Does Honesty Respond to Unrelated Luck?*

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Abstract

We conducted a lab experiment to examine (1) whether luck-based income influences honesty in a subsequent, unrelated decision, (2) whether the perceived agency over an uncertain event affects the interplay between luck and honesty, and (3) whether accumulated previous luck-based incomes influence honesty. Specifically, participants self-report a dice roll outcome *after* receiving an unrelated luck-based income. Additionally, we manipulated participants' perceived control over the luck-based income. In the *exogenous luck* treatment, computerized coin tosses determine the luck-based income. In the *endogenous luck* treatment, computerized coin tosses also determine the luck-based income, but the participants choose the coin's winning side beforehand. Our main findings are as follows: lying behavior increases when contemporaneous luck-based income is high, remains unaffected by perceived agency, and does not correlate with prior luck-based income. Furthermore, we find evidence suggesting that individual-specific heterogeneity may significantly influence dishonesty, contrasting with the common view that context is the primary driver.

JEL Classification: C91, D03, D82
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1 Introduction

People often have income components that are partially or entirely determined by luck, and they perceive their luck in different ways. How does luck-based income affect *unrelated*, self-serving dishonest actions? In this paper, we investigate whether and to what extent luck-based income affects the moral value of honesty in a separate, unrelated individual decision where self-serving dishonest actions are possible. More specifically, we examine whether (1) low income resulting from an unlucky outcome, (2) perceived agency in the luck-based income, and (3) prior low incomes resulting from unlucky outcomes, lead individuals to lie more in a separate, unrelated decision. To the best of our knowledge, our paper is the first study in experimental economics to assess the effect of unrelated luck on lying behavior.

A vast body of literature shows that people have preferences for honesty. Even in situations where unverifiable lying could secure additional benefits, individuals do not fully exploit this option (Abeler et al., 2019; Gerlach et al., 2019). For example, in several experiments where an individual privately rolls dice and gets paid more when the self-report of dice roll outcome is higher, the average report is significantly larger than 3.5 (complete honesty) yet significantly smaller than 6 (complete lying). Arguably, a possible explanation for this preference for honesty is the desire to protect one's self-image and avoid appearing dishonest to oneself or others (Gneezy et al., 2018). Specifically, the so-called justification strategies enable individuals to lie while preserving a positive self-image. As these strategies are context-specific, lying behavior can vary considerably across different situations (Shalvi et al., 2015; Markowitz and Levine, 2021). Previous studies show that lying behavior is affected by the opportunity to perform multiple dice rolls (Shalvi et al., 2011), loss framing (Grolleau et al., 2016; Schindler and Pfattheicher, 2017; Garbarino et al., 2019; Charness et al., 2019), time pressure (Capraro et al., 2019), and stake size (Kajackaite and Gneezy, 2017; Gerlach et al., 2019).

A primary focus of the literature on lying behavior has been to examine the factors driving the moral justification for dishonesty. In other words, many previous studies have focused on identifying contexts in which individuals are more likely to engage in self-serving dishonesty (Bereby-Meyer and Shalvi, 2015). These studies typically involve asking participants to observe a random outcome (which the experimenter does not observe) and then self-report it. Because the experimenter cannot verify the accuracy of the self-reports, participants can lie undetected about an unfavorable random outcome to secure a higher payoff.

There are two typical narratives on moral justification, which are based on an *income effect* and a feeling of deservingness, respectively. The income effect means that individuals engage in self-serving dishonest actions as a compensatory mechanism in response to privately observed, randomly generated outcomes that imply low income. Furthermore, individuals may justify dishonesty if they feel deserving of a good outcome. We examine two factors that may heighten a sense of deservingness in situations where outcomes are entirely luck-dependent. First, an individual may feel entitled to greater compensation and therefore engage in more profit-seeking behavior as a reward for their agency (Cappelen et al., 2013; Mollerstrom et al., 2015; de Oliveira et al., 2017; Fries and Parra, 2021), which we call the *entitlement effect*. Second, an individual may choose dishonest actions when "Nature has been unfair", i.e., when they feel accumulated unlucky outcomes render them (psychologically) entitled to pursue self-serving behavior (Houser et al., 2012; Gill et al., 2013), which we call the *accumulation effect*. Overall, these narratives of the income, entitlement, and accumulation effects regard people's dishonest behavior as optimal responses to realized random outcomes, aimed at fulfilling their psychological objectives.

To answer our research question, grounded in the narratives on moral justification, we conducted a laboratory experiment. Specifically, we implemented two treatments in which participants self-report a dice roll outcome—associated with an effort-based income—*after* receiving an exogenous income. In the *Exogenous Luck* treatment, the computer determines a luck-based exogenous income by randomly tossing a coin. In the *Endogenous Luck* treatment, the exogenous income is also luck-based; however, participants first choose the winning side of the coin before the computer tosses it. In each treatment, the coin toss and the subsequent, unrelated self-reporting of a dice outcome are repeated 15 times. A laboratory experiment is particularly suitable for addressing our research question, as it enables us to control various factors that might influence moral behavior, particularly by (1) creating a *de facto* exogenous, luck-based income and (2) varying participants' perceptions regarding their influence over determining the exogenous, luck-based income.

Conceptually, the moral justification narratives imply that contexts can enhance a decision-maker's "moral capacity" to lie, i.e., increase the psychological threshold for lying. Our null hypotheses posit that moral capacity is a function of the following factors: (1) luck-based incomes, (2) perceptions of agency regarding luck-based incomes, and (3) the accumulation of past luck-based outcomes.

