or being perceived as honest, even when the true outcomes cannot be observed and thus cheating cannot be detected.

By asking subjects to return the money in the loss treatment, we might also trigger impure altruism (Andreoni, 1989, 1990). The decision to return money could then be associated with warm-glow giving, especially in the money-manipulation loss treatment, where subjects had to place the amount to be returned in an envelope. Finally, subjects could also have different attitudes towards losing the money in the loss treatments with and without the manipulation of money, as paper losses may be treated differently from realized losses (Weber and Zuchel 2005; Imas, 2016); this could reflect mental accounting (Thaler, 1985).

One might nevertheless wonder why there was more cheating in the loss frame than in the gain frame in other studies. In our study, there is nothing regarding one's own ability, while the work of Cameron and Miller (2009), Cameron, Miller and Monin (2010) and Grolleau, Kocher and Sutan (2016) involves reporting one's own performance in a cognitive task. One's judgment about own ability seems to be more malleable than one's judgment about events over which one has no control, as seen in the updating studies mentioned earlier. Indeed, lying is likely to have some moral cost, but one would like to appear talented or capable in the eyes of those who may be watching or even one's self; in fact, this may not even be a conscious tendency (see Charness, Rustichini, and van de Ven, 2018).[21] Using the die-roll task, Schindler and Pfattheicher (2017) allows for multiple rolls of the die and find that reports are larger under loss aversion, but the authors do not find evidence that people cheat in a gain setting. We complement their findings by also considering the effects of the moral costs, which seems to be a crucial element in understanding cheating behavior.

The moral cost of lying or cheating may well be higher when one feels entrusted with money and has actually handled the money (as was the case in the Loss-MM treatment). It may feel like more of a personal breach when one is given money and one absconds with it.[22] One may well be more honest when one feels more trusted or responsible. Some evidence for this notion is provided in Charness (2000b), while Mazar, Amir and Ariely (2008) and Gino, Ayal and Ariely (2009) provide evidence that subjects refrain from cheating when the opportunity is made

[21] Some people even seem to care about reporting a higher number in the Baseline treatment, since more high numbers (5-9) are reported than low numbers in this case – more than 60% of the reports are high numbers.
[22] In the loss treatments (Loss-No and Loss-MM) subjects knew about or received the 5-Euros when they entered the room. In both settings, we induced the reference point at the beginning, so it should be the manipulation of money (not the point at which earnings were announced) that drives the results.

particularly salient (see also Jacobsen, Fosgaard and Pascual-Ezama (2017) for a discussion). Finally, Campbell (1935), May and Loyd (1993) and Haines et al. (1986) find that an honor system induces more honesty than does a proctor.

The results for loss aversion in our experiments surprised us. One might argue that the sense of ownership was not sufficiently strong in our standard treatments, although our mechanism was similar to others used previously. One might also argue that loss aversion was indeed present in the money-manipulation treatments, but that it was swamped by the relatively high moral cost of lying in the Loss-MM treatment. While further research is needed to disentangle these considerations, our evidence nevertheless suggests that loss aversion was not a strong force in our experiments.

## 5. Conclusion

Our paper investigates cheating behavior when experimental participants are asked to reveal a piece of private information that does not reflect on their personal ability and where one's choice does not affect the financial payoffs of other participants. This information concerns the result of a die roll. In the Baseline treatment, there is no financial incentive for misreporting the result and the reports made do not differ significantly from expected outcomes with random draws. On the other hand, reports when there are financial incentives to cheat generally show considerable evidence of lying on the reports made. In addition, we study cheating in the absence of payoff externalities, in that only one's own material payoff is affected by reported outcome.

We do not find evidence of loss aversion in our environment. There is no difference in behavior across gain and loss frames when payment is simply made at the end of the session. More remarkably, when we endow participants with prospective payment in advance, there is substantially *less* cheating in the loss frame. We presume that the observed behavior represents differences in the moral cost of cheating, reflected by either some form of guilt aversion or a desire to have a favorable self-image or social image.

The moral cost of dishonest behavior is an element that must not be ignored and this goes beyond simple considerations such as the distance of one's report from the truth or guilt for disappointing others and affecting their earnings. We expect more research will follow on this theme, as it is critical to understanding cheating and corruption in the world at large.

