The Effect of Chosen or Given Luck on Honesty*

Diogo Geraldes† Franziska Heinicke‡ Duk Gyoo Kim§

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Abstract

Does being lucky (or unlucky) affect honest decision-making? We examine (1) whether luck-based income strengthens or erodes the moral value of honesty; (2) whether the perceived level of agency over an uncertain event affects the relationship between luck and honesty; and (3) whether accumulated luck affects honesty. To this end, we conducted a lab experiment where participants self-report a dice roll outcome, which is associated with effort-based income, *after* having received luck-based income. We manipulated the participants' perceptions regarding their influence on luck-based income. In the *exogenous luck* treatment, computerized coin tosses determines the luck-based income, whereas in the *endogenous luck* treatment, the participants choose the coin's winning side before the computerized coin toss. Our results are as follows: (1) lying behavior increases when contemporaneous luck-based income is high, (2) lying is not affected by the perceived level of agency, and (3) lying is not affected by the previous outcomes of the luck-based income. Our observations challenge the relative importance of context that may render moral justification. Therefore, our findings indicate that differences in dishonest behavior may be largely due to individual-specific heterogeneity.

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†School of Economics and Geary Institute, University College Dublin, Ireland. Email: diogogeraldes@gmail.com

‡Department of Economics, University of Mannheim, Germany. Email: f.heinicke@uni-mannheim.de

§Department of Economics, Sungkyunkwan University, South Korea. Email: kim.dukgyoo@skku.edu

1 Introduction

People often have income components that are partially or entirely subject to uncertainty. A farmer's harvest depends on the weather, an investor's returns depend on market conditions, and on the extreme end, people can also receive fully luck-based income from, for instance, playing the lottery or gambling activities. Does being lucky (or unlucky) with luck-based income affect honesty in the determination of another income source?

Several experimental studies on dishonest behaviors have examined the drivers of "moral justification." In other words, numerous previous studies have focused on identifying contexts in which people are more prone to engage in self-benefiting dishonest behavior. These studies have a common procedure of asking participants to observe a random outcome (which is not observed by the experimenter) and self-report it. Since the experimenter cannot assess the veracity of the self-reports, participants can lie about a bad-luck random outcome without being detected in order to receive a higher payoff.

There are two typical narratives on moral justification, which are based on an income effect and a feeling of deservingness. The income effect means people choose dishonest[1] actions that counterbalance low income. In this study, the income effect relates to low income driven by bad luck. Further, people are able to justify dishonesty if they feel deserving of a good outcome. We consider two factors that might enhance a feeling of deservingness in the specific case of luck-dependent outcomes. First, the accumulation effect means people choose dishonest actions when they feel the accumulated bad luck (or unfair Nature) renders them a (psychological) right to pursue lying behavior. Second, the entitlement effect means people feel they deserve more compensation (and choose more profit-seeking actions) for their effort. Overall, these narratives about the income, accumulation, and entitlement effects view people's dishonest behaviors as optimal responses to the realized outcomes to achieve their (mental) objectives.[2]

In this study, we directly test for the three effects outlined above by introducing a second source of luck-based income. Thus, as in earlier studies, participants observe one outcome that can be misreported; however, they also receive income from a second random outcome that they cannot misreport. Collectively, our results challenge the relative importance of context. To ease the following discussion, we will refer to individual heterogeneity in lying

[1]In our context, we regard a person's behavior as dishonest when the deviation from the truth leads to a profitable deviation—either materially or psychologically—for the person.

[2]Conceptually, we suppose a decision maker who faces a true realized value of interest and decide what to report. The monetary payoffs monotonically increase with the reported value, but "moral capacity" prevents her from lying to the full extent possible. More details are found in Section 2.2.

and "moral capacity" in a specific context. As moral capacity, we understand the factors enabling or hindering lying in a particular context, which importantly include justification strategies. While individual heterogeneity predicts lying behavior independent of context, moral capacity acts as a constraint to the lying maximization problem in a given context and thus, predicts lying behavior to be responsive to context.

Geraldes et al. (2021) find that dishonest reports on two orthogonal dimensions are positively associated, undermining the idea that people lie until their moral capacity constraint binds. Additionally, good luck in one dimension might be associated with more lying in another dimension. In an extensive meta-study, Gerlach et al. (2019) find limited support for the relevance of situational factors in people's decision to lie. This is particularly true for individual tasks. These findings may suggest that individual-specific heterogeneity—rather than conditions or contexts[3] that render a different degree of moral capacity—could be more significantly associated with lying.

In practice, both context and individual preferences are elements that affect honest behavior. However, particularly for policymakers aiming to minimize the social costs of dishonest behavior, rigorously examining which elements are more strongly associated with dishonest behaviors is important. If we understand the contexts which encourage or discourage honest actions, our behavioral insights would be relevant for policymakers aiming to (re)design institutional setups. However, if we find that heterogeneous individual differences fundamentally explain dishonest behaviors, the implication for policymakers would be to direct their efforts and resources to improve monitoring capacity. Hence, our primary research question—whether luck-based income affects the moral value of honesty—is policy-relevant.