Alternatively, one might argue that dishonest behavior is primarily driven by individual-fixed effects, implying moral capacity varies across individuals and is relatively unaffected by external factors. Some previous studies are in line with this view, demonstrating individual heterogeneities in moral capacity and truthfulness preferences (Gibson et al., 2013; Heck et al., 2018). For instance, research has shown that dishonest behavior across different dimensions is positively correlated, indicating that some individuals are more predisposed to lying (Geraldes et al., 2021). Similarly, Barron (2019) finds that a considerable share of participants make high reports on both dimensions in an experiment involving high- and low-stake dimensions. Importantly, a meta-analysis found limited evidence for the influence of situational factors on lying (Gerlach et al., 2019).[1] Despite these findings, most of the literature emphasizes contextual factors as the primary determinants of moral capacity. Our experiment can also shed light on this debate: if individual-specific heterogeneity significantly influences lying behavior, we should observe dishonest behavior even if our null hypotheses are rejected.

Our paper provides the first evidence of the impact of luck on an unrelated decision in which a lying opportunity exists. First, we find that individuals tend to lie more in the dice roll in the rounds when they have experienced good luck in the coin flip, contradicting the income effect. Second, we find that perceived agency over uncertain events does not significantly affect lying behavior. Specifically, dice reports are indistinguishable between treatments, contradicting the entitlement effect. Third, we show no significant link between prior luck-based incomes and current dice roll reports, rejecting the accumulation effect. Fourth, we identify a substantial prevalence of lying overall, and a detailed exploratory

[1] Another interesting example is Urban et al. (2019), who find no effect of green consumption on subsequent honest behavior, which contradicts the findings of Mazar and Zhong (2010), who suggest that green consumption triggers cross-domain moral licensing.

analysis uncovers clear individual-level differences, which collectively challenge the dominant assumption that situational factors are the primary determinants of lying behavior.

The closest previous work to our paper is the studies comparing luck-dependent lying tasks to performance-based lying tasks (Kajackaite, 2018; Gerlach et al., 2019). While these studies are an important first step in understanding the connection between luck and lying behavior, the exogenous effect of luck on dishonesty remains an open question. We address this question by analyzing whether being lucky (or unlucky) with an exogenous luck-based income affects honesty in the determination of another, unrelated source of income.

The remainder of this paper is organized as follows. In Section 2, we outline the experimental design, state the hypotheses, and describe the procedures. In Section 3, we report the results of the experiment. In Section 4, we discuss the results and conclude.

2 Experimental Design

We conducted a laboratory experiment to observe how individual lying about a dice roll is affected by previously being lucky or unlucky in an unrelated source of income. In the experiment, a participant can receive two sources of income: an effort-based income and a luck-based income. These two sources of income are received separately and are unrelated.

Regarding the effort-based income, participants in all treatments first performed a real-effort task. We employ the counting-zeros task, in which participants are presented with a 10×15 matrix of ones and zeros and have to count the number of zeros (Abeler et al., 2011). Participants had four minutes to solve as many matrices as possible. In each matrix, the cell displays zero with a probability between 30% and 40%. Answers in the range of plus and minus one for the exact number of zeros were also recorded as correct. This performance measure will serve as the basis for determining participants' earnings related to a subsequent dice roll report, of which they are not aware at this stage. That is, before performing the real-effort task, the participants were merely informed that the more matrices they solved correctly, the higher their earnings would be. We designed this effort-based income so that we could incorporate a self-serving lying opportunity in a more natural setting, as opposed to merely associating the dice roll report with an offered lump sum.

2.1 Treatments

In the *exogenous luck* (EXO) treatment, the real-effort task was followed by 15 rounds of self-reports.[2] Two events occurred during each round. First, the computer reported the outcome of a virtual coin toss for each participant, which determined the participant's luck-based income. If the coin landed on *heads*, the participant received an income of 80 points. If the coin landed on *tails*, the participant received an income of 20 points. Thus, *heads* and *tails* present the lucky and unlucky outcomes, respectively. Second, *after* observing the coin toss, the participant privately rolled a dice and submitted a dice roll report. The dice roll report determines the piece rate for performance in the counting-zeros tasks, which in turn determines the effort-based income. The piece rate was set as: [dice report + 3]; for example,

[2] At the end of the experiment, one of the 15 rounds was randomly chosen for actual payment (Azrieli et al., 2018).

if a participant solved 10 matrices and reports 6, she earned 90 ($= 10 * [6 + 3]$) points. We chose parameters so that: (1) the expected income associated with each of the two income sources is similar, and (2) the lowest expected total income is guaranteed to be above €5, in line with a standard show-up payment.[3] While the coin toss was entirely exogenous and determined by the computer, the actual dice roll was only observed by the participants. This means that the actual piece rate to determine the effort-based income depended exclusively on self-reports.

The *endogenous luck* (ENDO) treatment closely resembled the EXO treatment in all aspects except for actively involving participants in the outcome determination of the luck-based income. Specifically, before the computer determined the outcome of the coin toss, participants actively chose the winning side of the coin. Thus, the participant decides whether the *head* or *tail* is associated with a high payoff of 80 points. Such a choice is nominal and does not affect the chance of receiving a high income from a coin toss. Figure 1 summarizes the experimental design. The complete experimental instructions are available in the Supplementary Material.