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**Table 4:** Regression analysis

| | (1) Pooled data | | (2) No Manipulation | | (3) Manipulation | | (4) Gains | | (5) Losses | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (a) | (b) | (a) | (b) | (a) | (b) | (a) | (b) | (a) | (b) |
| Constant | 6.624*** | 6.332*** | 6.628*** | 6.271*** | 5.368*** | 5.820*** | 6.636*** | 6.809*** | 6.678*** | 6.334*** |
| | (0.459) | (0.484) | (0.480) | (0.558) | (0.127) | (0.444) | (0.231) | (0.275) | (0.534) | (0.546) |
| Gain | 0.061 | 0.439 | 0.061 | 0.585 | 0.757** | 0.724** | | | | |
| | (0.505) | (0.479) | (0.527) | (0.478) | (0.365) | (0.337) | | | | |
| Money manipulation | -1.254*** | -0.964** | | | | | -0.531 | -1.023*** | -1.285** | -1.078** |
| | (0.476) | (0.465) | | | | | (0.386) | (0.315) | (0.553) | (0.529) |
| Gain × Money manipulation | 0.696 | 0.0257 | | | | | | | | |
| | (0.605) | (0.570) | | | | | | | | |
| Control previous experiment | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| Sigma | 3.350*** | 3.342*** | 3.376*** | 3.354*** | 3.326*** | 3.304*** | 3.043*** | 3.022*** | 3.685*** | 3.682*** |
| | (0.187) | (0.191) | (0.197) | (0.207) | (0.345) | (0.342) | (0.197) | (0.200) | (0.192) | (0.192) |
| Pseudo LL | -762.751 | -762.936 | -380.477 | -379.411 | -382.262 | -382.158 | -386.015 | -384.747 | -374.673 | -374.589 |
| # of obs.(uncensored) | 338(257) | 338(257) | 173(126) | 173(126) | 165(131) | 165(131) | 173(137) | 173(137) | 165 (120) | 165(120) |

*Notes:* Specification (1) includes data from all treatments except the Baseline. Specification (2) includes data from only the Gain-NO and Loss-NO treatments to investigate whether the frame has an effect on cheating when subjects do not manipulate the money and receive the earnings from the experimenter, while (3) includes data only from the Gain-MM and Loss-MM treatments to see whether cheating in the gain and loss treatments differ when subjects manipulate their earnings. Specification (4) includes data only from the Gain-NO and Gain-MM treatments to see the effect the money manipulation in the gain treatment, (5) includes only data from the Loss-NO and Loss-MM treatments to see the effect of the money manipulation in the Loss treatments. ** and *** indicate significance at the $p$ = 0.05 and $p$ = 0.01 levels, respectively, two-tailed tests.

# Appendix A: Experimental Instructions (originally in Spanish)

## BASELINE TREATMENT

### Instructions (to be read aloud)

The aim of this experiment is to study decision-making. We are not interested in your particular choices but rather on the individual's average behavior. Thus, all through the experiment you will be treated anonymously. Neither the experimenters nor the people in this room will ever know your particular choices.

Next, you will receive the instructions and a 10-sided die. Instructions should be easy to follow. Please read the instructions carefully and raise your hand if you have any doubt, as it is important that you understand the instructions before starting the experiment.

### Instructions

The aim of this experiment is to study decision-making. We are not interested in your particular choices but rather on the individual's average behavior. Thus, all through the experiment you will be treated anonymously. Neither the experimenters nor the people in this room will ever know your particular choices. Please do not think that we expect a particular behavior from you. However, take into account that your decisions along the experiment may affect your earnings. Below, you will find details of your task in this experiment. Please follow the instructions carefully, as it is important that you understand the experiment before starting. Talking with each other is forbidden during the experiment. If you have any questions, raise your hand and remain silent. You will be attended by the instructor as soon as possible.

### What is the experiment about?

Your task consists on throwing the 10-sided dice that you received memorizing the number that you obtain in the first throw. This number will determine your earnings as is shown in the table below.

| **Number** | **0** | **1** | **2** | **3** | **4** | **5** | **6** | **7** | **8** | **9** |
|---|---|---|---|---|---|---|---|---|---|---|
| **Amount** | 2.50 € | 2.50 € | 2.50 € | 2.50 € | 2.50 € | 2.50 € | 2.50 € | 2.50 € | 2.50 € | 2.50 € |

This means that you will earn 2.50€ regardless of the number that you report.

