Cheating and money manipulation

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Abstract

We use different incentive schemes to study how subjects cheat when they are asked to reveal a piece of private information. We find no significant evidence of cheating when there is no financial incentive associated with the reports, but cheating does occur when the reports determine financial gains or losses (in different treatments). We find no evidence of increased cheating to avoid a loss in the standard case in which subjects receive their earnings at the end of the session. When subjects manipulate the possible earnings, we find evidence of less cheating in the loss setting. 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 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 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) 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., 2017) can exacerbate cheating behavior.
In this paper, we study the effects of incentives on cheating behavior in both the loss and gain 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. 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). However, Cameron and Miller (2009) and Grolleau, Kocher and Sutan (2016) find that subjects cheat more in a loss frame when reporting their own performance on a real-effort task. Cameron, Miller and Monin (2010) argue that paying people in advance for performing a task might induce a feeling of entitlement, 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 money manipulation, as this may affect the “sense of ownership” and what could be termed the moral cost of lying. This device helps us to tease apart the effects of the frame and the manipulation of money 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 (of course considerations of self-image and social image may still 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
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, since the notion of loss
aversion (Kahneman and Tversky, 1979) is so pervasive. In our standard treatments, we give
subjects their earnings in a closed envelope at the end of the session, but we 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.\(^2\) Finally, having observed our results in
these treatments, we implemented treatments in which the participants either took their earnings

\(^1\) We use a 10-sided die to increase the number of possible outcomes. Studies by, e.g., Greene and Paxton (2009), Hao
and Houser (2010), Shalvi et al. (2011), Conrads et al. (2013), Gravert (2013), Jiang (2013) and Ploner and Regner
(2013) 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

\(^2\) 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, Kajackaita and Sobel (2016) for other experiments that
vary the probability of a (the) bad outcome.
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).

How would loss aversion actually apply in a cheating environment? As mentioned above, 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. 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, 2013) 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).

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). Another salient difference between our designs is that we ask subjects to report the outcome of a 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). In addition, we complement their findings by looking at the moral costs and the effects of money manipulation on the reported outcomes.

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3 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.
We expect that moral concerns and social norms will interact with the motivation to cheat. With the mounting evidence on cheating 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 cheating and that from not cheating (e.g., Lundquist et al.; 2009, Garbarino, Slonim, and Villeval, 2016). However, it is likely that other elements should be present in the arguments of a function reflecting the moral cost of cheating. 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 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.\(^4\)

In our setting, if you are given money and hold it in your possession, you may feel that you have been trusted (the mental-cheating condition could be seen as being a strong demand effect). Keeping the money with which you have been entrusted may feel more like stealing than taking money that you’ve been invited to take.\(^5\) Trust and morality are important and people may be sensitive to small clues and considerations (Mazar, Amir and Ariely, 2008). If one feels trusted, this could mean that one believes that the trustor believes that the trustee will behave in a trustworthy manner. If one doesn’t, then one may experience guilt (Charness and Dufwenberg, 2006; Battigalli and Dufwenberg, 2007, 2009; Battigalli, Charness, and Dufwenberg, 2013). We attempt to better understand the interplay between incentives to cheat in the gain and loss domain and the moral costs of cheating.


\(^5\) This resembles the idea of omission-commission in Spranca, Minsk and Baron (1991). However, participants in our experiment are 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)
In fact, some of our experimental results will surprise many readers. 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, we find no evidence of more cheating with a loss frame than with a gain frame. We felt that two elements could have helped to induce 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 shall not observe more cheating in the loss frame. Second, the sense of ownership might have been too weak in this design. We addressed these points by affecting the moral costs of cheating and implement treatments in which the participants actually physically handle the money. Indeed, requiring the participants to engage in money manipulation led to less cheating. To our surprise, however, 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.

Thus, we find ourselves swimming upstream with our experimental results. We do interpret our results as reflecting differences in perceived trust and beliefs. We suspect that there were different moral costs and beliefs in different treatments. Specifically, people in the money-manipulation loss treatment might have been more likely to feel that they had been 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. Further, the decision to return money in the loss framing could be seen as warm-glow giving (Andreoni, 1989, 1990), especially in the money-manipulation loss treatment, where subjects had to place the amount to be returned
in an envelope. Hence, it seems unrealistic to ignore the psychological (moral) costs and benefits that are likely to be involved in deciding whether (and by how much) to cheat.

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 and conclude in Section 4.

2. Experimental Design and hypotheses

2.1. Experimental design

A total of 426 subjects were recruited to participate in our experiment. We use the procedures in Fischbacher and Follmi-Heusi (2013) and add our experiment at the end of a previous experiment that took around 90 minutes. All sessions were run at the Laboratory for Research in Experimental Economics (LINEEX) at the University of Valencia.

