Lying Behavior When the Payoffs are Shared with Charity: Experimental Evidence

Scott Lee Chua[a1], Jessica Chang[2], and Guillem Riambau[3]

[1,2]Yale-NUS College
[3]IPERG – Universitat de Barcelona

October 4, 2019

Abstract

We investigate lying behavior when lying is undetectable and payoffs are split with charity. 524 Participants are randomly assigned to share all, some, or none of the payoff with a charity of their choice. The payoff earned depends on the number participants report after rolling a die in private (i.e., there are clear incentives to lie). This allows us to examine lying behavior as the share of the payoffs to charity gradually increases. Our results are as follows: (i) participants in all groups lie to inflate their number; (ii) lying decreases drastically when the charity is the sole recipient; and (iii) post-experiment surveys reveal that those participants who are most likely to have lied are the *least* likely to admit it. Finally, our data suggests that lying is not correlated with any observable sociodemographic characteristic.

Keywords: Dishonesty, Charity, Morality, Split, Experiment

JEL Codes: C91, D63, H26

Authors' declarations of interest: None.

Funding: This work was supported by Yale-NUS College.

[a]Yale-NUS College, 16 College Avenue West, Singapore. Contact info: Scott Lee Chua (corresponding author): scottleechua@u.yale-nus.edu.sg. Jessica Chang: jchang@u.yale-nus.edu.sg. Guillem Riambau: griambau@gmail.com. We gratefully acknowledge funding from Yale-NUS College. The funding source had no involvement in the design, collection, analysis, interpretation, writing, or decision to submit this paper. We would like to thank thank Eugene Choo and Lam Wingtung for their helpful insights, and Minwoo Choi, Dylan Leong, and Yogesh Tulsi for their assistance in conducting the experiment. Anonymized data and replication codes are available as online resources.

1 Introduction

For decades, traditional economics models predicted that people would lie as long as the lie went undetected and material gains could be made from it. However, recent studies have shown that, in fact, people lie surprisingly little: a meta study of 90 experiments involving 44,000 subjects across nearly 50 countries shows that subjects forgo on average three quarters of the potential private gains from lying when lying is both incentivized and undetectable (Abeler et al., 2019).

A few studies have examined how lying behavior varies when the beneficiary of the lie are not liars themselves. Typically, they compare lying behavior when payoffs are donated in full to other individuals (prosocial lying), versus lying behavior when payoffs are privately kept (selfish lying). Notably, some also examine behavior when payoffs are evenly split (Gino et al., 2013; Wiltermuth, 2011; Klein et al., 2017). These studies find a single-peaked-shaped propensity to lie. That is, lying is present in all situations, is maximal when spoils are evenly split, and reaches minimum rates when only others receive the payoff —i.e., selfish lying is larger than prosocial lying but smaller than lying for both.

Other studies have examined prosocial lying when the beneficiary is a charity rather than some other individual (Maggian, 2019; Lupoli et al., 2017). Both studies find that lying rates are invariant to the recipient: whether the ultimate beneficiary is the self or a charity, lying rates are exactly the same. Our study aims to examine whether the hump-shaped pattern of lies observed when the *other* recipient is an individual is reproduced when the *other* recipient is instead a charity. This way, we aim to establish whether participants find it easier to justify lies when these are told "for a noble cause". Further, by setting a gradual pattern of split payoffs similar to that in Klein et al. (2017), this study examines in detail how marginal variations in splitting shares with charity affect lying behavior.

We do this by means of a die-rolling task that follows the Fischbacher and Föllmi-Heusi (2013) paradigm. In our setting, participants are randomly allocated to one of five different groups. In the baseline treatment, participants keep all payoffs to themselves — we call this "self-only" group. In a second group, they donate all payoffs to an NGO they have previously chosen —we refer to this as "charity-only" group. In the other three, payoffs are split: 90% vs. 10%; 50% vs. 50%; and 10% vs. 90%; for the participant and their preferred charity, respectively. Participants are aware of the payoff scheme from the outset. Once all information is provided, they enter a room to privately roll a die once. While still in there, they are asked to write the number on a piece of paper previously provided to them. They return the paper to us with their reported number upon leaving the room. Before leaving the experimental site, they get paid and are further requested to oversee the online donation in case they belong to one the four groups that donates. As we describe in detail in section 2 below, we took all precautions to make sure that participants (i) were aware that lying was possible and completely undetectable; (ii) would care about the charity money would go to and; (iii) trusted that, indeed, we would make the donation as specified. Although we cannot detect dishonesty on an individual level, we can measure dishonesty at the aggregate level by comparing the distribution of reported outcomes against the expected discrete uniform distribution of a fair die roll.

Results unambiguously show that participants lie by inflating their numbers in all groups. Lying patterns are however heterogeneous: participants' propensity to lie is extremely sensitive to whether payoffs are fully donated or not. We estimate a flat rate of lying of around one in four participants for all groups in which they privately benefit from the lie (whether they keep the payoff in full or only part of it). However, only one in ten lie when payoffs are fully donated. This suggests there is a clear discontinuity in the costs of lying: they seem to be uniform as long as the share to be kept privately is strictly positive, but remarkably increase when payoffs are donated in full.