First, we ask you to roll the dice and memorize the number you obtain in the first throw.

Then, introduce this number in the computer screen.

You can throw the dice as many times as you want to test that it works properly. Still, your payment depends only on the number you report for the first throw.

At the end of the experiment, you will receive your earnings (in an anonymous way) in a sealed envelope.

## GAIN-NO TREATMENT

### Instructions (to be read aloud after the first experiment)

The aim of this experiment is to study decision-making. We are not interested in your particular choices but rather on the individual's average behavior. Thus, all through the experiment you will be treated anonymously. Neither the experimenters nor the people in this room will ever know your particular choices.

Next, you will receive the instructions and a 10-sided die. Instructions should be easy to follow. Please read the instructions carefully and raise your hand if you have any doubt as it is important that you understand the instructions before starting the experiment.

### Instructions (to be read privately)

The aim of this experiment is to study decision-making. We are not interested in your particular choices but rather on the individual's average behavior. Thus, all through the experiment you will be treated anonymously. Neither the experimenters nor the people in this room will ever know your particular choices. Please do not think that we expect a particular behavior from you. However, take into account that your decisions along the experiment may affect your earnings. Below, you will find details of your task in this experiment. Please follow the instructions carefully, as it is important that you understand the experiment before starting. Talking with each other is forbidden during the experiment. If you have any questions, raise your hand and remain silent. You will be attended by the instructor as soon as possible.

### What is the experiment about?

Your task consists on throwing the 10-sided dice that you received memorizing the number that you obtain in the first throw. This number will determine your earnings as is shown in the table below.

| **Number** | **0** | **1** | **2** | **3** | **4** | **5** | **6** | **7** | **8** | **9** |
|---|---|---|---|---|---|---|---|---|---|---|
| **Amount** | 0 € | 1 € | 1.50 € | 2 € | 2.50 € | 3 € | 3.50 € | 4 € | 4.50 € | 5 € |

This means that you will earn 0 € if the number you report is 0, 1 € if the number you report is 1, 1.50€ if the number you report is 2, so on, obtaining an amount of 5 € if you report a 9.

First, we ask you to roll the dice and memorize the number you obtain in the first throw.

Then, introduce this number in the computer screen.

You can throw the dice as many times as you want to test that it works properly. Still, your payment depends only on the number you report for the first throw.

At the end of the experiment, you will receive your earnings (in an anonymous way) in a sealed envelope.

## LOSS-NO TREATMENT

### Welcome (to be read aloud at the beginning of the session)

Welcome to the lab! Today, you have received an initial amount of 5 Euros for participating in an experiment that follows the one that is about to start. From now, this money belongs to you. Next, we will explain to you the instructions of the first experiment.

*(Subjects participate in the first experiment)*

### Instructions (to be read aloud after the first experiment)

The aim of this experiment is to study decision-making. We are not interested in your particular choices but rather on the individual's average behavior. Thus, all through the experiment you will be treated anonymously. Neither the experimenters nor the people in this room will ever know your particular choices.

Next, you will receive the instructions and a 10-sided die. Instructions should be easy to follow. Please read the instructions carefully and raise your hand if you have any doubt as it is important that you understand the instructions before starting the experiment.

### What is the experiment about?

Before starting the experiment you received 5€.

Your task consists on throwing the 10-sided dice that you received memorizing the number that you obtain in the first throw. This number will determine your earnings as is shown in the table below.

| **Number** | **0** | **1** | **2** | **3** | **4** | **5** | **6** | **7** | **8** | **9** |
|---|---|---|---|---|---|---|---|---|---|---|
| **Amount** | -5 € | -4 € | -3.50 € | -3 € | -2.50 € | -2 € | -1.50 € | -1 € | -0.50 € | -0 € |

This means that you will return 5 € if the number you report is 0, 4 € if the number you report is 1, 3.50€ if the number you report is 2, so on, returning an amount of 0 € if you report a 9.

First, we ask you to roll the dice and memorize the number you obtain in the first throw.

Then, introduce this number in the computer screen. We shall subtract the amount that you need to return from your initial 5 Euros.