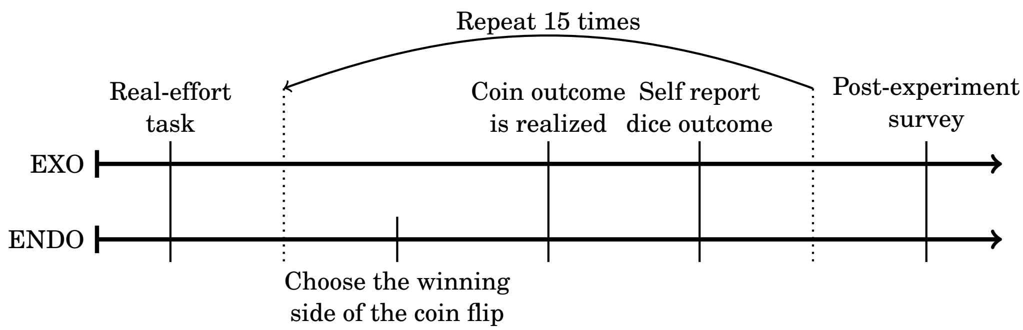


Figure 1 Experimental Design

We also preregistered a control treatment, where the dice roll procedure was identical to the other two treatments, but the additional income was fixed at 20 points in all rounds, i.e, it was not determined by a coin toss. As our primary testable hypotheses are not related to the observations from it, we omit the report about the control treatment for the sake of brevity (the data is available in the data repository (Kim, 2023)).

2.2 Hypotheses

We formulate hypotheses about how unrelated luck affects dishonesty through the lens of moral justifications. Specifically, our experiment enables a direct test of whether the income, entitlement, and accumulation effects change the decision-maker's "moral capacity." We postulate that moral capacity is a constraint for self-serving dishonest behavior, analogous to buying capacity being a budget constraint for utility-maximizing consumer behavior. Accordingly, the narratives regarding the income, accumulation, and entitlement effects can be

[3]In a previous study using a similar method, the average counting-zeros task performance was 7.7 (Vranceanu et al., 2015). Based on that, we expected the income associated with the dice to be 50.05 ($= 7.7 * [3.5 + 3]$) if everyone reports the dice roll honestly, which is aligned with the expected income associated with the coin toss (50 ($= (20 + 80)/2$)).

translated into the assumption that individuals' moral capacity is an increasing function of these effects. Although we establish our null hypotheses based on these narratives in terms of the observables within our experiment, it is important to note that these hypotheses are not normative predictions. This implies that, even if individual heterogeneities in moral capacity are unaffected by contextual changes, we may still reject these null hypotheses if significant deviations from honest behavior are observed.

For expositional simplicity, in what follows, we call losing rounds (winning rounds) the rounds where the coin lands on tails (heads) in the EXO treatment and on the chosen losing (winning) side in the ENDO treatment.

Hypothesis 1. *The reported dice roll is higher in losing rounds.*

Hypothesis 1 posits that when luck-based income is low, the decision-maker's moral capacity for lying increases. In contrast to the income from the coin toss, the participants can leverage their effort-based income by reporting a dice roll outcome higher than the actual outcome. If higher dice rolls are reported in the losing rounds of coin tosses, this hypothesis will be supported. If we reject this hypothesis but still observe overall lying behavior, it implies that the income effect does not influence moral capacity.

Hypothesis 2. *The reported dice roll in the winning rounds is higher in the ENDO treatment than in the EXO treatment.*

Hypothesis 2 posits that participants in the ENDO treatment, who believe that the luck-based outcomes are partially due to their choices rather than solely to luck, would lie more than those in the EXO treatment, who perceive the desirable outcomes as purely exogenous luck. In line with Cappelen et al. (2020), who show that agency increases participants' perceived control over uncertain outcomes, we hypothesize that participiants' perceived control over the luck-based outcome increases their moral capacity. Consequently, they lie more even when the income effect is muted.

Hypothesis 3. *The reported dice roll in round t increases with τ, where $\tau \leq t$ is the number of previous losing rounds.*

Only one out of 15 rounds is randomly chosen for actual payment. Thus, strictly speaking, previous unlucky outcomes do not affect the income level in a given round. Nevertheless, a higher accumulation of unlucky outcomes might lead participants to feel unfairly treated by Nature, resulting in increased profit-seeking lying behavior. Accordingly, we hypothesize that psychological or emotional responses may affect moral capacity.

2.3 Procedures

The experimental sessions were conducted with participants recruited from the Mannheim Laboratory for Experimental Economics (mLab) of the University of Mannheim. Instructions were provided in English. We invited participants to join an online meeting to receive instructions from the experimenter, where they received the unique link for participating in the experiment. As we could not provide participants with physical dice in an online setting, we implemented the following procedure: Participants were asked to use either a physical dice (if they had one readily available at home) or an online dice generator. Regarding the latter option, we provided a link to a dice generator and informed participants that they could use that link—or any other website they preferred—to generate the dice outcomes. Importantly, we highlighted that in either case, we (experimenters) could not observe actual dice outcomes. Providing participants with the choice between a physical or online dice yielded credibility to this claim as participants knew for certain that a physical dice could not be observed. At the same time, providing a choice ensured the feasibility of the experiment.

In each treatment, we conducted four sessions consisting of 11–15 participants. A total of 106 participants (54 in EXO and 52 in ENDO) participated in one of the eight sessions.[4] The experiment was programmed using the experimental software oTree (Chen et al., 2016). Before the participants joined the online meeting, they were asked to remove their profile photos. After they joined the online meeting, but while still in the virtual waiting room, the experimenter asked participants to turn off the webcam and rename their displayed names to two letters they arbitrarily chose to ensure that their identities—and hence decisions—remained anonymous. Subsequently, participants were required to read the instructions displayed on their screens carefully and to pass a comprehension quiz.