We conducted a lab experiment to answer our research question regarding the three effects of income, accumulation, and entitlement. Specifically, we conducted a lab experiment with three treatments wherein participants were required to self-report a dice roll outcome, which was associated with effort-based income, *after* receiving exogenous income. In the *exogenous luck* (EXO) treatment, the computer tossed a coin to determine the *luck-based* exogenous income. In the *endogenous luck* (ENDO) treatment, exogenous income was also luck-based, but a participant chose the coin's winning side before the computer tossed it. In the *control* (CON) treatment, the dice report was identical to the previous two treatments, but the additional income was not determined by luck. Instead, the participant received a

[3] We agree that the social contexts or social norms play a crucial role in shaping individual decisions (Frank, 2020). By conditions or contexts, we mean the marginally different context faced by an individual while the broader sense of social contexts is fixed.

fixed income. In each treatment, the coin toss and subsequent self-report of a dice outcome were repeated 15 times. A lab experiment is suitable for addressing our research question because it allows us to control several factors that might affect moral capacity, especially the participants' perceptions regarding their influence on luck-based income.

Our three main findings can be described as follows: First, we examine how being lucky for the luck-based income affects lying for the effort-based income. We find weak evidence that people lie more in rounds with good luck. The income effect's significance would have been reflected by a positive association between bad luck and more lying. However, as this is not the case, we do not find evidence to support the income effect narrative.

Second, we examine how the perceived level of agency over an uncertain event affects lying behavior. Due to the active choice of the winning side in the ENDO treatment, the perceived agency should be greater in this treatment than in the EXO treatment, where the corresponding "winning" is passively informed. Although the manipulation check indicates that the ENDO treatment significantly affects the participants' perception, we find that the dice reports are not distinguishable between treatments.

Third, we examine how accumulated luck affects lying. The accumulation effect's significance would have been reflected by a positive association between a streak of bad-luck outcomes and more lying.[4] However, we find no significant relationship between previous luck outcomes and current dice reports.

Overall, our observations challenge the view that dishonest behavior is mainly driven by the income, entitlement, and accumulation effects. Rather, in our setting, dishonest behavior seems to be rooted in individual-specific heterogeneity. Supporting the latter view, one unexpected but notable finding is that participants who self-reported that they used an actual dice[5] rather than an online dice generator were lying significantly more than others. In other words, this finding is in line with our claim that individual-specific heterogeneity is a strong predictor of lying behavior.[6]

[4]Note that this psychological effect must be distinguished from the accumulated income effect because the participants are well informed that only one round is selected for payment. More details follow in Section 2.

[5]We claim that a substantial fraction of the participants who self-reported the use of an actual dice were lying. More details follow in Section 3.

[6]The argument that the individual differences matter in (dis)honest behavior is not new; for example, Dai et al. (2018) find a predictive power of (dis)honest behavior in the lab to that in the field, and Konrad et al. (2021) elicit the heterogeneous individual lying costs that determine honest earnings. The difference between the previous studies and ours is in the claim that the individual differences may matter more strongly than the contextual changes. If being honest in the lab can be considered a social norm, our findings might be along with Kimbrough and Vostroknutov (2016), who report the heterogeneity of individual norm-sensitivity in the lab.

The remainder of this paper is organized as follows. The following subsection 1.1 provides an overview of the relevant literature. Section 2 outlines the experimental design, hypotheses, and procedures. Section 3 reports the results of the experiment, and Section 4 concludes.

1.1 Literature Review

A growing body of literature shows that people hold preferences for honesty. Even in situations where unverifiable lying allows for securing additional benefits, people do not entirely utilize this option (Abeler et al., 2019; Gerlach et al., 2019). A desire to protect their self-image and not appear as a liar to oneself or others arguably drives this preference for honesty (Mazar et al., 2008). Justification strategies allow people to lie while still maintaining a positive self-image, and as justification strategies are specific to a context, lying can vary considerably in different situations (Shalvi et al., 2015; Markowitz and Levine, 2021). For example, previous studies show that lying is affected by the opportunity to perform several dice rolls (Shalvi et al., 2011), loss framing (Grolleau et al., 2016), and time pressure (Capraro et al., 2019).

Naturally, luck and the perception thereof likely also affect lying on an independent matter. The majority of the related studies suggest that being unlucky could justify lying more after experiencing bad luck. Several reasons could explain the latter, but we primarily focus on two factors: the income effect and the entitlement effect—including psychological and emotional responses. First, after experiencing bad luck, participants might have a lower income and thus attempt to compensate for it by lying more. In contrast, people who are lucky in regard to the additional uncertain income component have already secured a high income and might not feel the need to increase their income through lying. Evidence on the effect of stakes on dishonesty is mixed; however, in support of this argument, Gerlach et al. (2019) find a negative correlation between stake size and honesty in a coin flip task. Further, Kajackaite and Gneezy (2017) show that stake size significantly increases dishonesty if participants do not fear being discovered. Additionally, Gneezy et al. (2018) provide a theoretical argument—with empirical support—that the chance of making a false claim is higher when the true observed outcome is lower.