You can throw the dice as many times as you want to test that it works properly, still your payment depends only on the number you report for the first throw.

At the end of the experiment, you will receive your earnings (in an anonymous way) in a sealed envelope.

**GAIN-MM TREATMENT**

## Instructions (to be read aloud after the first experiment)

The aim of this experiment is to study decision-making. We are not interested in your particular choices but rather on the individual's average behavior. Thus, all through the experiment you will be treated anonymously. Neither the experimenters nor the people in this room will ever know your particular choices.

Next, you will receive the instructions, <u>an envelope with 5 Euros</u> and a 10-sided die. Instructions should be easy to follow. Please read the instructions carefully and raise your hand if you have any doubt as it is important that you understand the instructions before starting the experiment.

*(The envelope was left on the table when instructions were given to participants. We underline the sentence to highlight differences with respect to other treatments.)*

## Instructions (to be read privately)

The aim of this experiment is to study decision-making. We are not interested in your particular choices but rather on the individual's average behavior. Thus, all through the experiment you will be treated anonymously. Neither the experimenters nor the people in this room will ever know your particular choices. Please do not think that we expect a particular behavior from you. However, take into account that your decisions along the experiment may affect your earnings. Below, you will find details of your task in this experiment. Please follow the instructions carefully, as it is important that you understand the experiment before starting. Talking with each other is forbidden during the experiment. If you have any questions, raise your hand and remain silent. You will be attended by the instructor as soon as possible.

## What is the experiment about?

Your task consists on throwing the 10-sided dice that you received memorizing the number that you obtain in the first throw. This number will determine your earnings as is shown in the table below.

| **Number** | **0** | **1** | **2** | **3** | **4** | **5** | **6** | **7** | **8** | **9** |
|---|---|---|---|---|---|---|---|---|---|---|
| **Amount** | 0 € | 1 € | 1.50 € | 2 € | 2.50 € | 3 € | 3.50 € | 4 € | 4.50 € | 5 € |

This means that you will earn 0 € if the number you report is 0, 1 € if the number you report is 1, 1.50€ if the number you report is 2, so on, obtaining an amount of 5 € if you report a 9.

First, we ask you to roll the dice and memorize the number you obtain in the first throw.

Then, report this number using the computer screen. There is an envelope with 5 Euros on your table. Take the money that corresponds to your throw and sealed it.

You can throw the dice as many times as you want to test that it works properly, still your payment depends only on the number you reported for the first throw.

At the end of the experiment, the instructor will pick up the sealed envelopes when you leave the room. Your earnings will be anonymous.

**LOSS-MM TREATMENT**

*(Subjects are given 5 Euros when entering the room)*

## Welcome (to be read aloud at the beginning of the session)

Welcome to the lab! Today, you have received an initial amount of 5 Euros for participating in an experiment that follows the one that is about to start. From now, this money belongs to you. Next, we will explain to you the instructions of the first experiment.

*(We observe that roughly 1/3of the subjects decided to leave the money on the table, while 2/3 of the subjects kept it in their pockets or bags. Subjects participate in the first experiment. After finishing the first experiment, we ask subjects to take their endowment and put it on the table.)*

## Instructions (to be read aloud after the first experiment)

The aim of this experiment is to study decision-making. We are not interested in your particular choices but rather on the individual's average behavior. Thus, all through the experiment you will be treated anonymously. Neither the experimenters nor the people in this room will ever know your particular choices.

Next, you will receive the instructions<u>, an empty envelope</u> and a 10-sided die. Instructions should be easy to follow. Please read the instructions carefully and raise your hand if you have any doubt as it is important that you understand the instructions before starting the experiment.

(*The envelope was left on the table when instructions were given to participants. We underline the sentence to highlight differences with respect to other treatments.)*

## What is the experiment about?

Before starting the experiment you received 5€.

Your task consists on throwing the 10 sided dice that you received memorizing the number that you obtain in the first throw. This number will determine your earnings as is shown in the table below.

| **Number** | **0** | **1** | **2** | **3** | **4** | **5** | **6** | **7** | **8** | **9** |
|---|---|---|---|---|---|---|---|---|---|---|
| **Amount** | -5 € | -4 € | -3.50 € | -3 € | -2.50 € | -2 € | -1.50 € | -1 € | -0.50 € | -0 € |

This means that you will return 5 € if the number you report is 0, 4 € if the number you report is 1, 3.50€ if the number you report is 2, so on, returning an amount of 0 € if you report a 9.