In both treatments, the participants filled out a post-experiment survey about their basic demographic characteristics, whether they used a physical or online dice, and risk preferences. Finally, participants were also asked two additional questions: (1) they were asked to recall how often the coin tosses were successful (i.e., yielded an income of 80 points). This elicitation was incentivized as follows: if their answer was in a range of plus and minus one of the correct number, a participant received an additional payoff of 5 points, (2) participants rated on a 7-point scale how lucky they felt about the coin toss.

The average payment per participant was €8.51. Payments were made via online transfers (either PayPal or bank transfer) after receiving the personal payment code generated at the end of the experiment. Each session lasted for less than 40 minutes.

3 Results

Before testing our hypotheses about the effect of unrelated luck on lying, we (1) assess whether unintended session-specific effects exist between the treatments, and (2) examine the overall extent of lying about dice reports in the two treatments.

The participants completed the real-effort task without knowing about the experiment's second phase. Thus, since we randomly assigned participants to treatments, the performance should not differ between treatments. On average, the participants solve 5.684 (SD 1.792) matrices. As expected, the performance does not differ significantly between the two treatments (Mann-Whitney U test (MW), $p = 0.984$). Moreover, the probability of winning in the EXO and ENDO treatments should be the same, regardless of whether the participants in the ENDO treatment can choose the winning side of the coin. Our data corroborate the latter point: the number of winning rounds is not statistically different between the two treatments (MW, $p = 0.480$).

[4] We preregistered the experiment at AsPredicted.org and aimed to collect a sample of 50 participants per treatment, based on a power analysis under the assumption of 0.4 standardized effect, a significance level of 5%, and a power of 80%.

To examine the overall extent of lying about dice reports, we consider the individual average dice report across the 15 rounds to account for individual effects. In the EXO and ENDO treatments, participants' average dice reports are 4.530 (SD 0.840) and 4.415 (SD 0.858), respectively. In line with previous studies using a self-report dice task, reports in both treatments are significantly higher than 3.5, which would be the expected average report under truth-telling (t-test (TT), p<0.001 for the two tests). In other words, participants are lying in each treatment. Moreover, there is no significant difference between the two treatments (MW, p=0.4224).

3.1 Hypotheses Testing

Table 1 Dice Reports in Winning and Losing Rounds

	Winning mean (sd)	Losing mean (sd)	Difference in means
EXO	4.735 (0.977)	4.336 (0.927)	0.399^{**}
ENDO	4.484 (1.110)	4.343 (0.885)	0.141
Pooled	4.612 (1.047)	4.339 (0.902)	0.273^{**}

** indicates statistical significance at the 1% level.

To test Hypothesis 1, we assess whether being lucky or unlucky on the coin outcome affects dishonest behavior in the dice report. Table 1 shows the average dice report broken down into lucky rounds (i.e., when participants earn 80 points in the coin toss) and unlucky rounds (i.e., when participants earn 20 points in the coin toss). In the EXO treatment, the average dice report in winning rounds is significantly higher than that in losing rounds (Wilcoxon signed-rank test (WT), $p = 0.003$). In the ENDO treatment, the average report in winning rounds is also higher than that in losing rounds, but the difference is not statistically significant (WT, $p = 0.178$). Moreover, pooling the two treatments, the average dice report in winning rounds (4.612) is significantly higher than that in losing rounds (4.339) (WT, $p = 0.002$).

These findings show an overall tendency of participants to make higher reports in winning rounds, which is more pronounced when participants have no agency at all in an uncertain event.[5] Thus, our results reject Hypothesis 1.

Result 1. *The dice roll reports are* ***lower*** *in losing rounds.*

This observation is interesting because our null hypothesis on the income effect seems innocuous. It is important to emphasize that this finding is not driven by insufficient statistical power. Note that we report a statistically significant *opposite* effect to the null hypothesis in EXO treatment, and a weak but opposite effect in ENDO treatment, which means that if we were to increase the sample sizes, such an opposite effect could have been statistically significant in ENDO treatment as well. Importantly, Result 1 is robust when considering the

[5]These findings also align with the OLS regression results on the effect of winning on dice reports, reported in Appendix Table A1.

first-round observations only and when controlling for other available variables and clustering the standard errors of the estimated coefficients at the individual level.

We reject Hypothesis 2 as well. The difference between treatments in the winning rounds is insignificant (4.735 vs. 4.484, MW, $p = 0.232$). In absolute terms, lying in winning rounds is higher in EXO treatment, contradicting Hypothesis 2. We also rule out an ineffective treatment manipulation (i.e., participants' perception of luck not being affected when they choose the winning side of the coin) as a reason for the lack of support for Hypothesis 2. As described in Section 2.3, we elicited participants' subjective perceptions of winning rounds at the end of the experiment. Table 2 presents the number of winning rounds reported by the participants versus the actual number of winning rounds. Although perceived wins in the EXO treatment are not statistically different from the actual number of wins (7.519 vs. 7.370, WT, $p = 0.874$), the perceived number of wins in the ENDO treatment is significantly higher than the actual number of wins (8.096 vs. 7.558, WT, $p = 0.008$). Since this belief elicitation was monetarily incentivized to reward accurate estimates, participants' increased optimism in the ENDO treatment indicates that the treatment manipulation was effective.