Second, the feeling of being treated unfairly by Nature may enable people to justify their lying. While no one else is responsible for the unfortunate outcome, people may consider their bad luck unfair. That is, they might believe that Nature treated them unfairly. People who feel treated unfairly have been found to cheat more (Houser et al., 2012). When workers are exposed to a compensation scheme based on random bonuses, they tend to cheat more (Gill et al., 2013). Geraldes et al. (2021) report weak but suggestive evidence that a streak of good luck on a high-stake prize decreases lying about a small-stake report. In further support of this argument, there is evidence that bad luck induces anger (Zitek and Jordan, 2021). Thus, some people have an emotional reaction to bad luck even though no other person is responsible for the outcome. Anger is also an emotion that affects dishonest behavior (Yip and Schweitzer, 2016).

In addition to the presence of an unrelated uncertain outcome, a few studies have suggested that perceived control over the uncertain outcome plays a significant role. The literature on redistribution preferences has shown that when outcomes are chosen, rather than by influenced by brute luck, people are more likely to perceive themselves to deserve these outcomes (Cappelen et al., 2013; Mollerstrom et al., 2015; de Oliveira et al., 2017). Cappelen et al. (2020) find that this tendency holds even if the choice is only a nominal one that does not allow control over the outcomes. Thus, having a choice leads to outcomes attributable to people's decisions. Based on this insight, one might expect that people who perceive that their choice affects uncertain income would have an additional justification for being lucky; they deserve desirable outcomes because they chose well. This effect might be further reinforced if we consider evidence showing that people engage in motivated recollection (Bénabou and Tirole, 2002); that is, they tend to view themselves as responsible for desirable outcomes, but stress external forces and bad luck for undesirable outcomes. Fries and Parra (2021) show that participants lie more to secure income generated through their effort than to secure windfall money. In their design, loss aversion is hold constant which indicates that treatment effects are due to participants feeling deserving of rewards for a job well-done. An extension of their argument could hold in the context of this study. Having been successful on the unrelated outcome could make people feel deserving of rewards beyond this initial outcome.

Based on the insights from these previous studies, we set our null hypotheses such that luck-related contexts and the perception thereof affect moral justification, and the affected moral justification allows people to lie differently. In contrast to the aforementioned arguments, only a few studies report that individual-specific heterogeneity, rather than the situation, might be a stronger predictor of dishonest behaviors. Geraldes et al. (2021) find that people who lie more in one dimension tend to simultaneously lie more in another dimension. If people's moral capacity were to be binding, the opposite should have been observed. In an experiment with a high-stake and low-stake dimensions, Barron (2019) finds half of the participants make a combination of a high and a low report. Nevertheless, the author still finds that a considerable share of the participants (26%) make high reports on both dimensions. Additionally, when some people prefer being seen as lucky, reporting luck could confirm this view (Darke and Freedman, 1997). Together, these studies suggest that dishonest reports are mainly driven by people's heterogeneous willingness to engage in dishonest behavior or to be seen as a liar, and not so much by people's (different extents of) moral capacity across contexts.

2 Experimental design

2.1 Treatments

We design laboratory experiments to observe how lying is affected by being lucky or unlucky in separate and unrelated income sources.[7] For all treatments, the participants first conducted a real-effort task to generate a performance measure. We employ the counting-zeros task, in which participants are presented with a 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 of the 10×15 matrices as possible. In each table, 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. Before performing the task, the participants were informed that the more matrices they solved correctly their earnings would be higher; however, the participants were not informed how their performance would be translated into the payoff.

In the *exogenous luck* (EXO) treatment, the real effort task was followed by 15 rounds of self-reports. Two events occurred during each round. First, the computer reported the outcome of a virtual coin toss for each participant, thus determining the first source of a participant's 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 gave a dice roll report. The dice roll report determines the piece rate for performance in the counting-zeros tasks, which in turn determines the income from the second source. The piece rate was set as: [dice report + 3]; for example, if a participant solved 10 matrices and reports 6, she earned

[7] The complete experimental instructions for all treatments are in Appendix B.

90 ($= 10 * [6+3]$) points.[8] While coin toss was entirely exogenous and determined by the computer, the actual dice roll was only observed by the participants. This means that the piece rate used to determine effort-related income was based exclusively on self-reports. At the end of the experiment, one of the 15 rounds was randomly chosen for actual payment (Azrieli et al., 2018).