At the beginning of our experiment, subjects received a 10-sided die and a copy of the experimental instructions. 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.

<table>
<thead>
<tr>
<th>Reported number</th>
<th>0</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>9</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baseline</td>
<td>2.5</td>
<td>2.5</td>
<td>2.5</td>
<td>2.5</td>
<td>2.5</td>
<td>2.5</td>
<td>2.5</td>
<td>2.5</td>
<td>2.5</td>
<td>2.5</td>
</tr>
<tr>
<td>Gain treatments</td>
<td>0</td>
<td>1</td>
<td>1.5</td>
<td>2</td>
<td>2.5</td>
<td>3</td>
<td>3.5</td>
<td>4</td>
<td>4.5</td>
<td>5</td>
</tr>
<tr>
<td>Loss treatments</td>
<td>-5</td>
<td>-4</td>
<td>-3.5</td>
<td>-3</td>
<td>-2.5</td>
<td>-2</td>
<td>-1.5</td>
<td>-1</td>
<td>-0.5</td>
<td>0</td>
</tr>
</tbody>
</table>

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

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6 Subjects in our experiment did not receive any feedback about the previous one until the end of the session. Our experiment was presented as an independent task to subjects, in which they could earn some additional money (all subjects decided to participate). This procedure is frequently used in the literature due to the short nature of the task.

7 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 (see Table 1) and the extent to which subjects manipulated their potential earnings.

- **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. This 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. They could keep this endowment on their table or put it in their pockets. After finishing the first experiment (90 minutes), subjects were asked to take their initial endowment and put it on their table. 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.

Before proceeding to the hypotheses, there are some aspects of our experimental design that worth mentioning. First, earnings associated with each reported number were equivalent in the Gain and the Loss treatments. Second, we announced the initial endowment at the beginning of the session to subjects in the Loss treatments to trigger loss aversion. Finally, subjects in

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8 Roughly 1/3 of the subjects decided to leave the money on the table. The rest put it in their pockets.
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.

2.2. Hypotheses

Consider first the Baseline treatment. If people have standard preferences, we should expect reports to follow an equal distribution. We should also expect an equal distribution of reported numbers if lying has a cost. Even if one cares about social image, it is not obvious that rolling a higher number is better. Since we are not aware of any paper that directly tests for cheating behavior in the absence of economic incentives, 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 (Fischbacher and Föllmi-Heusi 2013, Shalvi et al. 2011, Abeler, Nosenzo and Raymond 2016). 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 from 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.

In line with the literature on loss aversion (Kahenman and Tversky, 1979) and a plausible link between loss aversion and choices made with gain and loss frames, we expected more cheating with loss framing, leading 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 an intrinsic motivation to be honest (e.g., Lundquist et al., 2009), social image (the choice is observed by the experimenter) (e.g., Gneezy, Kajackaite and Sobel, 2016), a desire to hold a positive self view (e.g., Mazar, Amir and Ariely, 2008), or some form of guilt aversion (e.g., Battagalli and Dufwenberg, 2007, 2009). 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. We therefore feel that entrusting people with money (on their desk or at the beginning of the session) will increase the moral cost of lying, leading to our final hypothesis:

Hypothesis 4: The numbers reported in the money-manipulation treatments will be lower than in the corresponding treatments without money manipulation.

3. Results

Figure 1 displays the distribution of the reported numbers in each of the treatments. Table 1 summarizes our data by including information on the frequency and cumulative distribution of reported numbers. 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.9

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9 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,
Figure 1: Distribution of reported numbers per treatment

Table 1: Descriptive statistics

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Obs.</th>
<th>Mean</th>
<th>SE</th>
<th>0</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>9</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baseline</td>
<td>88</td>
<td>4.977</td>
<td>0.304</td>
<td>4</td>
<td>10</td>
<td>11</td>
<td>7</td>
<td>3</td>
<td>10</td>
<td>8</td>
<td>14</td>
<td>12</td>
<td>9</td>
</tr>
<tr>
<td>Gain-NO</td>
<td>89</td>
<td>6.281</td>
<td>0.270</td>
<td>3</td>
<td>3</td>
<td>3</td>
<td>6</td>
<td>6</td>
<td>8</td>
<td>8</td>
<td>16</td>
<td>16</td>
<td>20</td>
</tr>
<tr>
<td>Loss-NO</td>
<td>84</td>
<td>6.214</td>
<td>0.291</td>
<td>4</td>
<td>4</td>
<td>4</td>
<td>3</td>
<td>2</td>
<td>8</td>
<td>12</td>
<td>15</td>
<td>12</td>
<td>20</td>
</tr>
<tr>
<td>Gain-MM</td>
<td>84</td>
<td>5.952</td>
<td>0.271</td>
<td>3</td>
<td>5</td>
<td>2</td>
<td>5</td>
<td>7</td>
<td>6</td>
<td>8</td>
<td>24</td>
<td>14</td>
<td>10</td>
</tr>
<tr>
<td>Loss-MM</td>
<td>81</td>
<td>5.198</td>
<td>0.332</td>
<td>7</td>
<td>5</td>
<td>7</td>
<td>6</td>
<td>10</td>
<td>6</td>
<td>4</td>
<td>12</td>
<td>10</td>
<td>14</td>
</tr>
</tbody>
</table>