Most of our results are consistent with those studies whose "other" recipients are anonymous individuals. As in Gino et al. (2013), Wiltermuth (2011) and Klein et al. (2017), maximal lying happens when spoils are shared. Like them, we also find minimal rates of lying when the other (in our case, a charity) is the sole recipient. However, whereas these studies find that lying rates for selfish reasons are strictly smaller than lying rates when spoils are shared, we find there to be no difference: lying is virtually the same whether the participant is the sole recipient or the payoff is partially shared with a charity.

Our results are also consistent only to some extent with those studies that examine prosocial lying when the beneficiary is a charity. The design in Maggian (2019) is of a zero-sum game: all funds that are not given to participants are automatically donated to charity. Therefore, a negative externality is imposed on those who lie to increase their private gains. Our setting is closer to Study 3 in Lupoli et al. (2017) as there is no such trade-off: funds can only go to charity (or be kept by researchers). Both studies find similar rates of selfish lying and prosocial lying, which suggests that people do not find it easier to internalize the lie when the beneficiary is a charity. Our results are consistent with this, but are even more extreme, as they suggest that lying for a charity is in fact more costly than lying for onself.

Two are the main takeaways. First, our results support previous studies that show that charities do not increase lying rates when compared to selfish lying. Second, our results suggest that prosocial lying clearly depends on who the recipient is: when the other recipients are individuals, the pattern of lies is single-peaked (with maximum lying when payoffs are shared). However, in our case, where the "other" recipient is a charity, the pattern of lies is flat, with a single decline when the sole beneficiary is the charity. This is consistent with Maggian (2019), who suggests that such differential rates of prosocial lying that depend on whether the receiver is another individual or an organization can be explained by greater psychological distance and impersonal fashion —i.e., participants may find it easier to internalize a lie when the beneficiary is someone else that who they can connect with, rather than an organization, no matter how noble.

Finally, by means of a post-experiment questionnaire, we examine who is most likely to admit to having cheated. Results are unambiguous: we estimate that one in four in participants who lied admit to having cheated, with the exception of those who were the sole beneficiaries of the payoffs (selfish liars). For them, we find that only one in thirty admit to having lied. This strongly suggests that there exist two very different mechanisms at play: when it is implied that cheating benefits a noble third party like a charity, participants seem remarkably more likely to acknowledge their lies to others, but, at the same time, they seem to find a harder time to justify it to themselves compared to when they lie for purely selfish reasons.

2 Experiment design

2.1 Overview

Participants privately roll a fair six-sided die and report the outcome. The payoffs disbursed are the monetary equivalent of the outcome reported —e.g., if a participant reports a 4, the corresponding payoff is $4. Participants thus have opportunity and incentive to lie by misreporting the outcome. We vary whether participants take home all payoffs, split them with charity (via online instantaneous donation), or take none (i.e., all is donated). We observe how lying behavior changes accordingly.

2.2 Control and treatment groups

We randomly distribute participants across five different groups. In our control group, which we call "self-only" group, participants take home 100% of the payoff in line with previous experiments that use the die-roll paradigm (e.g. Hermann and Mußhoff, 2019). Participants in the three split-payoff groups ("90-self", "50-self", and "10-self") take home only a fraction of the payoff (90%, 50%, and 10%, respectively), and donate the rest to a charity of their choice. In the "charity-only" group, participants take home 0% of the payoff, and instead must donate 100% of the payoff.

All participants are aware of their payoff split before rolling the die. Participants could choose from five reputable charities which provide online-donation options and represent a diverse range of social issues participants might care about: women's rights, prisoner rehabilitation, animal welfare, crisis relief, and terminally ill children. We aimed to include a cross-section of popular social issues among our target population (undergraduate university students); for more details, see Appendix A.5. Table 2.2 summarizes the payoff distributions of the different treatment groups.

Treatment Group	Participant (% payoff)	Charity (% payoff)
self-only	100	0
split-payoff (90-self)	90	10
split-payoff (50-self)	50	50
split-payoff (10-self)	10	90
charity-only	0	100

Table 1: Comparison of payoff distributions in different treatment groups.

2.3 Die-rolling task

Participants were instructed to read the instructions for the task (Appendix A.1), then brought inside a private room by an experimenter, one at a time. Three items were provided inside the room: a pouch containing a die, a copy of the task instructions (Appendix A.2), and a timed lock-box containing pen and paper. As the lock-box was transparent, participants could see the pen and blank slips of paper inside. The experimenter then set the timer in the lock-box for one minute, and left the room. The timer countdown was displayed on a screen in the lid.

Participants were encouraged to spend this waiting time rolling the die "to practice".

Participants were instructed to roll the die exactly once as soon as the box unlocked, and write down the outcome on a now-accessible slip of paper. Participants were instructed to put the die back in the pouch, then proceed to a different room to submit their outcome slip to the experimenter. Clearly, since no one else was in the room, participants had an opportunity to lie, i.e. misreport the outcome of the final die roll.

We implemented the one-minute wait for two reasons. Some participants may have been suspicious about the fairness of the die. This minute allowed them to check whether the die was fair. Since our experiment ostensibly advertised "one-shot games of luck," which is somewhat vague, this re-assured them of the purpose of the experiment. Second, a time delay serves as a mandatory period of deliberation, which could increase participants' awareness of the opportunity to lie (Lohse et al., 2018) or override their intuition to cooperate with the experimenters, i.e., be honest (Rand et al., 2014). In other words, the delay helped ensure participants think rationally about their incentives and response.