Furthermore, the belief distribution of the number of wins shows an insignificant difference between treatments (7.519 vs. 8.096, MW, $p = 0.150$), implying that the significant difference in the perception of winning rounds in the ENDO treatment is mainly driven by each participant's small optimistic deviation from the actual number of wins.

Result 2. *Dice roll reports in the winning rounds are* ***not*** *higher in the ENDO treatment than in the EXO treatment.*

Table 2 Winning Rounds and Perceived Winning Rounds

	Perceived mean (sd)	Actual mean (sd)	Difference in means
EXO	7.519 (1.840)	7.370 (1.926)	0.149
ENDO	8.096 (1.973)	7.558 (1.754)	0.538^{**}

** indicates statistical significance at the 1% level.

To test our third Hypothesis, we assess whether accumulated lucky events affect dishonest behavior. Table 3 summarizes the distributed lag models of the dice reports on the current and previous coin toss outcomes. To examine the effect of the coin toss outcome history on the current dice roll report, we consider distributed lag models of the dice reports on the coin toss outcomes up to three lags. The models are well specified because (1) the previous dice reports do not Granger-cause the random coin toss outcomes, and (2) the correlation between the current and previous coin outcomes is near zero, free from the near-multicollinearity issue. If Hypothesis 3 is true, then the coefficients of the lagged variables necessarily have to be negative and significant. However, as Table 3 shows, all the lagged variable coefficients are insignificant, and the signs are inconsistent. Coefficients of longer lags are statistically insignificant as well. This implies that accumulated unlucky events do not lead to more lying. Therefore, we reject Hypothesis 3.

Result 3. *The dice roll reports in round t do* ***not*** *increase with the number of losing rounds in $\tau \in \{t-3, t-2, t-1\}$.*

Table 3 Distributed Lag Models

Dependent Var.:	*Dice report*					
	EXO			ENDO		
	(1)	(2)	(3)	(4)	(5)	(6)
win	0.4940***	0.4977***	0.4833**	0.1125	0.1042	0.0735
	(0.1265)	(0.1338)	(0.1466)	(0.1575)	(0.1592)	(0.1660)
Lwin	−0.0115	0.2795	−0.0171	−0.0570	−0.0502	−0.0359
	(0.1079)	(0.1139)	(0.1158)	(0.1247)	(0.1318)	(0.1280)
L^2win		−0.0691	−0.0988		0.1739	0.1361
		(0.0886)	(0.0921)		(0.1228)	(0.1210)
L^3win			0.1071			−0.1487
			(0.1132)			(0.1418)
R^2	0.0243	0.0253	0.0251	0.0015	0.0042	0.0047
N	756	702	648	728	676	624

OLS regression of reported dice roll in current round. The binary variable 'win' is the result of the coin toss in the current round. L is the lag operator. The standard errors clustered at individual level are in parenthesis. *, **, and *** indicate statistical significance at the 5% level, 1% level, and 0.1% level, respectively.

3.2 Exploratory Analysis

Thus far, we have shown that the potential drivers typically associated with moral justification—income, entitlement, and accumulation—have an opposite (or insignificant) impact on dishonest behavior than we hypothesized. These findings are not in line with previous work showing that changes in context affect lying behavior. Interestingly, though, an unexpected finding that we uncover—and report in this section—suggests that individual-specific heterogeneity facilitates more lying in our setting than the context.

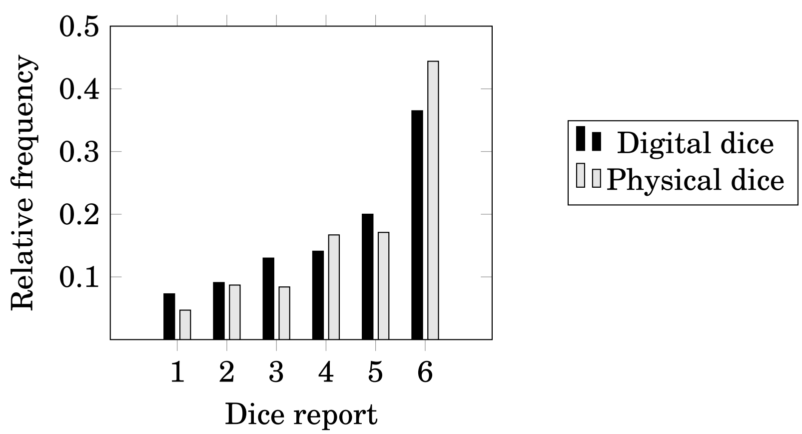
In our experiment, participants could choose to use either a physical dice (if they had one) or an online dice generator. At the end of the experiment, we asked participants to indicate the type of dice they used because we wanted to control for a (possible) difference in the type of randomization device used. Overall, 28.5% of participants indicate using a physical dice, and this proportion is highest in the EXO treatment (33.3%). However, the difference between treatments in the proportion of participants who used a physical dice is insignificant (χ^2 test, $p = 0.503$). Intriguingly, when considering the entire sample, we find that the average dice report (4.735) from the participants who indicated having used a physical dice is significantly higher than the average dice report (4.392) from the participants who indicated having used an online dice generator (MW, $p = 0.042$). Given this unexpected relationship between the (indicated) randomization device used and lying behavior, we perform the following robustness check: We rerun the regression analysis presented in Table 3 with including the randomization device as a control variable. The results reported in Table 3 are robust to this inclusion (see Robustness of Results section in the Supplementary Material).