Notes: Frequency is in whole numbers and cumulative distribution is in parentheses. SE = standard error.

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 ($\chi^2 = 14.95, p = 0.005$), 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 2. We first investigate whether the reported outcomes in each of treatment differ from the actual expected outcomes (i.e., the equal distribution) using a $\chi^2$ test. Using Wilcoxon rank-sum and Kolmogorov-Smirnov tests, we compare the reports in the Gain and Loss treatments with those in the Baseline (where subjects have no incentives to cheat).

**Table 2: Non-parametric analysis on cheating behavior**

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Expected outcomes</th>
<th>Baseline outcomes</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$\chi^2$ test</td>
<td>Wilcoxon rank-sum (Z)</td>
</tr>
<tr>
<td>Baseline</td>
<td>12.00 (0.213)</td>
<td>-</td>
</tr>
<tr>
<td>Gain-NO</td>
<td>38.98 (0.000)</td>
<td>3.128 (0.001)</td>
</tr>
<tr>
<td>Loss-NO</td>
<td>39.57 (0.000)</td>
<td>2.880 (0.002)</td>
</tr>
<tr>
<td>Gain-MM</td>
<td>45.11 (0.000)</td>
<td>2.165 (0.015)</td>
</tr>
<tr>
<td>Loss-MM</td>
<td>11.72 (0.230)</td>
<td>0.610 (0.271)</td>
</tr>
</tbody>
</table>

Note: p-values (in parentheses) reflect one-tailed tests. $^{10}$ The $\chi^2$ test compares expected to actual outcomes.

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, we see no significant evidence of distortion in the reports made when there is no financial incentive for misreporting.

$^{10}$ 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).
Result 1. *There is no 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 have 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 ($p > 0.230$).

Result 2. *Incentives affect cheating in all treatments except in the Loss-MM, where the distribution of reported numbers is very close to 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. *Incentives in the loss domain do not increase cheating behavior compared with incentives in the gain domain.*

Finally, Hypothesis 4 predicts that money manipulation will lead to less cheating due to an increased moral cost of lying. We find strong support for the hypothesis when we compare
reports made in the Loss-MM and Loss-NO treatments. 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. Here the Wilcoxon test gives $Z = 1.144, p = 0.126$, while the Kolmogorov test gives $KS = 0.119, p = 0.254$, both one-tailed tests. Thus, we see that money manipulation makes a real difference with a loss framing, but much less of a difference with a gain framing.

**Result 4.** *Money manipulation reduces cheating behavior, especially in the loss framing.*

We now proceed to the regression analysis. In Table 3, 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.\footnote{In our Tobit analysis in Appendix B (Table B2) the set of independent variables include dummies for each of the treatment conditions, which are then compared with the Baseline reports. We note that ordinary least squares regressions provide qualitatively the same results and similar levels of significance. Our findings are also robust to controlling for the earnings of the previous experiment. While the subjects were not informed about such earnings when rolling the die, one might argue that they might had formed some beliefs to be used as a reference point. Table B1 presents the correlation between previous earnings and the reports, which is not statistically significant in any of the treatments ($p > 0.165$).} The reported standard errors (in parentheses) are clustered at the session level. Our first regression in column (1) uses the data from all treatments except the Baseline. Specifications (2) to (5) give the results for different Tobit models, depending on whether or not subjects manipulate the money and the frame.

As already suggested, there is no evidence of loss aversion in our data. If there is no money-manipulation, the behavior in the Loss frame is not significantly different from the behavior in the Gain frame. Money-manipulation does reduce the reported outcomes, but the
effect is only significant in the loss frame, in fact subjects cheat more in the Gain than in the Loss treatment with money-manipulation.