We allowed the participants to lie about the piece rate of their performance to distinguish luck-based income from effort-based income which can be affected by lying. By linking the lying opportunity to the participants' performance, we examine the effect of luck on honesty, given the exogenous nature of the coin toss.

After the 15 rounds, participants were asked to recall how often the coin tosses were successful; that is, they yielded an income of 80 points. Their recall was incentivized; if their answer was in a range of plus and minus one of the correct number, they received an additional payoff of 5 points. Finally, participants rated on a 7-point-scale how lucky they felt about the coin toss.

The ENDO treatment closely resembled the EXO treatment but actively involved participants in the outcome determination of the luck-based income source. Specifically, before the computer determined the outcome of the coin toss, participants 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. This design closely resembles the *nominal choice* treatment of Cappelen et al. (2020), which showed that it increases participants' perceived control over uncertain outcomes. The self-reports of dice rolls and the recall of successful rounds elicitation are identical to those of the EXO treatment.

Finally, we conducted a control (CON) treatment. In the CON treatment, the dice roll was identical to the other two treatments, but the additional income was not determined by luck. Instead, no coin tosses occurred, and participants received a fixed income of 20 points in all rounds. Questions on the number of successful rounds and feeling lucky were omitted in the CON treatment.

[8] We chose parameters so that: (1) the expected income associated with each of the two income sources is similar, and (2) the lowest expected income is guaranteed to be above €5, in line with a standard show-up payment. In a previous study using a similar method (Vranceanu et al., 2015), the average counting-zeros task performance was 7.7. 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 ($= (20+80)/2$)).

2.2 Hypotheses

We have three testable null hypotheses under the assumption that the treatments affect moral capacity and dishonest behavior. Our hypotheses concern the income, entitlement, and accumulation effects. For expositional simplicity, we call the rounds where the coin lands on *tails* in the EXO treatment and on the losing side in the ENDO treatment as losing rounds. Analogously, we call the opposite scenario as winning rounds.

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

Hypothesis 1 reflects the argument that people lie more to compensate for low income induced by bad luck. Unlike the income from the coin toss, the participants can leverage another part of the income by reporting the dice roll outcome higher than the actual outcome. If we observed higher dice roll reports in the losing rounds of coin tosses, this hypothesis would have been supported.

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

Given that the ENDO treatment induces participants to perceive control over the luck-based outcome, Hypothesis 2 states that the participants in the ENDO treatment who believe that the luck-based outcomes are (to some extent) due to their choices rather than (solely due to) luck would lie more than those in the EXO treatment who perceive the desirable outcomes as purely exogenous luck. In line with the findings of Fries and Parra (2021), we hypothesize that participants in the ENDO treatment would have a strong feeling of deservedness regarding being lucky and, consequently, feel entitled to be (highly) rewarded for their good performance.

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

As described in the Experimental Design section, the actual payment is the payoff of the randomly selected round. Thus, strictly speaking, previous realizations of bad luck do not affect the income level of the current round. Nevertheless, we cannot rule out the existence of a psychological or emotional response that may affect moral justification. More accumulated bad luck might have made the participants feel that Nature had treated them unfairly, leading to more profit-seeking lying behavior.

In the following model, we restate our hypotheses formally. Recall the typical consumer choice problem where a decision maker maximizes her utility with a budget constraint. The indirect utility is the utility value at the optimal bundle of consumption goods, and the indirect utility function depends on the budget size. That is, the budget constraint describes the decision maker's "monetary capacity," and how the budget constraint is binding works as a shadow price of the capacity. That is, marginally relaxing the budget constraint increases the indirect utility to the extent that the current constraint binds.

We use these familiar results analogously. In short, "moral capacity" is described by the (mental) constraint of lying affects the decision maker's indirect utility, and how moral capacity binds works as a cost of lying. We explore the factors that affect the decision maker's lying decision, focusing on the potential determinants of moral capacity.

We postulate moral capacity as a constraint.[9] Conceptually, the narratives about the income, accumulation, and entitlement effects view people's dishonest behaviors as optimal responses to the realized outcomes to achieve their (mental) objectives. Suppose a decision maker faces a true realized value of interest, r_t, and she should decide what to report, r. The monetary payoffs monotone increase with the reported value, but moral capacity prevents her from lying further. The narratives can be represented by the following utility maximization problem:

$$r^*(r_t, L, E, \rho_i) = \arg\max_{r \in R} U(r; r_t, L, E)$$
$$\text{s.t. } r - r_t \leq v(L, E, \rho_i),$$

where $U(r)$ is monotone increasing in r on a finite set of support R, L is information about the luck-based outcome history, E is information about the effort-based outcome or achievement history, $\rho_i \in \mathbb{R}_+$ captures individual heterogeneity of subject i, and $v(L, E, \rho_i) \in \mathbb{R}_+$ represents "moral capacity," whose functional forms are unknown to researchers. A decision maker is called (but not detected as) dishonest when $r^*(r_t, L, E, \rho_i) \neq r_t$.