Indeed, in line with Fischbacher and Föllmi-Heusi (2013), we made every effort to convey that dishonesty could not possibly be observed and therefore never be punished, without ever explicitly mentioning so to avoid priming. Participants could "tell a smaller lie" by reporting a favorable practice roll instead of their final roll. Participants were also asked to put the die back in the pouch before leaving the room so that the number facing up need not be their actual outcome. Finally, participants wrote down their outcome in private – they did not have to lie to the experimenter in conversation.

2.4 Experimental overview

Our experiment was conducted over 17 days in March 2019 at four sites within the National University of Singapore. A total of 524 participants took part in it. We solicited demographic information from participants, summarized in Appendix B.2.

To detect a 0.5 shift in average reported dice roll outcome with 80% power and significance level of 0.05, we required at least 99 participants per treatment group. All our treatment groups have at least 101 participants. The experiment was advertised by means of online research recruitment portals and posters at high traffic hubs around campus. Participants could sign up through research recruitment websites or walk-in to the experiment site. On average, the experiment took 10 minutes per participant, who were paid a show up fee of $5 (5SGD $\sim$ 4USD).

We precluded participants from participating more than once.[1]

Upon arrival, participants randomly drew a unique ID, which also determined their treatment group. Once assigned to a group, participants, except those in the charity-only group, were informed that they could earn additional payoff based on their outcomes during the experiment. Since the additional payoffs varied by treatment group, the exact amount of potential bonus earnings was not specified. Participants were not given information on the other treatment conditions, and were prohibited from communicating verbally or with their cellphones. Participants received written instructions (Appendix A.1), a payoff chart showing how their additional payoff corresponds to the die outcome (Appendix A.3), and, with the exception of the self-only group, a menu of charities. Participants' questions were raised and addressed privately. Participants chose the charity and then entered the room to carry out the die-rolling task described in Section 2.3.

Upon completion, participants proceeded to a private payoff station, where an experimenter paid participants (in cash) and/or charities via instant online bank transfer accordingly. Participants were requested to verify all bank transfers, and to oversee the calculation and disbursement process. Subsequently, participants answered a questionnaire (Appendix A.4), which was later matched to their reported outcomes. Finally, participants were debriefed and paid their show-up fee.

3 Results

3.1 Maximal lying and pure honesty occur in all groups.

Table 2 shows the share of participants in each treatment group who reported each payoff (Appendix B.4 shows the same data in histograms). If all participants were honest, we would expect around one-sixth of them to report having rolled a 6. However, in every treatment group, 6 is reported significantly more frequently than one would expect from a fair die (one-tailed binomial tests are significant, at a 5% significant level). Notably, this includes the charity-only group, where participants cannot increase their own payoff.

If we assume that participants do not lie to *decrease* their payoff, then participants who reported a 1 must be honest. As seen in Table 2, a nonzero share of the participants in every

[1]As we collected no personally identifying information, we could not systematically check names or IDs. As precautions, the same experimenters conducted all sessions, and participants were all informed of the preclusion condition before they could be assigned to any group.

treatment group do report 1. This share is consistent across split-payoffs groups (11-12% of reported 1s in all of them), is smallest for the self-only group (5%) and largest for the charity-only (18%).

	Share of participants (%)						
Group	1	2	3	4	5	6	N
Self-only	4.63***	12.04	15.74	17.59	17.59	32.41^{+++}	108
90-self	11.88	11.88	9.90**	18.81	15.84	31.68^{+++}	101
50-self	11.82	10.00**	11.82	16.36	21.82^{+}	28.18^{+++}	110
10-self	10.68*	7.77*	10.68*	18.45	27.18^{+++}	25.24^{++}	103
Charity-only	17.65	11.76	11.76	18.63	15.69	24.51^{++}	102

***($^{+++}$) $p < 0.01$, **($^{++}$) $p < 0.05$, *($^{+}$) $p < 0.1$.

Table 2: Share of participants in each treatment group who reported each payoff. Stars (plus signs) indicate significance for one-sided binomial tests that observed share is smaller (larger) than expected share of 16.67%.

3.2 Partial lying only occurs in 50-self and 10-self.

"Partial lying" occurs when participants misreport to increase payoff, but do not claim the maximum possible payoff (e.g. reporting a 5 instead of 6). In 50-self and 10-self, 5 is reported significantly more than expected (see Table 2). Further, the share of reported 5s in 10-self is significantly different (two-sided, two-sample χ-square tests of independence, $p < 0.1$) from all treatments except 50-self ($p = 0.36$). We do not detect partial lying in self-only. In 90-self and charity-only, 5 is reported less frequently than expected from a fair die, but insignificantly so.

3.3 Overall lying behavior is significant in all groups except charity-only.

If all participants were honest, we would expect the mean outcome from repeatedly rolling a die to be 3.5. We find that the mean reported outcome significantly differs from 3.5 for all groups except for the charity only-group (two-sided, one-sample t-tests, $p < 0.001$). In the charity-only group, however, we find no such significance ($p = 0.14$).