First, we ask you to roll the dice and memorize the number you obtain in the first throw.

Then, introduce this number in the computer screen. Place the amount that you need to return in the envelope and sealed it.

You can throw the dice as many times as you want to test that it works properly, still your payment depends only on number obtained on the first throw.

At the end of the experiment, the instructor will pick up the envelopes. Your earnings will be anonymous.

# Appendix B

## B1. Previous task

Recall that our experiment is preceded by a previous (unrelated) task. Our data come from two previous experiments.

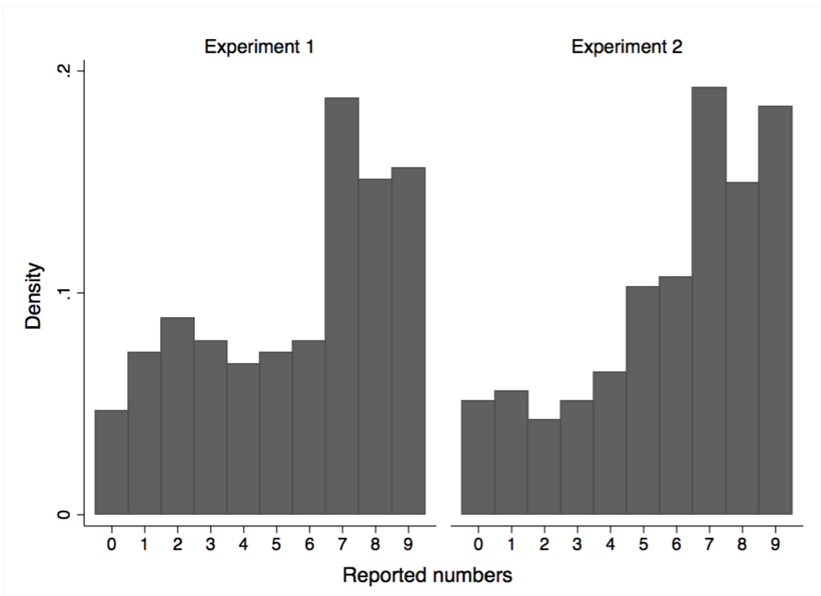
- *Experiment 1* (Rodriguez-Lara and Ponti, 2017). This experiment has a total of 3 phases. In Phases 1 and 2, the authors elicit (individual) risk and time preferences using multiple-choice lists. In Phase 3, subjects are matched in pairs and one of them (randomly selected as "Dictator") has to decide whether members of the pair receive the money immediately or in a future period. All subjects receive 5 Euros for participating in this experiment. In addition, some subjects are paid for their choices either in the risk or the intertemporal task. If subjects are paid for the later, the amount can be paid either the day of the experiment or in future (all payments are done using bank transfers). The aim of the paper is to study *social time preferences*. In particular, the authors want to investigate how subjects trade intertemporal payoffs for themselves and/or others, controlling for risk aversion.

- *Experiment 2* (Kinateder, Kiss and Pintér 2017; Kiss, Rodriguez-Lara and Rosa-Garcia 2018). In these experiments, subjects are matched in groups of three to play a bank-run game. Subjects face a situation that resembles a coordination problem in which they have to decide whether to withdraw their money from a common bank or keep it deposited. The most efficient outcome is the one in which subjects keep their money deposited in the bank but doing so is risky if the rest of depositors withdraw; i.e., subjects may be better withdrawing than waiting alone. The authors consider two different settings: a sequential setting (where decisions are made in sequence and subjects can observe the decision of others) and simultaneous (where subjects decide without knowing what others in their bank have done). In both settings, choices are elicited using the strategy method. More precisely, subjects participating in the sequential version of the game are asked to decide what to do if they are the first ones in the line, the second ones and observe that the first depositor waited/withdrew, etc… In the simultaneous setting, depositors only know their position in the line and have to decide whether or not to withdraw without conditioning this choice to the action of any other depositor. The authors investigate the determinants of withdrawing decisions and whether subjects are willing to pay to signal their decision in the simultaneous setting.