To illustrate the role of $v(L, E, \rho_i)$, let us assume no individual heterogeneity, that is, $\rho_i = \rho$ for all individuals at this moment. Consider two extreme situations: If $v(L, E) = 0$ for all L and E, then no one lies because r has to be r_t. If $v(L, E) = \infty$ for any L and E, then

[9] Alternatively, we could directly put "lying costs" into the utility function, as Abeler et al. (2019) posit it as $u(r) - \theta^{LC} c(r, \omega) - \theta^{RH} v(\Lambda(r))$, where r is the report, ω is the true state, LC refers to lying costs, and RH refers to reputation for honesty. $\Lambda(r)$ is the share of people reporting a lie of r in equilibrium. Although it would be suitable to examine the different weights of lying costs and the reputation for honesty, it is not suitable to capture the history of luck-based outcomes and changes in entitlement perception, which are our primary interests. Also, it is not easy to consider separately the individual heterogeneities and the treatment conditions we can change in the laboratory. Moreover, it is unusual to include "costs" to the utility function that a decision maker maximizes. For example, consumers maximize their consumption utility with considering the expenses of purchasing a consumption bundle, but we describe the utility function as $u(x, y)$, not $u(x, y) - p_x x - p_y y$.

the constraint is not binding, so the decision maker merely chooses $r^* = \max R$ that gives the largest utility value.[10] Hence, the marginal relaxation of moral capacity gives a decision maker a marginal room to lie more, and the Lagrange multiplier of the constraint can be interpreted as the cost of having such moral capacity.

The larger $v(L,E)$, the easier for the decision maker to lie. Thus, a smaller $v(L,E)$ makes her harder to lie because it works as a higher cost of lying. $1/v(L,E)$, the cost of lying, may capture the lying aversion and self-image concern.

The income, accumulation, and entitlement effects hypothesize the possible changes in $v(L,E)$. Although the histories of luck and effort may not necessarily be described as a set of countable entities, we impose some restrictions that correspond to our experimental design. Assume that the entities of L, the luck-based outcome history, are either 1 (lucky) or 0 (unlucky). That is, at time $t \in \mathbb{N}$, $L := \{l_1, l_2, \ldots, l_t\}$, where $l_\tau \in \{0,1\}$ for $\tau \le t$. In our experiment, the effort-based outcome is pre-determined before the decision rounds, so the history of the effort-based outcome, E, is constant over time. Our ENDO treatment could render the decision makers a little more sense of entitlement if the luck-based outcome is believed, to some extent, to be the outcome of their 'correct' decision. Assume that E consists of $\{e, \eta\}$, where e is the pre-determined level of effort, and η is the constant value that the decision maker subjectively attaches to the outcome of the luck-based event. Now our three hypotheses can be rephrased as follows:

H1: $r^*(L',E) \ge r^*(L,E)$ if $l'_t = 0$, $l_t = 1$, and $l'_\tau = l_\tau$ for all $\tau < t$.

H2: $r^*(L,E') \ge r^*(L,E)$ if $\eta' > \eta$.

H3: $r^*(L',E) \ge r^*(L,E)$ if $\sum_{\tau=1}^{t-1} l'_\tau < \sum_{\tau=1}^{t-1} l_\tau$ and $l'_t = l_t$.

One parameter not yet discussed in this context is ρ_i, the individual heterogeneity that scales the moral constraint. All of our hypotheses are based on the assumption that ρ_i is constant for all i or varies little. Thus, another way of interpreting our hypotheses is that our study investigates the relative importance of context changes over individual heterogeneity. Although we do not focus on determining which is right and which is wrong, illustrating the role of individual heterogeneity would be useful for understanding our analysis plan. Suppose our data do not support the three null hypotheses outlined above, yet the treatment

[10] In the context of reporting dice rolls, the meta-analysis results show that the average of the reported dice rolls is significantly greater than 3.5, and the average dice roll is significantly smaller than 6, implying $0 < v(L,E) < \infty$. This means that our model of moral capacity fits well with the previous findings.

condition anyways affects the lying behavior. Then, it might suggest the interplay of ρ_i with other changes in context.

2.3 Procedures

The experimental sessions, conducted in May and June 2021, included participants recruited from the Mannheim Laboratory for Experimental Economics (mLab) of the University of Mannheim. Instructions were provided in English. Due to COVID-19 restrictions, we did not take participants to the laboratory. Instead, we invited them to join an online meeting to receive instructions from the experimenter, distributed the unique link for participating in the online experiment, and paid them via online transfers (either PayPal or bank transfer). The online setting necessitated that physical dice could not be provided to participants. As an alternative solution, 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.

Four sessions were conducted for each treatment, and each session consisted of 11–15 participants. A total of 158 participants (54 in EXO, 52 in ENDO, and 52 in CON) participated in one of the 12 sessions.[11] 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, the experimenter asked them 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. Participants were required to read the instructions displayed on their screens carefully and to pass a comprehension quiz. In all the treatments, the participants filled a post-experiment survey about their basic demographic characteristics, whether they used a physical or online dice, and risk preferences.