We formally test the reported outcome distributions of each treatment group against discrete uniform distributions of comparable sample size. We conduct two non-parametric tests: the Kolmogorov-Smirnov (K-S) test, which examines the single largest vertical difference between two distributions, and the Wilcoxon rank-sum (WRS) test, which pools the two distributions and tests for clustering. Test results are summarized in Table 3. Because our data is (1) discrete and (2) has multiple ties across samples to be compared, the standard versions of both tests are known to produce conservative p-values. We use Arnold and Emerson (2011)'s K-S Monte Carlo

p-value simulation method (1,000 replicates) and Marx et al. (2016)'s dynamic programming WRS solution, and report both accurate and conservative p-values in Table 3.

We observe similar results under both tests. Reported outcome distributions deviate significantly from the uniform in self-only and all split-payoff groups. But once participants have no private earnings to make, overall lying behavior diminishes drastically —to the point that we cannot reject the null that the distribution of the charity-only group is equal to the uniform. Chi-squared goodness-of-fit tests yield the same pattern of results (see Appendix B.1). In line with the literature, we also check results under the Fisher exact test, where self-only and 10-self are different from expected values at $p < 0.1$.

	Kolmogorov-Smirnov test (two-sample, one-sided)		Wilcoxon rank-sum test (two-sided)	
Group	Accurate	Conservative	Accurate	Conservative
Self-only	0.001***	0.003**	0.001***	0.001***
90-self	0.004**	0.009**	0.012*	0.013*
50-self	0.001***	0.004**	0.008**	0.009**
10-self	0.000***	0.000***	0.003**	0.004**
Charity-only	0.182	0.405	0.270	0.270

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

Table 3: Nonparametric tests of observed outcome distributions against uniform.

3.4 Overall lying behavior does not vary by gender, age, major, or spirituality.

All our treatment groups have similar demographic compositions, such that no one characteristic was over- or under-represented in any group (see Appendix B.2). We split our data into two groups using different demographic variables: gender, above/below median age, whether a participant studies economics, whether a participant considers themselves spiritual. In no case can we reject the null hypothesis that the distributions of the two groups are equal (K-S and WRS test p-values are all insignificant even at the 10% level).

We also find no sociodemographic characteristics associated to lying: fitting a linear regression of reported outcomes on demographic variables yields no significant coefficients. This result is robust to combining business students with economics students, as per López-Pérez and Spiegelman (2019). Regression results are summarized in Table 4.

Variables	Reported outcome
Male	-0.112
	(0.187)
Age	0.022
	(0.027)
Economics student (dummy)	-0.217
	(0.245)
Spiritual (dummy)	-0.066
	(0.186)
Constant	3.80***
	(0.629)
N	342
R^2	0.005

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

Table 4: Linear regression using demographics as predictors of reported outcome. Robust standard errors in parentheses.

3.5 Self-only participants are most likely to lie about having lied.

We can estimate the share of participants who lied (whether partially or maximally) in each group comparing expected vs. reported shares of 5s and 6s. The estimated shares of liars are reported in the first column of Table 5. We also asked participants through the post-task questionnaire (Appendix A.4) whether they were honest in their report. 90.4% of all participants said "Yes", 4.3% said "No", and 5.2% did not respond. No participant who admitted to lying had reported a 1, 2, or 3. Admitted lying per treatment group is reported in the second column of Table 5. While dishonest participants might have reason to lie about lying, it is hard to imagine honest participants lying about telling the truth. Hence, self-admitted lying can be seen as a lower bound on the number of misreports.

In every treatment group, the estimated frequency of lying is greater than self-reported frequency —evidence that participants lie about lying. We report the ratio of admitted lying to estimated lying in the third column of Table 5. We find that only 3.72% of liars in self-only admit to having lied, compared to 22-29% in all other treatment groups. Indeed, as participants' take-home share increases, they become less likely to admit having lied, though this trend is not monotonic.

Perhaps non-respondents who reported a 5 or higher are all liars, and their very non-response is an admission of guilt. Including them in our measure of self-admitted lying does not change the result (see Appendix B.3). The result is also robust to including non-respondents who reported a 4 to the share of admitted liars.

	Share of participants (%)		
Group	Estimated lying	Admitted lying	Admitted/Estimated (%)
Self-only	25.00	0.93	3.72
90-self	21.28	4.95	23.26
50-self	25.00	5.45	21.80
10-self	28.63	7.77	27.14
Charity-only	10.30	2.94	28.55

Table 5: Discrepancies between estimated and self-admitted shares of lying. Estimated lying is computed as follows: $1 - \left|\frac{\text{actual \# of } \{1,2,3,4\}}{\text{expected \# of } \{1,2,3,4\}}\right|$. That is, if, as expected, 2/3 of respondents report {1,2,3,4}, this value is 0, and, if none report {1,2,3,4}, this value is 1.

4 Discussion

We can summarize findings presented thus far as follows: First, participants in all groups lie to inflate their number. Second, participants lie more often when the private share is strictly positive. Third, lying is not correlated with any observable sociodemographic characteristic. And, fourth, those participants who are most likely to have lied are the *least* likely to admit it. We discuss them in turn.