<table>
<thead>
<tr>
<th>Table 3: Regression analysis</th>
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<tr>
<td></td>
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<tr>
<td>Constant</td>
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<tr>
<td></td>
</tr>
<tr>
<td>Gain</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Money-manipulation</td>
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<tr>
<td></td>
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<tr>
<td>Gain × Money-manipulation</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Sigma</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Pseudo LL</td>
</tr>
<tr>
<td># of obs. (uncensored)</td>
</tr>
</tbody>
</table>

Notes: Specification (1) includes data from all treatments except the Baseline. Specification (2) includes data from only the Gain-NO and Loss-NO treatments, while (3) includes data only from the Gain-MM and Loss-MM treatments. Specification (4) includes data only from the Gain-NO and Gain-MM treatments, (5) includes only data from the Loss-NO and Loss-MM treatments. ** and *** indicate significance at the $p = 0.05$ and $p = 0.01$ levels, respectively, two-tailed tests.

4. Discussion

Our results confirm some expected patterns. For example, people do not lie 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.

But our other results are largely surprising. We expected to find evidence of more cheating when earnings associated to the report were framed as a loss, but we found absolutely no
evidence of this 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 the loss frame than in the 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. This last result would appear to turn conventional wisdom on its head.

What could explain these unexpected results? An important consideration regarding loss aversion is the reference point for gains and losses and this may be unclear in laboratory settings (Terzi et al. 2016). While loss aversion and reference dependence are widely accepted, the generality of loss-aversion seems 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 it occurs in our standard treatments. 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. We decided to affect the moral costs of cheating by asking subjects to manipulate the money. The spikes at higher numbers other than the maximum value in the money-manipulation treatments suggest that many people who choose to lie do not wish to either 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).

To our surprise, however, our null result was not driven by a lack of a sense of “ownership” of the funds as we find evidence of reversed loss aversion in the money-

---

12 Some people 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. While there is no overall difference between the reports in the Baseline and the expected true values, it is nevertheless the case that this 60% is meaningful. The binomial test tells us the probability that 53 or more of 88 random draws in this treatment being high numbers is only 2.8%.
manipulation treatments. Harinck et al. (2007) document also this effect 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 the negative feelings associated with small losses may be outweighed by the positive feelings associated with equivalent small gains. In our experiment, these feelings are likely to be affected by the damage to one’s image and the beliefs about what is expected.

We suspect that if one feels trusted, one is more likely to respond in an honest or trustworthy manner. It could be that it is more costly for subjects to cheat when they receive the money in advance because they feel they have been trusted. 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.\(^\text{13}\) Perhaps people in the money-manipulation treatments had different beliefs about the expectations of the experimenter than people in the other variable-pay treatments. 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. By asking subjects to return the money in the loss treatment, we might also trigger impure altruism (Andreoni, 1989, 1990). Subjects can also have different attitudes towards losing the money in the loss treatments with and without the manipulation of money, as paper losses can be treated differently from those that are realized (Weber and Zuchel 2005; Imas, 2016); this can be due to mental accounting (Thaler, 1985).

\(^\text{13}\) This goes to the issue of whether one is more honest if one feels trusted. Some evidence is provided for this idea in Charness (2000). Additionally, Campbell (1935), May and Loyd (1993) and Haines et al. (1986) find that an honor system induces more honesty than does a proctor. See also Mazar, Amir and Ariely (2008) or Gino, Ayal and Ariely (2009) for the related evidence on the importance of honor codes on cheating behavior.
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) or 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, 2014). 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 looking at the effects of the moral costs, which seems to be a crucial element in understanding cheating behavior.

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 state of the world. In the Baseline treatment, there is no financial incentive for misreporting the state of the world 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

14 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. While there is no overall difference between the reports in the Baseline and the expected true values, it is nevertheless the case that this 60% is meaningful. The binomial test tells us the probability that 53 or more of 88 random draws in this treatment being high numbers is only 2.8%.
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 that loss aversion translates into this 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.

Our results represent a challenge for the more standard behavioral theories such as loss aversion and reference points. It seems that the moral cost of behavior is an element that must not be ignored. 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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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.

<table>
<thead>
<tr>
<th>Number</th>
<th>0</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>9</th>
</tr>
</thead>
<tbody>
<tr>
<td>Amount</td>
<td>2.50 €</td>
<td>2.50 €</td>
<td>2.50 €</td>
<td>2.50 €</td>
<td>2.50 €</td>
<td>2.50 €</td>
<td>2.50 €</td>
<td>2.50 €</td>
<td>2.50 €</td>
<td>2.50 €</td>
</tr>
</tbody>
</table>

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.