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

[11] We preregistered this experiment at AsPredicted.org (#65770, "The Effect of Chosen or Given Luck on Honesty") and aimed to collect a sample of 50 participants per treatment. We aimed for this sample size based on a power analysis under the assumption of 0.4 standardized effect, a significance level of 5%, and a power of 80%.

3 Results

3.1 Hypotheses Testing

First, we assess whether unintended differences exist across the treatments. The participants completed the effort task without knowing about the experiment's second phase. Thus, since we randomly assigned participants to treatments, barring the influence of session-specific effects, the performance would not differ across treatments. On average, the participants solve 5.684 (SD 1.792) matrices. As expected, the performance does not differ significantly among the three treatments (Kruskal-Wallis test, $p = 0.952$). 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 that the number of winning rounds is not statistically different between the two treatments (Mann-Whitney U test (MW), $p = 0.478$; Kolmogorov-Smirnov test (KS), $p = 0.956$).

Before testing our hypotheses about the effect of luck on lying, we examine the overall extent of lying in the three treatments. To this end, we consider the individual average dice report across the 15 rounds to account for the individual effects. In the CON treatment, participants' average dice report is 4.522 (SD 0.953). 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 all treatments are significantly higher than 3.5, which would be the expected average report under truth-telling (t-test (TT), $p<0.001$ for all three tests). In other words, participants are lying in each treatment. Moreover, pairwise comparisons reveal no significant difference between treatments (MW, $p>0.1$ for all three tests). Thus, the overall dishonest behavior after receiving luck-based exogenous income is not significantly different from that after receiving fixed exogenous income.

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, $p = 0.178$).[12] Importantly, 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. Thus, our results reject Hypothesis 1.[13]

Result 1. *The dice roll reports are **not** higher in 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^{***}

Table 1: Dice reports in winning and losing rounds
*** indicates statistical significance at the 1% level.

We do not find any support for Hypothesis 2 either. The treatment difference for the winning rounds is insignificant (MW, $p = 0.232$). In absolute terms, lying in winning rounds is higher in the EXO treatment, which opposes Hypothesis 2. The lack of support for our hypothesis is not because the treatment manipulation—making the participants choose the winning side of the coin—does not affect their perception of luck. As a manipulation check, we elicited the participants' subjective perceptions of winning rounds. Specifically, after the 15 rounds were completed, we asked participants in both the EXO and ENDO treatments to indicate the number of successful coin tosses that they had observed. Table 2 presents the actual number of winning rounds against the number of winning rounds reported by the participants. While the perceived wins in the EXO treatment are not statistically different from the actual number of wins (WT, $p = 0.874$), the perceived wins in the ENDO treatment are significantly larger than the actual number of wins (WT, $p = 0.008$). Since belief elicitation was monetarily incentivized to reward accurate estimates (see details in the Appendix), we deem participants being more optimistic in the ENDO treatment as evidence that the treatment manipulation was effective.[14] Earlier literature has established a positive correlation between optimism and even overconfidence and perceived control (e.g., Stotz and von

[12] Reports are higher than expected under truth-telling for all four cases (TT, $p < 0.001$ for all four tests).

[13] Results 1 and 2 are robust when considering the first-round reports only (detailed results available upon request). Those are also robust to controlling for other available variables and clustering the standard errors of estimated coefficients at the individual level.

[14] Further, although the actual number of winning rounds in the ENDO treatment and perceived number

Nitzsch, 2005; Fontaine et al., 1993). Interestingly, our results on the manipulation check show that this not only holds *ex-ante* but also *ex-post*.

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

	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^{***}

Table 2: Winning rounds and perceived winning rounds
*** indicates statistical significance at the 1% level.

To test our third Hypothesis, we assess whether accumulated luck affects 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.[15] We claim that the models are well specified for the following: (1) Since the coin toss outcomes are random in both EXO and ENDO treatments, the dice reports do not Granger-cause the coin toss outcomes. (2) Since the correlation between the current and previous coin outcomes is near zero, we are 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.[16] This implies that accumulated luck does not lead to more lying. Therefore, we reject Hypothesis 3.

of winning rounds in the EXO treatment are virtually the same, the belief distributions about the number of wins show a non-significant treatment effect (MW, $p = 0.150$). This implies that the significance change 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.

[15]We do not intend the length of lags as a limited memory. We exclude longer lags as they do not increase the model fit. We could, instead, consider the entire history up to round t to assess how it affects the report in round t. However, taking the entire history as an explanatory variable in the regression models, the initial realizations have more weight than later ones, which we do not find any proper justification for it. Also, as the coin toss outcomes get accumulated, the variance of each participant's history (that is, the number of winning rounds) decreases. This small variation makes undesirably large standard errors of the estimate, thus making each coefficient appear insignificant.