Previous research has focused on our two "extreme" groups, self-only and charity-only. The frequencies of maximal lying and pure honesty in our self-only baseline group have magnitudes comparable with Fischbacher and Föllmi-Heusi (2013) and Gneezy et al. (2018), whose experimental designs most closely resemble ours. Regarding charity-only, we find that this group has the least maximal lying and the most pure honesty (Result 3.1). This result is consistent with Wiltermuth (2011) and Klein et al. (2017), who find that when the payoff goes only to others, there is still lying but significantly lower than when the self is involved.[2] Our results, however, contradict those in Zhao et al. (2017), who find that 3- and 5- year old kids are marginally more likely to engage in (potentially detectable) cheating when the beneficiary is another kid than when they are the beneficiaries.[3] Our result is also consistent with Maggian (2019) and Lupoli et al. (2017), who find that participants whose lies increase charity donations do not tend to lie more than those whose lies increase their own material payoff.[4]

[2]We note that Klein et al. (2017) uses a within subject design. I.e., all subjects were asked for their choices in each of the treatments.

[3]Cheating is potentially observable because the experimenter leaves the kids alone in the room specifying she will "be back soon". Since kids have to physically flip a deck of cards to cheat, they may fear they could be caught if the experimenter comes back *too soon*.

[4]In Lupoli et al. (2017), the study that most resembles ours is the "neutral" condition in Study 3.

As noted above, our main contribution are "non-extreme" groups. By and large, these reveal a monotone increase: if we look only at the proportion of reported 6s, we can see that it increases gradually as we increase the payoff share that goes to participants (24.5% < 25.2% < 28.2% < 31.7% < 32.4%). This suggests that incentives to lie increase smoothly as participant shares increase. However, the pattern does not look as smooth once we look at the distribution of reported numbers. If we split them in two groups, {1,2,3} vs. {4,5,6}, i.e., "small" vs. "big" numbers, we can see a clear discontinuity: for all four groups that include a non-zero private gain, the proportion of "small" numbers is extremely close to one third (Table 2) —i.e., half as much as one would expect.[5] However, the share of small numbers sharply increases once the private payoff fully disappears: more than 40% of participants in the charity-only group reported a small number (a 30% increase with respect to all other groups). This means that around one fourth of participants are estimated to have lied in all groups but the charity-only, in which only one tenth are estimated to have lied (see Table 5). This pattern is essentially the same if we include 4 as a "small" number.

These results taken together suggest that, while all participants are likely to lie, they tend to lie more as long as they are the recipients of a strictly positive fraction of the material payoffs. Before examining these results in more depth, we would like to discuss how the experiment design may have affected our results.

Participants may not really have regard for the charity the money goes to. If that is the case, then those in the charity-only group would have no incentive to lie because their utility gain from the lie would be essentially non-existent. To avoid this, five different types of charity were carefully chosen so that at least one would be truly appealing to all participants. Topics were chosen among those that are currently most popular among youngsters —women's rights, prisoner rehabilitation, animal welfare, crisis relief, and terminally ill children. All participants whose outcome affected charity donations were requested to choose one of the five in advance, and were told that this choice was to make a donation. While it is possible that some participants may have not been interested in any the charities offered to them, we believe that they genuinely appealed to most of participants.

Similarly, participants may not trust researchers. We took all possible precautions to minimize the risk of this happening. Upon receiving the instructions, participants were told that

[5] Proportion of "small" numbers: self-only (32.41%); 90-self (33.66%); 50-self (33.64%); 10-self (29.13%); charity-only (41.17%);

they would be requested to oversee the donation to be made online before they left the site. Furthermore, when choosing among the five charities, they were able to check they were legitimate, and that instantaneous small online donations could be made. Hence, we also discard the possibility that participants lied less in the charity groups because they did not trust the donation was going to be made.

All our evidence therefore points to the fact that differential rates of lying were due to participants' preferences and not to our experiment design or implementation. Therefore, if participants prefer more money to less, and minimally care about one of the five charities provided, they should *always* lie. Given that these two features can be safely assumed for all participants, if they do not always lie it must be because lying is costly. The literature has broadly identified two main reasons why people do not lie maximally when all the incentives are set for them to do so (see Abeler et al. (2019) and all references cited therein): a preference for being seen as honest (social image/reputational cost) and a preference for being honest (self image). Given the distinct pattern of lies we find, it must be the case that costs of lying depend on the share to be donated: either preferences for being seen as honest, or preferences for being honest are affected when the payoffs are shared with a charity.

Regarding the former (social image), Levine and Schweitzer (2014) find that individuals who tell lies with the intention of benefiting others are perceived to be more moral than individuals who tell the truth. If this is the case, participants in our setting would have a bigger incentive to lie the larger the share of the pie going to charity. That is, we should observe more lying as the fraction going to charity increases. We, however, find the exact opposite: maximal lying *decreases* with charity share, i.e., if anything, participants seem to be more shameless about lying when they are the sole beneficiaries of the lie.

We find no evidence of a willingness to signal prosocial behavior to others. How about self image? Some participants may be averse to lying, but may find it easier to internally justify the lie if the payoffs go to a charity they support. If that is the case, we should find that participants lie more as the share of the pie for charity increases. Again, we find the exact opposite: assuming self-image is the main concern, participants seem more likely to justify a selfish lie than a prosocial lie.