<table>
<thead>
<tr>
<th>Number</th>
<th>0</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>9</th>
</tr>
</thead>
<tbody>
<tr>
<td>Amount</td>
<td>0 €</td>
<td>1 €</td>
<td>1.50 €</td>
<td>2 €</td>
<td>2.50 €</td>
<td>3 €</td>
<td>3.50 €</td>
<td>4 €</td>
<td>4.50 €</td>
<td>5 €</td>
</tr>
</tbody>
</table>

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.

<table>
<thead>
<tr>
<th>Number</th>
<th>0</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>9</th>
</tr>
</thead>
<tbody>
<tr>
<td>Amount</td>
<td>-5 €</td>
<td>-4 €</td>
<td>-3.50 €</td>
<td>-3 €</td>
<td>-2.50 €</td>
<td>-2 €</td>
<td>-1.50 €</td>
<td>-1 €</td>
<td>-0.50 €</td>
<td>-0 €</td>
</tr>
</tbody>
</table>

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, an envelope with 5 Euros 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.

<table>
<thead>
<tr>
<th>Number</th>
<th>0</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>9</th>
</tr>
</thead>
<tbody>
<tr>
<td>Amount</td>
<td>0 €</td>
<td>1 €</td>
<td>1.50 €</td>
<td>2 €</td>
<td>2.50 €</td>
<td>3 €</td>
<td>3.50 €</td>
<td>4 €</td>
<td>4.50 €</td>
<td>5 €</td>
</tr>
</tbody>
</table>

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 1/3 of 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, an empty envelope 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.

<table>
<thead>
<tr>
<th>Number</th>
<th>0</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>9</th>
</tr>
</thead>
<tbody>
<tr>
<td>Amount</td>
<td>-5 €</td>
<td>-4 €</td>
<td>-3.50 €</td>
<td>-3 €</td>
<td>-2.50 €</td>
<td>-2 €</td>
<td>-1.50 €</td>
<td>-1 €</td>
<td>-0.50 €</td>
<td>-0 €</td>
</tr>
</tbody>
</table>

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.

27
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

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.$^{15}$

**Table B1.** Non-parametric analysis (Wilcoxon rank-sum and Kolmogorov-Smirnov tests), fraction of cheaters and correlation between reported outcomes and previous earnings in each treatment

<table>
<thead>
<tr>
<th>Treatment</th>
<th>$Z$</th>
<th>$KS$</th>
<th>% cheaters</th>
<th>Correlation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baseline</td>
<td>1.099</td>
<td>1.810</td>
<td>-0.125</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.272)</td>
<td>(0.405)</td>
<td>(0.245)</td>
<td></td>
</tr>
<tr>
<td>Gain-NO</td>
<td>4.095***</td>
<td>14.056***</td>
<td>62.43%</td>
<td>-0.148</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.165)</td>
<td></td>
</tr>
<tr>
<td>Loss-NO</td>
<td>3.886***</td>
<td>15.454***</td>
<td>48.37%</td>
<td>-0.028</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.802)</td>
<td></td>
</tr>
<tr>
<td>Gain-MM</td>
<td>3.296***</td>
<td>12.448***</td>
<td>60.12%</td>
<td>-0.068</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.541)</td>
<td></td>
</tr>
<tr>
<td>Loss-MM</td>
<td>1.563*</td>
<td>3.459*</td>
<td>21.99%</td>
<td>-0.004</td>
</tr>
<tr>
<td></td>
<td>(0.059)</td>
<td>(0.089)</td>
<td>(0.971)</td>
<td></td>
</tr>
</tbody>
</table>

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

The literature on cheating behavior has usually identified honest subjects as those who report the worst possible outcome (Fischbacher and Föllmi-Heusi, 2013). The third column of Table B1 reports the fraction of subjects who cheat to avoid the worst possible outcome (receiving nothing) using the estimation method in Garbarino, Slonim and Villeval (2016). The last column of Table B1 reports the correlation between the reported outcomes and the participants’ earnings in the previous experiment. While this is negative and never significant

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$^{15}$ 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 B2, we report the results of a Tobit analysis, where the set of independent variables include dummies for our treatment conditions. The reported standard errors (in parentheses) are clustered at the session level.

**Table B2: Regression analysis (using the Baseline as the benchmark)**

<table>
<thead>
<tr>
<th>(1) Pooled data</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Gain-NO</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Loss-NO</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Gain-MM</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Loss-MM</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Pseudo LL</td>
</tr>
<tr>
<td># of obs.</td>
</tr>
<tr>
<td>(uncensored)</td>
</tr>
</tbody>
</table>

In line with our previous analysis, we find that all 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 expected direction.