We consider two possible reasons for the uncovered association between the self-reported use of a physical dice and increased lying in the dice roll reports. First, participants using a physical dice may feel more certain that the experimenter cannot observe their roll than those using an online dice generator. With an online dice generator, participants might believe that the outcome is somehow being tracked, although we explicitly informed them that they could use any dice generator. Second, some participants may have lied about having used a physical dice, a behavior that correlates with their dishonesty in the dice self-reporting task.

We find the second explanation more convincing based on two observations. Firstly, participants who indicated having used a physical dice took significantly less time to complete the experiment. This observation is peculiar, as we would expect some delays for participants to gather the dice. That is, we consider it implausible to assume that a physical dice was readily accessible to those claiming to have used it.

Secondly, the difference in the distribution of dice reports between digital and physical dice users supports the second explanation as well. In Figure 2, we pool all dice reports and break down the data between participants using a physical dice and those using an online dice. The largest difference is found for the share of 6s reported, which is significantly larger for participants who indicated using a physical dice (two-sample proportion test, $p = 0.002$). In other words, physical dice users misreported more frequently overall and specifically reported a higher share of 6s. This behavior accommodates an explanation that these participants did not actually roll a dice but simply reported the most beneficial dice outcome.

Figure 2 Distribution of the Dice Reports by Randomization Device



Additionally, to gain more insight into our exploratory finding, we tested our conjecture that it seems unlikely that such a large proportion of participants in the experiment had a physical dice readily accessible. To this end, some time after the experiment, we conducted an online survey with participants from the same subject pool. Specifically, we asked other researchers conducting online experiments with participants from the mLab subject pool to survey, at the end of their experiments, how easily the participants could access a physical dice. We find that only about 6.7% of the responders (6 out of 90) answered that they possessed a dice within their reach. This 6.7% fraction is significantly lower than the 28.5% fraction of participants who claimed to have used a physical dice in our experiment (one-sample proportion test, $p<0.001$).

In this survey, we further learn that an additional 30% of respondents answered that they could find a dice in 30 seconds. Accordingly, if participants in our lab experiment spent time finding a physical dice, we should have observed a significant difference in the average time spent reading the instructions or at any point before they were asked to report the dice roll outcome. In stark contrast, we find that the time spent between learning the requirement of a dice and submitting the first report is, on average, 25 seconds longer for the participants who indicated having used an online dice generator. Moreover, we find that participants who indicated having used a physical dice completed the 15 rounds significantly faster than those who used an online dice generator (MW, $p < 0.001$).

Taking all these findings into account, our interpretation is that some participants in the experiment might not have used any randomization device and simply reported favorable dice outcomes. Moreover, these individuals may have felt more comfortable falsely indicating the use of a physical dice (which we asked only at the end of the experiment) precisely because they had already been dishonest, i.e., they have a higher moral capacity to lie.

4 Discussion

We investigated the connection between experiencing luck and engaging in dishonest behavior in a subsequent, unrelated decision. Specifically, we examined (1) whether contemporaneous luck influences dishonesty, (2) whether perceived agency over luck affects the relationship between luck and honesty, and (3) whether past luck influences honesty. As far as we are aware, this study is the first in experimental economics to investigate the impact of luck on unrelated individual lying decisions.

In light of the income, entitlement, and accumulation effects, we hypothesized that individuals would lie more reporting a dice roll under the following contexts: (1) experiencing bad luck in a contemporaneous unrelated luck-based income, (2) believing that their choices influence a contemporaneous unrelated luck-based income, and (3) experiencing bad luck in multiple previous unrelated luck-based incomes. These hypotheses are based on the assumption that such effects increase the decision-maker's moral capacity to lie. We reject all our hypotheses. Overall, our findings suggest that context (in the sense of treatment manipulations) does not seem to affect lying behavior.

Yet we observe a significant level of lying overall in self-reported dice roll outcomes. A careful exploratory investigation reveals clear differences at the individual level when one takes into account whether a participant indicated having used a physical dice or an online dice. Specifically, dice reports from participants who claim having used a physical dice are significantly higher than those from participants who claim having used an online dice. Accordingly, one can argue that these findings cast doubt on the paramount influence typically attributed to situations that compel honest individuals to lie. Colloquially speaking, our findings suggest that *liars will lie*.

In practice, both context and individual preferences for lying are elements that influence honest behavior. However, particularly for policymakers aiming to minimize the social costs of dishonesty, understanding which element is more strongly associated with dishonest behavior in a specific context is of utmost importance. Our results suggest that policymakers should be cautious in considering institutional changes to tackle dishonesty as a panacea. In light of our findings, directing resources exclusively toward establishing institutional changes might not be the best solution in some contexts. That is, considering policies to identify individuals prone to lie may be essential in designing effective policies to prevent dishonest behavior. Further research is necessary to understand how such liar-prone individuals can be identified and how institutional and individual-based policies can complement each other.

Data Availability

The datasets generated during the current study are available in the Open Science Framework repository, https://osf.io/8ymq3.

Competing Interests

The authors declare no competing interests.

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Supplementary Material
Does Honesty Respond to Unrelated Luck?