Our results hence suggest that the larger the share for one-self, the easier it is to justify a lie. Importantly, our experiment setting allows us to assess exactly where the charity motive "kicks in": as earlier noted, one in four participants in the self-only group are estimated to have lied, whereas this is the case for only one in ten in the charity-only group (see Table 5). What happens in-between these two extreme groups? We find that the transition is by no means smooth. As long as participants have some material gains to make, lying rates are around one in four. Only when private material payoffs are completely non-existent do lying rates drastically decrease. This suggests an asymmetric salience effect: reducing private material gains from 100% to 90%, to 50% or even to 10% has no apparent effect on lying rates. However reducing private material gains from 10% to 0% results in a reduction in lying of around 60%. Overall, participants are insensitive to the size of their private payoff; they are sensitive only to *the existence* of a (strictly positive) private payoff. This is in line with Kajackaite and Gneezy (2017), Mazar et al. (2008), or Fischbacher and Föllmi-Heusi (2013), who find that lying does not increase with incentives.

An alternative explanation to our results is that charities act as a nudge to behave honestly: since some participants are forced to think about NGOs before rolling the die, their willingness to lie may therefore be affected. If this was the case, then we would expect participants in the self-only group to lie significantly more often than all other participants. We do not find this to be the case: participants in all "split-groups" (90-self, 50-self, and 10-self) lie as much as those in the self-only group. We therefore do not find enough evidence pointing to the nudge as an explanation of the patterns we observe.

NGOs may still however help explain the patterns we observe, since in our experiments payoffs are split with an NGO, unlike most experiments that examine prosocial behavior, which have payoffs split with other individuals. Maggian (2019) suggests that people are more willing to act unethically against an organization (even a charitable one) than against an individual. Perhaps lying to benefit a charity provides less moral flexibility than lying to benefit an individual. This might explain why Wiltermuth (2011) and Gino et al. (2013) find that lying peaks when fellow research participants also benefit, while we do not observe such a peak for splits with charity.

Regarding individual sociodemographic characteristics, our results show that overall lying behavior does not vary by gender, age, major, or spirituality (Result 3.4). This supports and contradicts multiple studies investigating the demographic covariates of dishonesty. Regarding gender, our result is consistent with Capraro (2018), whose meta-analysis finds an inconclusive gender effect on "Pareto white lies", lies that benefit both the self and another. Unlike with gender, no such consensus exists for the other covariates. Regarding field of study, our result contradicts Muñoz-Izquierdo et al. (2019) who find that business and economics students are more likely to lie. Regarding spirituality, however, our result is consistent with López-Pérez and Spiegelman (2019) but not with Arbel et al. (2014), who find higher dishonesty among secular females.

Finally, we find another sharp discontinuity when it comes to admitted lying. In all groups but one, we estimate that around one in four liars admit having cheated. In this case, however, the exception is the self-only group: only one in thirty liars are estimated to have admitted failing to tell the truth. This is in sharp contrast to lying patterns themselves: whereas lying markedly increases the moment private benefits are strictly positive, admitted lying markedly increases the moment charity benefits are strictly positive. This suggests that participants are willing to admit having lied when they can claim that payoffs went to charity, but are extremely reluctant to admitting they cheated when they are the only beneficiaries of the lie. Participants might believe that the consequences of admitting to lying (whether punitive or reputational) are lower when they are not the sole recipients of the payoff.

5 Conclusion

This paper aims to shed light on our understanding of prosocial lying. Results show that around one quarter of participants are willing to lie to increase their private payoffs, regardless of the exact share of the pie they are getting. However, when only charities benefit from lying, so that participants do not privately benefit, propensity to lie decreases markedly: only one in ten participants are willing to cheat in such scenario.

Our findings are mostly consistent with previous studies, but differ in two aspects. Compared to Lupoli et al. (2017) and Maggian (2019), we find that the propensity to lie for charity is significantly smaller than the propensity to lie for selfish motives (they find no difference). Further, while lying patterns in Gino et al. (2013), Wiltermuth (2011), and Klein et al. (2017) peak when payoffs are split, we find no evidence of such peak —in our case, lying when payoffs are shared is not larger than selfish lying. This could be explained by the identity of the other recipient (charity in our case, other participants in their studies), but could also be explained by cultural norms: ours is the only study that was not carried out in a “Western” country. Hence, we cannot disregard that discrepancies in patterns observed arise from particular cultural norms prevalent in the South East of Asia.

References

Abeler, J., Nosenzo, D., & Raymond, C. (2019). Preferences for truth-tellings. *Econometrica*, *87*(4), 1115–1153.

Arbel, Y., Bar-El, R., Siniver, E., & Tobol, Y. (2014). Roll a die and tell a lie - what affects honesty? *Journal of Economic Behavior & Organization*, *107*, 157–172.

Arnold, T., & Emerson, J. (2011). Nonparametric goodness-of-fit tests for discrete null distributions. *The R Journal*, *3*(2), 34–39.

Capraro, V. (2018). Gender differences in lying in sender-receiver games: A meta-analysis. *Judgment and Decision Making*, *13*(4), 345–355.

Fischbacher, U., & Föllmi-Heusi, F. (2013). Lies in disguise: An experimental study on cheating. *Journal of the European Economic Association*, *11*(3), 525–547.

Gino, F., Ayal, S., & Ariely, D. (2013). Self-serving altruism? the lure of unethical actions that benefit others. *Journal of Economic Behavior & Organization*, *93*, 285–292.

Gneezy, U., Kajackaite, A., & Sobel, J. (2018). Lying aversion and the size of the lie. *American Economic Review*, *108*(2), 419–453.