[16]Coefficients of longer lags are statistically insignificant as well. We could consider the moving average as an explanatory variable, but it is nothing but a distributed lag model with the same-coefficient restriction to the lagged variable.

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

The dependent variable is the report of dice roll in the 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 10% level, 5% level, and 1% level, respectively.

3.2 Exploratory Analysis

So far, we have shown that the potential drivers that affect moral justification—income, entitlement, and accumulation—are either insignificant or have opposite impacts on dishonest behaviors. We argue that these results may suggest that the individual, not the context, facilitates more lying. In line with this argument, we report an unexpected and interesting finding.

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, $p = 0.503$). Notably, when considering the entire sample, we find that the average dice report (4.735) of the participants who indicated using a physical dice is significantly higher than the average dice report (4.392) of the participants who indicated using an online dice generator (MW, $p = 0.042$).[17]

We consider two possible reasons for the association between the use of a physical dice and more lying. First, a possible cause is that participants using a physical dice are 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 lied about using a physical dice, which correlates with their dishonest behavior in the dice self-report task. We find the second explanation more convincing based on three observations. First, we observe that participants who indicated having used a physical dice took significantly lesser time to complete the experiment. However, some delays would have been expected for participants to gather the dice because it is far-fetched to assume that the physical dice was easily accessible to those claiming to have used it.

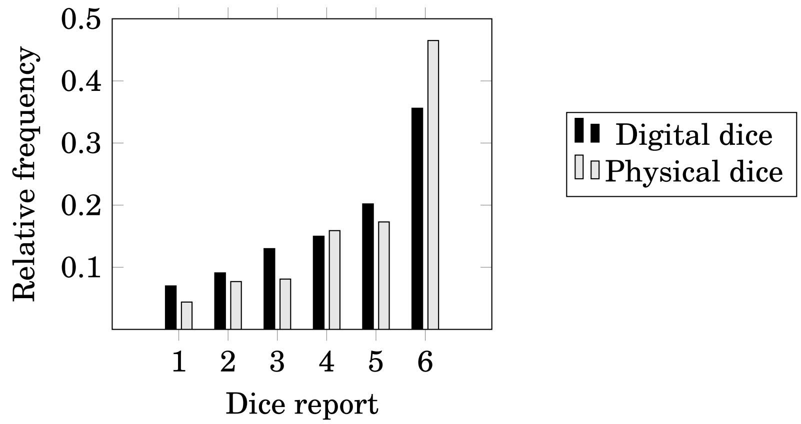
Further, we find support for the second explanation in the difference in distributions of dice reports between digital and physical dice users. In Figure 1, we pool all dice reports and break down the data between participants using a physical dice and those using an online dice. The largest difference is found for the share of 6 being reported, which is significantly larger for participants using a physical dice (two-sample proportion test, $p < 0.001$).[18] Thus, physical dice users did not lie more overall but specifically reported a higher share of 6. This could be an indication that they did not actually roll a dice but simply reported the most beneficial outcome.

Finally, it seems unlikely that such a large proportion of participants had a dice ready at the time of the experiment. Thus, in light of this unexpected finding, we subsequently explore our new conjecture regarding lying about the physical dice by conducting a survey to other participants from the same pool. A few months later, we asked other researchers conducting online experiments with participants from the mLab subject pool to survey at

[17]Given this surprising relationship between the randomization device 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 Appendix A).

[18]To account for individual effects, we also regress each individual's share of 6 on the type of dice used. The difference is still statistically significant ($p = 0.043$).

Figure 1: Distribution of the dice reports by randomization device



the end of their experiments how easily the participants could access a physical dice.[19]

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 (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 of Part 2 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 giving the first answer is, on average, 25 seconds longer for the participants who indicated having used an online dice generator. The latter finding further supports our interpretation that participants who indicated having used a physical dice in the experiment spent no time getting it. Second, 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, $p < 0.001$).

Based on these findings, we speculate that some participants did not use a randomization device at all and merely reported beneficial outcomes. Those participants might have felt more comfortable reporting using a physical dice.

[19] We asked the following: "Please answer as sincerely as possible. If someone asks you to bring a real (not virtual) dice right now, can you do it? 1. Yes, I have a dice within my arm reach. 2. Yes, I am sure that I can find a dice in 30 seconds in my current place. 3. There may be a dice somewhere, but I am not sure or it takes longer than 30 seconds to find. 4. No, I don't have a dice.

4 Conclusions

We examine (1) whether bad luck regarding luck-based income erodes the moral value of honesty, (2) whether the perceived level of agency over luck affects the relationship between luck and honesty, and (3) whether accumulated luck affects honesty.