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Appendix A: Robustness of Results

Table A1. Effect of Winning on Dice Reports

	EXO		ENDO	
Dice	(1)	(2)	(3)	(4)
win	0.457***	0.462***	0.128	0.125
	(0.121)	(0.117)	(0.144)	(0.142)
physical_dice	0.611**	0.671***	−0.226	0.007
	(0.249)	(0.247)	(0.255)	(0.312)
female		−0.339		0.461
		(0.211)		(0.235)
economics_background		0.384		0.103
		(0.214)		(0.247)
risk_preference		−0.070		0.097
		(0.067)		(0.072)
performance		0.027		0.006
		(0.060)		(0.059)
Constant	4.101***	4.229***	4.403***	3.571***
	(0.122)	(0.382)	(0.147)	(0.604)
N	810	810	780	780
R^2	0.055	0.081	0.005	0.023

OLS regression of reported dice roll in the current round. The binary variable 'win' is the result of the coin toss in the current round. The standard errors clustered at individual level are in parenthesis. *, **, and *** indicate statistical significance at the 5% level, 1% level, and 0.1% level, respectively.

Table A2. Treatment Differences in Dice Reports in Winning Rounds

Dice	(1)	(2)
endo	−0.262	−0.357
	(0.200)	(0.209)
physical_dice	0.208	0.207
	(0.222)	(0.217)
female		0.195
		(0.183)
economics_background		0.268
		(0.207)
risk_preference		0.029
		(0.058)
performance		−0.051
		(0.059)
Constant	4.697^{***}	4.711^{***}
	(0.146)	(0.436)
N	791	791
R^2	0.012	0.025

OLS regression of reported dice roll in the current round but only considering winning rounds. The binary variable 'endo' is the endogenous treatment. The standard errors clustered at individual level are in parenthesis. *, **, and *** indicate statistical significance at the 5% level, 1% level, and 0.1% level, respectively.

Table A3. Distributed Lag Models

	EXO				ENDO			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
winning	0.487***	0.498***	0.476***	0.492***	0.107	0.105	0.072	0.082
	(0.125)	(0.131)	(0.141)	(0.136)	(0.158)	(0.160)	(0.166)	(0.159)
L.winning	−0.030	0.014	−0.023	−0.008	−0.060	−0.053	−0.032	−0.018
	(0.112)	(0.116)	(0.117)	(0.120)	(0.124)	(0.132)	(0.127)	(0.126)
L2.winning		−0.089	−0.112	-0.094		0.169	0.132	0.139
		(0.092)	(0.091)	(0.091)		(0.122)	(0.120)	(0.115)
L3.winning			0.080	0.093			−0.153	−0.155
			(0.111)	(0.113)			(0.141)	(0.144)
physical_dice	0.602*	0.609*	0.596*	0.657*	−0.261	−0.294	−0.270	0.001
	(0.257)	(0.262)	(0.267)	(0.263)	(0.265)	(0.274)	(0.271)	(0.317)
female				−0.281				0.534*
				(0.228)				(0.233)
econ_background				0.424				0.141
				(0.241)				(0.270)
risk_preference				−0.106				0.114
				(0.0690)				(0.0776)
performance				0.0561				−0.001
				(0.0696)				(0.0618)
Constant	4.112***	4.097***	4.136***	4.171***	4.446***	4.376***	4.466***	3.508***
	(0.126)	(0.155)	(0.189)	(0.459)	(0.180)	(0.208)	(0.207)	(0.659)
N	756	702	648	648	728	676	624	624
R^2	0.056	0.058	0.057	0.088	0.006	0.010	0.009	0.033

OLS regression of reported dice roll in the current round. The binary variable ‘win’ is the result of the coin toss in the current round. The standard errors clustered at individual level are in parenthesis. *, **, and *** indicate statistical significance at the 5% level, 1% level, and 0.1% level, respectively.

Appendix B: Experimental Instructions

[*Notes: Phrases in double curly brackets are for the ENDO treatment only. Corresponding phrases for the EXO and CON treatments are followed in gray. Wordings used in examples and comprehension check questions are modified accordingly.]

Welcome.

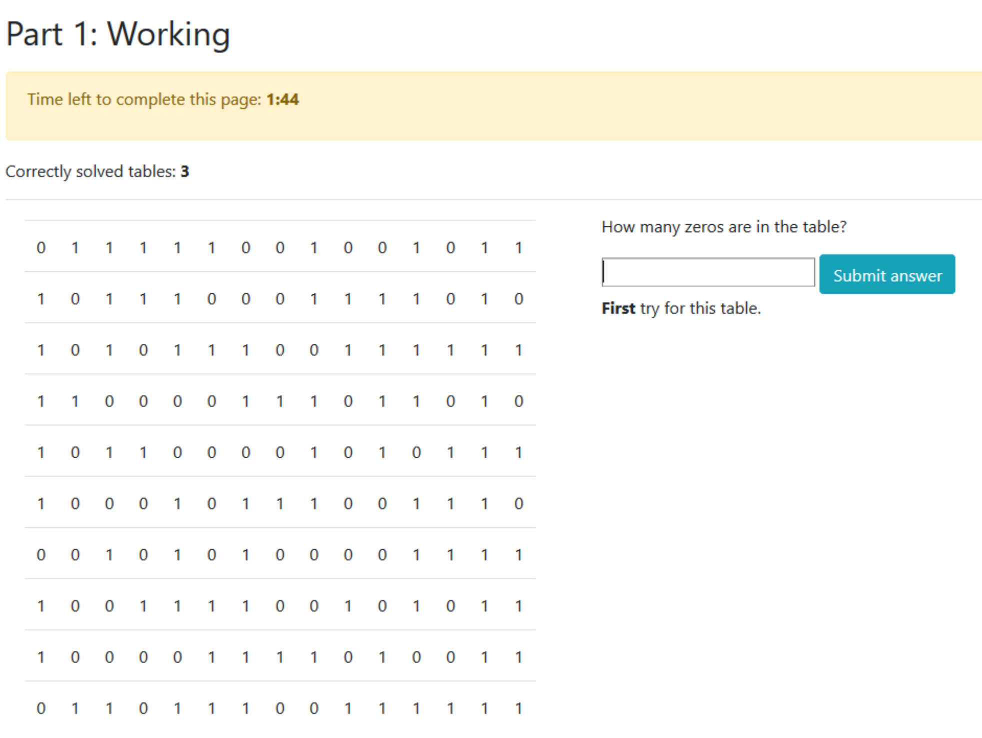
In this experiment, you can earn more money with the decisions you make during the experiment. Hence, it is important that you fully understand the instructions that follow. Please, read them carefully.