Hermann, D., & Mußhoff, O. (2019). I might be a liar, but i am not a thief: An experimental distinction between the moral costs of lying and stealing. *Journal of Economic Behavior & Organization*, *163*, 135–139.

Kajackaite, A., & Gneezy, U. (2017). Incentives and cheating. *Games and Economic Behavior*, *102*, 433–444.

Klein, S. A., Thielmann, I., Hilbig, B. E., & Zettler, I. (2017). Between me and we: The importance of self-profit versus social justifiability for ethical decision making. *Judgment & Decision Making*, *12*(6).

Levine, E. E., & Schweitzer, M. E. (2014). Are liars ethical? on the tension between benevolence and honesty. *Journal of Experimental Social Psychology*, *53*, 107–117.

Lohse, T., Simon, S., & Konrad, K. (2018). Deception under time pressure: Conscious decision or a problem of awareness? *Journal of Economic Behavior & Organization*, *146*, 31–42.

López-Pérez, R., & Spiegelman, E. (2019). Do economists lie more? In A. Bucciol & N. Montinari (Eds.), *Dishonesty in behavioral economics* (Chap. 3.1, pp. 143–162). Perspectives in Behavioral Economics and the Economics of Behavior. Academic Press.

Lupoli, M. J., Jampol, L., & Oveis, C. (2017). Lying because we care: Compassion increases prosocial lying. *Journal of Experimental Psychology*, *146*(7), 1026.

Maggian, V. (2019). Negative externalities of cheating: An experiment with charities. In A. Bucciol & N. Montinari (Eds.), *Dishonesty in behavioral economics* (Chap. 3.3, pp. 182–191). Perspectives in Behavioral Economics and the Economics of Behavior. Academic Press.

Marx, A., Backes, C., Meese, E., Lenhof, H.-p., & Keller, A. (2016). Edison-wmw: Exact dynamic programing solution of the wilcoxon–mann–whitney test. *Genomics, Proteomics & Bioinformatics*, *14*(1), 55–61.

Mazar, N., Amir, O., & Ariely, D. (2008). The dishonesty of honest people: A theory of self-concept maintenance. *American Marketing Association*, *45*, 633–644.

Muñoz-Izquierdo, N., de Liaño, B. G.-G., Rin-Sánchez, F. D., & Pascual-Ezama, D. (2019). Cheating and altruism by discipline. In A. Bucciol & N. Montinari (Eds.), *Dishonesty in behavioral economics* (Chap. 3.2, pp. 163–181). Perspectives in Behavioral Economics and the Economics of Behavior. Academic Press.

Rand, D., Peysakhovich, A., Kraft-todd, G., Newman, G., Wurzbacher, O., Nowak, M., & Greene, J. (2014). Social heuristics shape intuitive cooperation. *Nature Communications*, *5*, 3677.

Wiltermuth, S. (2011). Cheating more when the spoils are split. *Organizational Behavior and Human Decision Processes*, *115*(2), 157–168.

Zhao, L., Heyman, G. D., Chen, L., Sun, W., Zhang, R., & Lee, K. (2017). Cheating in the name of others: Offering prosocial justifications promotes unethical behavior in young children. *Journal of experimental child psychology*, *177*, 187–196.

Supplementary Materials for

Lying Behavior When the Payoffs are Shared with Charity: Experimental Evidence

Scott Lee Chua, Jessica Chang, and Guillem Riambau

A.1 Instructions given before the experiment

Authors' note: Text in black is for all participants. Text in blue changes is only for participants of certain treatment group/s. Text underlined indicates facility-specific details.

Welcome to the experiment!

You are about to participate in an economic experiment, in which you will play a game and answer a few follow-up questions. You have drawn a random identification number, and we will not be able to link your personal or contact information to your identification number. All your actions in this experiment are private, so only you will know them.

During the experiment, you will receive instructions telling you what to do. If something is unclear, please feel free to raise your hand and we will answer any questions individually.

There may be moments where you will have to sit and wait, and we ask you to be patient. Please refrain from using your phone for the duration of the experiment. You may not talk to each other during the experiment and we ask you not to discuss the experiment with others afterwards.

[***For Groups B through E only***] Please take a moment to read the menu of charities and pick one you want to support.

In a moment, you will be led to a separate room. In that room, there will be a pouch with a die inside, and a box containing pen and paper.

The timer on the container should be set to 1 minute. The experimenter will start the timer and leave the room. During the 1 minute before the container opens, you may practice rolling the die as many times, in any manner you wish.

[***For Groups A through D***] In addition to the $5 in your envelope, you have the chance to earn additional cash depending on the outcome of your roll.

OR

[***For Group E***] You have the chance to earn cash for the charity you chose, depending on the outcome of your roll.

[***For Groups B through D only***] 10% / 50% / 90% of your winnings will immediately be donated to your chosen charity, and you get to keep the remaining 90% / 50% / 10% in cash.

Please refer to the payoff sheet in front of you.

After you have recorded the outcome of your roll, please replace the pen in the container and the die in the pouch. Then go to room name to hand the paper to the experimenter. (These instructions are also duplicated in the room, for your convenience.)

[***For Group A***] The experimenter will hand you the payoff corresponding to your reported outcome, and then ask you to fill out a short questionnaire online.