Specifically, we test our null hypotheses on the income, entitlement, and accumulation effects. In line with most previous studies, we hypothesize that people would lie more for profits when (1) bad luck is accompanied with a lower income, (2) they believe that their choices lead to a good luck outcome, and (3) bad luck outcomes are accumulated for a longer period. The alternative hypothesis is that it is the individual, not the context, facilitates more lying.

We reject all of our null hypotheses. We argue that our findings suggest that, colloquially speaking, *liars will lie*, which casts doubt on the paramount influence usually attributed to situations that make honest individuals lie. One interesting but unanticipated observation in our setting is in line with our argument: Participants who claim to have used a physical dice seem to be lying about using the physical dice, and their dice reports are strictly higher than those of participants who claim to have used an online dice generator.

The policy implications of our results are that policymakers aiming to decrease the social costs associated with dishonest behaviors should be cautious in deeming institutional changes to tackle dishonesty as a panacea. In light of our findings, directing resources to identify individuals who lie rather than establishing institutional changes would be a more effective policy to prevent dishonest behavior. Further research is needed to understand how such individuals can be identified.

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A Appendix - Robustness of results

Table A.1: 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

The dependent variable is the report of 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 10% level, 5% level, and 1% level, respectively.

Table A.2: 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

The dependent variable is the report of 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 10% level, 5% level, and 1% level, respectively.

Table A.3: 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

The dependent variable is the report of 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 10% level, 5% level, and 1% level, respectively.

B Appendix - 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.

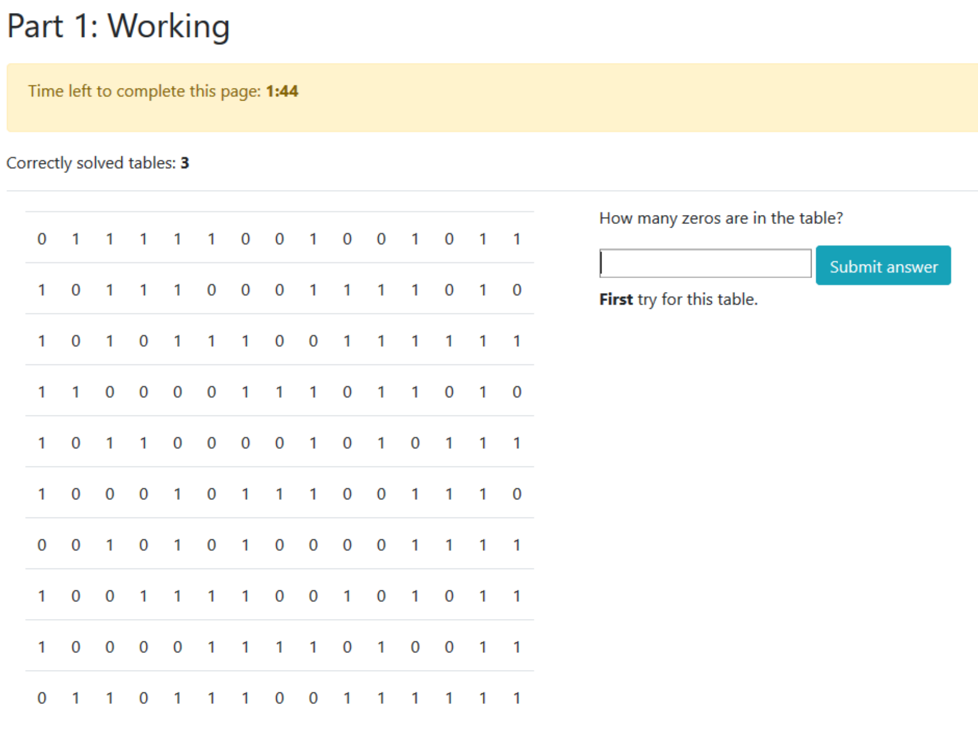
Counting tips: Of course, you can count the zeros any way you want. Speaking from experience, however, it is helpful to always count two zeros at once and multiply the resulting number by two at the end. Also, counting with the mouse cursor would be helpful.

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.}}


Part 1: Working

Time left to complete this page: **1:44**

Correctly solved tables: **3**

0	1	1	1	1	1	0	0	1	0	0	1	0	1	1
1	0	1	1	1	0	0	0	1	1	1	1	0	1	0
1	0	1	0	1	1	1	0	0	1	1	1	1	1	1
1	1	0	0	0	0	1	1	1	0	1	1	0	1	0
1	0	1	1	0	0	0	0	1	0	1	0	1	1	1
1	0	0	0	1	0	1	1	1	0	0	1	1	1	0
0	0	1	0	1	0	1	0	0	0	0	1	1	1	1
1	0	0	1	1	1	1	0	0	1	0	1	0	1	1
1	0	0	0	0	1	1	1	1	0	1	0	0	1	1
0	1	1	0	1	1	1	0	0	1	1	1	1	1	1

How many zeros are in the table?

Submit answer

First try for this table.

Figure A.1: Screenshots: counting zeros

{{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?