## B2. Reports and influence of the previous task

In our experiment, we have a total of 426 participants: 192 subjects participated in Experiment 1 (social time preferences) and 234 participated in Experiment 2 (bank runs). Figure B1 presents the distribution of the reported numbers from those who participated in each experiment. We observe no differences in the behavior of subjects in the die-task using the Wilcoxon rank-sum test ($Z$ = 1.270, $p$ = 0.204). The Kolmogorov-Smirnov test yields the same results ($KS$ = 0.089, $p$ = 0.328).

**Figure B1:** Distribution of reported numbers depending on previous experiment




Apart from the type of experiment, the role of subjects could also affect the reported numbers. When we look at Experiment 1, we see that those who were randomly selected as Dictators in Phase 3 of the experiment do not report different numbers than those who were randomly selected as Recipients at any common significance level ($Z$ = 0.346, $p$ = 0.730; $KS$ = 0.508, $p$ = 0.926). Similarly, participants in the sequential and the simultaneous version of the bank-run game of Experiment 2 do not behave differently with regard to the reported numbers ($Z$ = 0.148, $p$ = 0.138; $KS$ = 0.109, $p$ = 0.481). These findings, in turn, suggest that the type of experiment or the role of participants do not affect their reports in the subsequent task.

Finally, we test whether the earnings in the previous experiment affects the reports; in fact, participating in Experiment 1 earn more than those in Experiment 2 on average (€17.31 vs €7.85, $p$ < 0.001). Table B4 presents the correlation between previous earnings and the reports. This is never significant ($p$ > 0.165).

**Table B1:** Correlation between reported outcomes and previous earnings

| Treatment | Baseline | Gain-NO | Loss-NO | Gain-MM | Loss-MM |
|---|---|---|---|---|---|
| Correlation | -0.125<br>(0.245) | -0.148<br>(0.165) | -0.028<br>(0.802) | -0.068<br>(0.541) | -0.004<br>(0.971) |

**B3. Robustness checks**

In Section 3, we use a $\chi^2$test to investigate whether the reported outcomes differ from the expected ones (i.e., the equal distribution). In the first columns of Table B1, we show that our results are robust to the Wilcoxon rank-sum test ($Z$) and the Kolmogorov-Smirnov ($KS$) test of cumulative distributions, except for the Loss-MM treatment, where the one-tailed comparisons between the reports and the expected actual outcomes come close to statistical significance.[23] The third column of Table B2 reports the fraction of subjects who cheat to avoid the worst possible outcome using the estimation method in Garbarino, Slonim and Villeval (2016).

**Table B2**. Non-parametric analysis and fraction of cheaters in each treatment

| Treatment | *Z* | *KS* | % cheaters |
|---|---|---|---|
| Baseline | 1.099<br>(0.272) | 1.810<br>(0.405) | |
| Gain-NO | 4.095***<br>(0.000) | 14.056***<br>(0.000) | 62.43% |
| Loss-NO | 3.886***<br>(0.000) | 15.454***<br>(0.000) | 48.37% |
| Gain-MM | 3.296***<br>(0.001) | 12.448***<br>(0.001) | 60.12% |
| Loss-MM | 1.563*<br>(0.059) | 3.459*<br>(0.089) | 21.99% |

*Notes.* * and *** indicate significance at the $p = 0.10$ and $p = 0.01$ levels, respectively. p-values are reported in brackets

[23] We assume that the number of observations is 85 and perform one-tailed analysis, except for the Baseline treatment, where we consider a two-tailed hypothesis.

In Table B3, we undertake the approach in Ezquerra, Kolev and Rodriguez-Lara (2018) to investigate whether there is cheating in each of the five treatments. Ezquerra, Kolev and Rodriguez-Lara (2018) consider a linear regression model where the dependent variable is the standardized die outcome; i.e., they subtract the theoretical expected value of the die roll outcome (4.5), and divide by the theoretical standard deviation of the die outcome (2.872). The test statistics are computed with Eicker–White robust to arbitrary heteroskedasticity covariance matrix, which is necessary because given that the dependent variable has limited range (integers from 0 to 9). The estimated parameters in the model have the interpretation of amount of cheating that takes place in each treatment in units of standard deviation; e.g., in the Baseline treatment the observed average die outcome is 0.166 of one theoretical standard deviation above the theoretical expected die outcome.