OR

[***For Groups B through D***] The experimenter will make the corresponding donation in front of you, and hand you the remainder of your winnings in cash. They will then ask you to fill out a short questionnaire online.

OR

[***For Group E***] The experimenter will make the corresponding donation in front of you. They will then ask you to fill out a short questionnaire online.

After the questionnaire is submitted, the experimenter will give you a full debrief, and hand you your envelope, containing your $5. This will conclude the experiment.

A.2 Abbreviated instructions

Authors' note: This abbreviated version of the instructions was made available to participants inside the private dice-roll room. Text underlined indicates facility-specific details.

Welcome!

Duplicated below are the instructions:

- During this one minute before the container opens, please practice rolling the die in any manner you wish.

- As soon as the container opens, please roll the die exactly once, and write down the outcome.

- Replace the pen in the container, and the die back in the pouch.

- Go to room location to hand the paper to the experimenter.

A.3 Payoff chart

Authors' note: Participants received this payoff chart during the pre-experiment briefing, and surrendered it before commencing the experiment. Text in black is for all participants. Text in blue changes is only for participants of certain treatment group/s.

The possible payoffs are listed below.

[*For Group A*] You will receive the payoff amount, in addition to the $5 in your envelope.
OR
[*For Groups B through D*] You will receive 90% / 50% / 10% of the payoff amount, in addition to the $5 in your envelope. The other 10% / 50% / 90% will be donated to your chosen charity immediately after the experiment.
OR
[*For Group E*] Your chosen charity will receive the payoff amount. You will still receive the $5 in your envelope.

Dice	Payoff
1	$1
2	$2
3	$3
4	$4
5	$5
6	$6

A.4 Post-task questionnaire

Authors' note: This questionnaire was administered online, split over four "screens" that participants clicked through. Participants were free to skip any question except for "Experiment ID number."

Screen 1.
1. The winnings from your game should have been paid out before you answer this questionnaire. If they have not, please inform the experimenter.
2. Please do not divulge your personal data (name, IC, student ID number) anywhere in this questionnaire. Only identify yourself by your Experiment ID number.
3. Please tell the experimenter once you have completed this survey to receive your $5 reimbursement and debrief.
Thank you for your participation!

Experiment ID number: _____________

Screen 2.
What were you thinking about during the one-minute wait? _____________________

Screen 3.
Were you honest in your report? [Yes OR No]

Screen 4.
Gender: _____________
Age: _____________
Religion, (if any): _____________
Major: _____________

A.5 Charities

Authors’ note: Participants in split-payoff and charity-only groups were asked to select one charity to donate to, from the five charities listed below.

1. Make-A-Wish Foundation Singapore
Beneficiary: Terminally ill children
Website: https://www.makeawish.org.sg/

2. Yellow Ribbon Project
Beneficiary: Ex-offenders
Website: https://www.yellowribbon.org.sg/

3. Aidha
Beneficiary: Low-income women and foreign domestic workers
Website: https://www.aidha.org/

4. Oasis Second Chance Animal Shelter
Beneficiary: Stray animals
Website: https://www.oscas.sg/

5. Red Cross Singapore
Beneficiary: Vulnerable populations and populations in crisis
Website: https://www.redcross.sg/

B.1 Chi-squared goodness-of-fit tests

Group	Chi-squared goodness-of-fit test
Self-only	0.0001***
90-self	0.0015**
50-self	0.0054**
10-self	0.0008***
Charity-only	0.2163
** $p < 0.001$, * $p < 0.01$.	

Table 1. Chi-squared goodness-of-fit test of observed outcome frequencies against expected frequencies.

B.2 Participant demographics by treatment group

Authors' note: These are the results of the demographic questions from the questionnaire outlined in Appendix A.4, broken down by treatment group. For completeness we also report below nonresponses per question as a percentage of each group.

Group	Male (%)	Spiritual (%)	Studies economics (%)	Within-group Median age
Self-only	36.11	41.67	16.67	22
90-self	41.58	45.54	14.85	22
50-self	37.27	44.55	10.90	21
10-self	34.95	41.75	16.50	21
Charity-only	39.22	47.06	14.71	21

Table 2. Participant demographics broken down by treatment group.

Group	Gender Nonresponses (%)	Spirituality Nonresponses (%)	Major Nonresponses (%)	Age Nonresponses (%)
Self-only	4.63	25.00	15.7	4.63
90-self	4.95	21.78	17.82	4.95
50-self	9.09	22.73	22.72	9.09
10-self	4.85	25.24	17.48	3.88
Charity-only	3.92	26.47	19.61	3.92

Table 3. Nonresponses to demographic questions broken down by treatment group.

B.3. Estimated versus self-admitted shares of lying, including non-respondents

	Share of participants (%)		
Group	**Estimated lying**	**Admitted lying (incl. non-respondents)**	**Admitted/Estimated (%)**
Self-only	25.00	2.78	11.12
90-self	21.28	6.93	32.56
50-self	25.00	10.00	40.00
10-self	28.63	10.68	37.30
Charity-only	10.30	3.92	38.05

Table 4. Discrepancies between estimated and self-admitted shares of lying. Estimated lying is computed as in Table 5 in the main manuscript. That is, if, as expected, 2/3 of respondents report {1,2,3,4}, this value is 0, and, if none report{1,2,3,4}, this value is 1.

B.4 Histograms of reported outcomes, per treatment group