Overview

This experiment consists of two parts. In Part 1 of the experiment, which we call WORKING, you will work on a task for 4 minutes. In Part 2 of the experiment, which we call REPORTS, you will report the results of what we ask to do. Your earnings in this experiment will depend on both your task performance and random factors. More details will follow.

Part 1: WORKING

In this part of the experiment, your task is to count zeros in a series of tables. While working on the task, you will see a screen similar to the figure below:



To provide your answer, you will enter the number of zeros into the box on the right side of the screen. After you have entered the number, click on [Submit answer]. If you enter the correct result, a new table will be generated. If your input is wrong, you will have two additional tries to enter the correct number of zeros. You, therefore, have a total of three tries to solve each table. You will have 4 minutes to tackle as many tables as possible. The more correct answers you give, the more money you will earn. We will provide you more detail in the second part of the experiment. The upper left corner of the screen will display the remaining time.

Counting tips: Of course, you can count the zeros any way you want. Speaking from experience, however, it is helpful to always count two zeros at once and multiply the resulting number by two at the end. Also, counting with the mouse cursor would be helpful.

If you have a question, please ask. Otherwise, please click on [Next] to start working.

[Participants perform the real-effort task for four minutes.]

Part 2: REPORTS

{{This part of the experiment consists of 15 rounds. In each round, a computer randomly tosses a fair coin. Before the coin toss, you **choose Head or Tail as the winning side**. You win if the coin toss lands on the winning side you chose, and you lose otherwise.}}

{{EXO: This part of the experiment consists of 15 rounds. In each round, a computer tosses a fair coin, and the outcome of the coin flip will be displayed on the screen.}}

{{CON: This part of the experiment consists of 15 rounds.}}

After knowing the coin toss result, your second task in each round is to **roll a fair 6-sided dice once and report the outcome**. If you have a 6-sided dice nearby, please pick it up before you click on [Next]. If you don't have a dice nearby at the moment, you can alternatively use the following online dice: https://flipsimu.com/dice-roller (or any similar dice roller you can find online). Note that the experimenters can't track your online activities. That is, regardless of whether you use a physical dice or an online dice roller, the actual outcome of the dice roll is private to you.

Earnings

Suppose you correctly solved N tables in Part 1. In each round, you earn points calculated as follows:

Total points = {{[Coin points]}} + N*[Dice points]
{{EXO: [Coin points]}}, {{CON: 20}}

{{Coin points are **80 if the coin lands on the winning side**. Coin points are **20 if the coin lands on the losing side**.}}

{{EXO: Coin points are **80 if the coin lands on Head**. Coin points are **20 if the coin lands on Tail**.}}

{{CON: You get **20 base points** regardless of your performance in Part 1.}}

Dice points are **[dice report + 3]**. For example, a dice report of 4 corresponds to dice points of $7(=4+3)$.

For illustration, we provide three examples below. Suppose you solved 8 tables correctly.

- Example 1: If the coin lands on the winning side, and you report a dice outcome of 1, you receive $80+8*4=112$ points.
- Example 2: If the coin lands on the losing side, and you report a dice outcome of 3, you receive $20+8*6=68$ points.
- Example 3: If the coin lands on the winning side, and you report a dice outcome of 6, you receive $80+8*9=152$ points.

Actual payment

At the end of the experiment, the server computer will randomly select one round with equal probability, and **your earnings in that selected round will be your actual payment**. Since each round is equally likely selected, it is in your best interest to take every round equally seriously. Your points will be converted into Euros at the exchange rate of 10 points = 1 EUR.

To ensure your understanding of the instructions, we will ask you to answer a brief quiz. When ready, please continue.

Quiz

To ensure your understanding of the instructions, we provide you with a quiz. If you have one or more wrong answers, you have to re-take the quiz. This quiz is only intended to check your understanding of the instructions. It will not affect your earnings.

Q1 Suppose you solved no tables correctly. If the coin lands on the losing side and you report a dice outcome of 1, how many points do you receive?

Q2 Suppose you solved 7 tables correctly. If the coin lands on the losing side and you report a dice outcome of 4, how many points do you receive?

Q3 Suppose you solved 10 tables correctly. If the coin lands on the winning side and you report a dice outcome of 6, how many points do you receive?

Q4 Suppose you solved 4 tables correctly. If the coin lands on the winning side and you report a dice outcome of 3, how many points do you receive?

[Participants choose the winning side.]
[After learning the outcome of the coin flip, participants report the dice roll.]
[Repeat for 15 times.]

Recall the coin tosses.

You now have the chance to earn additional 5 points.

You have to recall how often you won the coin toss. If your answer is in a range of plus and minus 1 of the right number, you receive an additional 5 points. Otherwise, you receive no additional points. Thus, you have to give an answer between 0 and 15.

How often did you win the coin toss in the 15 rounds?