**Table B3**. Standardized die outcome: Linear regression

| Treatment | *b* | *t-stat* | p-value |
|---|---|---|---|
| Baseline | 0.166<br>(0.106) | 1.57 | (0.117) |
| Gain-NO | 0.454***<br>(0.141) | 3.21 | (0.001) |
| Loss-NO | 0.431***<br>(0.146) | 2.94 | (0.003) |
| Gain-MM | 0.339**<br>(0.142) | 2.39 | (0.017) |
| Loss-MM | 0.0767<br>(0.157) | 0.49 | (0.625) |
| # of obs. | 426 | | |
| R-squared | 0.038 | | |

*Notes.* Dependent variable is the standardized die outcome, (Die Outcome—Theoretical Expectation of Die Outcome)/(Theoretical Standard Deviation of Die Outcome) = (Die Outcome—4.5)/2.872. We use *, ** and *** to indicate statistical significance at the $p$ = 0.10, p = 0.05 and $p$ = 0.01 levels, respectively. p-values are reported in brackets in the last column.

The results suggest that there is no significant cheating in the Baseline and the Loss-MM treatments, but cheating is significant in the Gain-No, Loss-NO and Gain-MM treatments. In line with previous evidence, there is no support for loss aversion when there is no money manipulation, as the amount of cheating in the Gain-NO and Loss-NO is not statistically different (p = 0.867). In addition, subjects cheat less in the Loss-MM than in the Gain-MM (p = 0.079).

Finally, we use the Baseline as the benchmark. Our econometric analysis in Table 3 relies on the comparison between the behavior in the Gain and the Loss treatments, with and without the manipulation of money but we have run a Baseline treatment in which subjects had no incentives to misreport the outcome. We can look at whether reported outcomes in each of the treatments differ from the ones reported in the Baseline treatment using a Tobit analysis. Our results are summarized in Table B4. In this case, the set of independent variables include dummies for our treatment conditions (the omitted category is then the Baseline treatment). The reported standard errors (in parentheses) are clustered at the session level. These results are robust to controlling for the previous experiment.

**Table B4:** Regression analysis (using the Baseline as the benchmark)

| | (1) Pooled data | (2) No Manipulation | (3) Manipulation | (4) Gains | (5) Losses |
|---|---|---|---|---|---|
| Constant | 5.079*** | 5.079*** | 5.077*** | 5.070*** | 5.087*** |
| | (0.203) | (0.209) | (0.208) | (0.203) | (0.215) |
| Gain-NO | 1.602*** | 1.602*** | | 1.578*** | |
| | (0.392) | (0.410) | | (0.396) | |
| Loss-NO | 1.541*** | 1.541*** | | | 1.563*** |
| | (0.502) | (0.524) | | | (0.522) |
| Gain-MM | 1.046*** | | 1.045*** | 1.041*** | |
| | (0.372) | | (0.376) | (0.384) | |
| Loss-MM | 0.289 | | 0.288 | | 0.294 |
| | (0.241) | | (0.241) | | (0.247) |
| Sigma | 3.326*** | 3.326*** | 3.297*** | | 3.515*** |
| | (0.146) | (0.130) | (0.212) | | (0.158) |
| Pseudo LL | -973.023 | -590.762 | -592.508 | -596.425 | -585.544 |
| # of obs. (uncensored) | 426 (332) | 261 (201) | 253 (206) | 261 (212) | 253 (195) |

Our regression (1) suggests that reports in all the treatments are statistically different from the Baseline ($p < 0.01$) except for the Loss-MM treatment ($p = 0.231$). Pairwise comparisons confirm that there is no significant difference between reports in Gain-NO and Loss-NO ($p = 0.674$), while there is a significant difference between the reports in the Gain-MM and Loss-MM treatments ($p = 0.016$). We also see that there is a significant difference between the reports in the Loss-NO and Loss-MM treatments ($p = 0.003$), while the difference between the reports in the Gain-NO and Gain-MM treatments is weakly significant ($p = 0.062$), this suggesting that the manipulation of money can also have an effect in the Gain treatments in the direction predicted by Hypothesis 4.